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The IRP Summer Research Workshop celebrated its 20th anniversary in 2010. This meeting has become a time-honored tradition in the poverty studies community. The four-day workshop held in Madison each June brings together as many as 90 junior and senior scholars from a variety of disciplines, primarily Economics and Sociology, to discuss research related to the low-income population. Designed to build a community of research interest around topics related to the poor and their labor market connections, the workshop focuses on frontier statistical methods for the empirical study of these issues and seeks to mentor the next generation of poverty researchers. This issue of Focus *is devoted to the keynote papers delivered at the June 2010 Workshop.*

Progress toward improving the U.S. poverty measure: Developing the new Supplemental Poverty Measure

David Johnson

David Johnson is the Chief of the U.S. Census Bureau's Housing and Household Economic Statistics Division.

> "We define poverty as economic deprivation. A way of expressing this concept is that it pertains to people's lack of economic resources (e.g., money or near money income) for consumption of economic goods and services (e.g., food, housing, clothing, transportation). Thus, a poverty standard is based on a level of family resources (or, alternatively, of families' actual consumption) deemed necessary to obtain a minimally adequate standard of living, defined appropriately for the United States today."
>
> National Academy of Science Panel on Poverty and Family Assistance, 1995[1]

The official U.S. poverty measure has been calculated nearly the same way since it was first developed in the early 1960s. Criticisms of this measure as a current indicator of poverty in the United States have been well-documented: it is based only on family cash income, and uses an absolute poverty threshold that has not kept pace with changes in the standard of living and which does not differ by geographic area.

The president's fiscal year 2011 budget provides funding to develop a new Supplemental Poverty Measure. With this funding, the U.S. Census Bureau, in cooperation with the Bureau of Labor Statistics (BLS), is developing this new measure to provide further insight into economic conditions and trends. This measure will not replace the official poverty measure, and will not be used for resource allocation or program-eligibility determination.

An Interagency Technical Working Group has provided guidance to the Census Bureau on how to develop the Sup-

The Institute for Research on Poverty has long committed itself to improved poverty measurement (see http://www.irp.wisc.edu/faqs/faq2.htm#reading). This issue of Focus *summarizes recent news on this issue, which was presented in the opening session of the 20th annual IRP Summer Research Workshop. In the first talk, David Johnson, chief of the Census Bureau's Housing and Household Economic Statistics Division, spoke about the new federal Supplemental Poverty Measure (SPM). IRP researchers presented the second talk, which described new research on poverty measurement at the state level and presented the first results from the new Wisconsin Poverty Model (see* http://www.irp.wisc.edu/research/wipoverty.htm#wipovreports*). The Wisconsin Model is part of a new genre of state and local measures using National Academy of Sciences and SPM-related guidelines. More on the creation of state and local measures can be found at* http://www.irp.wisc.edu/research/povmeas/regional.htm.

—Timothy M. Smeeding, IRP Director

plemental Poverty Measure, drawing on the recommendations of a 1995 National Academy of Science report, as well as extensive research on poverty measurement that has been done over the past 15 years.[2] The Census Bureau and BLS will produce an initial measure using recommendations from the Working Group, and will improve the measure over time as new data, new methods, and additional research become available.

Poverty threshold

The threshold for the official poverty measure is based on the cost of a minimum food diet for a two-adult and two-child family in 1963, multiplied by three in order to cover all other expenses. In contrast, the supplemental measure uses data from the BLS Consumer Expenditure Survey to calculate expenditures over the most recent five years on food, clothing, and shelter including utilities and all mortgage expenses (FCSU). The supplemental measure threshold is the 33rd percentile of the distribution of all consumer units with exactly two children plus 20 percent to cover all other expenses.[3]

Family type

The official measure has separately developed thresholds by family type, and uses lower thresholds for elderly singles and couples. In the supplemental measure, a reference family threshold is adjusted using a three parameter equivalence scale, which assumes that children need less than adults, and also incorporates economies of scale for larger families.

Economic unit of analysis

The unit of analysis for the supplemental measure is more detailed than that used for the official measure, which is based on families, with unrelated individuals residing at the same address being considered separately. For the supplemental measure, the economic unit of analysis is all related individuals who live at the same address; co-resident unrelated children who are cared for by the family (such as foster children); and any cohabitors and their resident children.

Shelter type

The official measure includes no adjustment for shelter type. In contrast, the supplemental measure applies adjustment factors to the shelter component of the FCSU threshold to reflect relative expenditures of housing groups. There are three separate thresholds: for renters; homeowners with a mortgage; and homeowners without a mortgage.

Geographic area

Again, the official measure includes no adjustment for geographic area. The supplemental measure does, adjusting for housing cost differences using five years of American Community Survey data on rental costs. The supplemental measure also includes adjustments for each Metropolitan Statistical Area (MSA) and non-MSA in each state. Finally, the Census Bureau in cooperation with BLS will continue to research interarea price indices in order to refine the geographic adjustment techniques.

Family resource definition

The official measure is based on gross (before-tax) cash income from all sources using data from the Annual Social and Economic Supplement of the Current Population Survey (CPS). The supplemental measure also begins with gross cash income from the CPS, but adds the value of near-money federal in-kind benefits for FCSU such as Supplemental

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Nutrition Assistance Program assistance (formerly food stamps) and housing subsidies, as well as tax credits such as the Earned Income Tax Credit. The supplemental measure also subtracts income and payroll taxes and other nondiscretionary expenses such as child care, work-related expenses, child support payments, and out-of-pocket medical care expenses including health insurance premiums.

Method for updating poverty threshold

The official poverty threshold uses the 1963 level, updated annually for price changes with the Consumer Price Index for All Urban Consumers. As stated above, the supplemental poverty threshold will be recalculated annually using data from the BLS Consumer Expenditure Survey from the most recent five years. Factors used to adjust thresholds by housing status and for interarea price variation will also be recalculated regularly.

Next steps

An Interagency Steering Committee and a Census and BLS development and implementation team are being created. The Census Bureau and BLS will continue to work with the research community and the general public to obtain comments on this new measure. As part of this conversation, a Federal Register Notice was posted to solicit comments, and the Census Bureau will post these comments and responses on their new Supplemental Poverty Measure Web site later this year.[4] In addition, the Bureau has participated in academic conferences such as the Association for Public Policy Analysis and Management Annual Research Conference and the American Economic Association meetings. Finally, the Bureau will post a variety of methodological documents describing the details of this new supplemental measure. These documents will include papers on the thresholds, poverty rates, geographic indices, taxes, housing subsidies, child care, work expenses, the unit of analysis, and the use of new survey questions on child support, mortgage status, and medical out-of-pocket expenditures.

If the president's fiscal year 2011 budget is approved, the first Supplemental Poverty Measure will be released on the same day as the official poverty measure in September 2011. As mentioned above, official poverty estimates will continue to be used for allocations of federal funds and as guidelines for program eligibility, and the supplemental measure will provide a new gauge of the economic well-being of American families and the effectiveness of federal antipoverty programs.■

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[1]C. Citro and R. Michael, eds., *Measuring Poverty: A New Approach* (Washington, DC: National Academy Press, 1995).

[2]More information may be found at the U.S. Census Bureau Poverty Web site www.census.gov/hhes/www/poverty/poverty.html.

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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. Full texts of Discussion Papers and Special Reports are available on the IRP Web site.

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Edited by Emma Caspar

[3]The expenditures for other family sizes (for example, those with one adult and two children) are adjusted (using equivalence scales) to be similar to those of a two-adult and two-child family unit.

[4]See http://www.census.gov/hhes/www/povmeas/povmeas.html.

Measuring poverty in Wisconsin

Joanna Marks, Julia Isaacs, Katherine Thornton, and Timothy Smeeding

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In collaboration with the State of Wisconsin and federal and nongovernmental entities, researchers at IRP have undertaken a new initiative related to the measurement of poverty. The Wisconsin Poverty Project involves innovative use of data to gain a broader, more complete view of both needs and resources on the state and local levels. IRP has prepared two annual reports on poverty in Wisconsin using the American Community Survey (ACS), administrative data, and new methods of poverty measurement. The first *Wisconsin Poverty Report*, released in May 2009, relied on the innovative use of data from the Supplemental Nutrition Assistance Program (formerly known as food stamps) to forecast growing need in the state. In September 2010, IRP researchers released the second *Wisconsin Poverty Report*, which uses a more complete accounting of both resources and need than traditional measures in order to determine the state poverty rate.

The new Wisconsin Poverty Measure was devised to improve understanding of the level of need in a given region and of the efficacy of public programs aimed at reducing economic hardship. The official federal poverty measure considers pretax cash income, whereas the new Wisconsin Poverty Measure counts other resources as well, such as food assistance and tax credits. The measure includes state-specific policies such as the Wisconsin State Earned Income Tax Credit (EITC) and the Wisconsin Homestead Credit, in addition to estimating payments for federal taxes, Social Security and Medicare payroll taxes, and the federal EITC. The research team also considered work-related expenses such as transportation and child care and out-of-pocket medical expenses, which reduce income that could be spent on food, housing, and other basic needs. The new Wisconsin Poverty Measure also looks at geographic differences in cost of living both within the state and relative to the nation as a whole.

The Wisconsin Poverty Measure allows researchers to assess the effects of new and proposed policies. A recent issue of IRP's *Fast Focus* research brief illustrates the impact of the American Recovery and Reinvestment Act on poverty rates in Wisconsin (see http://www.irp.wisc.edu/publications/fastfocus/pdfs/FF7-2010-rev.pdf). In the coming year, IRP researchers will use the measure to analyze new data on poverty for 2009, provide guidance to neighboring states and others on using the ACS for alternative poverty measurement, and assess the impact of the proposed federal Supplemental Poverty Measure.

Work on the Wisconsin-specific measure is one of several research efforts of university, government, and private agency researchers across the United States to develop alternative poverty estimates at the local, state, and national levels. The U.S. Census Bureau and the Bureau of Labor Statistics are leading federal efforts to develop a Supplemental Poverty Measure (SPM) based on recommendations from experts on the 1995 National Academy of Sciences' Panel on Poverty and Family Assistance, as well as on subsequent research on alternative measures of poverty. To support this work, IRP is developing on its Web site an SPM technical resource to disseminate ongoing research to all researchers interested in this topic at http://www.irp.wisc.edu/research/povmeas/spm.htm.

How well do we understand achievement gaps?

Eric A. Hanushek

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An underlying principle of U.S. social policy is that education is the key policy lever for addressing poverty. In the United States and around the world, education is almost always heavily subsidized by government. The justifications for government involvement vary, but increasingly rely on the suggestion that expanded educational investments both strengthen the national economy and improve the societal distribution of income and welfare. Education, for example, had a prominent role in the U.S. "War on Poverty," with many of the programs developed in the 1960s continuing through today. The expansion of public colleges and universities over the past three decades has also rested on distributional arguments.

This article assesses what we currently know about the role of education in improving the welfare of the disadvantaged population by looking at one particular aspect of the subject, achievement gaps for disadvantaged students. Specifically, I review literature related to measured cognitive skills, focusing on achievement rather than school attainment. For the most part, I interpret cognitive skills as measured by student achievement tests as a direct measure of human capital. In the end, although this is a smaller and thinner body of work than the broader topic related to education in general, I think it is fundamental to much of the rest of the literature.

An overview of achievement issues

Focusing on achievement rather than school attainment has several advantages in discussing the interaction of research and policy. First, most current policy discussions relate directly to issues of quality and student learning. For example, policy discussions of accountability and the No Child Left Behind Act of 2001 (NCLB), or of preschool and the preparation of disadvantaged students for school, are concerned with what students know at any point in time. Second, a focus on achievement allows for the fact that much of education actually takes place outside of schools. Finally, a focus on achievement allows for the possibility that other policy-relevant factors, such as health and neighborhoods, are important for education.

There are also disadvantages to focusing on achievement. With many different tests, the reliability and validity of specific measures is often unknown. Additionally, achievement may not reflect an individual's full range of education. This disadvantage may be greater at higher levels of education;

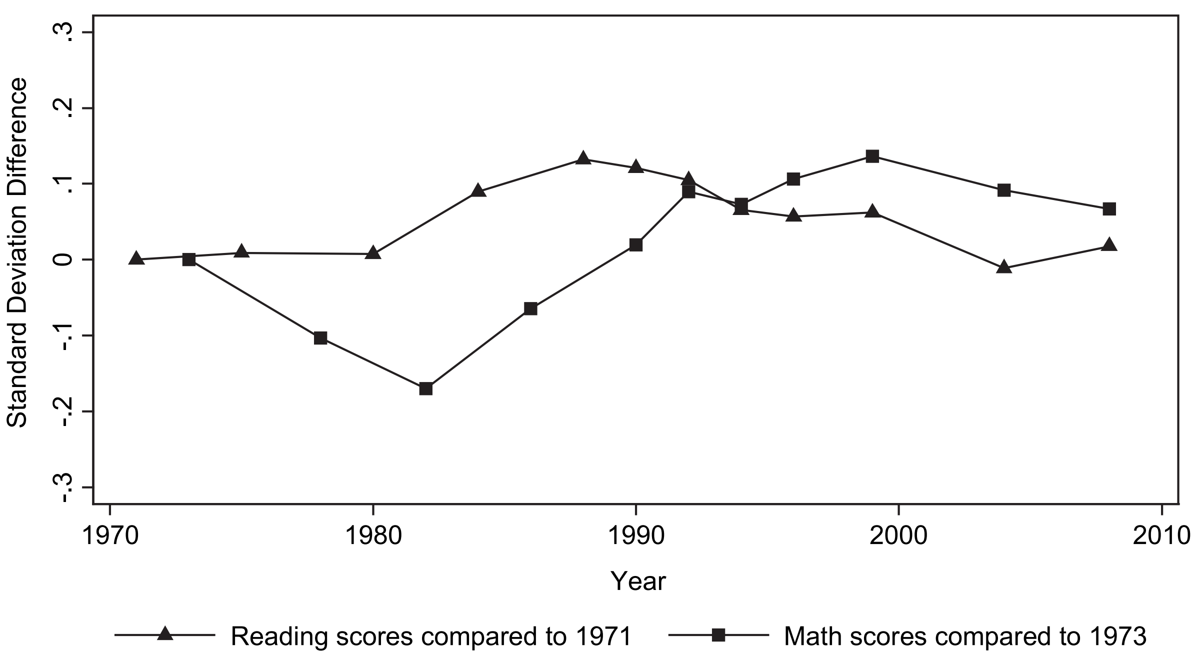

Figure 1. National Assessment of Educational Progress (NAEP) Scores, 17-year-olds 1971–2008.

Note: Figure shows standard deviation differences relative to initial performance.

Source: Author calculations based on data from http://nces.ed.gov/nationsreportcard/.

for example, few believe that various college tests accurately reflect what anybody has learned in college.

Until recently, school attainment has been the focus of most empirical education work, particularly as it relates to the labor market. This choice is one of convenience, because census data and other surveys have tended to measure attainment but not achievement. From the extensive evidence of inputs into educational production functions, it is apparent that school attainment is not a complete indicator of human capital and that in most situations it would need to be augmented by other determinants of achievement.[1]

Returns to achievement

Three U.S. studies provide very consistent estimates of the impact of test performance on earnings of young workers.[2] These studies use different nationally representative datasets that follow students after they leave school and enter the labor force. When scores are standardized, they suggest that one standard deviation increase in mathematics performance at the end of high school translates into 10 percent to 15 percent higher annual earnings. Hanushek and Zhang find even larger returns to achievement (20 percent) for a more age-representative sample.[3] These consistent findings demonstrate the need to pay attention to achievement gaps.

Achievement over time

The backdrop for this analysis—which emphasizes distributional, or equity, concerns—clearly must include what has been happening in terms of overall school performance. Figure 1 provides the overall performance of U.S. 17-year-olds on the National Assessment of Educational Progress (NAEP). The NAEP provides consistent national testing of a random sample of students in different subjects, so it is possible to observe any changes in performance over time.[4] The remarkable thing about this picture is that performance appears roughly flat for almost four decades. This constancy is particularly remarkable given the amount of resources expended over that time period in an attempt to improve performance. Of the myriad changes, probably the most obvious policy response has been continued increases in the funding and resources of schools. The commonly discussed policy instruments—reduced pupil-teacher ratios, retaining more teachers, and having more-educated teachers—have been systematically employed over the past decades. Between 1960 and 2007, U.S. pupil-teacher ratios fell by 40 percent; teachers with a master's degree more than doubled to over 50 percent; and average experience increased (see Table 1). Bringing about these changes is, of course, expensive; real spending per pupil more than tripled over the period.

The simple picture is that school policy has not been directed primarily at overall student performance (at least as seen by outcomes). Thus, it is also useful to see what happened in terms of the distribution of outcomes. This distributional discussion concentrates largely on racial differences in performance patterns because data by family income and other measures of socioeconomic status are generally not available over time. And when such data are available, they tend to be unreliable, utilizing such measures as eligibility for free and reduced-price lunch, which is known to be incompletely reported at the high school level.

Table 1
Public School Resources in the United States, 1960–2007

	1960	1980	2000	2007
Pupil-teacher ratio	25.8	18.7	16.0	15.5
Percentage of teachers with master's degree or more	23.5 %	49.6 %	56.8 %	not available
Median years of teacher experience	11	12	14	not available
Real expenditure per student	$3,170	$6,244	$10,041	$11,674

Source: U.S. Department of Education Digest of Education Statistics, 2009 (Washington, DC: National Center for Education Statistics, 2010).

