INTENDED AND UNINTENDED EFFECTS OF THE WAR ON POVERTY: WHAT RESEARCH TELLS US AND IMPLICATIONS FOR POLICY

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Abstract

During the mid-1960s, the United States adopted a series of cash and in-kind transfer programs, as well as human capital investment strategies, as part of the War on Poverty. A number of other programs were first proposed as part of this "war" but were not implemented until the mid-1970s. These programs had noble goals: to increase incomes at the bottom of the income distribution, reduce poverty, and improve nutrition, heath, and human capital. However, various features of the programs also had the potential to produce unintended consequences: for example, means-tested programs can discourage work. In this paper, we comprehensively evaluate the main War on Poverty programs that were aimed at the low-income nonelderly population along with several follow-on programs. We focus on both intended and unintended consequences, drawing on the most compelling causal evidence. We conclude with a series of lessons learned and questions that are outstanding. © *2015 by the Association for Public Policy Analysis and Management.*

INTRODUCTION

In his 1964 State of the Union address, President Lyndon Johnson declared an "unconditional war on poverty" and proceeded to work with Congress and his administration to implement a series of cash and in-kind transfers, as well as human capital investments, designed to reduce absolute poverty. The Equal Opportunity Act (1964), the Food Stamp Act (1964), the Elementary and Secondary Education Act (ESEA) (1965), the Higher Education Act (HEA) (1965), the Social Security Act (1965), the Housing and Urban Development (HUD) Act (1965), and the Child Nutrition Act (1966), among others, introduced or expanded a cascade of programs targeted to low-income families, children, and youth, with the aim of providing basic income support (Aid to Families with Dependent Children [AFDC]); nutrition supports (Food Stamps, the School Breakfast Program [SBP]—adding to the National School Lunch Program [NSLP]); subsidized or free health care (Medicaid and Medicare); subsidized housing; early childhood education (Head Start); compensatory K-12 education funding (Title I); subsidized higher education through grants, loans, and work study; and job training (Job Corps).

Most of the initial War on Poverty programs targeted to nonelderly adults and children continue five decades later, although often with a new name or having undergone substantial reforms. The core programs from the 1960s have been joined by several others initiated in the mid-1970s and beyond that are central to the safety-net system. These later additions—many of which had been considered when the War on Poverty was launched or were extensions of original programs—include the Special Supplemental Nutrition Program for Women, Infants and Children (WIC) in 1973, Supplemental Security Income (SSI) in 1974, the Earned Income Tax Credit (EITC) in 1975, the Child Care Development Fund (CCDF) in 1990, and the State Children's Health Insurance Program (SCHIP) in 1997.

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In marking the 50th anniversary of the War on Poverty, various efforts to take stock of the effectiveness of individual programs or the safety net as a whole illustrate the range of views held by policymakers and the public at large. The War on Poverty "progress report" compiled by the President's Council of Economic Advisors (CEA) in 2014 emphasized the decline in poverty in the past five decades when the value of both cash and in-kind transfers are taken into account, while still acknowledging that economic hardship is far from being eliminated with nearly 50 million persons counted among those officially considered poor as of 2012. The CEA (2014) assessment further emphasized the poverty-reducing effects of programs such as the EITC and SNAP (Supplemental Nutrition Assistance Program, the renamed Food Stamp Program), while concluding that there is little evidence that safety-net programs have unintended consequences such as substantial reductions in work effort. A contrasting view is found in the House Budget Committee's (2014) appraisal of the War on Poverty, which emphasized the existence of a multiplicity of programs that create a "poverty trap," leading to hundreds of billions of dollars in spending with unclear benefit given that the official poverty rate has remained little changed over five decades. The report calls attention to the disincentives to work inherent in means-tested programs and cites supporting empirical evidence.

The goal of this paper is to assess what we know about the effects of the key War on Poverty programs targeting the nonelderly population on individual and family decisionmaking and well-being. Did the programs have their intended effects in reducing poverty and promoting self-sufficiency? Were there unintended consequences that resulted from the programs? And did other factors hinder or reinforce the policy changes? Our primary interest is in behavioral impacts with respect to economic outcomes such as employment and earnings, income and poverty, and program participation. We are also interested in the effects on other aspects of family decisionmaking such as investments in education, marriage, childbearing, and investments in children. Given these outcomes of interest, we center our attention on antipoverty programs that target children, youth, and working-age adults, thereby omitting programs that exclusively target the elderly or the behavioral consequences for the elderly or programs that are aimed at both the nonelderly and the elderly. We also limit ourselves to programs that have federal oversight and fairly uniform rules. Thus, we omit some Office of Economic Opportunity (OEO) funded grant programs (e.g., family planning grants or grants for community health centers).

In particular, we focus on the set of antipoverty programs featured in Table 1, which comprise the core programs initiated under the Johnson administration 50 years ago, as well as the other key components of the safety net for the nonelderly introduced or expanded in subsequent decades. Table 1 lists the programs and when they were founded, their target population, and their goals. Table 1 also describes whether the program is an entitlement, which must cover all those who are eligible, or is restricted in the number it can serve. (Table A1 in the Appendix also shows, for each program, 2013 federal outlays, annual aggregate participation as of 2013, and the typical monthly benefit amount, when relevant.[1]) Viewed together, the programs we cover in this review vary widely in terms of how broadly targeted they are, how generous their eligibility rules are, take-up, and whether they are an entitlement or not. Together, federal outlays for the set of programs listed in Table 1 exceeded $600 billion in federal fiscal year (FY) 2013.

To set the stage for our review, we begin in the next section with a brief discussion of the basic economic model that posits a set of expected behavioral effects of the

[1] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Table 1. Major antipoverty programs serving the nonelderly population and associated evaluation strategies.

Program	Target population	Description	Sources of variation for causal analysis: Experimental evaluation	Variation in program rollout	Federal, state, or local policy variation	Exogenous assignment and other variation
			(a) Means-tested cash transfers			
AFDC (1935–1996)/ TANF (1996–present)	Low-income families with dependent children	Entitlement (AFDC)/Funding limited (TANF): cash transfers and other supports conditioned on work requirements	• AFDC: State waiver based demonstration projects	–	• 1996 PRWORA • AFDC: benefit level • TANF: benefit level, income disregards, benefit reduction rate, sanctions, time limits, family cap	• Birth of twins versus singleton
SSI (1974–present)	Low-income aged, blind, and disabled children and adults	Entitlement: cash transfers to eligible population	–	–	• 1990 Zebley ruling • 1996 PRWORA • State supplements	• Regional differences in effect of oil price shocks
EIC or EITC (1975–present)	Low- and moderate-income working families with (or without) children	Entitlement: refundable tax credit that varies with earnings and the number of dependent children	–	–	• 1990s expanded eligibility (varied by number of children) • State EITCs	• Calendar timing of lump sum payment

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Table 1. Continued.

Program	Target population	Description	Sources of variation for causal analysis			
			Experimental evaluation	Variation in program rollout	Federal, state, or local policy variation	Exogenous assignment and other variation
(b) Means-tested in-kind transfers						
Food Stamps (1964–2008)/ SNAP (2008–present)	Low-income (<130% of poverty) households	Entitlement: Voucher-based subsidy for food	• Demonstration project linking SNAP benefits to consumption of fruits and vegetables	• County variation in program rollout	• Outreach spending • Immigrant eligibility post PRWORA • Asset requirements • Eligibility determination • Use of EBT • Recertification period	–
Child Nutrition Programs (1946–present)	Low-income (<185% of poverty) school-age children	Entitlement: free- or reduced-price breakfast and lunches provided through subsidies to schools	• Breakfast in the classroom and universal free breakfast demonstration projects	• 1962 policy changes	• State school participation mandates • School districts with universal provision or breakfast in the classroom	• Day-to-day variation in student participation • Summer versus school-year participation

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Table 1. Continued.

Program	Target population	Description	Sources of variation for causal analysis			
			Experimental evaluation	Variation in program rollout	Federal, state, or local policy variation	Exogenous assignment and other variation
WIC (1973–present)	Low-income (<185% of poverty) pregnant women, lactating women, and infants and children under age 5	Funding limited: voucher-based subsidy for nutritious food and infant formula; nutrition education; breastfeeding support	–	• County variation in program rollout	–	• Changes in location of WIC clinics
Medicaid (1965–present)/ SCHIP) (1997–present)	Low-income (varies by state) families with dependent children and blind or disabled or elderly adults	Entitlement (Medicaid)/funding limited (SCHIP): subsidized health care	• Oregon lottery-based Medicaid expansion	• State variation in Medicaid rollout	• 1980s and 1990s state Medicaid and SCHIP expansions	• –
Housing Assistance (1965–present)	Low-income individuals and families (including elderly and disabled)	Funding limited: public housing and voucher-based rental subsidies	• MTO demonstration project	–	–	• Housing project demolitions • Opening of waiting lists
CCDF (1990–present)	Low-income working families with children less than 13 years old	Funding limited: voucher-based subsidies for child care services	• IL eligibility expansion demonstration • WA copayment demonstration	–	• State funding • Income eligibility thresholds • Co-payments • Recertification period	–

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Table 1. Continued.

Program	Target population	Description	Sources of variation for causal analysis: Experimental evaluation	Variation in program rollout	Federal, state, or local policy variation	Exogenous assignment and other variation
(c) Other antipoverty programs focused on education and job training						
Head Start (1965–present)/ Early Head Start (1994–present)	Low-income (<poverty) children ages 0 to 5	Funding limited: comprehensive child development services	• Oversubscribed program sites	• County variation in funding, in part because of initial federal technical assistance	–	• Sibling differences in participation
Title I of ESEA (1965–present)	Children in grades preschool to 12 in schools with high concentrations of children in poverty	Funding limited: funding to local education agencies for compensatory programs	–	–	–	• School-level poverty rates determine Title I schools
Student Financial Assistance for Higher Education (1965–present)	Low-income postsecondary students without a bachelor's degree	Funding limited: grants and subsidized loans	–	–	• 1992 change in federal financial aid formula	• Pell Grant funding formula includes family size
Job Corps (1964–present)	Low-income youth age 16 to 24	Funding limited: academic and vocational training	• Oversubscribed program sites	–	–	–

Source: Authors' analysis.
Notes: Program starting year refers to year when benefits were first paid or program services were first offered. –, not applicable; EBT, electronic benefit transfer.

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War on Poverty transfer programs and human capital investment initiatives. We use this framework to focus on a set of key outcomes of interest for the discussion that follows. Next, we highlight the analytic challenges associated with quantifying the impacts of the War on Poverty programs, given that, in most cases, researchers must rely on observational data from which it is difficult to make inferences about causal impacts of the programs. Analysts must also confront the need to control for other factors that were changing at the same time and, in that regard, the concomitant changes in demographics, the economy, and other social forces are important to acknowledge. With that background, we then turn to a review of the evidence of program impacts, both in terms of intended and unintended effects, first for the shorter-term outcomes that we might expect to see from cash and in-kind transfer programs—AFDC (now Temporary Assistance for Needy Families or TANF), SSI, EITC, Food Stamps (now SNAP), child nutrition programs, WIC, Medicaid and SCHIP, housing assistance, and subsidized child care—and then for the shorter- and longer-term impacts targeted by human capital investment strategies—Head Start and Early Head Start, Title I funding for elementary and secondary education, targeted higher education financial aid, and Job Corps. Given that there is a vast body of scholarship examining the major social safety-net programs, we give more weight in the discussion to recent research that has endeavored to employ rigorous statistical methods to make causal inferences. (The citations for these causal studies, discussed in the context of each safety-net program, are provided in Appendix B separately by program.[2]) As we will note, however, there are a number of areas where research has not fully explored—using convincing methods—the behavioral or other consequences of the programs we consider. Nevertheless, we seek to draw out, in the concluding section of the paper, a set of broader lessons for policy that come out of our review.