Notes: Expenditures are in 2007–2008 dollars.

Early attention to distributional issues was provided by the massive government report, *Equality of Educational Opportunity*, commonly referred to as the "Coleman Report."[5] This report was mandated by the Civil Rights Act of 1964, which instructed the U.S. Office of Education to report on the lack of educational opportunity by reason of race or ethnicity. To address this issue, the Coleman research team tested some 600,000 students in the United States in 1965.

The analysis vividly underscored the huge difference in the achievement of students by race and background. A simple summary of the magnitude of differences comes from equating test scores to grade-level equivalents. If white twelfth graders in the urban Northeast (in 1965) were the standard for the knowledge that a twelfth grader should have, black students also in the urban Northeast were achieving at the ninth grade level, and black students in the rural south were achieving at the seventh grade level. The magnitude of these differences never received much attention, as most of the attention went to the analysis of the determinants of achievement.

The achievement differences have been consistent across studies. For example, when disaggregated by race, the SAT tests showed differences of approximately one standard deviation. The SAT relied on voluntary test taking for a changing group of students, however, and thus the interpretation is somewhat ambiguous. Once again, the clearest picture comes from the National Assessment of Educational Progress.

Figure 2 displays the average performance gap between whites and blacks in math and reading at age 17. Across each of the tests there is a very consistent pattern: racial gaps tended to shrink noticeably during the 1980s and then to be flat or widen somewhat during the 1990s. If anything, the white-black gap expanded in the 1990s.[6]

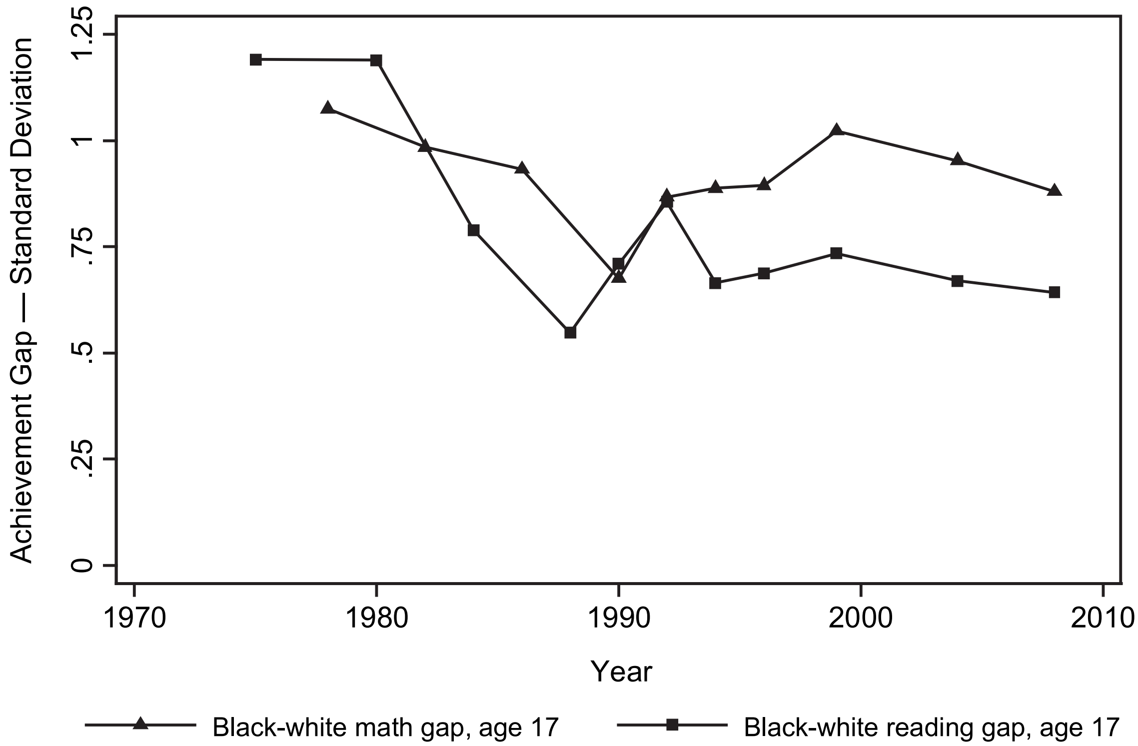

Figure 2. Black-White Achievement Gap, 1975–2008.

Source: Author calculations based on data from http://nces.ed.gov/nationsreportcard/.

The magnitude of the gap is stunning. The average difference in math and reading in 2008 is 0.77 standard deviations—implying that the average black 17-year-old is achieving at the 22nd percentile of the overall distribution. Other things equal, existing earnings studies indicate that the average skill differences alone imply roughly a 10 percent annual earnings difference.

Much has been made of the narrowing of the black-white achievement gap, including a widely cited conference book.[7] The one-time nature of the test score convergence, however, was not generally anticipated and has received less attention than the significant closing of the gaps that occurred over a decade ago.

One other point of comparison is relevant. Figure 3 provides the Hispanic-white achievement gaps over time. These gaps have been flatter than the black-white gaps, and are also smaller, averaging 0.64 standard deviations across subjects in 2008. Even though there are more Hispanics than blacks in public schools (21 percent compared to 17 percent in 2007), consideration of Hispanic performance still remains limited.

Achievement outcomes

In the following section, I review studies that look at three factors that may affect the outcomes of disadvantaged students: teacher effectiveness, racial concentration in schools, and preschool education.

Distributional aspects of schools and teachers

Estimates of variations in teacher quality suggest that having a good teacher for three to five years would eliminate the average gap between children who do and do not receive free or reduced-price lunch, and between whites and blacks or Hispanics.[8] In practice, this potential is unlikely to be realized, as few students actually get such a long exposure to good teachers.

The question remains whether or not teachers are distributed adversely for low-income and minority students. On this, the evidence is less clear. A considerable amount has been written about differences in spending across schools within large districts.[9] These differences, however, largely reflect teacher salaries, which in turn reflect experience and graduate degrees—things mainly uncorrelated with effectiveness. Concern about uneven distribution of teachers within districts also motivated parts of NCLB that called for "highly qualified" teachers in schools serving disadvantaged students, again with little evidence that the standards were related to teacher effectiveness.

Existing evidence shows that teachers who switch schools tend to move to schools with higher achieving, higher income, and fewer minority students than their previous schools, and those changing districts tend to get slightly higher wages on average once the wages are adjusted for changes in student demographic composition.[10] On average in 2005, 13 percent of teachers left schools with 5 percent

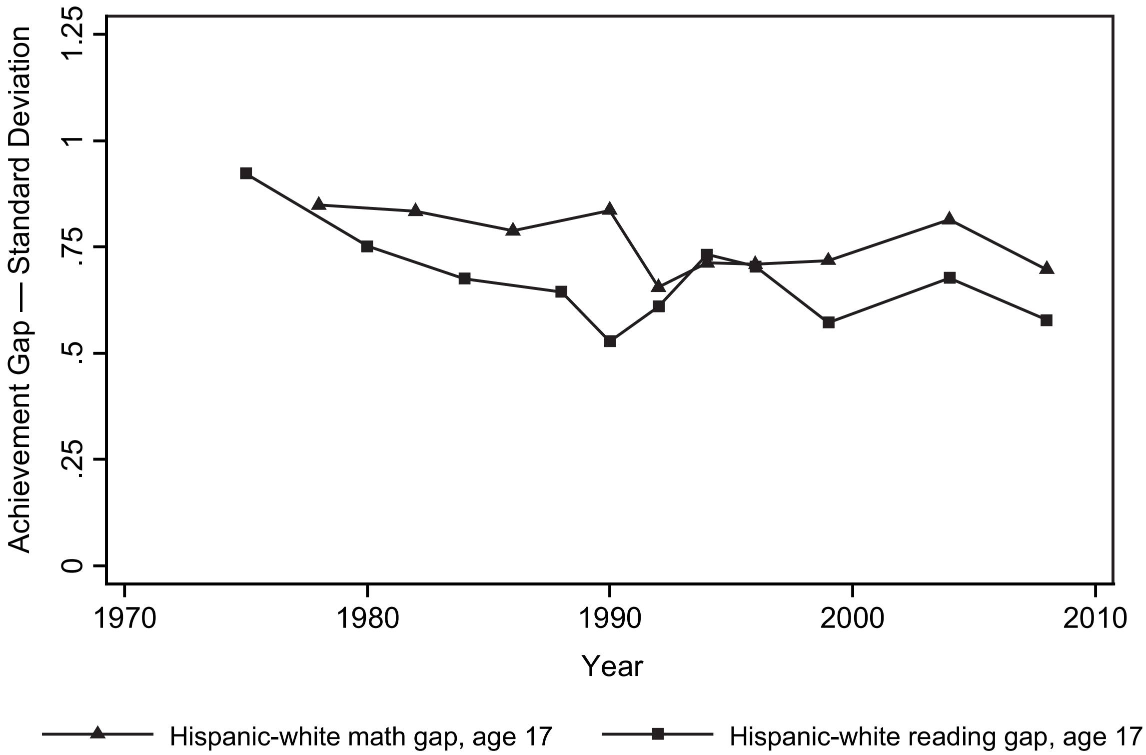

Figure 3. Hispanic-White Achievement Gap, 1975–2008.

Source: Author calculations based on data from http://nces.ed.gov/nationsreportcard/.

or less minority students, while 20 percent of teachers left schools with 50 percent or more minority students.[11] Since demographic composition is likely to be related to working conditions, these findings suggest that salary is outweighed by other considerations in job decisions of teachers. As more-experienced teachers move away, they are replaced by rookie teachers, implying that schools serving disadvantaged students will tend to have a greater proportion of new teachers. Preliminary analysis of principals finds that they follow similar mobility patterns, suggesting that administrator skills may also vary with the student population. Boyd, Lankford, Loeb, and Wyckoff also find that teacher labor markets tend to be highly localized, which further disadvantages high-poverty, lower-achieving schools located in urban centers and rural areas that tend to produce few college graduates.[12] However, even with the differences in teacher mobility, the first-year-teaching effect cannot account for much of the observed achievement gap.

Segregation and school outcomes

Poverty, race, and schooling are very highly correlated with location. A variety of people have traced different dimensions of residential locations and segregation by race and income. Cutler, Glaeser, and Vigdor describe black migration to urban areas from 1890 to 1940, which led to racial ghettos.[13] As the migration continued between 1940 and 1970, ghettos expanded and racial segregation increased continuously. Since 1970, there has been a modest decline in segregation as blacks moved to suburban areas and central cities became less segregated. Despite these large changes in segregation over time, segregation across cities remains very persistent and is strongly related to city size. Iceland and Weinberg examine residential segregation in metropolitan areas for the four major racial and ethnic minority groups in the United States—American Indians and Alaska Natives, Asians and Pacific Icelanders, blacks, and Hispanics or Latinos.[14] They conclude that blacks are the most residentially segregated of the four groups examined, but that their segregation declined between 1980 and 2000. Hispanics are the second-most-segregated group, and their overall concentrations by neighborhood have not changed over the period. Swanstrom, Casey, Flack, and Dreier analyzed economic segregation among municipalities for 50 major metropolitan areas.[15] They conclude that economic segregation among municipalities is rising, but the trends vary significantly across time and in different regions of the country.

Fischer, Stockmayer, Stiles, and Hout study trends in residential segregation in the United States from 1960 to 2000 along several social dimensions, including race, income, and family status, and across several geographic levels: region, metropolis, the center city-suburb division, municipality, and tract.[16] They report that the segregation of blacks decreased considerably after 1960, largely because neighborhoods became more integrated. While the central city-suburb barrier lessened for blacks, suburbs themselves became more segregated. The segregation of Hispanics, however, changed little. Economic segregation increased between 1970 and 1990 mainly because the affluent were clustered more in both specific metropolitan areas and in specific municipalities within metropolitan areas. An important element, however, is that economic segregation is significantly less than racial segregation.

Perhaps the most significant social policy of the 20th century was the desegregation of schools following the 1954 *Brown v. Board of Education* decision. From the late 1960s through 1980, black exposure to whites rose dramatically.[17] After the late 1980s, there was some decline in white exposure, but it remained improved over the 1960s.[18]

Currently, the fundamental force behind school segregation is the residential location of blacks and whites across jurisdictions. In particular, completely balanced schools within districts would yield very small differences in segregation beyond that across districts.[19] Moreover, minority enrollment is very much an issue in large urban areas. Over 30 percent of blacks and one-quarter of Hispanics attend schools in one of the top 50 districts.[20]

Surprisingly, relatively little attention has been given to identifying the impacts of racial and ethnic segregation on achievement, and the available analyses provide a mixed picture. The Coleman Report provided early empirical evidence that racial isolation harms academic achievement, although Armor raises questions about the findings.[21] Subsequent work also finds that school racial composition affects academic, social, and economic outcomes.[22] On the other side, Rivkin finds no evidence that exposure to whites increases academic attainment or earnings for black men or women in the high school class of 1982; Card and Rothstein find that neighborhood but not school racial composition affects achievement; and Cook and Evans indicate that little of the black-white difference in NAEP scores can be attributed to racial concentration.[23] While varying in details, a recurring concern throughout these studies is the lack of a convincing identification strategy for uncovering the causal impact of racial concentration in the schools.

Hanushek, Kain, and Rivkin attempt to sort out the independent impact of racial composition on achievement in a framework similar to that described for teacher-effects studies.[24] They provide strong evidence that increases in the proportion of black students in a school adversely affects mathematics achievement of blacks. These effects are much larger and more precisely estimated for blacks than the corresponding estimated impacts on whites, which are generally not significantly different from zero. Moreover, Hispanic enrollment share exerts a far smaller effect, indicating that it is the proportion of black students rather than proportion of minority students that is the key aspect of peer race and ethnic composition in terms of achievement for blacks and whites.

The magnitudes of the black-composition effects are significant. On average, the black share of school enrollment in Texas is almost 30 percentage points higher for black students than for white students. Elimination of this gap implies, according to the direct estimates, that the racial achievement gap would fall by 0.05 standard deviations in a single year. Such a reduction for grades 5 to 7 (the sample grade span of the estimates) suggests that a three-year cumulative effect of racial composition equalization would reduce the race achievement gap by roughly 14 percent.[25] Moreover, Hanushek and Rivkin suggest that it is high-achieving black students who are most harmed by increased racial concentration in schools.[26]

Preschool education

A recent focus of policy discussions is preschool education. Various types of preschool education, such as universal or means-tested, are frequently mentioned as the next "obvious" fix for the current schooling problems, particularly for disadvantaged students who come to school far behind their middle-class peers in language and other skills.

There are three arguments for why broad provision of preschool education is a good idea. First, the problems of disadvantaged children at entry to school have received increased attention, particularly with the availability of new longitudinal data for early childhood.[27] The deficits in preparation of disadvantaged children are significant. For example, in evaluating the vocabulary of disadvantaged children, Hart and Risley found that they were exposed to dramatically less vocabulary. More-advantaged children at age 3 had vocabularies that were four times as large as disadvantaged 3-year-olds. Moreover, the quality of parent-child communication was vastly different. These differences in preparation have potentially lasting effects on student outcomes, where the previous charts indicate that schools have been unable, on average, to close these gaps.

Second, a variety of conceptual arguments for early investments in human capital—most notably by James Heckman and his colleagues—have received scholarly and policy attention. In a series of articles, Heckman has argued that early investments are critical, since "learning begets learning."[28] Investments made early in life enhance learning later in school and even into careers, making such investments attractive.

Third, key studies with strong research designs have supported the efficacy of preschool education. The most well-known is the Perry Preschool Program, but others, such as the Abecedarian Program and the Early Training Program, also provide important evidence.[29] A set of benefit-cost analyses of the Perry Preschool Program shows that this appears to have been an effective program that was worth the expenditure.[30]

Given this background, it is natural that discussions of preschool enter into the educational policy debate and into judicial proceedings and judgments. For example, courts in South Carolina and New Jersey have found preschool education to be an essential element of an adequate education.[31]

Despite the popularity of preschool programs, there are serious questions concerning the interpretation of the underlying evaluations and whether their results have general application. For example, the evaluations of the Perry Preschool, Abecedarian, and Early Training programs relied upon a random assignment methodology that followed subjects over extended periods of time, but the numbers of children taking part in the experiments were relatively small, with only around 50 children in each treatment group. Clearly, with samples of this size, one must be concerned about whether the evaluation results can be generalized to much larger programs, especially when, upon reanalysis, many of the originally reported findings have turned out to be fragile.[32]

Moreover, even the beneficial results are quite varied. First, virtually all of the positive programmatic results are for females, with male children having primarily zero or negative impacts.[33] Second, a substantial part of the beneficial impact falls outside of schools and the development of cognitive skills. In particular, a substantial part of the benefits found for females relates to reduced criminal behavior.[34] Third, the results differ across programs, so that it is impossible simply to refer to "preschool," but it is necessary to identify the precise kind of treatment.