FRAMEWORK FOR CONSIDERING INTENDED AND UNINTENDED CONSEQUENCES OF ANTIPOVERTY PROGRAMS

Cash and in-kind transfer programs targeted to low-income nonelderly individuals and families are designed to provide support for basic needs, setting a floor on living standards. Such transfers recognize that individuals, sometimes through no fault of their own, may not have the ability to earn sufficient income to maintain a basic standard of living to support themselves or their dependent children. Cash transfers allow the recipient to allocate funds to those goods and services they choose, according to their own preferences. In-kind transfers constrain the recipient to consume the category of goods—food, housing, medical care, child care—provided directly or through a voucher. Both cash and in-kind transfer programs aim to raise an individual's consumption above what it would have been otherwise, with the goal of improving well-being (and thereby increasing social welfare). This may be especially important for families with young children, when low resources, poor nutrition, or limited access to medical care may have both short- and longer-term consequences for child development, for success in school, and for other teen and adult life course outcomes. Often the expectation is that the cash or in-kind supports will be transitory, available for a time until the individual is able to meet their needs through employment or other private supports (e.g., private transfers from family or friends).

A perennial challenge with means-tested programs is that, depending on their design and the individual's earnings potential, they can create a disincentive to

[2] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

work. The disincentive to work is evident upon considering the standard economic model of utility-maximizing behavior. In that framework, individuals are assumed to compare the utility of receiving cash or in-kind transfers or both, alone or in combination with varying levels of work effort, and to choose the combination of work and transfers that maximizes utility. Depending on the generosity of the cash and in-kind supports and the implicit tax rate programs impose on those supports as work effort increases (i.e., the amount that benefits decrease for each dollar of earnings or the benefit reduction rate), some individuals with low earnings potential will choose to receive transfers. Moreover, these individuals will typically work less than they would have in the absence of the transfer programs and may maximize their utility when they do not work at all.

The disincentive to work is particularly salient when the tax rate on benefits is 100 percent, as it was prior to 1967 for the AFDC program.[3] With programs such as the EITC, the marginal tax rate can even be negative or be a subsidy (during the phase-in range), zero (during the plateau), or positive (during the phase-out range), depending on the individual's family earnings (or adjusted gross income, AGI). The marginal tax rate can exceed 100 percent for programs such as Medicaid where the benefit remains fixed until the maximum income threshold is reached and the recipient is no longer eligible for any benefits, creating a notch in the budget set.

The relationship between earnings and marginal tax rates for low-income individuals can be particularly complex when the structure of multiple means-tested programs is taken into account. As an example, Congressional Budget Office (CBO) (2012, see especially Figure 2) illustrates how the marginal tax rate for a hypothetical single-parent family with one child varies with the level of earnings. When transfers and taxes are considered, this figure looks like a series of step functions up and down, with many cliffs. Furthermore, the period over which earnings matter and how income is counted for determining program eligibility can vary by program, making the choice of earnings levels complex even if an individual faces a constant wage offer and the ability to freely choose work hours. Nonetheless, there is evidence that some thresholds are quite salient while others are not (e.g., Saez, 2010 finds clear evidence that individuals bunch at earnings levels right around where the EITC phase-in ends but not at other kinks in the budget set).

According to the predictions of the economic framework, other features of means-tested programs may also affect behavior. In particular, when eligibility is based on a characteristic under an individual's control, unintended behavior (from the perspective of the policymaker) may result. Programs available only to unmarried parents could create a disincentive to marry (or an incentive to divorce) or an incentive to bear children outside of marriage. Programs that are available only to those without assets above a given threshold may create a disincentive to save or accumulate other assets (e.g., automobiles). Programs available only to those who are disabled may create a disincentive to recover and return to work after a disabling event. In addition, through impacts on adult time use, family income, and other investments in children, means-tested programs may affect aspects of child well-being. If means-tested programs allow a parent to stay home and care for a young child, there may be developmental benefits if the quality of the parental time investment exceeds the quality of the time spent with other potential caregivers.

[3] After that year, the implicit tax rate in the AFDC program was reduced to 67 percent and an earned income disregard of $30 per month was introduced. Thus, after the first $30 in earnings, a recipient on AFDC gained $1 in net income for every $3 in earnings. Under federal waivers implemented in the 1990s, states further experimented with reforms to "make work pay" by increasing the earned income disregard, reducing the marginal tax rate on additional earnings and extending the time period over which earnings were disregarded (Grogger & Karoly, 2005).

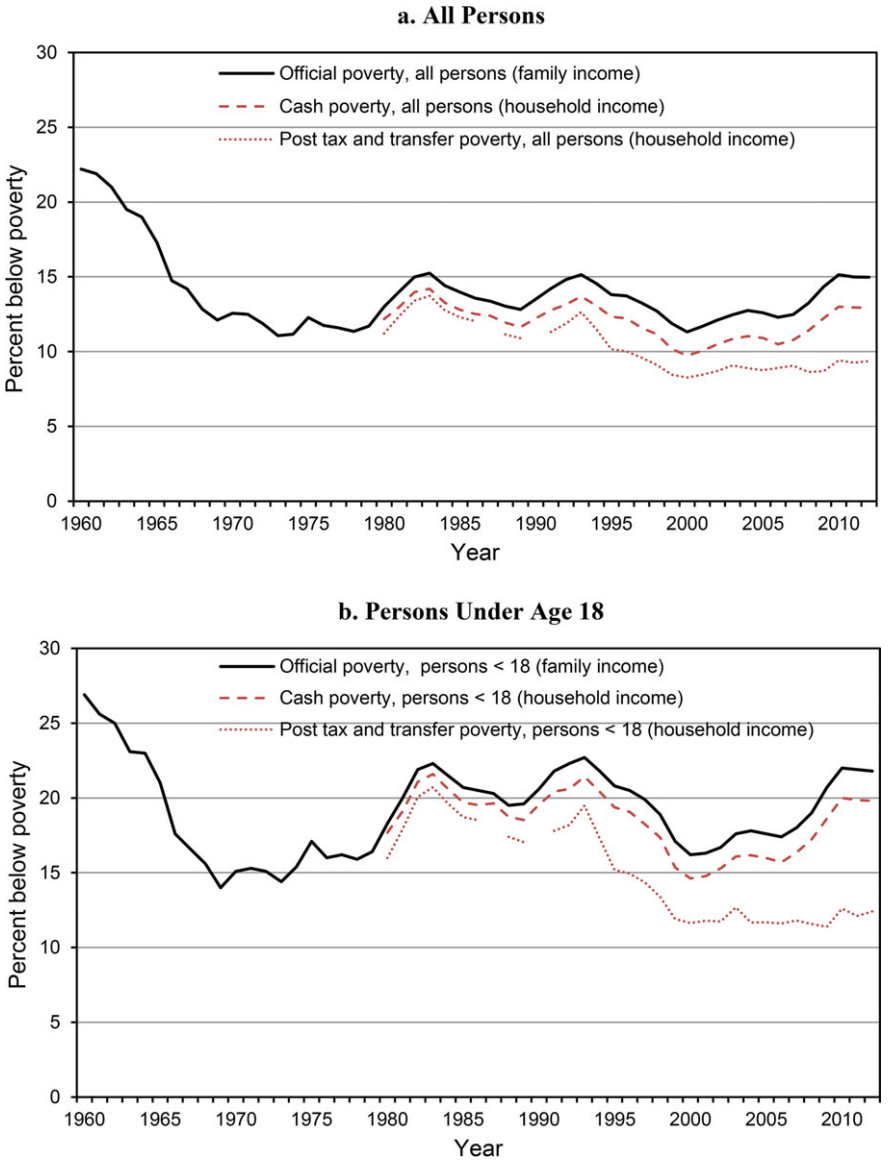
Conversely, high-quality early learning programs targeted to low-income children may improve children's developmental trajectories compared with less-structured care options. Means-tested cash and in-kind programs may also benefit children by increasing the resources families have to invest in child nutrition, health, and other developmental supports.

As a result of concerns about some of these unintended consequences, U.S. welfare policy has shifted in the last several decades from providing primarily unconditional transfers for targeted groups to incorporating transfers that are conditioned on work or other gainful activity. The EITC follows this model in making the tax credit available only to those with nonzero earned income. The welfare reform experiments in the 1990s that culminated in the substitution of the TANF program for AFDC under the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) likewise tied the receipt of cash benefits to work or other qualifying activities, with sanctions that applied to those who did not comply with the requirements (Grogger & Karoly, 2005). Furthermore, the PRWORA reforms instituted lifetime time limits on the receipt of TANF benefits to eliminate long-term dependency. Nevertheless, a number of the means-tested transfer programs that comprise the current social safety net continue as transfers that are not time-limited or conditioned on work requirements or other qualifying activities (e.g., SNAP for most individuals). And both conditional and unconditional transfer programs may alter the calculus of individual decisionmaking relative to a world where such transfer programs do not exist.

The key empirical issue is whether such behavioral consequences exist and the magnitude of the effects. In terms of intended effects, there is interest in understanding whether antipoverty programs serve to raise incomes or resources net of transfers and taxes, or to lower the incidence of poverty, or to do both. Other dimensions of well-being targeted by safety-net programs include improved nutrition and health for children and adults, better parenting for adults, and better educational performance for children. In terms of unintended effects, of paramount interest are the effects of antipoverty programs on work effort, typically measured as employment, hours worked, and earnings. Other domains of decisionmaking that could be affected include savings, marriage/divorce, cohabitation, and childbearing. Programs targeted at increasing human capital have educational attainment and, increasingly, cognitive and social-emotional development as goals. However, human capital programs can also have unintended effects. These unintended consequences can include crowd-out of private expenditures or leaky targeting of public funds. Also, they can give rise to cases where there is public provision of lower quality goods and services than would be provided by the private sector.

CHALLENGES IN DETERMINING THE IMPACTS OF THE WAR ON POVERTY PROGRAMS

According to the official measure of poverty produced by the Census Bureau based on pretax money income (see panel a of Figure 1), the U.S. poverty rate as of 2012 stood at 15.0 percent, compared with 19.5 percent 50 years earlier in 1963, a rather modest showing for a five-decade-long battle against poverty (DeNavas, Proctor, & Smith, 2013). The poverty rate for children under age 18, according to the official measure (see panel b of Figure 1), also shows virtually no net improvement over the past 50 years, starting at 23 percent in 1963 and measuring 22 percent in 2012. However, even setting aside the confounding issues with the business cycle, the existence of a number of flaws in the official poverty measure distorts the long-term perspective. As discussed in great detail in Citro and Michael (1995), the official poverty measure includes only pretax and pre-transfer cash income; it does not account for in-kind transfers and net taxes (including tax credits). It also fails to



Sources: U.S. Census Bureau (2014), Tables 2 and 3, and Bitler and Hoynes (forthcoming).

Figure 1. Official U.S. Poverty Measure and Alternative Poverty Measures: 1960–2012.

account for the value of employer-paid health insurance; for various necessities such as fixed costs of working (e.g., child care), housing costs, or required out-of-pocket medical expenses; and for differences in the cost of living across places and family types.