Probably most important, these programs are not typical community or school-based programs found in most states. The Perry Preschool Program, estimated to cost over $15,000 per child per year (in 2000 dollars), involved intensive treatment by teachers with master's degrees in child development, student-teacher ratios of 6 to 1, and regular home visits.[35] The Abecedarian Program is full day, five days per week, 50 weeks per year, for five years beginning at birth and including medical care and intensive home visitation.[36] It is estimated to cost $76,000 per child (in 2002 dollars).

Many people also forget that we have, in fact, a large public preschool program, introduced with the War on Poverty programs in 1965. Over 900,000 3- and 4-year-olds from families in poverty are currently enrolled in Head Start programs around the country. The federal Head Start program is considerably different from the Perry and Abecedarian programs. In 2005, just 35 percent of its teachers had a bachelor's degree, and the programs varied considerably in length and intensity.[37] The cost of Head Start is usually reported as slightly over $7,000 per pupil per year (in 2003–04 dollars), derived by dividing total program costs by the number of participants. However, this mixes together a variety of different programs; if run on a full-time, full-year basis, the program costs would exceed $20,000 per pupil per year.[38]

Support for the educational efficacy of Head Start is limited. The early education program in Head Start was complicated by its emphasis on local community employment activities, and, after initial evaluations found little lasting impact on student achievement, was redefined as a health and nutrition program instead of an educational program. Subsequent evaluations have consistently found small achievement effects that generally disappear relatively quickly.[39]

In 2005, 70 percent of the 4-year-olds and 5-year-olds who were not in kindergarten were in center-based care arrangements that averaged 27 hours per week.[40] Indeed, for all children ages 0 to 5, blacks (36 percent) and Hispanics (29 percent) were more likely than whites (27 percent) to be in a center-based program. (The differences largely reflected differential participation in Head Start programs.) Thus, preschool programs have already reached large portions of the young population.

In sum, there are reasons to be favorably disposed to instituting expanded preschool programs for disadvantaged students, but there are also potentially huge costs and problems associated with doing it right. The idea has been to supplement what goes on in the home in order to provide stronger educational development. Such preschool investments recognize that it is easier to remediate earlier rather than later. At the same time, the educational outcomes of existing programs that have been evaluated, except perhaps the most intensive and expensive, have been small and short lived. The limited number of models that have been evaluated provides uncertain guidance about design of effective programs, particularly programs that reach male children.

One other aspect of the design is also important. Any proposals of governmental support for preschool must consider which groups should receive programmatic help, how the programs should be organized, and how they should be financed.[41] The existing evidence on preschools is limited largely to their impact on disadvantaged students. There is no evidence about positive impacts for middle- and upper-income students.[42]

Conclusions

The starting point for this article is that achievement gaps by race (and income) are large and substantively important. Moreover, except for the gap closing during the 1980s, there has been little systematic movement.

Some conclude that schools lack the power to effect significant changes in achievement gaps. But there is a difference between having the capacity to lessen existing gaps and having an institutional structure and set of policies that accomplish such an objective.

The existing research suggests that there are three places to look for improvements. First, without a doubt, the biggest influence of schools comes through teachers. Improving teacher effectiveness could dramatically improve the achievement of disadvantaged students. Second, at least for blacks, it appears that racial concentration in schools is a significant factor. Here, the policies that would be effective are quite unclear, given the importance of residential segregation and the force of legal restraints. Third, some sort of preschool education for disadvantaged students could potentially deal with the typical lesser preparation these students have at entry to school, yet, the exact policies and nature of any new preschool programs need to be developed.■

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[1]Simple Mincer earnings models have shown that school attainment has an independent effect on individual earnings even when achievement is controlled for. However, in an international analysis, Hanushek and Zhang (E. A. Hanushek and L. Zhang, "Quality-Consistent Estimates of International Schooling and Skill Gradients," *Journal of Human Capital* 3, No. 2 (Summer 2009): 107–143) indicate that the estimated "Mincer return" falls by 40 percent on average after measures of mother's education, health, and ability are added to the Mincer earnings equation. This impact on the estimated schooling gradients is far larger than those reported in D. Card, "The Causal Effect of Education on Earnings," in *Handbook of Labor Economics*, Eds. O. Ashenfelter and D. Card (Amsterdam: North-Holland, 1999): 1801–1863, where other inputs to human capital are not considered.

[2]See C. B. Mulligan, "Galton Versus the Human Capital Approach to Inheritance," *Journal of Political Economy* 107, No. 6, part 2 (1999): S184–S224; R. J. Murnane, J. B. Willett, Y. Duhaldeborde, and J. H. Tyler, "How important are the cognitive skills of teenagers in predicting subsequent earnings?" *Journal of Policy Analysis and Management* 19, No. 4 (Fall 2000): 547–568; and E. P. Lazear, "Teacher Incentives," *Swedish Economic Policy Review* 10, No. 3 (2003): 179–214.

[3]It is convenient to convert test scores into measures of the distribution of achievement across the population. E. A. Hanushek and L. Zhang, "Quality-Consistent Estimates of International Schooling and Skill Gradients," *Journal of Human Capital* 3, No. 2 (Summer 2009): 107–143, analyze data from the International Adult Learning Survey that covers a sample across all ages of adults. A separate review of the normalized impact of measured cognitive skills on earnings in S. Bowles, H. Gintis, and M. Osborne, "The Determinants of Earnings: A Behavioral Approach," *Journal of Economic Literature* 39, No. 4 (2001): 1137–1176, finds that the mean estimate is only 0.07, or slightly over half of the specific studies here.

[4]Testing is conducted at ages 9, 13, and 17, and there have been some larger changes at younger ages.

[5]J. S. Coleman, E. Q. Campbell, C. J. Hobson, J. McPartland, A. M. Mood, F. D. Weinfeld, and R. L. York, *Equality of Educational Opportunity*, Washington, DC: U.S. Government Printing Office, 1966.

[6]Scores at age 17 are obviously the product of schooling received over the prior ten years. A review of achievement gaps for 13-year-olds shows that the gains seen during the 1980s for the oldest students have their antecedents in the 1970s. Most recently, the achievement gap for 9-year-olds narrowed in reading and math. Some popular statements have attributed this narrowing to increased national accountability and particularly the introduction of the federal *No Child Left Behind Act of 2001*, but detailed analysis is unavailable.

[7]C. Jencks and M. Phillips, eds., *The Black-White Test Score Gap* (Washington, DC: Brookings, 1998).

[8]This calculation compares having an average teacher to having a teacher one standard deviation above the mean. It has uncertainties about the accumulation of gains, since it is based on one-year growth.

[9]For example, see M. Roza and P. T. Hill, "How Can Anyone Say What's Adequate if Nobody Knows How Money is Spent Now?" In *Courting Failure: How School Adequacy Lawsuits Pervert Judges' Good Intentions and Harm our Children*, ed. E. A. Hanushek (Stanford: Education Next Books, 2006), pp. 235–255.

[10]See H. Lankford, S. Loeb, and J. Wyckoff, "Teacher Sorting and the Plight of Urban Schools: A Descriptive Analysis," *Educational Evaluation and Policy Analysis* 24, No. 1 (Spring 2002): 37–62; E. A. Hanushek, J. F. Kain, and S. G. Rivkin, "Why Public Schools Lose Teachers," *Journal of Human Resources* 39, No. 2 (2004): 326–354; and B. Scafidi, D. L. Sjoquist, and T. R. Stinebrickner, "Race, Poverty, and Teacher Mobility," *Economics of Education Review* 26, No. 2 (2007): 145–159. These three studies find that working conditions as measured by student demographic composition tend to be a more important determinant of school choice than salaries. Average salary changes following a move, and adjusted for working conditions, are positive but small.

[11]For the high minority schools, those leaving teaching altogether and those changing schools were roughly the same percentages; see U.S. Department of Education, *Digest of Education Statistics*, 2008, Washington, DC: National Center for Education Statistics, 2009, Table 73.

[12]D. Boyd, H.Lankford, S. Loeb, and J.Wyckoff, "The Draw of Home: How Teachers' Preferences for Proximity Disadvantage Urban Schools," *Journal of Policy Analysis and Management* 24, No. 1 (Winter 2005): 113–132.

[13]D. M. Cutler, E. L. Glaeser, and J. L. Vigdor, "The Rise and Decline of the American Ghetto," *Journal of Political Economy* 107, No. 3 (1999): 455–506.

[14]J. Iceland and D. H. Weinberg, *Racial and Ethnic Residential Segregation in the United States: 1980–2000, Census 2000 Special Reports*, Washington, DC: U.S. Bureau of the Census, 2002.

[15]T. Swanstrom, C. Casey, R. Flack, and P. Dreier, "Pulling Apart: Economic Segregation among Suburbs and Central Cities in Major Metropolitan Areas, 1980–2000," In *Redefining Urban and Suburban America: Evidence from Census 2000*, eds. B. Katz, A. Berube, and R. Lang (Washington, DC: Brookings Institution, 2006): pp. 143–165.

[16]C. S. Fischer, G. Stockmayer, J. Stiles, and M. Hout, "Distinguishing the Geographic Levels and Social Dimensions of U.S. Metropolitan Segregation, 1960–2000, " *Demography* 41, No. 1 (2004): 37–59.

[17]S. G. Rivkin and F. Welch, "Has School Desegregation Improved Academic and Economic Outcomes for Blacks?" In *Handbook of the Economics of Education*, eds. E. A. Hanushek and F. Welch (Amsterdam: North-Holland, 2006), pp. 1019–1049; and C. T. Clotfelter, *After Brown: The Rise and Retreat of School Desegregation* (Princeton, NJ: Princeton University Press, 2004).

[18]Hanushek (E. A. Hanushek, "Black-White Achievement Differences and Governmental Interventions," *American Economic Review* 91, No. 2 (2001): 24–28) pointed out that the only obvious factor that could explain the pattern of NAEP achievement gaps between blacks and whites was the implementation of school desegregation in the aftermath of *Brown*.

[19]Rivkin and Welch, "Has School Desegregation Improved Academic and Economic Outcomes for Blacks?"

[20]U.S. Department of Education, *Digest of Education Statistics*, 2007, Washington, DC: National Center for Education Statistics, 2008.

[21]D. J. Armor, "The Evidence on Busing," *Public Interest* 28 (Summer 1972): 90–126.

[22]See, for example, E. A. Hanushek, and M. E. Raymond, "Does School Accountability Lead to Improved Student Performance?" *Journal of Policy Analysis and Management* 24, No. 2 (Spring 2005): 297–327.

[23]S. G. Rivkin, "School Desegregation, Academic Attainment, and Earnings," *Journal of Human Resources* 35, No. 2 (Spring 2000): 333–346; D. Card and J. Rothstein, "Racial Segregation and the Black-White Test Score Gap," *Journal of Public Economics* 91, No. 11–12 (2007): 2158–2184; M. D. Cook and W. N. Evans, "Families or Schools? Explaining the Convergence in White and Black Academic Performance," *Journal of Labor Economics* 18, No. 4 (2000): 729–754.

[24]E. A. Hanushek, J. F. Kain, and S. G. Rivkin, "New Evidence About *Brown v. Board of Education*: The Complex Effects of School Racial Composition on Achievement," *Journal of Labor Economics* 27, No. 3 (2009): 349–383.

[25]If the racial composition factors were similar for earlier grades, this change in racial composition would mean closing the seventh grade achievement gap by 21 percent.

[26]E. A. Hanushek and S. G. Rivkin, "Harming the Best: How Schools Affect the Black-White Achievement Gap," *Journal of Policy Analysis and Management* 28, No. 3 (Summer 2009): 366–393.

[27]See the description of the three separate panels created under the Early Childhood Longitudinal Program (ELCS) at http://nces.ed.gov/ecls.

[28]See, for example, F. Cunha, J. J. Heckman, L. Lochner, and D. V. Masterov, "Interpreting the Evidence on Life Cycle Skill Formation." In *Handbook of the Economics of Education*.

[29]A comprehensive description and evaluation of different preschool programs can be found in D. J. Besharov, P. Germanis, C. Higney, and D. M. Call, "Summaries of Twenty-Four Early Childhood Evaluations," Welfare Reform Academy, College Park, MD: University of Maryland, 2008.

[30]See, for example, C. R. Belfield, M. Nores, S. W. Barnett, and L. J. Schweinhart, "The High/Scope Perry Preschool Program," *Journal of Human Resources* 41, No. 1 (Winter 2006): 162–190.

[31]*Abbeville v. South Carolina* (on appeal) and *Abbott v. Burke*; see E. A. Hanushek and A. A. Lindseth, *Schoolhouses, Courthouses, and Statehouses: Solving the Funding-Achievement Puzzle in America's Public Schools* (Princeton, NJ: Princeton University Press, 2009).

[32]An extensive re-analysis of the data from these programs has been conducted by Anderson: M. L. Anderson, "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevalution of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association* 103, No. 484 (December 2008): 1481–1495. He attempts to correct for attrition and multiple outcome evaluations along with statistical innovations.

[33]Anderson, "Multiple Inference and Gender Differences in the Effects of Early Intervention."

[34]The impact of differences in criminal activity are particularly important in the case of the benefit-cost analyses; see Gramlich, Edward M. 1986. "Evaluation of Education Projects: The Case of the Perry Preschool Program." *Economics of Education Review* 5, No. 1: 17–24. The females did, nonetheless, generally have positive school completion results; Anderson, "Multiple Inference and Gender Differences in the Effects of Early Intervention."

[35]Cost estimates and programmatic comparisons are found in J. F. Witte, "A Proposal for State, Income-Targeted, Preschool Vouchers," *Peabody Journal of Education* 82, No. 4 (2007): 617–644.

[36]F. A. Campbell and C. T. Ramey, "Cognitive and School Outcomes for High-Risk African-American Students at Middle Adolescence: Positive Effects of Early Intervention," *American Educational Research Journal* 32, No. 4 (Winter 1995): 743–772.

[37]Head Start Bureau, *Biennial Report to Congress: The Status of Children in Head Start Programs*, Washington, DC: U.S. Department of Health and Human Services, 2005.

[38]D. J. Besharov, J. A. Myers, and J. S. Morrow, "Costs Per Child for Early Childhood Education and Care: Comparing Head Start, CCDF Child Care, and Prekindergarten/Preschool Programs (2003/2004)," Welfare Reform Academy, Washington, DC: American Enterprise Institute for Public Policy Research, August 2007.

[39]See, for example, the review in M. A. Vinovskis, "Do Federal Compensatory Education Programs Really Work? A Brief Historical Analysis of Title I and Head Start," *American Journal of Education* 107, No. 3 (1999): 187–209.

[40]U.S. Department of Education, *Digest of Education Statistics, 2009*, Washington, DC: National Center for Education Statistics, 2010, Table 44.

[41]For a sense of the disagreements about programs and purposes, see D. L. Kirp, *The Sandbox Investment: The Preschool Movement and Kids-First Politics* (Cambridge, MA: Harvard University Press, 2007); B. Fuller, *Standardized Childhood: The Political and Cultural Struggle Over Early Education* (Stanford, CA: Stanford University Press, 2007); and C. E. Finn Jr., *Reroute the Pre-School Juggernaut* (Stanford, CA: Hoover Institution Press, 2009).

[42]Fuller, *Standardized Childhood*, chapter 6.

Transfers and taxes and the low-income population: Policy and research trends

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The 20th anniversary of the IRP Summer Research Workshop offers a time to reflect on the evolution of policy relating to tax and transfer programs in the United States (and antipoverty policy more generally), the evolution of social science research, and the relationship between them. While there have been a number of papers that have characterized the evolution of antipoverty policy over the last several decades, there have been fewer efforts to study the evolution of antipoverty *scholarship* over a similar period.[1]

In this article, we review the evolution of antipoverty policy over the last few decades. Second, we document the evolution of scholarship on antipoverty policy over the last 20 years in both economic journals and at the IRP Summer Research Workshop. Finally, we suggest future topics for research on the tax and transfer system.

Evolution of antipoverty policy

We first provide a context for discussing the evolution of scholarship, by summarizing selected developments in antipoverty policy and taxation in recent decades.