The new Census supplemental poverty measure (SPM), available from 2009 onward, attempts to incorporate all of these components to produce a more accurate measure of lack of resources. Wimer et al. (2013) extend the SPM resources and absolute poverty thresholds backward in time and conclude that this poverty rate,

measured consistently over time, declined from 26 percent to 16 percent between 1967 and 2011.[4] Meyer and Sullivan (2013) estimate an even larger 26 percentage point decline in a consistent consumption-based poverty measure between 1960 and 2010.

Figure 1 plots another poverty rate series produced by Bitler and Hoynes (forthcoming) both for all persons—panel a—and for children—panel b. The series starts in 1980 and uses the same method to measure cash poverty as the official rate, but pools income at the household level and compares it to the poverty threshold for the household size (rather than the family level, as is done for official poverty). A second posttax and posttransfer poverty series, also calculated with income pooled at the household level compared to household size thresholds, accounts for the value of in-kind benefits and the net effects of taxes (including tax credits) in the income measure. As seen in Figure 1, accounting for income pooling within the household rather than the family lowers the cash poverty rate slightly, relative to the official measure. More importantly, the posttax and posttransfer measure demonstrates the substantial additional poverty-reducing effect of in-kind transfers and taxes that are not counted in the official measure. The bottom line is that while official poverty shows an increase between 1980 and 2012 of 2 and 4 percentage points among individuals and children, respectively, the measure that fully accounts for the safety net shows a decline of a similar magnitude.

Another approach for assessing the impact of specific means-tested transfers on poverty is to use individual data on incomes, including cash and in-kind transfers, to compare poverty rates (using a comprehensive measure such as the SPM) with and without the contribution of the cash or in-kind transfers. (See, e.g., Bitler & Hoynes, forthcoming.) However, neither such static comparisons nor the analyses of poverty trends discussed above reveal whether antipoverty programs can take credit for the measured drop in poverty in the last 50 years—as modest or large as it may be—or whether they can carry the blame for the failure to bring the poverty rate down even further. Neither approach tells us what the poverty rate would have been in the absence of a given antipoverty program or the entire safety net because they do not account for possible changes in behavior given the incentives or disincentives that antipoverty programs create. Rather, the appropriate counterfactual, one that is not observed, is what the poverty rate would have been if individuals had made their decisions about work and other life choices in a world without the program or the combined safety net.[5]

Evaluating the Impact of Specific Antipoverty Programs

In order to estimate the causal impact of individual components of the War on Poverty, researchers have employed various approaches to create the relevant counterfactual. Table 1 summarizes the evaluation strategies used to measure the causal impact of each of these War on Poverty programs based on our literature review. In many cases, evidence comes from one or more randomized control trials (RCTs)

[4] One important critique of backward anchoring the current thresholds from the SPM is that families had a very different distribution of spending on necessities such as food, clothing, and shelter historically compared to the present and another concern pertains to use of the CPI-U as a valid measure of inflation through time. Fox et al. (2015) generate a historical SPM poverty series, which adjusts for changing consumption patterns and find that the poverty rate fell from 19 percent in 1967 to 16 percent in 2012, a more modest change.

[5] A further challenge arises when using survey data either to look at poverty or income due to the level of underreporting of various income sources that is widespread and has become worse across time (e.g., Meyer, Mok, & Sullivan, 2009).

(see column four of Table 1). When implemented well, RCTs are the gold standard for generating unbiased estimates of the causal impact of a program or policy reform. RCTs, however, may miss general equilibrium effects or face other issues (e.g., Hawthorne effects, effects due simply to there being an experiment).

Because RCTs are not always an option, researchers have turned to a number of increasingly used quasi-experimental methods that exploit other potentially exogenous sources of variation (see the other columns of Table 1). In the case of the Table 1 antipoverty programs, researchers have used the difference-in-difference (DD) approach based on (1) variation in the timing of program rollout, particularly at the county level; (2) based on federal policy changes that affected some groups and not others; and (3) based on policy variation across states and localities. Such a DD design provides consistent estimates of program effects under two assumptions. The first is that trends would have been the same in the treatment and control groups absent the program (one can assess this, in part, by looking at pretrends in the outcome). The second is that the composition of the treatment group does not change as a function of the program. This can be tested if the treatment group is characterized by choice variables (e.g., being a low-income single parent). Another common approach is instrumental variables, where a program rule—outside the control of the participant but which affects participation in the program—can allow researchers to isolate the causal effects of program participation.

Researchers have also used regression discontinuity (RD) designs when eligibility for a program is contingent on a continuous assignment variable, with those scoring above a certain limit being eligible for the program and those scoring below ineligible. Here, one assumption is that individuals on both sides of the limit are otherwise equivalent if close enough to the threshold, and the only reason they differ is because of the assignment rule. RD presumes that agents cannot perfectly control which side of the limit they are on. The plausibility of this can be tested by seeing if other observables look smooth across the limit or whether there is an excess mass of individuals on one side of the limit.

When there is a significant behavioral response to a rule near a kink or change in the rule (such as cutting earnings to keep AGI right below the eligibility limit for the EITC), one can use this bunching response to estimate elasticities of interest (see, e.g., Chetty et al., 2011; Saez, 2010). Finally, when none of the above approaches can be applied, comparisons of periods of nonparticipation and participation within person across time or within families across members, controlling for person- or family-specific fixed effects, can offer some purchase, to the extent that the decisions to participate or not are random across persons through time or within families.

The Influence of Other Forces

Another challenge to causal analysis of the role of policy in shaping changes in poverty over time is the array of other factors that changed in the decades after the launch of the Great Society programs, factors that could also affect behavior and confound our understanding of the policy impacts.[6] In the remainder of this section, we briefly highlight several of the most salient factors.

Arguably most important, it is necessary to account for the evolution of the business cycle and trends in economic growth. The recent Great Recession has led to unemployment rates unseen since the late 1970s and early 1980s, and even absent program changes, the composition of the caseload for many safety-net programs

[6] See the discussion by Haveman et al. (2015, this issue) of the various economic, social, and demographic forces that influenced antipoverty policy and the share and composition of the poor.

would be expected to have changed (Danziger, Chavez, & Cumberworth, 2012). To the extent that the safety net does not offset the loss of earned income in an economic downturn, poverty can rise. If a worsening economy is coincident with the adoption of safety-net programs and is not controlled for, program impacts might be understated. Conversely, the program impacts could be overstated if implementation corresponds to more favorable economic conditions.

Another feature of the past several decades has been a rise in income and wage inequality (Atkinson, Piketty, & Saez, 2011; Autor, 2014; Fisher, Johnson, & Smeeding, 2013; Mishel et al., 2012). While adults with low education could make a reasonable living in the 1970s, the lack of real wage growth has made this more challenging. Research suggests this has been the result of many factors, including technological change, globalization, an influx of low-skill immigrants, the decline in unionization, and changes in the minimum wage, among others (Acemoglu, 2002; Autor, 2014; Goldin & Katz, 2008). At the same time, returns to education have increased, likely incentivizing more human capital accumulation. Further, there has been a large increase in women's labor force participation, a pattern of more education for men and women, and less occupational segregation (Karoly & Panis, 2004). In some racial and ethnic groups, increased incarceration rates associated in part with the War on Drugs have led to substantial changes in marriage markets, also affecting family structure and paternal involvement for many children in low-income and minority families (Wilson, 1987).

Finally, the past five decades have also been marked by changes in social norms about sex, marriage, divorce, single parenthood, and out-of-wedlock childbearing (Cherlin, 2005; Edin & Reed, 2005). The War on Poverty coincided with the changes in the technology for preventing pregnancy arising from the birth control pill and legalized access to contraception and abortion and also with the rollout of family planning programs across the United States (Bailey, 2013).[7] Most of these factors would be expected to affect poverty, as well as other outcomes of the War on Poverty programs we study; causal estimates should control for these factors.

EVIDENCE OF IMPACTS FOR CASH TRANSFERS

The War on Poverty initially began with a focus on in-kind transfers—those providing food, housing, and health care—rather than cash transfers. Although the AFDC program was already in place by 1964, the two other major sources of means-tested cash transfers available to the nonelderly today—SSI (cash assistance) and the EITC (a tax credit)—were introduced a decade later. Nevertheless, since these programs command considerable policy attention and are quite large, we begin our discussion with these three cash transfer programs.

AFDC and TANF

When President Johnson launched the War on Poverty, the AFDC program was already a fixture of the social safety net, dating to the 1935 Social Security Act.[8] The program had been instituted during the New Deal to provide cash assistance to single mothers who were required to be the primary breadwinner for their family, due to the death, disability, or absence of their husbands. President Kennedy expanded

[7] The roll-out of family planning funding was one of a set of 1960s decisions stemming from actions of the OEO, which we do not have space to survey. This type of grant is on the extreme end of local control, with programs such as SSI or Food Stamps marking the other end, where there is little scope for modification by states or localities.

[8] This section draws on Grogger and Karoly (2005).

the program in 1961 to include two-parent families where the father was unemployed, and subsequent changes in the War on Poverty made AFDC more generous (both directly and through other new programs linked to it). AFDC, an entitlement available to all who met eligibility rules, was jointly administered and funded by the federal government and the states, and states set benefit levels, which also varied with the number of dependent children. The resulting state benefit level variation has been large historically, even considering cost-of-living differences. In July 2013, the basic monthly cash grant for a single adult with two children ranged from $170 (Mississippi) to $923 (Alaska).

The history of AFDC and its eventual replacement with TANF under the 1996 PRWORA has been shaped by an understanding of the impact of cash aid on individual behavior, from its discouraging effect on work to the way that the program may have encouraged greater dependency by promoting childbearing outside of marriage and reducing the incentive to marry or stay married. However, much of this perception has been driven by anecdote, as much as by research. As it is a national program, it was challenging to identify a counterfactual to measure how outcomes would have differed had AFDC not existed. Researchers did exploit variation across states in benefit levels (controlling for other possible confounding factors, like the state of the economy), but this approach was problematic if states set benefit levels in response to concerns about the size of caseloads or outcomes of the low-income population such as rates of single parenthood (see, e.g., the discussion in the review by Moffitt, 1992).

Ultimately, understanding of the impact of welfare policy was informed by a feature of the 1962 Social Security Act that allowed the U.S. Department of Health and Human Services (DHHS) to allow states to undertake experimental, pilot, or demonstration projects with their AFDC programs. In granting such waivers, starting in the 1990s, DHHS required that any reforms be evaluated through a rigorous evaluation, typically an RCT. The resulting state-level reforms usually incorporated some combination of carrots and sticks to lessen the AFDC program disincentives for work and family formation.[9] Major reforms to provide an incentive to work by "making work pay" included raising the earned income disregard, lowering the benefit reduction rate, and providing more generous transitional child care and health insurance. Other key reforms aimed to shorten the time on aid and promote a faster transition to self-sufficiency through stronger work requirements, costlier sanctions for noncompliance, a cash grant that would not increase if a recipient had another child while on welfare, and time limits on lifetime benefits. Beyond the wavier-based evaluations, the shift in the 1990s from what was effectively a national AFDC program to a TANF program that had varied features across the states provided another source of policy variation that could be exploited.