Social insurance and transfer program expenditures

Figure 1 shows the evolution of spending on four programs specifically targeted to nondisabled, non-elderly poor families and individuals: Aid to Families with Dependent Children (AFDC, now Temporary Assistance for Needy Families, or TANF); food stamps (now called the Supple-

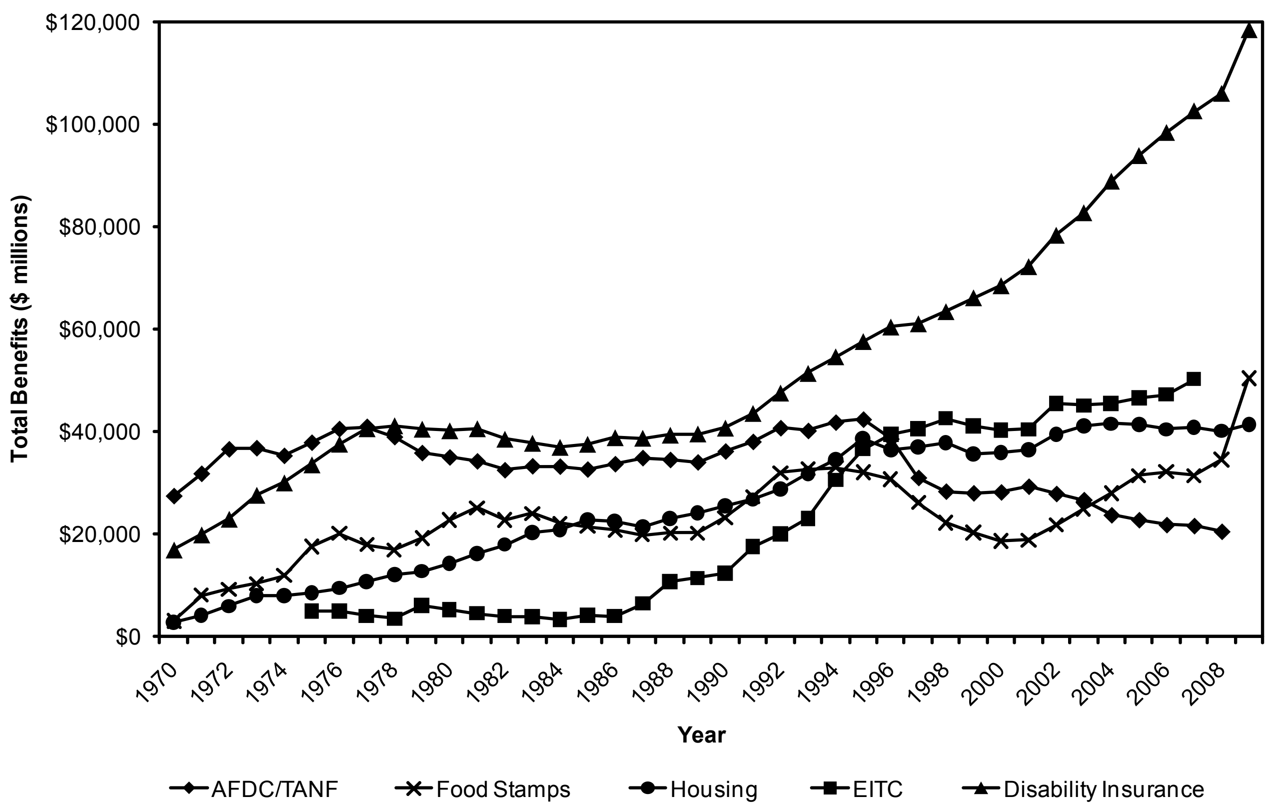

Figure 1: Total AFDC/TANF, Food Stamp, Housing, EITC, and Disability Insurance benefits, 1970–2009 (constant 2009 dollars).

Source notes can be found in J. K. Scholz, R. Moffitt, and B. Cowan, "Trends in Income Support," Table 8 A.1, in *Changing Poverty, Changing Policies*, eds. M. Cancian and S. Danziger (New York: Russell Sage Foundation, 2009).

mental Nutrition Assistance Program, or SNAP); housing assistance; and the Earned Income Tax Credit (EITC); as well as Disability Insurance, which is funded through the social security system. In general, transfer program expenditures rose from 1984 to 2004, but the increase was spread unevenly across different demographic groups and income classes. Several major developments in the time-series patterns of antipoverty spending are evident:

- *AFDC/TANF spending and recipiency have been sharply reduced*. Aid to Dependent Children (the precursor to AFDC) began in 1935 as part of the Social Security Act. The program was designed to provide cash support for children living in poor, generally single-parent households. In 1996, AFDC was replaced by TANF, which mandates work and lifetime time limits on federal assistance, and provides block grants to states with few restrictions. A combination of changes in AFDC and TANF, the longest economic expansion in history, sharp increases in the EITC, and other factors contributed to a 52 percent decline in welfare caseloads between January 1993 and December 1999. After the recession in 2001, TANF caseloads did not increase substantially from their historic lows. Real spending on TANF fell by 3.9 percent a year from 2000 to 2008, despite the weakest economy since the Depression.
- *Food stamps (SNAP) experienced highly variable spending*. Between 1994 and 2000, real food stamp expenditures fell to $19 billion from $33 billion, even though only modest changes to food stamp program rules were made by the 1996 welfare reform (primarily affecting immigrant households). The General Accounting Office concluded that participation fell “faster than related economic indicators would predict” and speculated that some former cash welfare recipients thought they were also no longer eligible for food stamps.[2] Food stamp (SNAP) spending has increased sharply since 2000.
- *Tax expenditures targeting low- and moderate-income families have increased substantially*. This increase was driven primarily by EITC expansion. The EITC, which began in 1975, subsidizes earnings up to a point, after which it phases out. It has been expanded in Republican administrations (in 1986 and 1990) and in Democratic ones (in 1996). In contrast to TANF, the EITC is designed to encourage low-skilled workers to enter the labor market, since nonearners do not receive the credit and the EITC amount rises with earnings up to about the poverty line.
- *Disability Insurance spending has grown*. Disability Insurance is a social insurance program administered as part of the social security system. Rules for eligibility are stringent and recipients must have substantial work experience. Less than 40 percent of all applications are granted benefits. Around 9.7 million people (including children) receive disability benefits, which cost $118 billion in 2009. Most recipients are pre-tax-and-transfer poor. Concerns over rapidly escalating costs have given rise to periodic “continuing eligibility reviews” that attempt to reduce disability rolls by moving those able to work back into paid employment. In practice, it may be difficult to determine how particular health conditions affect ability to work, and this correlation may also change over time for some health conditions.
- *Medicaid expanded rapidly*. Figure 2 shows outlays on Medicaid, which provides subsidized medical care for families with low income and assets or to families with a disabled member. At over $300 billion, Medicaid outlays greatly exceed expenditures on other safety net programs. The largest share of Medicaid expenditures pay for nursing homes of the low-income, low-asset elderly.

We make several general observations in reviewing all program expenditure trends. First, the U.S. population is aging, particularly as the baby boom generation reaches retirement. The elderly also have high voting rates relative to other demographic groups. There is considerable political pressure to support and even expand programs that provide benefits to the elderly. Figure 2 shows that Old Age and Survivors Insurance (commonly called Social Security), and Medicare (which provides near-universal health insurance for households over 65), have had upward spending trends in recent decades. Second, medical care prices have increased rapidly. The consumer price index for medical care increased 78 percent faster than the overall index between 1979 and 2007. Third, voters and policymakers appear comfortable providing support for specific needs such as medical care, food, and housing, rather than providing unrestricted cash payments to low-income families or individuals. The growth of Medicaid provides evidence for this; over the past 20 years, there have been far-reaching expansions of Medicaid to low- and moderate-income families with children. Finally, there are certain favored groups within the disadvantaged population. These include the elderly (if for no other reason than their willingness to vote), children (though not necessarily the adults that care for them), working individuals, and families with a disabled individual.

The distribution of income support

We use analyses of data from the Survey of Income and Program Participation to summarize changes in the level and distribution of income support.[3]

What is the composition of poor families?

The percentage of all families in deep poverty (below 50 percent of the poverty line) rose slightly (from 21.1 percent to 22. 3 percent) between 1984 and 2004. This increase is entirely attributable to growth in the number of very poor disabled families and childless families. The growth in the number of childless families in deep poverty represents a general increase in the percentage of childless adults in the United States over this period, rather than a shift downward in the income distribution within the childless group.

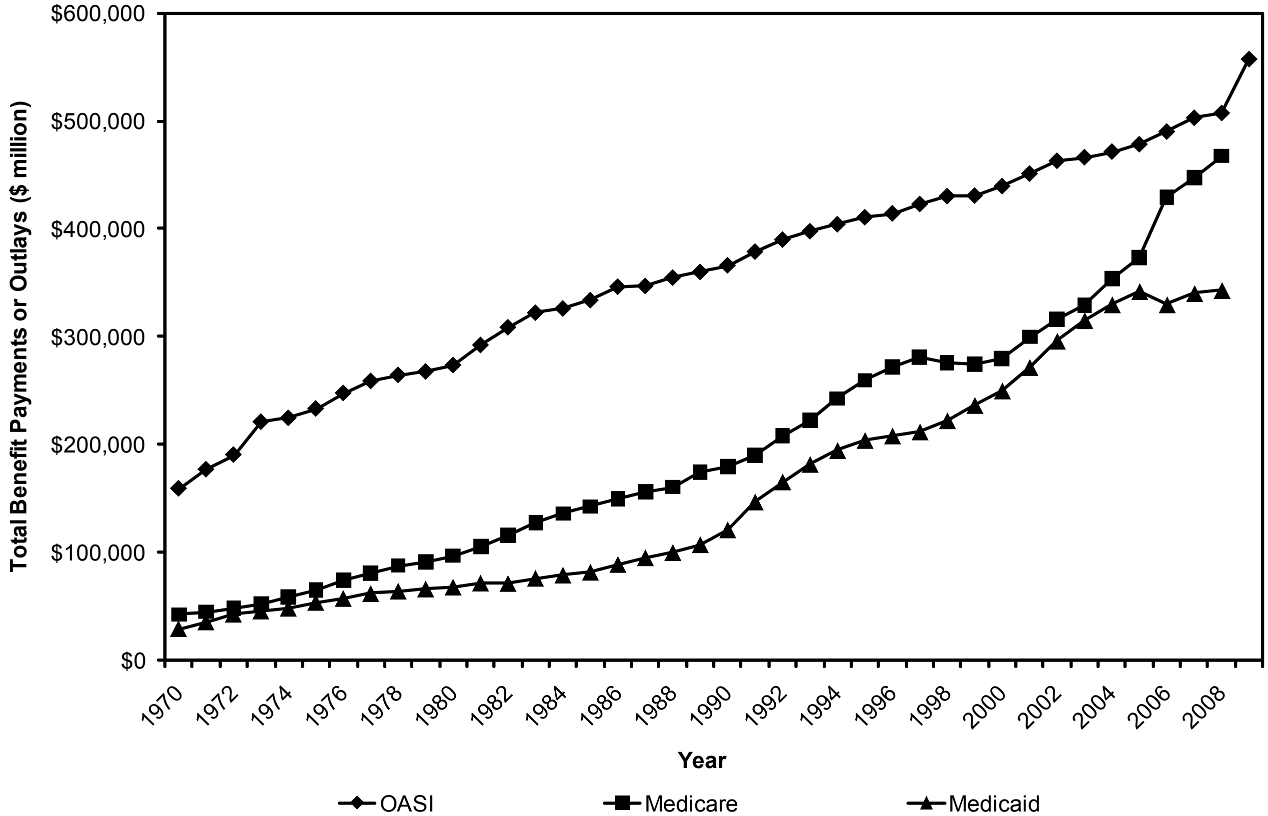

Figure 2. Total benefit payments on Old Age and Survivors Insurance (OASI) and outlays for Medicare and Medicaid, 1970–2009 (constant 2009 dollars).

Source notes can be found in J. K. Scholz, R. Moffitt, and B. Cowan, "Trends in Income Support," Table 8 A.1, in *Changing Poverty, Changing Policies*, eds. M. Cancian and S. Danziger (New York: Russell Sage Foundation, 2009).

What is the targeting of antipoverty spending across different groups of the poor?

Aggregate expenditures rose fairly sharply from 1984 to 2004 for poor families, including those in deep poverty. These increases occurred for elderly and childless families, and particularly for those receiving disability-related benefits. Aggregate expenditures fell, however, for one- and two-parent families with children who were in deep poverty. These aggregate patterns are driven primarily by an enormous reduction in AFDC and TANF participation for families in deep poverty. Around 60 percent of single-parent families with children and with incomes in deep poverty received AFDC in 1984 and 1993. Only 24 percent received TANF in 2004. Similar reductions (from 25 percent to 10 percent) applied to two-parent families with children who were in deep poverty. Participation in SNAP also fell, though not as much. Several income sources increased for single-parent families with children who were in deep poverty, including EITC benefits, Medicaid benefits, and unemployment compensation.

How has the targeting of antipoverty spending evolved over time?

United States income transfer programs are a patchwork, so families in different categories but with similar incomes can receive substantially different benefits. The core non-health safety net programs available to non-elderly, nondisabled families and individuals, for example, grew by 44 percent between 1984 and 1993, far faster than the growth in the number of families. Between 1993 and 2004, however, these benefits fell in real terms by 13.4 percent.[4]

Implications of income support changes

The policy developments affecting poor families with children were purposeful. The substantial EITC expansions were made in part with the idea that they rewarded work, augmenting the incomes of low-income working families "playing by the rules." One goal of "ending welfare as we know it" was to create a safety net that better reflects the norms of broader American society. The hope was that by providing states with greater flexibility and by imposing lifetime limits on TANF receipt, families would become much less reliant on welfare. In some sense that hope has been realized—TANF receipt today is much lower than past AFDC receipt.

When focusing on market income, there are significantly fewer single-parent families in deep poverty. The reduction in the number of single-parent families in 2004 is consistent with TANF achieving at least part of its goals. But these changes also make benefits less available to poor families with children. Those who are either unable or unwilling to work now have to get by with fewer publicly provided resources.

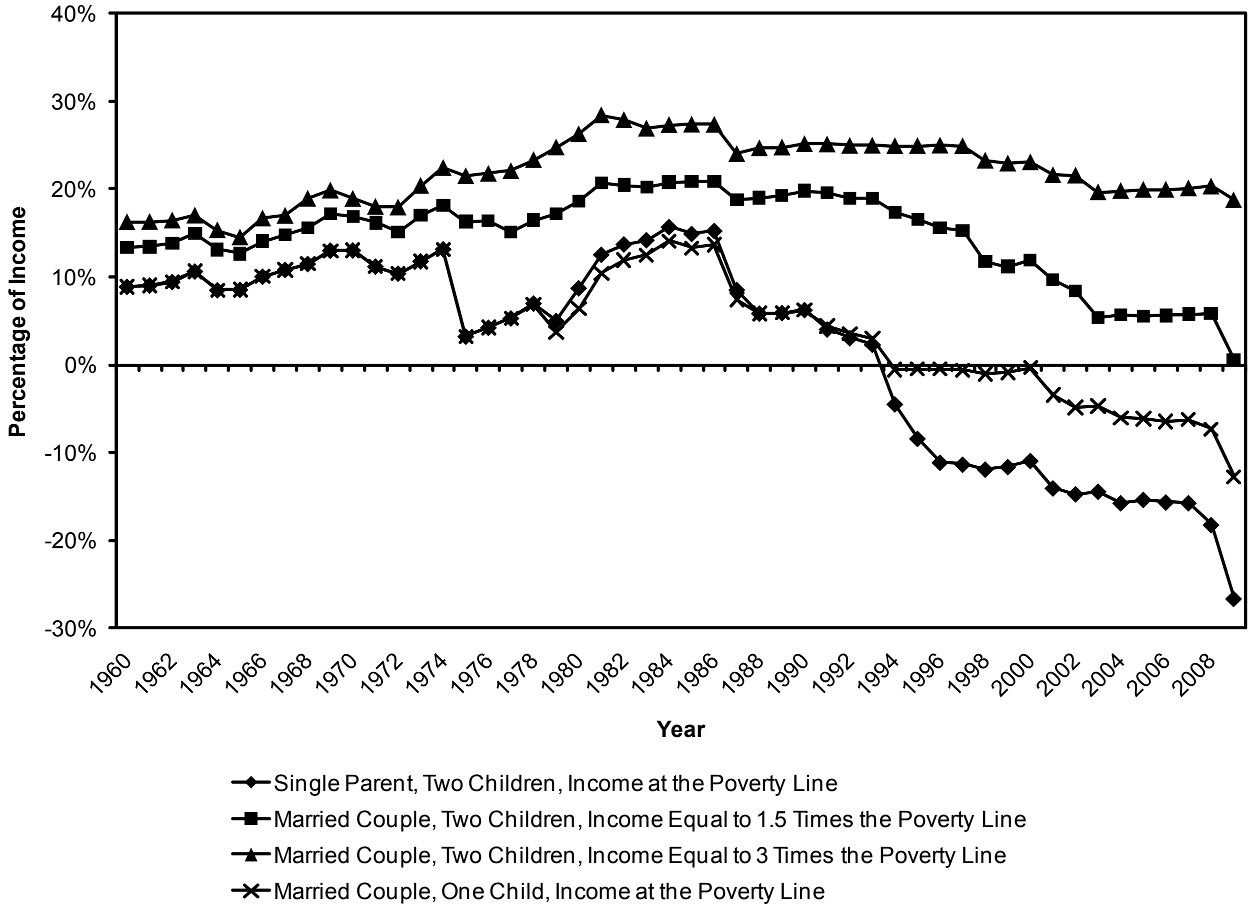

Figure 3. Average tax rate: Federal income tax plus payroll tax, 1960–2009.

Source: NBER's TAXSIM and authors' calculations as described in the text. See http://www.nber.org/~taxsim/taxsim-calc8/ and D. R. Feenberg and E. Coutts, "An Introduction to the TAXSIM Model," *Journal of Policy Analysis and Management* 12, No. 1 (Winter 1993): 189–94.

Taxation

Figure 3 shows the sum of average effective federal income and payroll tax rates from 1960 through 2009 for four different family types.[5] Between 1960 and 1974, tax burdens tended to be substantial, and the pattern of average tax rates was strikingly compressed. The difference in average effective tax rates between the families with income three times the poverty line and families with income equal to the poverty line ranged from 6.0 to 9.3 percentage points. By 1974, average effective tax rates on the representative poor families exceeded 13 percent.