In the remainder of this section, we highlight the key lessons learned about the structure of the AFDC/TANF cash welfare program and its effects on employment and earnings, welfare use, marriage and fertility, income and poverty, and child well-being. We begin with the evidence regarding unintended effects on such outcomes as employment and welfare use, as the implications for the intended effects of improved self-sufficiency and child well-being depend on the interplay between increased income from the cash benefit and the potential loss in earnings from the disincentive to work, as well as the impact of changes in parental time.

[9] A substantial earlier literature studies the negative effects of AFDC on labor supply and family formation, as summarized in Moffitt's (1992) review. The causal evidence from this body of work is that AFDC led to less work but had no effects on family formation.

Unintended Effects on Employment, Welfare Use, and Other Outcomes

As noted above, the economic framework predicts that the availability of a cash grant would induce some individuals to work less than they would otherwise. Moreover, the 100 percent tax on earnings that originally applied to AFDC would provide a further disincentive to increase work effort for those on welfare. The high tax rate originally in place with AFDC had little relevance in an era when few women worked outside the home. However, the rising labor force participation of women from the 1960s onward called into question the premise that single mothers should not be expected to work. Thus, in 1967, to encourage greater work effort as part of the War on Poverty, the $30 earned income disregard was introduced and the marginal tax rate on earnings was reduced from 100 percent to 67 percent—the $30-and-a-third rule. However, by the time of the state waivers of the 1990s, the tax rate had returned to 100 percent (after the first few months on the program). Moreover, the loss of ancillary benefits—such as Medicaid or child care (first introduced as part of the 1988 Family Support Act)—when earned income exceeded the relevant eligibility thresholds created a further disincentive to leaving the rolls.

Even as early as the 1960s, AFDC had drawn attention for the increase in participation, particularly among single-mother families formed from out-of-wedlock childbearing or divorce, families viewed by some as less deserving of aid. In the ensuing three decades, caseloads continued to expand, growing from 4 million recipients in 1964 to 14.2 million in 1994. As a result of rising rates of divorce and nonmarital childbearing, by 1994, a sizeable majority of recipients were never-married mothers. Moreover, Bane and Ellwood (1986), Blank (1989), and others documented high dependency rates, with one-quarter of recipients on aid for a decade or more. Increasingly AFDC was viewed as the cause of dependency (Murray, 1984), first by promoting childbearing and discouraging marriage, and then by providing a mechanism for long-term dependence on aid and even the transmission of dependency across generations, although not all the research supported this view.

For the most part, the recent experimental and observational research confirms the predictions of economic theory regarding the relationship between welfare policies and work effort. With few exceptions, the experimental evidence shows that reducing the financial disincentives to work or conditioning benefit receipt on work, as well as combining those policies with time limits, served to increase employment relative to AFDC rules, with effects as large as a 16 percentage point increase in the employment rate (where larger effects were associated with work mandates) (Bloom et al., 2000; Gennetian, Miller, & Smith, 2005; Hendra, Michalopoulos, & Bloom, 2001; Miller et al., 2000). Earnings largely increased as well, although there was some evidence that effects on total income disappeared once time limits on benefits were reached (Bitler, Gelbach, & Hoynes, 2006; Hendra, Michalopoulos, & Bloom, 2001). Observational studies that exploit the variation in state policy tend to reach similar conclusions although all of these studies struggled to separate out confounding effects of a booming economy (Klerman & Haider, 2004; Schoeni & Blank, 2000; Ziliak et al., 2000). Together, these studies suggest that the structure of the AFDC program, with its high tax on earnings, had discouraged work, whereas the state reforms and national reforms under PRWORA served to increase the employment of the welfare-eligible population.

In terms of welfare use, reducing the work disincentives in AFDC/TANF by increasing the earned income disregard or lowering the benefit reduction rate has been estimated to increase welfare use according to the experimental evidence, whereas welfare use appears to decline when welfare receipt is conditional on work. For example, the financial work incentives in Minnesota's Family Investment Program (MFIP) were estimated to increase welfare use by 10 percentage points up to

2.5 years after random assignment, compared with the control group (Miller et al., 2000). Among the various experiments evaluating mandatory work requirements, welfare use fell by as much as 12 percentage points in the Portland site of the National Evaluation of Welfare-to-Work Strategies (NEWWS) (Freedman et al., 2000). Reforms that tied more generous work incentives to a work mandate tended to decrease welfare use, except when the financial incentives were particularly generous (as in MFIP). States that implemented a bundle of reforms such as TANF—that is, combining time limits with work incentives and work mandates—tended to experience declines in welfare use, as well. The series of observational studies that examined the effects of various reforms or TANF as a whole tend to show similar findings, although the evidence was not always consistent, likely as a result of variation in study methods, including the set of controls for confounding factors (see Grogger & Karoly, 2005).

While employment and welfare use are linked to welfare policies, largely in the expected way, the more limited experimental evidence and observational studies have generally failed to find support for a strong link between welfare policies and marriage, fertility, or family structure. A number of the reforms in the 1990s, such as the cap on increased benefits if more children were born on aid, specifically targeted marriage and childbearing. Indirect effects might be expected as well from impacts on employment and income. Yet, hardly any of the experimental studies document significant impacts on childbearing, perhaps because the dollar amount affected by the family cap was small. MFIP had some impacts on marriage, suggesting that more generous financial work incentives, especially when targeted to two-parent families, may support marriage (Grogger & Karoly, 2009; Miller et al., 2000). Further, the studies making use of policy variation across time also fail to find large effects of the reforms on marriage and divorce (Bitler et al., 2004) or childbearing (Kearney, 2004; see also the review by Lopoo & Raissian, 2012).

One challenge is that changes in marriage and fertility as a result of changes in welfare policies may require a longer time horizon to observe than what was available in the experimental studies. Moreover, changing welfare policies may affect the behavior of women who are not yet on welfare (so-called entry effects), a population that is not observed in the experiments. Indeed, Lopoo and Raissian (2012) review the recent literature assessing the effect of welfare reform on teen fertility and conclude that there is suggestive evidence that the more restrictive provisions of TANF on teen mothers, as well as the lifetime time limits, contributed to the recent decline in teen childbearing.

The findings from these studies that exploited variation in the post-reform era accord with work by Grogger and Bronars (2001) that used 1980 census data to examine the effect of state variation in welfare benefits for initially unwed mothers who had twins rather than a single child on time to first marriage and to subsequent birth. The unexpected twins provide exogenous variation in the size of the welfare payment and the DD methods control for other differences between mothers with and without twins. They find that higher base welfare benefits (AFDC and Food Stamps) increase the time to first marriage for unmarried white mothers and lessen the time to the next birth for unmarried black mothers. However, the estimated effects are quite small in magnitude: a 10 percent decrease in welfare benefits is predicted to raise the share of white mothers married by a year after their first birth by 2 percentage points, while a 10 percent increase in welfare benefits is estimated to increase the share of black mothers with another child within two years by 1 percentage point. Effects five years later are equally modest.

Finally, there is some mixed evidence of effects on savings. For example, Bansak, Mattson, and Rice (2010) estimate that the loosening of asset requirements in PRWORA led to higher car ownership rates. Sullivan (2006) finds that while asset limits have little impact on asset holdings, vehicle exemptions have large effects

on vehicle assets. In contrast, Hurst and Ziliak (2006) report little effect of PRWORA on overall savings.

Intended Effects on Income, Poverty, and Child Well-Being

To the extent that welfare policy is designed to alleviate poverty, there is a tension between more generous benefits (even ones tied to work) that tend to raise incomes and lower poverty, and the accompanying reliance on welfare as a source of income, potentially for a longer period of time in the move to self-sufficiency. The state welfare experiments in the 1990s bear out this trade-off. In particular, both experimental and observational studies document that more generous financial work incentives, such as those in MFIP or New Hope, can raise incomes and lower poverty (Bos et al., 1999; Miller et al., 2000), but as noted above, such policies are also linked to greater welfare use. Moreover, the income gains associated with any given welfare reform policy or bundle of reforms tend not to be very large, and neither are the associated reductions in poverty. Indeed, estimates by Matsudaira and Blank (2013), using observational data and cross-state and cross-time variation in the generosity of earned income disregards, suggest little effect of work incentives on incomes, possibly because eligible single mothers are not taking advantage of the benefit. On the other hand, conditioning welfare receipt on work requirements tends not to raise incomes either because, in the absence of generous financial work incentives, the gain in income from greater employment is offset by the loss in welfare benefits. Likewise, studies find that implementing time limits does not lead to strong increases in income or declines in poverty (Grogger, 2003; Grogger & Karoly, 2005).

An original motivation for AFDC and a stated goal of TANF is to promote the well-being of children. The welfare reform literature also provides evidence as to the implications of welfare policy for children's outcomes such as early cognitive and behavioral development, later school performance, and risky behavior during adolescence (Duncan & Chase-Lansdale, 2001; Grogger & Karoly, 2005, 2009; Morris et al., 2001; Morris, Gennetian, & Duncan, 2005). For the most part, children's well-being tends to be tied to the pattern of income changes: if reforms raise family income, then child well-being, especially for younger children, tends to improve. Thus, more generous financial work incentives, such as those offered in MFIP and New Hope, are tied to improved outcomes for children (Gennetian & Miller, 2000; Huston et al., 2003). However, when policies such as work requirements alone do not lead to higher income, children may not be better off, but possibly not worse off either.

Supplemental Security Income

The SSI program, enacted in 1972 and implemented in 1974, is a federal cash assistance program for the low-income elderly, disabled, and blind.[10] Although SSI was implemented after the War on Poverty was underway, it was the result of a multiyear discussion about a universal negative income tax program for those with low income (the Family Assistance Program), first proposed by President Nixon. SSI replaced several federal programs—as well as a host of state-specific programs—providing support to the elderly or disabled, and the program required states to maintain the generosity of their existing programs to some extent if the state program was superseded by SSI. Children were included in SSI almost as an afterthought, with

[10] This section draws on Daly and Burkhauser (2003).

the result that a relatively small number of children with disabilities shifted from state AFDC programs to SSI. Originally a small program, beginning with changes in the late 1980s and early 1990s and evolving further in the wake of the welfare reforms of the late 1990s, SSI is now larger than TANF, although TANF covers more children (see Appendix Table A1[11]). The bulk of states provide state supplements, some mandated when SSI was created.

SSI is means tested and categorical. Means testing under SSI requires a low level of countable income near poverty (with some exemptions) and low assets (notably, housing is exempted). In-kind benefits are not counted but the benefit reduction rate is 100 percent. The blind and those over 65 need only satisfy the income and asset requirements to be eligible. The disabled, who can be adults or children, must show an inability to work or exhibit a commensurate disability. Applicants to SSI are assessed for disability status by the Social Security Administration (SSA), using the same general process as the Social Security Disability Insurance (SSDI) program. Adults need to show an inability to engage in substantial gainful activity (earn more than the countable income amount) from a disability that is expected to result in death or last a year or longer. There is a list of medical impairments, and if an SSI applicant is judged to have one, they are labeled disabled. Otherwise, applicants are examined to see if they could engage in the same work as before, and if they can, they are rejected. If not, the examiners assess whether the applicant could work at all (in a job relevant for their age, human capital, and past work history) and if they can, they are rejected. Individuals who participate in SSI are then periodically reexamined to ensure they are still disabled, with the rates of these reexaminations varying widely over time.