Average effective rates for families with incomes at the poverty line fell sharply in 1975 with the implementation of the EITC, only to rise to their 1974 level by 1983. Payroll tax rates account for much of this change, rising from 11.7 percentage points in 1974 to 14.3 percentage points in 1986. Effective average tax rates on one-parent, two-child poor families were 15.3 percent in 1986, the highest level of taxation seen over this 50-year period.[6]

If the establishment of the EITC in 1975 was the first landmark piece of legislation affecting taxation of poor families with children, the Tax Reform Act of 1986 (TRA86), which was fully phased in during 1988, was the second. Policymakers made an explicit decision to eliminate federal income taxes on families with incomes below the poverty line. They further increased the EITC to the point where the maximum credit in 1987 equaled the real value of the credit in 1975. TRA86 also indexed for inflation the EITC, exemptions, the standard deduction, and brackets.

The tax legislation in 1990 and again in 1993 marked the beginning of the third important set of developments in the taxation of poor families, as the EITC increased in six consecutive years beginning in 1991. By 1997, the maximum EITC had increased to $4,887; over $3,100 more than its level in 1975. A one-adult, two-child family where the adult was working in a job with poverty-line wages (and filing a tax return) would have had $4,000 more in disposable income in 1997 than it did in 1986.

The gap in effective tax rates between single, two-child families with incomes at the poverty line and married, two-child families with incomes three times the poverty line is now 45.4 percentage points. As recently as 1986, it was 12.1 percentage points. By using the tax system as a tool for antipoverty policy, government substantially varies the tax treatment of families at different points in the income distribution.

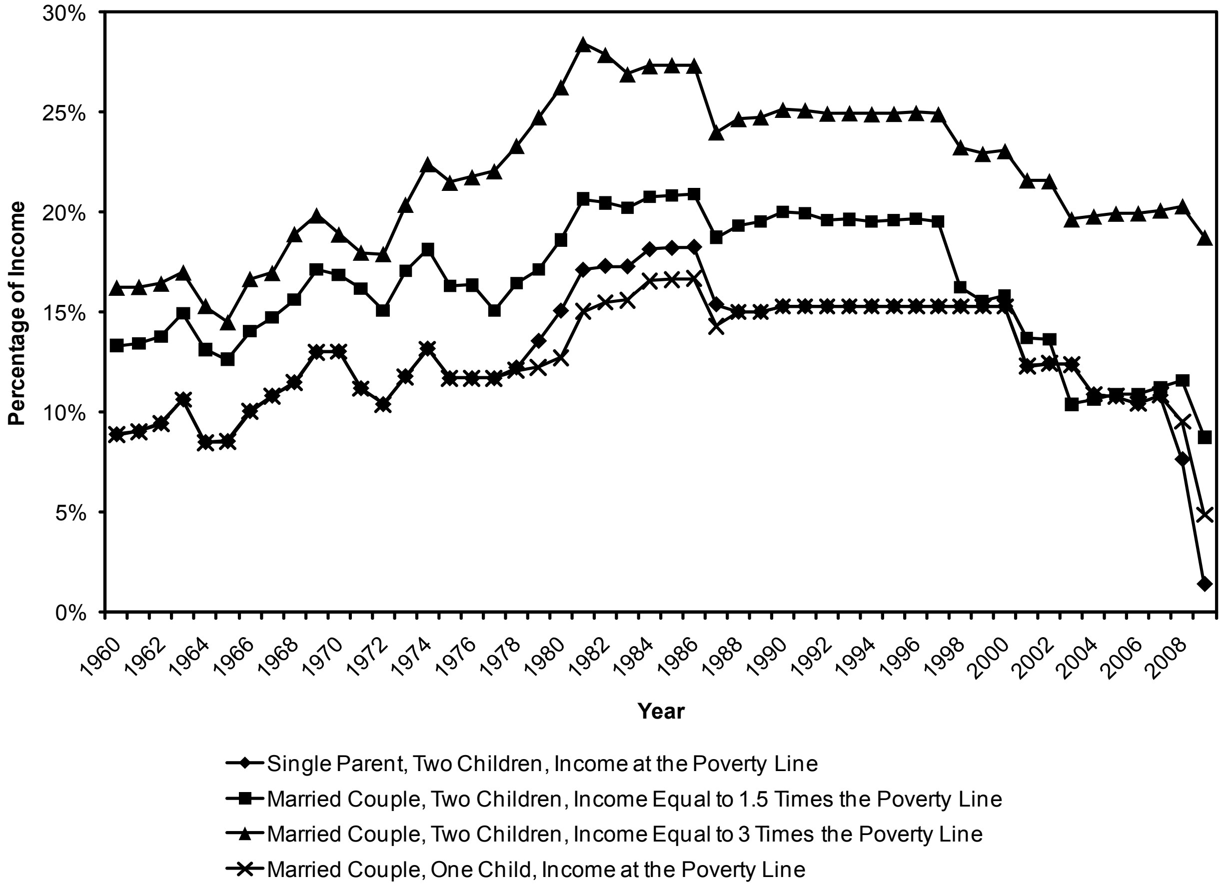

Figure 4. Average tax rate: Federal income tax plus payroll tax (excluding EITC), 1960–2009.

Source: NBER's TAXSIM and authors' calculations as described in the text. See http://www.nber.org/~taxsim/taxsim-calc8/ and D. R. Feenberg and E. Coutts, "An Introduction to the TAXSIM Model," *Journal of Policy Analysis and Management* 12, No. 1 (Winter 1993): 189–94.

The most recent major development affecting the taxation of poor families with children was the adoption in 2001 of a partially refundable child tax credit. The child credit is refundable for many taxpayers and equals $1,000 per child in 2010. The combination of the child credit (for upper-income families) and the partially refundable portion of that credit (for poor and near-poor families) account for the further reductions in effective tax rates beginning in 2000.

Figure 4 shows the pattern of effective federal income and payroll tax rates for the same four family types, excluding the EITC. A comparison of Figures 3 and 4 shows just how important the EITC is in augmenting incomes of working-poor households.

The focus of the academic literature

There is likely to be a complicated relationship between policy developments and academic research on poverty-related issues. It would be naïve, for example, to expect policy to be entirely driven by research developments. There is considerable inertia to policy, due to the nature of funding streams and administrative infrastructure. If policy rarely changes, it is unlikely to be responsive to quickly moving research developments. Moreover, many factors in addition to cost-benefit and efficiency considerations drive policy decisions.

It would be similarly naïve to expect research developments to be entirely driven by policy trends. Scholars frequently look forward, anticipating issues that will likely be of future interest. Policymakers also set policy, at least in part, on the basis of what they think is right from a normative standpoint. Economists are trained to focus on the positive (and not normative) aspects of their work, which may reduce economists' involvement in policy development. Lastly, rewards in academia come from making technical as well as policy contributions. Thus, much poverty-related scholarship is devoted to methodological advances that may or may not have ties to current policy developments. Given that policy innovation is rarely a central focus of academic reward systems, we would be surprised to see an overly strong link between research topics and policy developments.

Before speculating further on the relationship between policy developments and academic research, it is useful to first document the topics that have been the focus of academic poverty research. We examined nine economics journals using the following keywords: poverty, poor families, welfare, TANF, food stamps, EITC, SSI, subsidized housing, public housing, WIC (a supplemental nutrition program for Women, Infants, and Children), Medicaid, training programs, and child care.[7]

The ten most popular topics appearing in top-ranked economics journals between 1990 and 2010, based on the search keywords we used, were, in order:

- Poverty and inequality, 10 percent of the papers;
- Intergenerational linkages, 10 percent;
- Welfare and welfare reform, 9 percent;
- Fertility, 8 percent;
- Education, 6 percent;
- Employment, unemployment and unemployment insurance, 6 percent;
- Child care, 6 percent;
- Health, 4 percent;
- Medicaid, 3 percent; and
- Wealth or consumption, 3 percent.

The "poverty and inequality" category is even more heterogeneous than the other categories and also varies more across journals and over time than other topics. This category is also focused, at least in part, on measurement and administrative issues. The papers on intergenerational linkages are generally about factors affecting child well-being, though the category also includes papers measuring intergenerational correlations between children and parental attributes. The other categories listed above are largely self-explanatory.

In reviewing the frequency of each topic, we are first struck by the relatively broad representation of poverty-related research in top-tier economics journals. Roughly 30 papers per year appear in top outlets. To some extent we cast perhaps an inappropriately broad net since, for example, some of the intergenerational linkages papers have, at best, only a tangential poverty-related focus. At the same time, our keywords did not explicitly focus on low-wage labor markets, education, or a host of additional keywords that might have generated additional poverty-related papers. So we do not claim comprehensiveness, even in this fairly substantial list of poverty-related papers.

Second, we were surprised by the small number of papers on specific programs. Only Medicaid and welfare or welfare reform made the top 10 topics list. We do not think it is essential (or even necessarily desirable) for scholarly work to follow policy trends. Nevertheless, to the extent that researchers would like to influence policy, it seems useful for research to address issues related to major tax and transfer program expenditures.

Third, while there is relatively little peer-reviewed work investigating specific details of the large-budget antipoverty programs, the general outcomes that are the focus of the peer-reviewed scholarly work are what we would expect to see. Namely, factors influencing child well-being, fertility decisions, education, employment, and health are, to us, first-order correlates of economic well-being. It is not surprising that these topics are the focus of the bulk of poverty-related writing in economics, but it is nevertheless encouraging to us.

The IRP Summer Research Workshop

The IRP Summer Research Workshop (SRW) provides a complementary perspective on academic research on poverty. There are two good reasons for assessing the 20 years' worth of workshop research, in addition to the fact that the 2010 SRW marked its 20th anniversary. First, our keyword search making use of EconLit (the American Economic Association's electronic bibliography) is necessarily less than comprehensive. As is clear from the SRW programs, the applied poverty research community is doing a great deal of education-related work. This work is only partially captured by the journal tabulations described above. Second, it is possible that the organizers of the IRP workshop are more willing to highlight policy-oriented work than the editors of leading academic journals. Hence, summarizing the topics from the SRW gives a potentially valuable perspective on the focus of and trends in poverty-related research.

At least two issues arise with using the IRP conference as a window on poverty-related research. First, IRP has constraints that arise from its funders. For example, IRP does almost no work on development and global poverty, so the IRP workshop has not been a platform to showcase the resurgence of program evaluation in developing-country contexts. Second, the low-income workshop has tried to highlight work being done by younger, tenure-track poverty researchers rather than to necessarily showcase a representative sample of poverty research being conducted at a given time. Nevertheless, the SRW programs give insight into poverty research topics.

Table 1 shows the most popular SRW topics in order, along with the ranking of each topic according to our journal paper tabulation. The most striking result is the importance of education-related papers. Fifty-two education papers have been presented, representing 15 percent of the total number of workshop papers. We think the fact that education-related papers are far more common at the workshop than in our tabulation from academic journals is more a reflection of our EconLit search strategy than a fundamental difference between the culture of the SRW and the tastes of academic journals.

The next tier of papers at the IRP workshop are on employment and unemployment, welfare reform, earnings and wages, black-white issues, intergenerational linkages, marriage, fertility, and poverty and inequality. Three of these topics—earnings and wages; marriage; and black-white issues—do not appear in the academic journals' "top 10" topics. We

Table 1
Paper (or Presentation) Topics at the IRP Summer Research Workshop, 1990–2010

	Number of Papers	Percentage of Total Workshop Papers	Topic Ranking from Journal Tabulation
Education	52	15%	5
Employment and unemployment	27	8	6
Welfare and welfare reform	26	7	3
Earnings and wages	25	7	19
Black-white issues	24	7	21
Intergenerational linkages	22	6	2
Marriage	19	5	13
Fertility	18	5	4
Poverty and inequality	17	5	1
Child care	13	4	7
Job training	10	3	17
EITC	10	3	22
Health	9	3	8
International issues	9	3	30
Neighborhoods	9	3	24
Migration and immigration	9	3	14
Total	354		

suspect the lack of evidence on earnings and marriage is also a reflection of the specification of our EconLit search. We are certain that with a more targeted search on wages and employment affecting low-income families and individuals, we would have a more extensive set of references. We are less sure this is the case with black-white issues, as the SRW has shown sustained attention to black-white differences that, we think, is likely not mirrored by academic publications.

The third tier of IRP workshop papers comprises child care, job training, the EITC, health, international issues, neighborhoods, and migration and immigration. Only child care and health appear on the top 10 list of peer-reviewed paper topics in economics journals. These topics touch on vital issues affecting the low-income population. But they share the characteristic noted with the peer-reviewed papers: few papers address specific programs. Only 7 percent of SRW papers address welfare or welfare reform, while less than 3 percent address the EITC. Beyond this, there are few studies conducting evaluations or studies of specific programs. This suggests that the absence of studies focusing on specific programs in peer-reviewed outlets is likely to be a consequence of people not writing these papers, rather than there being an abundance of these papers that, for one reason or another, journal editors are unwilling to print.

The most striking intertemporal pattern that occurs with the summer workshop programs is the time pattern of papers on welfare and welfare reform. Over the 20 years, there have been 26 papers presented on this topic. The first year for this topic, however, was 1996 (when two papers were presented). This is the year when AFDC was eliminated and replaced by TANF, and several years after a large number of state welfare waivers were enacted. Then 24 papers were presented between 1998 and 2006. None were on this topic in 2007 to 2010. This is clearly a case where policy developments affected research topics.

Does academic research lead or lag policy?

It is not coincidence that work on welfare reform spiked from 2005 to 2010: research likely began immediately after the 1996 reforms, but there is generally a substantial lag between doing research and getting it published. It is also not surprising there has been a striking surge in education-related research in recent years, given the No Child Left Behind legislation (and we note that education was not a search keyword). Academic work sometimes also alters the trajectory of policy, as might be argued in the case of the Family Support Act in AFDC, the Negative Income Tax debates in the 1960s and 1970s, and in political efforts to cut the EITC in the 1990s. In general, we think most scholars would say that they are doing work that should provide useful background information for policymakers, perhaps identifying problems that need addressing, even if they are not suggesting specific policy changes.

We are struck by the broad range of topics that have been the focus of antipoverty scholarship. Nevertheless, it appears that policy and academic writing have only modest connections. We do not necessarily think the modest connection highlights a problem with academic research. The types of questions that lend themselves to high-quality academic research may not be the same as those that are central to policy developments.

Directions for research

Juxtaposing program trends with patterns of academic research positions us to comment on gaps in the existing literature. Four come immediately to mind. First, between Disability Insurance and SSI, over $100 billion a year is spent on individuals with disabilities. Yet only nine papers have appeared in leading peer-reviewed economics journals on disability over the past 20 years and only one paper was presented at the Summer Research Workshop.[8] There are a large number of critical issues that deserve further exploration. Disability caseloads have exploded. According to unpublished calculations from Rich Burkhauser, rates of SSI recipiency per 1,000 income-eligible children was 22 in 1990, and then rose sharply to 65 in 1996, largely due to the Zebley decision, a Supreme Court case that revised the childhood mental health impairment eligibility criterion to be consistent with the criterion that applies to adults. It rose to 80 by 2006 and is still rising. Further research on factors affecting SSI and Disability Insurance participation could be valuable. More work may also be useful in examining the link between participation in disability programs and take-up of other safety net benefits. To what degree, for example, are policymakers and citizens using disability programs to reduce pressure on other safety net programs? Finally, work is a centerpiece of safety net programs targeting non-elderly, nondisabled families and individuals. To what extent are the lessons that motivated the work-based approach to antipoverty policy applicable to the disabled population?

Second, one of the explicit goals of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), the 1996 welfare reform, was to increase marriage among low-income individuals. A great deal has been written on welfare reform and family structure, and the EITC and family structure; the effects appear to be small or nonexistent. Given the existing body of work, there may just not be much more to do. But the stakes of enhancing understanding of the factors affecting marriage and fertility decisions—including wage rates, employment, neighborhood characteristics, crime, and housing arrangements—are high. It is widely believed that having two adults in the household generally enhances child well-being. And in general, an overarching goal of much poverty research is to discover insights that may help people to escape poverty, whether through education, broader dimensions of human capital acquisition, training, and through choices they make about marriage and fertility.

Third, a striking result is that there are few direct studies of major programs, including Disability Insurance, SNAP, housing, and a host of smaller safety net programs. Given that few such studies have been presented at the Summer Research Workshop, we suspect the absence of studies on these topics in top peer-reviewed journals is because they are not being written. We further suspect high-quality economic and statistical analyses of these programs could be valuable. Most importantly, are there changes in program design that could enhance economic efficiency? To what degree are programs meeting their intended objectives? What are the behavioral responses to programs and to what extent do they mitigate program objectives? In general, we suspect there is a great deal more to learn about key safety net programs.