Until 1989, children were assessed to see if they had impairments similar to those conferring eligibility for adults. In the 1990 *Sullivan vs. Zebley* case, the Supreme Court ruled that the process for children must be made more comparable to that for adults. This led to two new paths for eligibility for children: having a condition on a list of disabling medical conditions that automatically qualify for SSI and doing poorly on an individual functional assessment (IFA). The IFA for children considers their ability to engage in substantial gainful activity or whether their impairment leads to marked functional limitations, and also assesses their ability to perform adequately in school. The *Zebley* ruling loosened the eligibility standards considerably for children, and the caseloads went up. PRWORA then tightened the standards for children again, and caseloads fell at first, and then rose again as SSI became an attractive substitute for TANF for some.

To some extent, from the very beginning, SSI and AFDC/TANF served overlapping populations. SSI is fully federally funded, while AFDC and TANF are not. Thus, states are better off if a family is on SSI, as they pay none of the cost. Furthermore, SSI has relatively high benefits, and thus in states with low AFDC/TANF benefit levels, families may benefit from moving a child to SSI, while perhaps leaving some other family members on AFDC/TANF. (Alternatively, if a mother is on SSI, in some cases her children may be child-only AFDC/TANF cases.) And a substantial share of AFDC/TANF recipients has physical or mental impairments (see, e.g., studies summarized by Nadel, Wamhoff, & Wiseman, 2003/2004). SSI recipients are also typically eligible for Medicaid and Food Stamps, as are AFDC recipients, so there is no loss of other program eligibility if a family shifts from AFDC/TANF to SSI.[12] And

[11] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[12] California is an exception to the eligibility for SNAP. Instead, California provides individuals with an amount of additional cash that is equivalent to the SNAP benefits that they would be eligible for.

like AFDC, SSI has a 100 percent tax rate on earnings. Finally, following PRWORA when TANF became a block grant and time limited, states had an even greater incentive to shift recipients to the federally funded SSI entitlement.

Intended Effects on Income, Poverty, and Child Outcomes

SSI benefits are relatively large as a share of the poverty line for single individuals and can lessen the burden on other family members of the disabled. Thus, mechanically, SSI helps alleviate poverty. Duggan and Kearney (2007) use SIPP data and a household fixed effects design to examine outcomes in households after the first time the family reports the presence of child SSI income. They estimate an increase in household income that is smaller than the increase in SSI benefits, with about a $0.72 increase for each $1 increase in benefits. They also find that receipt of child SSI leads to a decline in poverty but no impact on health insurance coverage, as most new recipients are already on Medicaid. There has been some effort to examine the longer-term effects of SSI participation on children by Coe and Rutledge (2013), however it is primarily descriptive.

Unintended Effects on Work and Other Outcomes

Following the theoretical framework discussed earlier, we expect more generous SSI benefits and the high marginal tax rate to discourage work and to distort behavior in other ways. The work disincentive can only be exacerbated by the fact that one needs to show an inability to engage in meaningful work activity to be eligible for SSI. An additional concern with child SSI recipients is that they may never enter the labor force.

Autor and Duggan (2003) suggest that the generosity of the SSDI program for low-wage adults and declining returns to work may have led to higher participation in SSDI over the 1990s and 2000s. It is easy to imagine some of the same changes happening in the SSI program, although they find no such relationship. Black, Daniel, and Sanders (2002) use variation from oil price shocks to areas where the economy was very coal dependent and estimate that negative economic shocks are associated with an increase in SSI payouts. Rupp and Stapleton (1995) document that applications are procyclical, but awards are less so, in a design with state and year fixed effects. (Note if awards were procyclical, it might suggest a degree of moral hazard.) At the same time, Bound, Burkhauser and Nichols (2003) find that the bulk of adult SSI recipients were not working three years before applying to SSI, suggesting some limit to the negative work effects of the program.

As noted above, when AFDC/TANF becomes less generous, families with a child who could be considered disabled face an increased incentive to enroll that child in SSI and several studies bear out this expectation. Kubik (1999) and Garrett and Glied (2000) both examine these incentives for cross-program shifting. Kubik uses a DD strategy and finds that higher SSI benefits relative to AFDC benefits prewelfare reform were associated with higher rates of SSI participation and less work. Garrett and Glied examine shifting of children from AFDC to SSI post *Zebley*, when disability determination became easier, and estimate a substantial effect on increased caseloads and decreased labor supply for mothers.

As noted above, the incentive to consider applying to SSI versus TANF became much larger for individuals post PRWORA and several studies confirm this result. Schmidt and Sevak (2004) find, also in a DD design, that the states with welfare waivers before PRWORA had higher levels of SSI receipt for single-mother families. Schmidt (2013) uses a panel of administrative state caseload data to study how SSI caseloads for nonelderly men, women, and children have evolved postwelfare

reform, controlling for other factors such as the unemployment rate, demographics, health, and income. She finds welfare reform led to a substantial increase in SSI caseloads, with a larger increase in places with more punitive welfare reforms. She also estimates that SSI caseloads have become more cyclical in the post-reform era and interprets this as SSI having become a more important part of the safety net. This contribution is particularly useful because of concerns about underreporting of social programs such as SSI in survey data, which could have led to bias in previous findings.

In recent work, Deshpande (2014a) takes advantage of a policy implemented as part of PRWORA that required redetermination of SSI eligibility for participating children when they turn age 18 using the stricter adult eligibility criteria, in contrast to prior policy when such medical reviews rarely took place. Using an RD design, the estimates show earnings to be higher and more volatile for those removed from SSI at 18, but this increase in earnings does not fully replace the lost SSI income. A similar design in Deshpande (2014b) uses an RD (and DD) based on the change in frequency of continuing medical reviews for child SSI recipients at the start of FY 2005, which affects the probability that SSI children are removed from the program. The estimates show that when children are removed from SSI, the loss of SSI benefits is fully offset by increased parental earnings.

Neumark and Powers (1998) consider the effect of the state variation in SSI generosity due to the state add-on programs and find lower savings for individuals likely to qualify for higher SSI benefits. Neumark and Powers (2000) report that generous SSI benefits reduce preretirement labor supply of men likely to be on SSI after retirement, using the same state variation.

Earned Income Tax Credit

The EITC was created in 1975 to offset payroll taxes paid by low-income workers. Thus, although it was not originally intended to be part of the safety net, the EITC now provides a large share of the total cash benefits paid to low-income individuals, reaching nearly 27 million tax filing units as of the 2012 tax year. Unlike the bulk of the War on Poverty programs, the EITC was conditioned on work from its inception. The program began by providing a small credit to families, with a subsidy for work in what is called the phase in range, where the credit increases with earnings. It then tops out for a range of incomes and then is taxed away. In its current incarnation, benefits depend on the number of children, with childless workers only qualifying for a small credit, while there is an increment for each child until the fourth child. The program was expanded first in the early 1990s and then even further in 1993, which is when the benefits for families with children were greatly increased. About half of the states have created their own state EITC program, which typically adds a percentage to the federal benefits. The EITC is refundable: even if a family does not owe any taxes, they can claim the credit.

Being primarily a national program (except for the state add-on EITCs) complicates evaluation. Studies examining the effects of the EITC use several approaches. Some papers characterize expected EITC benefits for a given individual based on benefit levels for others like them, and use a DD design, usually leveraging the variation from the expansions in the mid-1990s. Others use the fact that the expansion of the late 1990s resulted in a much larger increase in expected benefits for families with two or more children than for those with one child. These papers use a DD design, but with two or more child families being the treatment group, interacted with being after the expansions (and one child families being the control group). A third set of papers leverages the extra funding provided by state EITCs in a DD design. A fourth group of papers exploits the feature that the EITC is nearly always

dispensed in March and April in a lump sum. Finally, other papers use the methods from the taxable income bunching literature to study the effects of the program.

Intended Effects on Employment and Well-Being

The original goal of the EITC was to mitigate the regressive nature of payroll taxes for low-income workers while encouraging work. The expansions of the program in the mid-1990s were aimed at having an antipoverty role as well. Because of the nature of the EITC schedule, static calculations show it plays an important role in increasing family income and reducing poverty. But, as noted earlier, such calculations do not account for the behavioral effects of the program.

With a phase-in and a phase-out region, the EITC program would be expected to have effects on labor supply that vary according to where an individual is on the schedule. In the phase-in region, the program provides a subsidy to working and research generally confirms the intended positive employment effects. Eissa and Liebman (1996) estimate the effects of the 1986 Tax Reform Act expansion in the EITC on work for single women and find the EITC leads to more work, especially for single women with children. Hotz, Mullin, and Scholz (2006) estimate a large effect of the EITC on the employment of women who have used or will use welfare and are likely in the phase-in region. Meyer and Rosenbaum (2001) estimate the effects of the EITC and a host of other social programs, finding that much of the increase in work for single mothers in the 1990s is due to the EITC.

A number of papers have used various approaches to estimate the effects of the EITC on health as a measure of well-being beyond income or poverty and the findings are generally favorable. Hoynes, Miller, and Simon (2015) show that for single low-education mothers, the EITC leads to a reduction in the incidence of low birth weight and an increase in average birth weight. The results are consistent across several approaches, including DD leveraging the expansions of the mid-1990s differentially by family size, event studies, and a DD assigning treatment as the maximum EITC credit for a family of a given size and marital status. Strully, Rehkopf, and Xuan (2010) likewise find that state EITCs are associated with higher birth weight in a DD design. Evans and Garthwaite (2014) use the 1993 expansion and variation by number of children to examine health impacts of the EITC on women. They estimate that women in families with at least two children had better self-reported health and reductions in risky levels of biomarkers after the expansions.

One would expect that if the EITC increases net resources, consumption should rise as well, and the research again bears this out. Here, the main research design is to contrast consumption in March and April, when most benefits are paid out, with the rest of the year. For example, Barrow and McGranahan (2000) find increased spending in refund months for those they impute as eligible for the EITC, with spending concentrated on durables. More recently, McGranahan and Schanzenbach (2013) employ Consumer Expenditure Survey data to examine food choices as a function of imputed EITC eligibility and document an increase in expenditures on specific healthy foods (meat, poultry, fish and eggs, diary, fresh fruit, and vegetables) in refund months.

Unintended Effects on Employment and Other Behaviors

For those in the flat region of the EITC schedule, there is no substitution effect of the program, and the income effect will likely lead individuals to work fewer hours. In the phase-out region, the EITC claws back benefits, and both the income and substitution effect should lead to less work. For couples, if one assumes that there is a primary earner and a secondary earner who treats the primary earner's income

as fixed (like nonlabor income) when deciding whether to work, if the family is in the phase-out region, the secondary worker will face the family's existing marginal tax rate plus the clawback rate. The EITC would therefore be expected to reduce the probability of the secondary worker being employed. Eissa and Hoynes (2004) find evidence consistent with this model (where the secondary workers are assumed to be married women, and the primary workers are married men), with the EITC leading to less work by married women using several approaches.

Another possible unintended effect of programs such as the EITC can be to alter marriage and fertility decisions. Baughman and Dickert-Conlin (2009) use variation from state EITCs and report no impact on fertility. Dickert-Conlin and Hauser (2002) and Ellwood (2000) consider effects on family formation, finding no significant effects of the EITC on marriage or divorce. Herbst (2011) examines the effects of the state plus federal EITC, leveraging state variation, estimating a small decline in abortions with the EITC.