Fourth, while the 1996 welfare reform increased work, the earnings of most individuals who left welfare were still well below the poverty line, even many years after their exit. Hence, the degree to which work can be the primary antidote to poverty depends on the ability of low-skilled people to maintain employment that, over time, leads to higher incomes that allow families to be self-sufficient. More work is needed to develop effective ways of increasing the earnings of disadvantaged workers. To date, we do not have the research base needed to sort through approaches policymakers can take to enhance the economic self-sufficiency of disadvantaged workers. More could usefully be learned on these issues.■

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[1]For examples of papers characterizing the evolution of antipoverty policy, see G. Burtless, "Public Spending on the Poor: Historical Trends and Economic Limits," in *Confronting Poverty: Prescriptions for Change*, eds. S. Danziger, G. Sandefur and D. Weinberg (Cambridge: Harvard University Press, 1994), 51–84; J. K. Scholz and K. Levine, "The Evolution of Income Support Policy in Recent Decades," in *Understanding Poverty*, eds. S. Danziger and R. Haveman (Harvard University Press and Russell Sage Foundation, 2002), 193–228; and J. K. Scholz, R. Moffitt, and B. Cowan, "Trends in Income Support," in *Changing Poverty, Changing Policies*, eds. M. Cancian and S. Danziger (New York: Russell Sage Foundation, 2009), 203–241. For a study of the evolution of antipoverty scholarship, see R. H. Haveman, *Poverty Policy and Poverty Research: The Great Society and the Social Sciences* (Madison, WI: University of Wisconsin Press, 1987), which examines the content of journals in economics and sociology to examine the extent to which the War on Poverty affected academic research.

[2]General Accounting Office, "Food Stamp Program: Various Factors Have Led to Declining Participation," GAO/RCED-99-195.

[3]R. Moffitt and J. K. Scholz, "Trends in the Level and Distribution of Income Support," *Tax Policy and the Economy* 24, No. 1 (2010): 111–152; and Y. Ben-Shalom, R. Moffitt, and J. K. Scholz, "An Assessment of the Effectiveness Of Anti-Poverty Programs in the United States," in *Handbook of the Economics of Poverty* (Oxford University Press, forthcoming) examine changes in the level and distribution of income support using data from the 1984, 1993, and 2004 Surveys of Income and Program Participation.

[4]The programs in this calculation are AFDC and TANF, the EITC, general assistance, other welfare, food stamps, and housing assistance.

[5]We used the TAXSIM program, an Internet tax simulation program developed by Daniel Feenberg and colleagues at the National Bureau of Economic Research. See D. R. Feenberg and E. Coutts, "An Introduction to the TAXSIM Model," *Journal of Policy Analysis and Management* 12, No. 1 (Winter 1993): 189–94; and http://www.nber.org/~taxsim/taxsim-calc8/ for details on the current version of the model. We calculated tax burdens on four hypothetical families: Two have incomes exactly equal to the poverty line for that family type in the given year: a single parent with two children, and a married couple (single earner) with one child. The third "near poor" family consists of a married couple with two children and an income equal to 1.5 times the poverty line. The fourth family is a married couple with two children and an income three times the poverty line. The "effective" tax rate (in contrast to a "statutory" tax rate) is calculated as total taxes paid divided by income. We assume that all income in each family is earned, and that the incidence of the payroll tax is fully on wages.

[6]They were also at their highest level of 13.7 percent for poor married couples with one child in 1986.

[7]The nine journals we searched include what are generally regarded to be the five leading economics journals: the *American Economic Review*, *Journal of Political Economy*, *Quarterly Journal of Economics*, *Econometrica*, and *Review of Economic Studies*. We also included the four highest quality next-tier journals in which economists place their poverty-related research: the *Review of Economics and Statistics*, the *Journal of Labor Economics*, the *Journal of Public Economics*, and the *Journal of Human Resources*. We used EconLit to search the journals for keywords. We eliminated duplicate entries, as well as those that were not relevant to the design and understanding of antipoverty programs in the United States.

[8]We note that we did not search specifically for disability and instead searched only for SSI, poverty, and poor families, so we might have missed a handful of papers on Disability Insurance.

Revisiting an old question: How much does parental income affect child outcomes?

Susan E. Mayer

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Even casual observers note that the children of affluent parents are more likely to succeed in life than the children of poor parents. For example, compared to more affluent children, poor children:

- Score lower on tests of cognitive skill in early childhood;
- Have more behavior problems in school and at home;
- Are more likely to drop out of high school, and those who do graduate are less likely to enroll in or graduate college;
- Are more likely to have children at a young age; and
- Are more likely to be poor themselves when they are adults.

The most intuitive explanation for this difference is that rich parents can spend more than poor parents on their children and that these "investments" lead to better outcomes for their children. This intuition fit the interests of policymakers looking for simple solutions to alleviate poverty and its apparent by-products: If poor children fail because their parents cannot make sufficient monetary investments in their future, then government can improve the life chances of poor children by providing families with the means to make the investments or by providing the investments directly in the form of schooling, health care, and other human capital inputs. Such investments presumably also promote economic growth as the "higher quality" children grow to adulthood.

Consequently, it is no surprise that by far the most money spent by the federal government and states on income-tested programs goes to programs that increase the income of poor families. While most families benefit from universal transfers such as education, Table 1 shows that not counting medical care, the vast amount of income-tested government spending goes to cash transfers. The combined amount spent on cash transfers, food stamps, and housing subsidies (which are near-cash transfers) was almost three times the amount spent on education, job training, and services for the poor combined in 2002. Historically the government spent even less on non-transfer help for families. In 1993, before the 1996 welfare changes took effect, government spending on income and near-income support for low-income families was almost four times more than education and services for the poor. The largest increases in non-income support programs since TANF has been in services to help parents work. These include child care and transportation services for parents receiving TANF. The program today uses only one-third of the 1996 block grant for cash benefits, the rest going towards services.

However, poor parents' inability to invest in their children is not the only possible explanation for the relationship between family poverty and child well-being. Other parental characteristics associated with their poverty have been implicated, especially parental education and marital status. Neighborhood characteristics and parental behavior or "culture" have also been implicated. These explanations argue for policies other than income support to improve children's well-being as adults.

Because our support for the poor largely relies on income support, I reassess the evidence on the importance of parental income to adult well-being before comparing the effect of income to the potential effect of other family background characteristics and the potential benefits of programs other than income support for improving the well-being of poor children.

For many years, research on the relationship between parental income and children's outcomes followed the standard research trajectory of many big questions. First, correlational studies reinforced the basic observation that poor children did worse than rich children on an increasing list of outcomes. Then researchers began to increase the list of covari-

Table 1
Total (Federal, State, and Local) Spending for Income-Tested Benefits by Form of the Benefit (in Millions of Constant 2002 Dollars)

Fiscal Year	Medical	Cash	Food	Housing	Education	Jobs and Training	Services
1973	$44,485	$57,011	$15,843	$15,519	$7,484	$4,024	$9,128
1993	178,294	93,260	45,309	36,171	18,800	6,649	13,506
1998	214,412	101,403	38,890	37,432	20,068	5,416	18,896
2002	282,468	102,157	39,306	35,566	30,484	7,808	22,215

Source: V. Burke, "Cash and Noncash Benefits for Persons with Limited Income: Eligibility Rules, Recipient and Expenditure Data, Fiscal Years 2000–2002," Congressional Research Service, November 25, 2003.

Table 2
Recent Research on the Effect of Family Income on Years of Schooling[a]

Study	Outcome	Data	Model Notes	Estimated Effect of Parental Income
Ellwood and Kane	College enrollment	HSB, NELS88	One year of parental income; nonparametric nonlinear measure of income (quartiles); controls gender, race, ethnicity, mother's education, other background variables (not test scores), and tuition costs.	Going from 1st quartile (poor) to 2nd quartile = 10% greater chance for enrollment in 4-year college; 4% greater chance of enrolling in any post-secondary schooling. When high school achievement is controlled for = no differences. Magnitude of income increase is unknown.
Acemoglu and Pischke	College enrollment	NLS72, HSB, NELS88	Instrumental variable model based on changes over time in parental income net of income quartile; controls region fixed effects and returns to college.	10% increase in income = 1.1% increase in chance of enrolling in any college and 1.5% increase in chance of enrolling in 4-year college. Effects not bigger for poor and possibly bigger for families in the richest quartile.
Akee et al.	Educational attainment at age 19 and 21; High school graduation	Great Smoky Mountain Study of Youth	Compares children in Native American families who benefited from Casino profits to non-Native families that did not benefit; compares families by number of Native parents, which determine the size of the income increase; compare children by age which indicates length of higher income; uses child fixed effects for education outcomes.	No income effect on high school graduation or educational attainment for never poor children; for families that were ever poor receiving additional income = nearly 1 additional year of school and 30% greater chance of graduating high school. Note that the income increase was \$5,000–\$10,000/year or 1/4 to 1/3 of income for most families and as much as 100% for poor families.
Duncan, Ziol-Guest and Kalil	Years of completed schooling	PSID	Controls parents' test scores, expectations, personality variables, mother's age. Variables for income in early, middle childhood and adolescence; allows different linear estimates of the effect of income <\$25,000 and >\$25,000 and other functional forms.	No effect of parental income measured when child < 5; parental income measured at child age 6–10, \$10,000 increase in parental income = .65 additional years school for families <\$25,000, no effect for families >\$25,000; parental income measured at age 11–15 = no effect for families <\$25,000, increase of .09 years of school for families >\$25,000.

Notes: HSB is the High School and Beyond survey. NELS88 is the National Education Longitudinal Study begun in 1988. NLS72 is the National Longitudinal Study of the High School class of 1972. PSID is the Panel Study of Income Dynamics. Highlighted papers indicate some attempt at estimating a causal model.

[a]Mayer's prior review found that a 10 percent increase in income increased years of schooling by .024 to .104 years.

Studies referenced in this table are: D. Ellwood and T. Kane, "Who Is Getting a College Education? Family Background and the Growing Gaps in Enrollment," in *Securing the Future: Investing in Children from Birth to College*, eds. S. Danziger and J. Waldfogel (New York: Russell Sage Foundation, 2000); D. Acemoglu and J. F. Pischke, "Changes in the Wage Structure, Family Income, and Children's Education," NBER Working Papers No. 7986, 2000; R. K. Q. Akee, W. Copeland, G. Keeler, A. Angold, and J. Costello, "Parents' Incomes and Children's Outcomes: A Quasi-Experiment," *American Economic Journal: Applied Economics 2*, No. 1, (2010): 86–115; and G. Duncan, K. Ziol-Guest and A. Kalil, "Early Childhood Poverty and Adult Body Mass Index," *American Journal of Public Health* 99, No. 3, (2009): 527–532.

ates added to standard OLS models predicting the effect of parental income on children's outcomes. In the late 1990s, researchers seriously questioned the causal effect of parental income.[1] In 2000 I wrote a review of the research up to that time.[2] This article briefly summarizes my primary conclusions on what we have learned since then, and what that tells us about antipoverty policies.

The research

In this article I focus on the "effect of parents' income" literature, which tries to isolate the effect of parental income on children's outcomes, in particular the effect of low parental income on poverty. In this review I consider only research in the United States.[3]

Educational outcomes

Research on the relationship between parental income and educational outcomes can broadly be divided into research on general educational attainment and borrowing constraint for college enrollment.

Studies on educational attainment usually find that an increase in parental income modestly increases the educational attainment of children. These studies are described in Table 2. In my previous review, I concluded that the evidence suggested that a 10 percent increase in parental income was associated with .024 to .104 additional years of schooling.[4] Most of these effects occur before high school. There is no strong evidence that the income effects are greater for children from low-income families compared to children from high-income families, or that income effects vary by age of child.

Borrowing constraint and college enrollment research is motivated by the fact that going to college is expensive. This research is summarized in Table 3. Poor families have fewer resources and more limited access to credit than richer families, which should make the children of poor families less

Table 3
Recent Research on the Effect of Short-Term Credit Constraints on College Enrollment

Study	Outcome	Data	Model Notes	Estimated Effect of Parental Income
Carneiro and Heckman	College enrollment and completion	NLSY79	Non-parametric nonlinear measures of parental income (quartiles) measured in adolescence; controls race, gender, mother's age at birth, family composition, mother's education, and student AFQT.	Parental income has little effect on college enrollment net of test scores; about 5% of white males face a credit constraint to college entry, less for females and blacks, while similar rate for Hispanics. Income effects greater for richer quartiles.
Cameron and Heckman	College enrollment	NLSY79	Uses a dynamic discrete choice model of schooling decisions from age 15–24 to separate the influence of family income, other family background factors, AFQT scores, tuition and labor market opportunities.	Parental income in high school is weakly related to college going and does not explain much of the black-white gap. Parental income may be more important for educational transitions at younger ages.
Cameron and Taber		NLSY79	Estimates response of presumably constrained students to changes in cost of college (proxied by location of a college in the country) and opportunity costs (proxied by wage in low wage industry in county). Estimate 4 models including instrumental variable and structural models.	No evidence of borrowing constraint in any model; small measured effects are not statistically significant.
Keane; Keane and Wolpin	Educational attainment	NLSY79	Structural model of schooling decisions allowing for individual heterogeneity, borrowing limits, parental transfers (inferred from parental education) and labor market work while in school.	Parental transfers increase educational attainment but mainly for children of more highly educated parents. Reducing borrowing constraints has little effect on college going but reduces student labor supply.
Belley and Lochner	College attendance at age 21; High school graduation	NLSY79 and NLSY97	Income averaged over 3 years; controls student AFQT, race, gender, mother's age and education, family structure, and year of birth.	In NLSY79 going from 1st to 2nd quartile (on average about doubling income) = 1.3% increase in college attendance; in NLSY97 going from 1st to 2nd quartile = 2.4% increase in chance of going to college. Effects not bigger for poor. No effects for high school graduation.

Notes: NLSY is the National Longitudinal Sample of Youth; there are two samples, one begun in 1979 and one begun in 1997. AFQT is Armed Forces Qualification Test. I combine papers by the same authors that use substantially similar estimation models and come to the same conclusion.

Studies referenced in this table are: P. Carneiro and J. J. Heckman, "The Evidence on Credit Constraints in Post-Secondary Schooling," *The Economic Journal* 112 (October 2002): 705–734; P. Carneiro and J. J. Heckman, "Human Capital Policy," in *Inequality in America: What Role for Human Capital Policies*, eds. J. J. Heckman and A. Krueger (Cambridge: MIT Press, 2003); S. Cameron and J. J. Heckman , "Life Cycle Schooling and Dynamic Selection Bias: Models and Evidence for Five Cohorts of American Males." *Journal of Political Economy* 106 (1998): 262–333; S. Cameron and J. J. Heckman, "The Dynamics of Educational Attainment for Black, Hispanic, and White Males," The *Journal of Political Economy* 109, (2001): 455–499; S. V. Cameron and C. Taber, "Estimation of Educational Borrowing Constraints Using Returns to Schooling," *Journal of Political Economy* 112, No. 1 (2004): 132–182; M. Keane, "Financial Aid, Borrowing Constraints, and College Attendance: Evidence from Structural Estimates," *American Economic Review* 92 No. 2 (2002): 293–297; M. Keane and K. Wolpin, "The Effect of Parental Transfers and Borrowing Constraints on Educational Attainment," *International Economic Review* 42, 1051–1103; and P. Belley and L. Lochner, "The Changing Role of Family Income and Ability in Determining Educational Achievement," NBER Working Paper No. W13527, 2007.

likely to attend college. However, parental income is correlated with parental and therefore student cognitive skill, so at least part of the gap in college going between children from rich and poor families is presumably accounted for by differences in cognitive skill. Most recent research on borrowing constraints controls for students' cognitive test scores.

There is little evidence that short-term credit constraint reduces college enrollment.[5] However, as the costs of college have increased, the influence of credit constraint may have increased. Belley and Lochner find that the effect of parental income is greater using data from the National Longitudinal Sample of Youth (NLSY) panel that began in 1997, compared to the NLSY panel that began in 1979. Even with the more recent sample, their estimates imply that almost doubling income for families in the poorest income quartile only increases their children's chance of going to college by 2.4 percent.[6] Even with this small effect, the work demonstrates that the effect of parental income can change over time as the factors that influence the importance of money change.

Adult earnings and employment

In my earlier review I noted that research on the effect of parental income on children's adult economic status left considerable uncertainty about the size of the effect but a best guess was that a 10 percent increase in parental income would increase a (male) child's wages by no more than 2 percent per year.[7] More recent studies find positive effects of parental income on adult wages and hours worked but there remains uncertainty about the size of the effect. These studies are described in Table 4. It appears that we still do not have sufficient research to draw strong conclusions about the effect of family income in childhood on adult earnings.

Table 4
Recent Research on the Effect of Childhood Family Income on Adult Income and Employment[a]

Study	Outcome	Data	Model Notes	Estimated Effect of Parental Income
Wagmiller et al.	Employment at age 25	PSID	Uses a latent class model that captures duration, timing and length of exposure to poverty; controls race, gender, family structure, education and employment status of family head.	Never poor = 84.2% chance employed, long-term poor = 65% chance. Families poor some of the time had same probability as never poor.
Ellwood and Kane	Earnings	HSB, NELS88	1 year of parental income; nonparametric nonlinear measure of income (quartiles); controls gender, race, ethnicity, mother's education, other background variables, and tuition costs.	Children from 1st quartile earn 19% less than children from 4th quartile; 3% points of that is due to demographics, 4.2% points to high school achievement, 4.4 % points to schooling and remainder is unaccounted for.
Duncan, Ziol-Guest and Kalil	Earnings, hours worked	PSID	Controls parents' test scores, expectations, personality variable. Separate variables for income in early, middle childhood and adolescence; allows different linear estimates of the effect of income <$25,000 and >$25,000 and other functional forms.	Parental income measured when child <5 years, $1,000 increase in income = 5% increase in earnings for family income <$25,000; additional .05% in earnings when family income >$25,000. No effect when income is measured at ages 6–10 or 11–15. For parental income measured when child < 5 years, $1,000 increase in income = 50 more annual work hours when income <$25,000; 2 more annual work hours when income >$25,000. No significant effect for income measured at later ages.
Shea	Son's income	PSID	Controls father's education, occupation, race; whether son lives in urban area and South. Uses father's union membership as an instrument for income assuming that, for the same job, union members earn more than nonunion workers and union membership is due to luck.	Effect close to zero and not statistically significant.