The design of the program with discontinuous changes in tax rates in the standard utility-maximizing model suggests that when there are increases in the marginal tax rate, individuals should choose hours such that their earnings put them right below the point of a rate increase. Saez (2010) finds evidence that there is considerable bunching at the first kink point of the EITC, concentrated among self-employed individuals who can most easily manipulate their earnings and reporting of income. Further evidence comes from Chetty, Friedman, and Saez (2013) using a panel of tax returns to examine variation in earnings after families have children. They divide neighborhoods into two types: those where individuals typically maximize their EITC benefit (suggesting manipulation of hours worked) and those where individuals do not. The thought experiment is that the low-manipulation neighborhoods are ones where parents do not understand the tax incentives and thus should serve as a control group for the other neighborhoods where parents should find it easy to learn how to change their earnings to maximize their EITC. They estimate that parents in low-manipulation areas do not have large changes in their earnings in ways that maximize their EITC benefits after having children, while those who live in high-manipulation areas exhibit more refund-maximizing behavior.

EVIDENCE OF IMPACTS FOR IN-KIND TRANSFERS

We now turn our attention to the major in-kind transfer programs listed in Table 1 that make up the safety net. Four of the means-tested in-kind transfer programs—Food Stamps, Child Nutrition Programs, Medicaid, and housing assistance—were major components of the War on Poverty. Together with the later additions of WIC, SCHIP, and CCDF, spending on in-kind transfers pertaining to food/nutrition, health care, housing, and child care dwarfs the cash transfer programs discussed in the prior section.

Food Stamps and the SNAP

After being a pilot program from 1961 to 1964, the Food Stamp Program was made permanent in the Food Stamp Act of 1964. The legislation required states to develop eligibility standards, required individuals to spend a certain amount on food before gaining food stamps to supplement their diet (the "purchase" requirement), and included all domestically produced food items at grocery stores as eligible for purchase with the stamps. Later laws reduced (in 1971) and then eliminated (effective in 1979) the requirement to purchase a large amount of food before gaining stamps. All areas were mandated to adopt the program by 1974. Starting in the late 1980s, states began to move toward electronic benefit delivery, eliminating the use of paper

vouchers. In 2008, the program was renamed the SNAP, and different states now have different names for the program.

For most of the existence of the Food Stamp Program, benefits have been means tested, with eligibility since the late 1970s being limited to those with net income (after some exemptions) of 100 percent of the federal poverty guideline (FPG), gross income under 130 percent of the guideline, and limits on assets. The benefit reduction rate is 30 percent, much lower than that for AFDC (and some state TANF programs) or SSI. Over the 2000s, many states eliminated their asset requirements. Together with the income shocks from the Great Recession, this has led to unprecedented levels of participation, with as many as one in seven Americans receiving benefits. SNAP is currently the only universal safety-net program in the United States, available to individuals of any age or family type with sufficiently low income and assets (with minor exceptions discussed below).

Unlike several of the other major War on Poverty programs, SNAP benefits are fully funded by the federal government, with the states sharing administrative costs. Maximum allotments are tied to the cost of a nutritious low-cost diet (the "thrifty food plan"), and the benefits are meant to provide for food costs for such a diet. For example, for FY 2014, the maximum allotment for a family of four was $649 per month. SNAP also has a nutritional education component.

Intended Effects on Food Consumption, Nutrition, and Other Outcomes

The goals of the Food Stamp Program (now known as SNAP) are to alleviate hunger and improve nutrition. Like any in-kind transfer compared with an equivalent cash transfer, economic theory predicts that only those who would have spent less than their benefit level on food should change their food spending if they obtain food stamps. Estimates from the 1990s suggest that each dollar of SNAP benefits leads to only slightly more spending on food than a dollar of cash would, suggesting that a large share of recipients are inframarginal (Fraker, Martini, & Ohls, 1995; earlier research suggested a smaller share of recipients were inframarginal).

Some of the best evidence we have about the Food Stamp Program comes from a series of papers by Hilary Hoynes, Diane Schanzenbach, and Doug Almond. These studies rely on an event study or DD design, comparing outcomes at the county level before and after counties started to participate in the Food Stamp Program. Hoynes and Schanzenbach (2009) use data from the Panel Study of Income Dynamics (PSID) to show that the introduction of the Food Stamp Program led to a decline in out-of-pocket food spending and an increase in overall food expenditures. They also find that during this early period of the program (when there was a binding purchase requirement), Food Stamp benefits were like cash, leading to similar spending on food as equivalent increases in cash resources. Almond, Hoynes, and Schanzenbach (2011) examine the effect of adopting the program on birth outcomes, finding that exposure to the program in the third trimester leads to increased birth weight, especially at the bottom of the birth weight distribution, with larger effects for blacks but no statistically significant effects on infant mortality.[13] Finally, Hoynes, Schanzenbach, and Almond (2012) estimate the long-run effects of exposure to the program while aged 0 to 5, again using the PSID. The analysis shows exposure to the program reduces the incidence of metabolic syndrome (obesity, high blood pressure, and diabetes) and, for females, increases economic self-sufficiency. In all of these papers, in addition to a DD approach, the authors also interacted county roll-out with a control for the individuals' demographic group level of participation

[13] Currie and Moretti (2008) use a similar strategy in California, finding less consistent evidence.

in the program in 1976 to 1978. This scales the program effects on the dummy for program implementation by the groups' participation in the program to yield a treatment effect on the treated, assuming that each group's participation in the program in 1976 to 1978 after it has been rolled out everywhere is homogeneous by the time of implementation.

These papers carefully consider exogeneity of county adoption of the program. They show that 1960 county characteristics from the Census (race/ethnicity, poverty, land in farming) explain a small share of the variation in the timing of adoption (and they control for these variables interacted with county-specific time trends). They also control for expenditures on other welfare programs at the county level. They conduct event studies and show there are no important pretrends in the available outcomes of interest in advance of the program's roll-out. Unfortunately, because much of the data (including the microdata on birth weight and the PSID data) start in 1968 after much of the roll-out, their ability to fully test for pretrends is limited to the later adopting counties. With this county-based design, they are also able to control flexibly for state by year interactions in some specifications, and they also control for other programs rolled out at the county level, such as community health centers.

One limit to these roll-out findings is that individuals in the past facing the newly established Food Stamp Program experienced both a different counterfactual (with hunger being more frequently present) and a program that required participants to be more heavy food consumers or have more cash on hand (as a result of the purchase requirement). This suggests some caution in assuming that findings from the roll-out period are indicative of the program's current effects.

In addition to studies using the roll-out strategy, there are several papers that explore bounds to the average treatment effect while imposing assumptions about the degree of underreporting and selection in the data. Kreider et al. (2012) tackle both of these issues when examining the effects of SNAP on children using the National Health and Nutrition Examination Survey (NHANES). They restrict the data to individuals under 130 percent of poverty (using the NHANES' relatively imprecise income measure) and compare participants to nonparticipants while applying various assumptions about the measurement error and selection. They find SNAP is favorable for children's health if strong assumptions are made, but even the sign of the program's impact on average is unclear with weaker assumptions.

It has been challenging to evaluate the impact of Food Stamps or SNAP in the period after implementation because, in large part, the program is national. An additional challenge is that the program is most valuable for those who are nutritionally at risk, making comparisons of participants and nonparticipants not very useful due to selection (see, e.g., Gundersen, Jolliffe, & Tiehen, 2009). Some studies have attempted to confront these selection concerns by examining the effects of state experimentation on participation and nutrition (Kabbani & Wilde, 2003; Ratcliffe, McKiernan, & Zhang, 2011; Yen et al., 2008).

It is clear that the changes associated with welfare reform in the late 1990s also affected SNAP. Until these reforms, legal immigrants faced the same eligibility rules as citizens. But PRWORA and other welfare reforms first denied Food Stamps to noncitizens who had been in the United States less than five years unless they were refugees. These same groups were also made largely ineligible for TANF and for Medicaid, although a number of states filled in for one or more of these programs (Zimmerman & Tumlin, 1999). Borjas (2004) finds that these fill-in programs have buffered outcomes for some groups, both in levels of participation and in food security. Bollinger and Hagstrom (2008) examine how decisions about coding refugees and measurement error affect these results.

Over the 2000s, many states that broadened eligibility through reductions in asset requirements, loosened restrictions for able-bodied adults without dependents due

to effects of the Great Recession, and implemented other policies such as broad-based categorical eligibility rules (Ganong & Liebman, 2013; Ziliak, 2015). This state experimentation has led to some variation in participation, but no causal research as yet has focused on the nutrition or health effects of these policy changes.

Unintended Effects on Employment, Savings, and Other Outcomes

Hoynes and Schanzenbach (2012) provide evidence about effects on labor supply associated with the roll-out of Food Stamps. They use the same county roll-out design and PSID data as in the previous work, and find that both employment and hours worked fell when the Food Stamps Program was introduced. This evidence is supplemented by evidence from the 1960, 1970, and 1980 decennial Censuses. Effects are concentrated in families headed by single women. Effects on hours worked that inflate the intent-to-treat estimate are similar in magnitude to the effects from Moffitt (1983) on the impact of AFDC.

Evidence about the program under rules closer to the current program includes that from Fraker and Moffitt's (1988) analysis using more structural approaches and a nonlinear budget set. Their results show a work disincentive of the program of about 9 percent. Keane and Moffitt (1998) estimate effects of the Food Stamp Program together with that of other welfare programs, finding that a large reduction in marginal tax rates (from combined sources) would lead to increased labor supply but at the cost of many more entering the program (due to the higher breakeven point). This trade-off between lower work disincentives and increased participation reflects the "Iron Triangle" of welfare: one cannot simultaneously reduce work incentives and maintain program generosity without increasing program costs. Among other possible unintended consequences, Ganong and Liebman (2013) consider the impact of asset rules in the program (while thinking carefully about many other program rules) and find a significant effect of loosening asset rules on participation.

Another possible unintended effect of the program that has received considerable attention in policy circles is effects on nutrition and obesity. As mentioned earlier, those who would most gain from the program are likely those with the worst nutritional outcomes and thus comparisons that do not adjust for this selection likely will yield negative effects of the program (Bitler, forthcoming).

One final interesting area of much policy attention is the open-ended nature of SNAP benefits, which can be spent on a wide array of goods. With a nod to concerns about obesity, New York State recently proposed a waiver to the U.S. Department of Agriculture (USDA) to limit the use of SNAP to purchase sodas. This effort to use a stick to restrict use of SNAP benefits was denied, and the Food and Nutrition Service has published a report summarizing the difficulties with such proposals including the challenges with identifying "healthy foods" both scientifically and in practice (USDA, 2007). However, the USDA has been experimenting with carrots to encourage consumption of fruits and vegetables, in the Healthy Incentives Pilot (Bartlett et al., 2013). In this experiment, treatment group members in Hampden County, Massachusetts, were randomized to receive an additional $0.30 in SNAP benefits for each $1 spent on fruits and vegetables. Early findings suggest increased consumption of the targeted fruits and vegetables by 25 percent (Klerman et al., 2014).

Child Nutrition Programs

In addition to the broadly targeted Food Stamp Program, there are a series of more targeted child nutrition programs. The 1946 NSLP was initially established to provide matching grants to states based on the number of school-age children and

the state's ranking in the distribution of per capita income. Needier states had to match less of the program cost. In 1962, the NSLP was modified to allocate funding based on both the number of lunches served by schools in the program and a need-based formula where states with above-average neediness received more funding. The Child Nutrition Act of 1966 ushered in the SBP to provide grants to schools to serve breakfast to nutritionally needy children, first as a two-year pilot program and then as an established program in 1971. The Act also extended the Special Milk Program, which provided milk to children.