Notes: PSID is the Panel Study of Income Dynamics. HSB is the High School and Beyond survey. NELS88 is the National Education Longitudinal Study begun in 1988. Highlighted papers indicate some attempt at estimating a causal model.

[a]Mayer's prior review found that the best guess is that 10 percent increase in parental income was associated with an increase in adult wages of less than 2 percent.

Studies referenced in this table are: R. Wagmiller, M. C. Lennon, L. Kuang, P. Alberti, and J. L. Aber, "Dynamics of Family Disadvantage and Children's Life Chances," *American Sociological Review* 71, No. 5, (2006): 847–866; D. Ellwood and T. Kane, "Who Is Getting a College Education? Family Background and the Growing Gaps in Enrollment," in *Securing the Future: Investing in Children from Birth to College*, eds. S. Danziger and J. Waldfogel (New York: Russell Sage Foundation, 2000); G. Duncan, K. Ziol-Guest and A. Kalil, "Early Childhood Poverty and Adult Body Mass Index," *American Journal of Public Health* 99, No. 3, (2009): 527–532; and J. Shea, "Does Parents' Money Matter?" *Journal of Public Economics* 77, No. 2 (2000): 155–184.

Cognitive skill

In my earlier literature review, I concluded that the best evidence implied that doubling parental income was likely to increase cognitive test scores by around 0.10 standard deviations. Two recent papers using different techniques both find that a $1,000 increase in income is associated with an increase in cognitive test scores equal to 6 percent of a standard deviation.[8] These papers are described in Table 5. These more recent results are not necessarily inconsistent with the finding that an exogenous increase in income has a small effect on children's cognitive skills. These two studies consider additional income plus work requirements. Additional parental income with work may have beneficial effects by forcing (and enabling) low-income mothers to put their children into more-structured child care settings, whereas extra cash in isolation may do little to improve children's outcomes.

My updated conclusion is that parental income combined with work requirements may have a nontrivial effect on the cognitive test scores of young children in very poor families. Whether the improvements from an increase in parental income are maintained through the remainder of childhood is unknown.

Implications and issues for future research

When we ask about the relationship between poverty and child outcomes it is not completely clear whether we are asking about the low income of poor families or the complex set of circumstances that results in low income. If we are asking specifically about the relationship between parental income and children's outcomes, a fairly clear answer is emerging: parental income itself has a modest effect on children's outcomes and this effect is not necessarily greater for children from poor families compared to children from rich families.

In the United States today the poverty of a family has many causes and these causes rather than the poverty itself may create problems for children. This means that the policies that we implement to reduce the consequences of poverty on children must be aimed at the causes of parental poverty. If parents were the only source of investment in children, parental income would have a large effect on children's out-

Table 5
Recent Research on the Effects of Income on Cognitive Achievement in Childhood[a]

Study	Outcome	Data	Model Notes	Estimated Effect of Parental Income
Guo	PIAT math and reading, PPVT children aged 5–14	NLSY and CNLSY (over sample of low income families)	Income measures are poverty ratio, years below poverty line and average income all measured birth to when the outcome is measured; controls mother AFQT, education and age, race, gender, and prenatal behaviors.	No effect of income on PIATs in early childhood. For adolescents PIAT-R 4.4 points lower if family always lived in poverty; weak income effects on PIAT-M. Young children in poverty four years before PPVT scored 6.9 points lower on PPVT, adolescents in poverty all years before test scored 4.1 points lower.
Dahl and Lochner	Combined PIAT math and reading scores for children aged 4–14	CNLSY (over sample of low income families)	Instrumental variable model based on changes in the EITC; controls for age, gender, mother's education, AFQT, and marital status.	$1,000 increase in income raises combined reading and math scores by 6% of a standard deviation; effects fade after a year; effects are bigger for younger children and for children from poorer families. Note: all low income sample.
Morris, Duncan and Rodriguez	Achievement = parent or teacher report of child's relative achievement and PPVT; different measures at different ages	Micro data from 4 experimental welfare programs; children 2–15 years old at random assignment	Instrumental variable model based on random assignment into programs. Income data from administrative records and parent survey.	$1,000 increase in income raises achievement by .01–.06 of a standard deviation for 2 to 5-year-olds, very little for older children.

Notes: NLSY is the National Longitudinal Sample of Youth. CNLSY is the Children of the NLSY. PIAT is the Peabody Individual Achievement Test. PPVT is the Peabody Picture Vocabulary Test (-R is Revised and -M is Form M). Highlighted papers indicate some attempt at estimating a causal model.

[a]Mayer's prior review found that doubling parental income increased test scores by around .10 of a standard deviation.

Studies referenced in this table are: G. Guo, "The Timing of the Influences of Cumulative Poverty on Children's Cognitive Ability and Achievement," *Social Forces* 77, No. 1, (2008): 257–287; G. Dahl and L. Lochner, "The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit"; and P. Morris, G. Duncan, and C. Rodriguez, "Does Money Really Matter? Estimating Impacts of Family Income on Children's Achievement with Data from Random-Assignment Experiments," unpublished manuscript, 2004. http://www.gse.uci.edu/person/duncan_g/docs/1doesmoneymatter.pdf.

comes because investments in children would be highly correlated with parental income. A talented child born to bright, diligent, well-meaning parents who are too poor to feed the family might have trouble in school. When the government makes this relatively rare, other investments become more important in determining who succeeds and who does not. When poor children can get enough to eat but often cannot afford to go to school, variations in access to schooling rather than a nutritious diet will predict success. If the government then requires everyone to attend free public school up to age 16, variations in schooling after age 16 will predict success. Thus if the state equalizes most important material and pedagogic investments in children, social and psychological differences between parents and between children will explain a large percentage of the variation is the success of their children. The marginal returns to additional parental income will also fall. In the United States, antipoverty programs have largely focused on income support for poor families. But direct government investment in low-income children has also increased. Over the last 30 years or so in the United States, subsidies for child care, per-pupil expenditures for primary and secondary schooling, and college tuition aid have all increased. Government investments tend to increase total investment and equalize child outcomes.[9] In this context future efforts to reduce the effects of poverty should be aimed at ameliorating the causes of parental poverty.

It is also not entirely clear what the goal of policy might be when it comes to poverty and children, which is to say that we have conflicting ideas about equality of opportunity. Would we be satisfied with policies that resulted in children of parents who were ever poor having the same probability of outcomes as the children of parents who were never poor? Or is the goal to reduce poverty in the next generation by reducing the effect of poverty on the current generation of children and thereby reduce the "cycle of poverty?" These are two very different goals requiring entirely different policies.

The difference in the outcomes of children who were ever in a poor family and children who were never in a poor family are not as different as most statistics on the effect of poverty on child outcomes suggest. A short bout of poverty has little lasting effect on children. Long-term poverty is harmful but it is harmful partly because of endowments. To assure that the outcomes of chronically poor children are equivalent to the outcomes of never poor children would take a set of policies that provided intense services and aids to these children. The income transfer programs that we currently rely on are unlikely to accomplish this goal even if they were made much more generous.

If our goal is to prevent poverty in the next generation by preventing the children of the poor from growing up to be poor, we might be able to accomplish it with a combination of education, training, and services that would maximize the employability of such children. We could also try to reduce the number of children who grow up "at risk" of becoming poor by increasing the number of families that "follow the rules" by, for example, graduating high school, marrying, and working full time. However, neither of these strategies is likely to make much of a dent in poverty in the next generation. A good back-of-the-envelope estimate is that if we could have ensured that every child born in the 1960s or 1970s grew up in a household with both parents and with at least one parent employed full time, the poverty rate for these children once they were adults would decline by 10 percent to 15 percent. That is not a trivial amount, and this figure could probably be increased somewhat with even more energetic and effective government efforts to improve poor children's outcomes. It is impossible to reduce the future poverty rate appreciably by correcting the behavior of current parents because most children who will grow up to be poor do not live in poor dysfunctional families.[10]

The fact that children from "low-risk" families account for most of tomorrow's poor adults is what we call the "poverty prevention paradox."[11] Adults who graduate high school, work full time, and marry have a very low risk of being poor. But having parents who do these things does not assure that their children will do them. Even children from "good" families become poor and there are so many more of these families that the poor families of the future mostly come from these "low-risk" families.

Unfortunately we do not yet have sufficient high-quality research to understand the relationship between parental poverty and children's outcomes. This is partly because we have not defined the question well and partly because we have a paucity of relevant research. In particular, more work is required in five areas:

1. We need more research on the relationship between parental poverty and the factors that cause parental poverty on adult outcomes of children. It is easy to imagine that if poverty influences childhood outcomes such as cognitive skill or behavior problems, it will also affect the adult outcomes that are correlated with cognitive skill or behavior. But these pathways can prove to be very weak.
2. We need to assess the effect of childhood circumstances on a broader range of adult outcomes, notably marital status or adult relationships.
3. We need more causal research on factors associated with poverty such as parental mental and physical heath, marital status (or the complexity of parental relationships), and drug and alcohol use, and we need research that tries to model complex clusters of circumstances. Some research on multiple risk factors tries to do this but these models are in an early stage of development and often lack strong theory.
4. In terms of policy we need more research on the long-term effects of programs and we need better comparisons of effects across programs. We especially need to look carefully at programs in adolescence and programs that specifically are aimed at increasing employability and earnings rather than cognitive skill.
5. We have surprisingly little research on the noncognitive skills developed in childhood that are associated with adult labor market success and relationship stability. ■

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[1]D. Blau, "The Effect of Income on Child Development," *Review of Economics and Statistics* 81, No. 2, (1999): 261–276; G. Duncan and J. Brooks-Gunn, "Urban Poverty, Welfare Reform, and Child Development," in *Locked in the Poorhouse: Cities, Race, and Poverty in the U.S.*, eds. F. Harris and L. Curtis, (Lanham, MD: Rowman and Littlefield, 1998): 21–32. These studies used sibling fixed effects to control unobserved heterogeneity. S. Mayer, *What Money Can't Buy: Family Income and Children's Life Chances* (Cambridge: Harvard University Press, 1997), which assesses the causal effect of parental income, and an edited volume by Duncan and Brooks-Gunn, G. J. Duncan and J. Brooks-Gunn, eds., *Consequences of Growing up Poor* (New York: Russell Sage Press, 1997), emphasize nonlinearity and other methodological problems with previous research and attempt to overcome some of these problems to produce better estimates.

[2]S. E. Mayer, *The Influence of Parental Income on Children's Outcomes*, Report to the New Zealand Ministry of Social Development, 2002. http://www.msd.govt.nz/about-msd-and-our-work/publications-resources/research/influence-parental-income/index.html

[3]All of the research estimates the marginal benefit of income; that is, the benefit of additional income given all the earnings, cash and non-cash transfers that families already have. If the effect of parental income is nonlinear, the effect should be smaller in societies where on average families are richer whether they are richer because of greater earnings or greater cash or non-cash transfers. This makes understanding differences in the estimated effect of parental income across countries difficult.

[4]Mayer, *The Influence of Parental Income on Children's Outcomes*.

[5]See P. Carneiro and J. J. Heckman, "The Evidence on Credit Constraints in Post-Secondary Schooling," *The Economic Journal* 112 (October 2002): 705–734; P. Carneiro and J. J. Heckman, "Human Capital Policy," in *Inequality in America: What Role for Human Capital Policies*, eds. J. J. Heckman and A. Krueger (Cambridge: MIT Press, 2003); S. V. Cameron and C. Taber, "Estimation of Educational Borrowing Constraints Using Returns to Schooling," *Journal of Political Economy* 112, No. 1 (2004): 132–182; M. Keane, "Financial Aid, Borrowing Constraints, and College Attendance: Evidence from Structural Estimates," *American Economic Review* 92, No.2 (2002): 293–297; and M. Keane and K. Wolpin, "The Effect of Parental Transfers and Borrowing Constraints on Educational Attainment," *International Economic Review* 42 (2001): 1051–1103.

[6]P. Belley and L. Lochner, "The Changing Role of Family Income and Ability in Determining Educational Achievement," NBER Working Paper No. W13527, 2007.

[7]Mayer, *The Influence of Parental Income on Children's Outcomes*.

[8]G. Dahl and L. Lochner, "The Impact of Family Income on Child Achievement: Evidence From the Earned Income Tax Credit," NBER Working Paper No. 14599; and P. Morris, G. Duncan, and C. Rodriguez, "Using Welfare Reform Experiments to Estimate the Impact of Income on Child Achievement," unpublished manuscript, Northwestern University, 2004. Both papers use techniques to control unobserved heterogeneity and focus on low-income populations.

[9]S. E. Mayer and L. Lopoo, "Government Spending and Intergenerational Mobility," *Journal of Public Economics* 92, No. 1–2 (2008): 139–158; and S. E. Mayer, "How Did the Increase in Economic Inequality Affect Educational Attainment?" *American Journal of Sociology* 107, No. 1 (2001):1–32.

[10]J. Ludwig and S. E. Mayer, "'Culture' and The Intergenerational Transmission of Poverty: The Prevention Paradox," *The Future of Children* 16, No. 2 (2006): 175–196.

[11]Ludwig and Mayer, "'Culture' and the Intergenerational Transmission of Poverty."

An alien parachutes into economic research on low-income populations

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This article is about how applied microeconomics and applied econometrics have changed over the last 20 to 40 years, as reflected in their conceptualization and evaluation of initiatives to improve the welfare of lower income populations. I define such initiatives broadly to include research on: welfare provisions, poverty alleviation (including family income support), job training, education for poorer families, housing, and community development.

I am neither an economist nor a historian of science. I have not been trained in economic theory or in any of the econometric methods microeconomists use most. And while professional historians can construct a coherent narrative and remain sensitive to the many nonrealized futures that might have emerged, I cannot do this and will doubtless construct a history whose causal links seem more inevitable than they really were. To add to the embarrassment, I have not conducted systematic content analyses of past writings in applied microeconomics or applied econometrics. Reported here are merely personal impressions.

So why read on? Voltaire was fond of criticizing conditions in France. One way he did this was by inventing creatures from outer space who visited France and reported on what they saw there, as in *Zadig* or *Micromegas*. Underlying this version of the comparative method is the assumption that outsiders are particularly able to identify the big assumptions that insiders take for granted. My conceit is to pose as an outsider parachuting into certain parts of microeconomics and econometrics in order to identify and comment on big assumptions and how they have changed.

Substantive changes

In this first section, I discuss what we might call *substantive* issues in poverty and welfare research.

The shift away from labor economics to a broader set of substantive topics

The substantive concerns of applied microeconomists have become broader and less dependent on labor economics and studies of family income support, welfare rolls, job training, and other aspects of the low-end labor market. Up to about a decade ago, issues like the above were dominant, and labor economics was king. More recently, studies of welfare and family income support have declined in frequency, perhaps because of the implementation of Temporary Assistance for Needy Families, expansion in the Earned Income Tax Credit, and decreases in welfare rolls. Studies of the low-end labor market have also become less numerous, especially evaluations of the effectiveness of job training strategies—a major enterprise until about 1995. Individual job training evaluations are now rarer, perhaps in part because they generally failed to find large, sustained effects on subsequent employment or family life. Recent work on job training has gravitated towards synthesizing the policy and methodological lessons learned from past studies in job training.[1]

National policy agendas have now changed, and topics such as education, criminal behavior, and housing are receiving considerably more attention from economists. An institutional change has also accompanied this shift in fields of interest. Many of the economists doing this work are to be found in Schools of Public Policy, Schools of Social Work, and Schools of Education, rather than in Departments of Economics. Microeconomists long ago made the shift into Schools of Business, where they work on issues of labor, organizational, financial, and institutional economics.

The shift to other countries

Poverty research topics have also shifted away from research in the United States to research abroad. Some of the international welfare research has taken place in Western Europe or the Organization for Economic Cooperation and Development (OECD) countries. Much of this work is survey-based and descriptive-comparative, like the Luxemburg Income Study, but some is also experimental.[2] However, the most fundamental changes are taking place in developing countries. The last two decades have witnessed an explosion in the vigor and rigor of developmental economics, as well as in the willingness of developmental economists to structure their work around microeconomic theories and methods. This has weakened the formerly dominant tradition based on designing and evaluating large capital improvement projects. More recent are smaller, locally grounded initiatives often evaluated using randomized control trials. More and more developmental economists are using theories and methods I associate more with microeconomics than macroeconomics. Playing a role in this internationalization of research is probably the internationalization of graduate education in economics over the last 20 years. Many of the economists leading the charge are not American-born, though most were trained in the United States.

The findings of this international work have not gone unnoticed in American welfare policy. One example is the substantive interest in providing poorer families with micro-credit, following the model of the Grameen Bank. Another is using conditional cash transfers to shape the education-relevant behavior of both students and their caregivers. Indeed, the influence of Progresa/Opportunidades from Mexico on the New York conditional cash transfer program is publicly acknowledged.[3] We should hope that North American policymakers continue to scan foreign research for leads to improving American poverty policy, realizing that many specifics will need to be adapted to fit our unique national circumstances.

The multidisciplinary shift

A third change is worth noting—the growing use of study outcomes and substantive explanatory processes that are not traditionally considered economic. Some of this change comes from within economics, particularly from behavioral economics. As a subfield, it mirrors two concerns that other disciplines have long voiced about neoclassical theory: the assumption that individuals have access to, and fully use, all the information potentially pertinent to a decision; and the assumption that profit-maximization is the prime motivator of individual human behavior. Often inspired by cognitive psychology, the many demonstration studies in behavioral economics reveal individuals who consistently fail to respect formal logic when making decisions and who instead rely on many different cognitive shortcuts.

Behavioral analysis also reveals that individuals and groups do not act only, or sometimes even primarily, to maximize their own financial benefit. For example, nearly half of the families offered a housing voucher in the Moving to Opportunity for Fair Housing demonstration program (MTO) did not obtain a lease.[4] While some of this was attributable to delays in finding suitable housing, some was also because families realized that they did not want to leave their social ties and face the possible social isolation of more affluent settings (which often have limited access to affordable transportation).

I do not want to exaggerate the extent to which concepts from other disciplines have already seeped into microeconomics. To most social scientists, economists still speak a strange language, while most microeconomists believe that their training arms them with flexible theoretical and methodological tools that can quickly get them to the heart of any research matter. Bringing knowledge from other fields to bear on economic research requires a great deal of cooperation between individuals from disparate disciplines.

Methodological changes for causal analysis

Methodology is a broad construct and in the space available it must be dealt with extremely selectively. I concentrate here on causal issues. The biggest change over 40 years in econometrics applied to poverty research has been the movement away from statistical control to design control. A large part of this is the movement away from multivariate modeling towards methods that test the impact of one or a few potential causes while making assumptions that are as few and as transparent as possible. In this movement, the role of the randomized clinical trial has been fundamental, and over the last 40 years many more studies have been conducted using this method.[5] Random assignment is also the foundation of Rubin's influential causal model cited by many microeconomists.[6]

Over the shorter period of the last 20 years or so, the main changes have been within the experimental agenda. One change is towards marginal improvements in the theory and practice of randomized experiments. Another is towards exploring nonexperimental design and analysis options that result in minimal selection bias.

The randomized control trial

Randomized control trials crept increasingly into welfare studies during the 1970s and early 1980s and then became quite common.[7] Partly it was because of their impeccable statistical pedigree, and their high reputation in the eyes of many policy officials already acquainted with them through agriculture and medicine. The acceptance was likely advanced by work done by the social policy research organization MDRC, and to that organization's effective strategy for disseminating what it learned about poverty programs and their evaluation. The surge of randomization was also partly attributable to increasing evidence that other nonexperimental strategies were often ineffective in controlling for selection. Simple regression methods came under attack for reasons of hidden bias. Instrumental variables were faulted for their failure to show that the exclusion restriction held in real research examples. And Heckman-type selection models consistently failed to reproduce the results of randomized experiments. As old certainties were undermined, randomized experiments seemed to be one of the few things worth doing in order to determine causality.

Of course, critics of the flight to random assignment emerged, particularly among old guard econometricians and microeconomists with decades of experience using the very methods now being denigrated. They believed their own preferred methods to be more generalizable, since they were not limited to manipulable causal agents and to settings and persons willing to volunteer for random assignment. Their methods could often be used with extant datasets, particularly longitudinal ones, seeming to make them less expensive than launching a randomized trial. They also saw their own model-based methods as producing results that could be validly extrapolated to populations, settings, and times different from those actually studied.[8]

Advocates of randomization took some of these objections seriously. They too were concerned that many individuals do not take up treatments offered them, leading Angrist, Imbens, and Rubin to prove that random assignment can be used as an instrumental variable for estimating the effects of actually receiving a treatment as opposed to being offered it.[9]

Other issues taken seriously by randomization advocates included differential attrition, the proper computation of statistical power, and the need to be specific about the populations to which results could be generalized. Some of these practical problems in implementing or analyzing experimental data were completely or partially solved. Other problems have been acknowledged yet remain unsolved. The most serious of these is the inability to explain why or how effects occurred using conventional statistical methods—except where random assignment is the instrumental variable for examining the effects of a single mediator.

One continuing criticism of randomized experiments is the lack of external validity; in practice, many randomized trials are limited to a single historical time, one set of human populations, one set of social settings, one way of implementing the treatment, and often one way of measuring the outcome. Moreover, for ethical reasons, most experiments are limited to those who volunteer to be in the study and who know of all the treatments to which they could have been assigned. An obvious response is to note that randomized experiments aim to maximize internal validity and, if properly implemented, they do this well. They were not designed to foster external validity, and so why blame them for not doing what they are not meant to do? However, this merely admits the method's limited range. It acknowledges that, by itself, random assignment is irrelevant to selecting the cause and effect variables worth study, to conducting correct statistical tests, and to incorporating a useful set of persons, settings, and times into the study sampling design. It even acknowledges that certain biases might be repeated across a broad array of experiments—for example, all are limited to volunteers.

Another criticism of random assignment studies has to do with causality; it is nearly impossible to identify all the contingencies on which any particular cause-and-effect relationship depends. Thus, it is extremely likely that in other circumstances, similar manipulations of what appears to be the same cause will lead to a different effect. While we need practical new ways of thinking about causal contingency at the research design stage, this only highlights two other deeper problems. One is the mismatch between the manipulability theory of causation that underlies random assignment, and the more explanatory theory of causation to which most philosophers of science and many practicing natural and social scientists adhere. Randomized experiments serve to describe causal connections and not to provide an explanation of them. A very large experiment is still a single case study of a quite particular treatment, under a restricted set of all the possible conditions that might affect the size and direction of its effects.

Although a single causal mediator can be tested using random assignment as the instrumental variable, testing more elaborate causal models is only possible by relaxing current standards of evidence. While such relaxation would be unacceptable to most microeconomists, Manski has questioned the value of unbiased causal findings if achieving them requires selecting populations of persons, settings, and times that have little or no relevance to policy actors.[10] In this view, policy research should first determine who the policy audiences are, and then learn what each needs to know about a given population in a given setting before choosing methods that depend on the kind of questions asked. If they happen to be causal, questions should be addressed using whatever blend of experimental design and statistical manipulation permits capturing all the cause and effect constructs, as well as all the populations, settings, and times that surfaced when formulating the policy problem. Implicitly to be avoided is deliberately limiting oneself to questions that are causal, or to only using experiments to test cause, or to tailoring the experiment's sampling and measurement design solely to accommodate implementing the randomized experiment, come what may. Then, the desire to do an experiment takes precedence over meeting the necessarily contextually embedded causal knowledge needs of specific policy audiences. The cart precedes the horse.

I suspect that very few young microeconomists would be open to this criticism, so strong is the current ethos to privilege internal over external validity. Logically, it is correct that one should be sure of a causal connection before seeking to generalize it. But in the real world, trade-offs are common and one might benefit from asking: If a particular randomized experiment marginally increases certainty about cause, is this worth the more limited generalization that often results? Of course, no perfect answer to this question is possible, and enlightenment comes more from examining specific instances of the trade-off than from abstract ruminations about it. Nonetheless, I suspect that forging any kind of a compromise with younger microeconomists will not be easy. Without randomization, they can rarely be sure of no or minimal causal bias; and Manski's bounds require more assumptions and create less "certainty" than many applied microeconomists are now willing to accept. Still, it is worth having a debate about the conditions under which internal validity is more important than external validity in concrete research applications. Before this can happen, at least two preconditions would be helpful; clarification about the many meanings of external validity, and the willingness of applied microeconomists and econometricians to consider a quite broad range of alternatives to the randomized control trial.[11] I report on some of these alternatives below.

Nonexperimental alternatives

Most microeconomists interested in poverty came to believe early in the virtues of various forms of causal modeling, that they were more flexible, comprehensive, and theory-linked than a tool like random assignment. Also, they were attentive to their econometrician colleagues who, from the 1970s to the later 1990s, pursued a very visible agenda to discover ways of justifying causal inferences from observational data that, to varying degrees, required substantive theoretical assumptions. One direction this agenda took was towards the use of instrumental variables, given convincing proof that strong instrumental variables can reduce all selection bias on observed and unobserved variables. This led to countless and often unfruitful debates about the dependability of individual causal conclusions, especially about whether the restriction assumption was met. Other econometricians took to linking instrumental variables to various other assumptions designed to deal with selection bias. The foremost theorist of such selection models was James Heckman who, between about 1980 and 2000, created many such models in hopes of discovering a general theory of how to achieve unbiased causal inference from observational studies. However, this agenda failed to fulfill its promise, and the considerable excitement about it had abated by the turn of the century, when many microeconomists came to feel the need to explore a different and more modest causal agenda. First, they advocated doing more randomized experiments. When these could not be carried out, they next turned to the causal techniques discussed below that are of limited scope when compared to the earlier econometric agenda. Nonetheless, they were gradually able to develop a toolbox with many different tools for causal design and analysis, each limited in its range of application but collectively covering many situations where causal knowledge is needed.

One causal tool that emerged was the *natural experiment*. These are like randomized experiments in that the potential cause is considered to be exogenous to the processes otherwise generating variation in the outcome. Taking advantage of exogenous variation had long been a staple of interrupted time-series studies such as those on the effects of natural disasters or of macroeconomic shocks. However, natural experiments are intrinsically opportunistic and cannot be used often enough to function as a major knowledge-building tool about human behavior, especially when one needs to know about the effects of human-controlled interventions. This is because first impressions of exogeneity can be deceiving; some interventions occur in order to respond to prior performance. When causal agents are embedded in ongoing social or economic systems, all claims to be studying a natural experiment require close scrutiny.

The next observational study discovery was of the *regression-discontinuity design (RDD)*. As I have described elsewhere, this was discovered in the 1970s but rarely used in economics until the 1990s.[12] It is applicable when an intervention is offered to all those on one side of a quantitative cutoff score that has been used to determine treatment assignment. This design came to be seen as an unbiased causal tool for use in the many contexts where allocation to a scarce resource is based on some quantified need, merit score, birthdate, or on a "first-come, first-served" basis. By now, RDD is in every younger microeconomist's toolbox. However, it too is limited in scope, as it requires treatment allocation to occur at a single point on an assignment variable where the causal impact is estimated. While some interventions are allocated this way now, and more could be in the future, it is not yet the norm in welfare policy.

Much more flexible is *matching*, trying to use a study's sampling and measurement design to mimic the initial group comparability that random assignment achieves. With random assignment, the comparability is on all observed and unobserved variables, whereas with matching it is only on observed covariates. The matching agenda in applied econometrics has been extensive, including comparative work on different ways of creating matches. But the most visible effort has been with propensity scores, primarily developed by the statistician Donald Rubin.[13] Propensity scores entail constructing multi-item composites that predict selection into treatment. The point is to balance the propensity scores across the nonequivalent populations being examined and then adjust the study outcomes for the influence of the scores. Balance entails that all scores in the treatment and comparison group fall within the same range so that they totally overlap and create an area of common support. However, since propensity scores do not explicitly handle hidden variable bias, they cannot guarantee removing all selection bias and have received a mixed reception among applied econometricians and microeconomists. One reason for concern comes from the many within-study comparisons in job training that compare the results from a randomized trial to those from a statistically adjusted quasi-experiment with the same treatment group, but with a comparison group that was nonequivalent prior to matching. Propensity scores did not reproduce the experimental results in any of these comparisons.[14]

However, in empirical comparisons outside of job training, propensity score studies have recreated experimental results in several specific contexts.[15] A strategy for better propensity score practice is emerging, but is not yet definitive. This strategy involves first using theory, literature reviews, or pilot studies to develop several different selection models that seek to mimic the unknown true selection process. The key variables in each model are then measured reliably prior to treatment. Measures from other domains are also included among the covariates in order to increase the chances of obtaining measures that correlate with any individually unmeasured variables that might account for some part of the selection process that is correlated with the outcome. Only after covariates have been collected that seek to index *both* the true selection model *and* other forces with no known link to selection should the propensity score be computed. As the job training literature shows, propensity scores do not work universally. However, when carefully done in large sample work, they have sometimes been shown to nearly reproduce experimental causal estimates, and we now also have some idea of the conditions under which they are most likely to do so. So propensity scores rightly deserve to be in many microeconomist's causal toolbox, albeit for use with great care. The current dirty secret, though, is that propensity scores have rarely done better than careful simple regression analysis with the same covariates. The empirical case for propensity scores is therefore limited and mostly derives from the fact that they do not depend on extrapolation. Causal influence is instead limited to where estimated propensity scores overlap across the treatment and comparison groups.

Another causal tool is *interrupted time series*. Economists have used this almost exclusively with archival data, but it can also be used with original data collection at least in single case studies, as has been done in studies of special education.[16] Interrupted time series is the obvious methodology for evaluating changes in laws and regulations, and was recently used to evaluate the impact of No Child Left Behind at both the state and national levels and also at the national level through comparing all public school students with students in two comparison series (all Catholic private schools and all non-Catholic private schools).[17] In job training, employment and wage data have been used over many quarters both prior and subsequent to an intervention.[18]

With a single interrupted time series design, causal interpretation is only possible when the pre-intervention functional form is very clear, the effect is very large and occurs immediately after the intervention, and the time intervals are quite close to each other. These are daunting requirements. In most interrupted time series work, one or more comparison series are necessary in order to test whether intercepts and slopes change differentially from before to after an intervention. Such comparison series will not always be forthcoming, especially with changes at the national level. Comparison series can also be constructed from non-equivalent dependent variables rather than non-equivalent comparison populations. Ross et al. showed this in evaluating the British Breathalyzer data where traffic deaths and serious injuries were assessed during the hours when pubs were open and immediately after, and then compared to deaths and injuries during the hours when pubs were closed and thus drinking and driving were less prevalent.[19]

Although it would be an overstatement to claim that comparative interrupted time series studies are common in applied microeconomics (except in some areas of finance), I suspect that it is only a matter of time before they experience a renaissance like that of RDD. At present, interrupted time series is a very minor tool in the applied microeconomist's toolbox, but its potential for future utilization is high, albeit in a restricted set of circumstances where new laws or regulations are passed, where relevant administrative data exist (as is increasingly the case), where a no-treatment comparison series is available, and where no immediate policy answer about effectiveness is required.

Conclusions

In my view, we have witnessed over the last 20 to 40 years a shift in the substantive concerns of applied microeconomists interested in low-income populations. The dominance of labor economics and issues of unemployment, job training, welfare, and family income support has now ended. There is growing research interest in education, housing, criminal behavior, community and neighborhood development, and in policy lessons from other rigorously-studied countries. Also beginning to enter the field are cognitive, motivational, and social-network insights from qualitative sociologists who study poorer families at the ground level, plus an interest in individual and collective outcomes and explanatory concepts that most often come from sociology and individual and developmental psychology. Applied microeconomics seems broader today than 20 or 40 years ago, and slightly more integrated into the other social sciences.

The shift in method preferences has been considerable. Forty years ago, the use of instrumental variable analyses, complex selection modeling, and substantive modeling using simple regression were rampant. Now, they are noticeably less dominant, each subject to both theoretical and empirical attack. They have been partly replaced by the growing use of randomized control trials, and by research into improving the design, statistical power, and statistical analysis of those experiments. Also apparent is growth in observational study methods based on approximating the structure and logic of randomized experiments. Especially notable here is growth in RDD, propensity scores, and other matching methods, but we should also remember natural experiments and interrupted time series methods. The modern applied microeconomist now has a better provisioned causal tool chest than ever before. Each of the newer tools is limited in when and where it applies, but collectively the range is quite large. Microeconomists seem to have given up on developing a general theory of selection control. It is as though the field went from searching for one big arrow to fill a small but elegant quiver to requiring many causal arrows that collectively require a larger and certainly more ungainly quiver.

I suspect there are still some issues with which applied microeconomists interested in poverty and welfare need to struggle. One is the conflict between the manipulability theory of causation that underlies the field's thinking about causation, and the contrary belief that this particular theory is not as useful or comprehensive as other theories of cause. Science has always put a higher premium on the identification of novel causal mechanisms with broad applicability, compared to the identification of a specific link between a particular treatment and a particular outcome. The second challenge concerns the value of placing so much emphasis on achieving the last bit of uncertainty reduction about internal validity if this compromises external validity. In pure logic, internal validity is a necessary condition for external validity. However, practice in applied microeconomics is more concerned with satisfying the knowledge needs of specific consumers, especially policymakers, who tend to be less interested in the compulsive elimination of all uncertainty about cause, and more interested in learning whether a given causal result applies to the specific groups, settings, and times for which they are responsible. Applied microeconomics currently seems willing to tolerate many losses in external validity. But is this the right trade-off? In the case of experiments, this trade-off may mean that causal conclusions are limited to volunteers who are aware of the different treatments. In some observational studies, this trade-off favors gaining causal knowledge about discrete entities rather than about combinations of factors that more strongly affect human behavior, such as the combination of housing, family, neighborhood, and school factors in child and youth studies. The current causal strategy in applied microeconomics seems like a way to identify mostly small or null effects, and like an invitation to study convenient causal agents instead of serving as a test-bed for truly bold thoughts.■

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