In its current incarnation, the NSLP provides subsidies for lunches in participating schools. Schools must follow meal pattern and nutrition standards and also provide free- and reduced-price lunches to low-income children, with free lunches for those at or below 130 percent of poverty and reduced-price meals for those in the range of just over 130 percent to 185 percent of poverty. Reimbursement is also provided for snacks for afterschool programs. The SBP operates in a similar fashion, although fewer schools participate compared with the NSLP. Over time, as concerns have been raised about the possible impacts of school meals on obesity, Congress has tightened up the rules about what types of meals schools can be reimbursed for.

The nutrition programs impose a nontrivial administrative burden on schools. Schools typically send home application forms at the start of the school year, and those families who would like to participate in free- or reduced-price programs must document their income. Families who participate in the SNAP or TANF programs are automatically eligible, and over the last decade or so, the USDA has required states to "directly certify" such children. The reimbursement schools receive is highest for the free student lunches and declines successively for reduced-price meals and full-price meals.

Districts can also choose several reimbursement methods that lessen the administrative burden if the district is willing to provide free meals to all students, through "provision programs." Districts, or in some cases, schools, can be reimbursed based on the breakdown of free, reduced-, or full-price lunch students for one year as estimated based on past claiming behavior or population-based or administrative data based estimates of direct eligibility. Schools in communities with high enough participation in TANF and SNAP can also qualify. This has several advantages for schools and children. First, it reduces the burden of collecting applications and tracking meal categories. Second, it reduces the burden of verifying applicants are actually eligible, which otherwise districts must do on an annual basis. Third, it reduces the stigma for children participating in the program and has been shown in some cases to increase participation. Leos-Urbel et al. (2013), using a DD model, estimate that when New York City moved to universal free school breakfast provision (while also increasing the lunch price for those paying full price), participation in breakfast went up, even for groups who were already eligible for free meals, suggesting a reduction in stigma.

Intended Effects on Child Nutrition, Health, and Education Outcomes

The goals of these child nutrition programs are to improve nutrition for children, and by this mechanism, improve their ability to learn in school. In fact, the original NSLP was motivated by the effects of malnutrition on rejection rates for drafted individuals in World War II. To assess aspects of the portfolio of child nutrition programs, the USDA conducts regular School Nutrition Dietary Assessment Studies. These studies document some impacts on dietary intake but are by necessity nonexperimental. For example, Fox and Condon (2012) summarize findings from the fourth such study done during the 2009 to 2010 school year, finding most meals offered met the standards for nutrient amounts but that they were less likely to meet minimum calorie guidelines.

Like many other programs in the War on Poverty, the fact that many of the parameters of the NSLP and SBP are primarily national complicates evaluation, although some recent experimentation with providing free meals universally has allowed for some better identification. Hinrichs (2010) studies the effects of NSLP participation when the program was relatively new on adult health and educational achievement. He leverages variation in the funding formula with the 1962 law changes across children's cohort's exposure to the NSLP while in school. He models the effects of more participation by cohorts in the program by state and year of birth. He instruments for this potentially endogenous and poorly measured outcome with changes in the funding formulas introduced with the 1962 changes, finding no consistent evidence that the program produced statistically significant health improvements but some evidence that it may have improved educational attainment.

Frisvold (2015) examines the effects of the SBP by leveraging state mandates that require schools with a combined share of free- and reduced-price lunch students above certain thresholds to participate in the SBP, with evidence of positive impacts on test scores using both a DD and an RD design. As another source of variation, Bhattacharya, Currie, and Haider (2006) estimate effects of the SBP by comparing objective health measures in the NHANES during the school year and outside the school year and across places with a DD design, finding the program to be beneficial for both Health Eating Index scores and objective measures of reduced inadequate nutrition. At the same time, Leos-Urbel et al. (2013) estimate that universal school breakfast provision had small or no effect on attendance and no impact on school outcomes, despite the increase in program uptake. Ribar and Haldeman (2013) examine the effects in North Carolina of removing universal access to free breakfast, finding a decline in participation for those not eligible for free- or reduced-price meals but little or no effect on test scores or attendance.

Recently, districts have experimented with providing school breakfasts to everyone in particular schools, with meals being served in the classroom rather than in the cafeteria. Several studies have examined the effects of these breakfast-in-the-classroom programs on both nutrition outcomes and learning as measured by test scores. Imberman and Kugler (2014) find that breakfast in the classroom in Houston led to nontrivial improvements in accountability scores, especially for low-performing, low-income minority groups. They conclude that this is driven by effects on test taking, since they find no effect on grades. Dotter (2013) estimates that staggered introduction of breakfast in the classroom in San Diego increased test scores, while leaving attendance unchanged. Corcoran et al. (2014) report that breakfast in the classroom in New York City produced increases in participation but no effect on school lunch or obesity, and no impact on test scores. They use a DD design, leveraging differences in the timing of adoption. Schanzenbach and Zaki (2014) reanalyze experimental data from the USDA's original experiments, which evaluated universal free breakfast, but allowed treatment group schools to choose whether to do so as a traditional, cafeteria-based program or whether to do so as a breakfast in the classroom school. They estimate that breakfast in the classroom leads to a much larger increase in participation than does a free cafeteria program. They find that only breakfast in the classroom children are more likely than the controls to consume a nutritionally substantive breakfast, and find no impacts of being in either treatment group on test scores and little impact on attendance.

Unintended Effects on Nutrition and Obesity

One area of concern with all food assistance programs is their possible association with poor nutrition and obesity. Descriptive work by Gleason and Dodd (2009) compares students who are usual participants in these programs with other students while controlling for a host of observable characteristics, documenting no

relationship between NSLP participation and obesity and a possibly protective effect of the SBP. Of course, evidence such as this is by necessity a cross-sectional, point-in-time comparison of those on and off the program. A further analytical complication compared to some of the other programs we have discussed is that the decision to participate takes place first at the school or school district level, and then at the individual level.

In an effort to move past cross-sectional variation, Gleason and Suitor (2003) study the effects of participation in the NSLP on 24-hour dietary intake using data from the 1994 to 1996 Continuing Survey of Food Intakes by Individuals. Two days of food diaries were used for students in schools offering the NSLP. Intake for children was modeled according to whether or not the children participated in the NSLP on each day with student fixed effects to control for unobserved heterogeneity. Thus their results derive from students who had school lunch on one day and not another. They find both increased consumption of vitamins and minerals and increased fat intake as a share of calories with school lunch participation in the fixed effects models. They also estimate a reduction in added sugars with school lunch.

Schanzenbach (2009) uses an RD design around the eligibility threshold for reduced-price lunch (185 percent of poverty) as well as comparisons of participants and nonparticipants preschool entry, finding reduced-price school lunch is associated with increased obesity. This RD design yields local estimates of the effect of the program near the 185 percent eligibility limit.

Millimet, Tchernis, and Husain (2010) employ data from the ECLS-K, and find selection into the SBP is negative in terms of weight gain at preschool entry. They also estimate that participation in either the SBP or the NSLP in first grade is associated with more weight in third grade. Further investigation suggests selection is important in their results, but their preferred estimates are consistent with Bhattacharaya, Currie, and Haider's (2006) finding of positive effects of the SBP and Schanzenbach's (2009) estimate of negative effects of the NSLP.

Gundersen, Kreider, and Pepper (2012) use various bounding assumptions on selection and the effects of the program and a partial identification framework. They document further that those children in the program look worse off than observationally similar children who do not participate. With various assumptions about either the propensity to participate being decreasing in income or that participation cannot make participants less healthy, they conclude the program has positive effects on food insecurity, poor health, and obesity. Once they further account for underreporting of the program, however, only the poor health outcome is positive unless they also allow for participation to only be nonnegative in effect. Schanzenbach and Maki's (2014) reanalysis of the universal free breakfast experiment finds that breakfast in the classroom leads to a slightly higher probability of eating two breakfasts, but no increase in overall calories.

Special Supplemental Nutrition Program for WIC

In 1972, WIC was established as a pilot program to help provide nutritious foods, nutrition education, and links to other service providers for low-income, nutritionally at-risk pregnant and postpartum or breastfeeding women, infants, and children up to age 5. The program was made permanent in 1974. Like the child nutrition programs, WIC is operated by local agencies (e.g., county health departments, community centers, schools, or hospitals).

To be eligible for the program, applicants must be in one of the categorical groups above, and also live in the state in which they are applying and satisfy the income eligibility rules. Families are income eligible if they are under the income standard in their state (currently 185 percent of the FPG), or if they are automatically eligible (primarily those enrolled in SNAP, Medicaid, or TANF). Applicants need to also be

examined by a health professional and found to be nutritionally at risk based on such factors as anemia, being underweight or overweight, having had poor previous pregnancy outcomes, or showing dietary risk. Participants gain vouchers (or now, in some states, electronic benefit transfer cards) to use at authorized food stores for specific foods high in micronutrients. Foods include infant cereal, fortified adult cereals, food rich in vitamin C, fruits and vegetables, baby food, peanut butter, dried/canned beans and peas, milk, and canned fish. In 2009, WIC introduced new food packages and a fresh fruit and vegetable cash voucher, but researchers have yet to study this reform with convincing designs.[14] The program also seeks to encourage breastfeeding but for those who do not breastfeed, the program provides infant formula.

WIC is a block grant program. In the early years of the program, funding limits were binding, and only the most at risk were enrolled in the program. In recent years, the program has been fully funded and, due in part to the high income limits for Medicaid eligibility, the average income of participants has gone up over time.

Intended Effects on Pregnancy Outcomes, Nutrition, and Health

The goals of the WIC program are to improve nutrition and health of women and their infants and children. It also has a goal of encouraging breastfeeding and improving adherence to immunization. Much of the available literature examines whether WIC achieves these intended outcomes. Unlike many safety-net programs, a wealth of data shows that WIC participants have healthier babies, with lower health care costs (e.g., Devaney, 1992; Devaney, Bilheimer, & Schore, 1980; Devaney & Shirm, 1993). However, much of this work does not account for the fact that WIC participants may be positively selected on observable or unobservable characteristics, leading them to have better outcomes than others who are income eligible but do not participate in WIC. In sum, we know the most about causal effects of the program on pregnancy and birth outcomes, and the least about effects on children. We know almost nothing about long-run effects.

Using a by now familiar strategy, Hoynes, Page, and Stevens (2011) employ detailed natality data and exploit differences in the timing of roll-out by county. Their estimates show that the roll-out of the WIC program led to an increase in birth weight and a decline in low birth weight. Bitler and Currie (2005) use data from the Centers for Disease Control and Prevention's Pregnancy Risk Assessment Monitoring System, which collects outcomes at about six months after birth from women with live births, to examine targeting and explore the impact of state variation in nutritional standards. They find little evidence of positive selection into WIC on observables for a sample of women whose births were paid for by Medicaid. They also report little predictive power of state policy variation on participation. Currie and Rajani (2014) use data from New York City from 1994 to 2004 and a sibling comparison design. Their results show that women who use WIC for one but not all of their pregnancies are more likely to be younger, unemployed, and unmarried when on WIC while pregnant compared to pregnancies when they do not use WIC.

[14] Work based on pre–post comparisons among WIC recipients in Los Angeles by Whaley et al. (2012) finds an increase in self-reported consumption of whole grains and a decrease in consumption of whole milk, both targets of the change. Andreyeva and Luedicke (2013) estimate increased whole-grain intake using scanner data. Wilde et al. (2012), using administrative data, find that the changes designed to incentivize breastfeeding by making the full breastfeeding package more valuable in monetary terms relative to the partial breastfeeding or full formula packages led to the unexpected effect of more women receiving full breastfeeding packages and more women receiving full formula packages, with no net effect on breastfeeding.

They find that WIC is associated with reductions in low birth weight and being small for gestational age, but also that women on WIC and their infants use more medical care.

Using data from Florida, Figlio, Hamersma, and Roth (2009) estimate that among women very near the WIC income-eligibility threshold, those marginally eligible while pregnant have higher participation levels after a specific policy change. They use this to instrument for WIC use, and find WIC reduces the probability of low birth weight.

In a series of papers, Ted Joyce and co-authors question much of this previous work on WIC, showing that some share of the findings are due to the fact that women who participate in WIC very late are mechanically likely to have longer pregnancies than some counterparts, a feature known as gestational age bias (e.g., Joyce, Gibson, & Colman, 2005; Joyce, Racine, & Yunzal-Butler, 2008). However, their own results suggest some positive effects of the program, even net of the bias they discuss.

Rossin-Slater (2013) uses changes in the locations of WIC clinics in Texas from 2005 to 2009 to estimate causal effects of WIC on benefit use, pregnancy gain, birth weight, and breastfeeding initiation. She finds WIC to be beneficial, with larger effects for women with a high school education or less education. Her strategy combines maternal fixed effects with zip code variation in the location of clinics, and addresses many of the selection concerns about previous work.

Medicaid and the State Child Health Insurance Program

Along with Medicare, the Medicaid program was established in 1965 when Title XIX of the Social Security Act created access to coverage for health care services for low-income children without parental support, their caretaker relatives, and the elderly, disabled, and blind. Almond, Decker, and Simon (2012a) describe the process by which states took up this new program between 1966 and 1972 (with the lone outlier, Arizona, adopting the program in 1982).

The Medicaid program is a joint federal–state program. The federal share of funding is tied to state income levels, with poorer states receiving more federal funding. States must cover some categories of persons (e.g., AFDC/TANF recipients) and cover some categories of services (screening, doctor services, hospital care), but states have a lot of autonomy about the precise rules for eligibility for some groups and what specific services to cover.

Over time, the reach of Medicaid has expanded in several ways. In 1972, the SSI program was created and states were required to cover these individuals through Medicaid. Other large program changes included expansions in the late 1980s and early 1990s to cover women and children with incomes higher than the AFDC eligibility limits, changes in the late 1990s associated with the decoupling of cash welfare from Medicaid as part of PRWORA, and the creation of SCHIP in 1997. More recently, the Patient Protection and Affordable Care Act (PPACA) of 2010 produced a host of other changes to Medicaid, some aimed at expanding the program to cover low-income adults under 138 percent of the federal poverty level and others standardizing rules across different state Medicaid programs.

Through providing health insurance coverage, the primary goal of the Medicaid program was to improve the health of indigent individuals without coverage and likely with limited access to the health care system. Undoubtedly, there was some hope that the program would have longer-term effects on well-being by improving human capital. One issue in evaluating the effects of Medicaid on health is that health is a stock measure, one that changes slowly through more immediate changes in health care utilization and health behaviors. As a result, providing access to public health insurance is likely to have immediate effects on reported health insurance coverage (to the extent that people know their coverage has changed), to have more

gradual changes on health care use as individuals respond to the change in insurance coverage, and to more slowly have effects on most health outcomes.

As shown in Table 1, researchers have exploited several strategies for estimating the causal effects of Medicaid on health care utilization, health behaviors, measures of health, and other outcomes. The primary sources of identifying variation include the period of Medicaid roll-out and the coverage expansions in the 1980s and 1990s, although other approaches have been used as well. In the discussion that follows, we first touch on evidence regarding the intended immediate effects and then the longer-run effects. Next we turn to an accounting of the unintended consequences.

Intended Effects on Health Care Utilization, Health-Related Outcomes, and Other Outcomes

A number of studies examine the impact of the Medicaid roll-out on health care utilization and health-related outcomes. A challenge with this approach is that states chose when to implement the program, with all but Arizona doing so between 1966 and 1972. Furthermore, state decisions about the generosity of their preexisting AFDC programs and individual AFDC take-up decisions within states also may make identifying the immediate effect of the Medicaid roll-out using cross-state variation tricky. This challenge is reflected in the mixed findings across the papers using this approach, differences that have yet to be reconciled. For example, Almond, Decker, and Simon (2012a) estimate, in models with state, year, and month fixed effects, that the states' introduction of Medicaid led to more fertility among the nonwhite population. This result is robust to including state trends and even state by year fixed effects (when pooling whites and others). Using NHIS data and a similar design, Almond, Decker, and Simon (2012b) estimate that Medicaid adoption increased hospital expenditures among children living with low-education adults and among unmarried mothers. They find no effects on child or maternal health however, and the NHIS data used in the analysis do not capture the use of outpatient ambulatory care. They also analyze death rates by cause of death, age, state of residence, race, and gender, and find no impacts of Medicaid adoption on mortality of children or mothers.

Goodman-Bacon (2015) layers differences in take-up of AFDC on the empirical model in the Almond et al. papers, and, by contrast, finds a significant decline in nonwhite child mortality in states with relatively high rates of implied new Medicaid eligibility because of high preexisting AFDC participation. Boudreaux, Golberstein, and McAlpine (2014) use the PSID to examine the long-run impacts on adult health and economic status as a result of Medicaid implementation and report significant effects on health but no evidence of economic effects.

A key assumption for these studies is that the timing of the adoption of Medicaid across the states is exogenous. The Almond, Decker, and Simon (2012a, 2012b) papers include lead effects in models of hospitalizations and find no evidence that Medicaid was adopted more quickly in states with higher hospitalization rates for children. Strumpf (2011) also finds no systematic link between state per-capita income, state revenues, and state expenditures and the timing of adoption, while Decker and Selck (2012) reach a similar conclusion based on AFDC caseloads. Likewise, Goodman-Bacon's (2015) estimates indicate no evidence of differential trends in mortality in his treatment and control states using event study analysis. He further finds no evidence that Medicaid implementation in high- or low-welfare-use states was differentially tied to preimplementation state characteristics. Other suggestive evidence of the exogeneity of Medicaid implementation comes from the fact that, according to political discussions when Medicaid was established, it was viewed as an afterthought to the SSA changes that led to the creation of Medicare.

Other research has exploited the variation associated with the expansions in Medicaid eligibility introduced during the late 1980s and early 1990s. First, expansions were made to allow for those otherwise income-eligible individuals whose family structure did not meet AFDC rules to be eligible for Medicaid. Then, more broadly, states were allowed to increase their generosity via higher income thresholds or, more definitively, they were later required to cover income-eligible pregnant women, infants, and children (in most states through age 18) up to federally set limits. Some states took advantage of this opportunity to cover pregnant women and children earlier, while others only complied when forced too. Regardless, a large literature has explored the short- and long-run effects of these expansions.

The first goal of these expansions was to increase health insurance coverage. The seminal paper on this question is Cutler and Gruber (1996). They model generosity of the expansions based on applying state rules to a national sample of women and children, aiming to address measurement error issues with identifying families' own eligibility, endogeneity issues with states' changing policy based on the evolution of within-state coverage, and other endogeneity. Simulated eligibility varies then by state and year, and for children, by age of the child, based on state generosity. They then estimate the overall effect on any health insurance coverage, private coverage, and public coverage and find that the expansions led to increased coverage for children but at the cost of considerable crowd-out of private coverage for newly eligible children. Currie and Gruber (1996a, 1996b) devised and were the first to apply this simulated eligibility strategy to examine effects of the expansions on health. Their results show large favorable impacts on infant mortality and child health from the expansions despite the substantial crowd-out found by Cutler and Gruber (1996). This approach has been employed by a number of other researchers and is the main approach in the literature. That said, while it has been subjected to many validity tests, there may still be scope for using event study models or other approaches from the roll-out literature to evaluate exogeneity. Baughman and Milyo (2008) consider the endogeneity of Medicaid policy adoption but find no evidence that either the share of uninsured children in the state or the eligibility policy of neighboring states were tied to state Medicaid eligibility thresholds in place from 1990 to 2002.

The late 1990s brought two additional significant changes to Medicaid. First, as part of PRWORA, Medicaid eligibility was delinked more fully from TANF. States were required to retain the eligibility requirements in place as of August 1996 when PRWORA was signed into law. PRWORA, combined with another immigration act, also changed the rules of most social assistance programs for authorized immigrants, requiring a five-year waiting period. These changes led to reductions in Medicaid caseloads and a reduction in participation among immigrants. The second major change was the establishment of the SCHIP program, which allowed states to cover uninsured children with incomes up to 200 percent of poverty or 50 percent higher than the highest Medicaid limit at the time.[15]

A growing literature examines the long-run effects of the SCHIP/Medicaid expansions on the later health and education outcomes of the children affected by the change. Overall, the studies suggest favorable impacts, but a careful accounting of all of the potential short- and long-run effects remains to be done, and there is still some dispute about the findings concerning the contemporaneous effects of the expansions, which have not been totally reconciled. Among the studies examining health outcomes, Currie, Decker, and Lin (2008) estimate that Medicaid eligibility

[15] In the current system, some states cover children with incomes as high as 380 percent of poverty (Kaiser Family Foundation, 2014c).

from ages 2 to 4 results in better self-reported health in early adulthood. Meyer and Wherry (2012) use RD, and find that the coverage expansions led to significant declines in mortality for black children during the teen years. Miller and Wherry (2015) report that exposure in utero leads to less obesity for adults and lower likelihood of preventable hospitalizations and hospitalizations that are tied to causes they argue are plausibly affected by in utero health insurance coverage. Levine and Schanzenbach (2009) estimate that eligibility at birth leads to better reading but not math scores in middle school, while Cohodes et al. (2014) report that the expansions led to substantial increases in secondary and postsecondary educational attainment.

Taken together, the research evidence suggests that the introduction of Medicaid led to more health care spending and hospitalizations, and there is some suggestion of better health as a result. The later Medicaid expansions and addition of SCHIP appear to have led to better health as well, but there have been some costs in terms of crowd-out of private coverage. One additional piece of evidence comes from the recent Medicaid expansion in Oregon. Amy Finkelstein, Kate Baicker, and others have taken advantage of a natural experiment which increased eligibility for low income otherwise uninsured adults in Oregon's Medicaid program. Access was determined using a lottery, likely eliminating selection bias. The results of this experiment have been many faceted. In findings from the first year of the experiment, those in the treatment group had increased health insurance eligibility and coverage, as well as increased access to care (hospital care, outpatient care, and prescription drug use went up) and fewer unpaid bills sent for collection (Finkelstein et al., 2012). Mental health also improved for treatment group members. Findings from the first 18 months suggest more emergency room visits (Taubman et al., 2014). At the same time, many objective physical health measures were not significantly different across the treatment and control groups (Baicker et al., 2013b).

Unintended Effects on Employment, Savings, and Other Outcomes

The Medicaid program, like all means-tested programs, has scope for unintended consequences. It includes disincentives for work and incentives for keeping earnings and other sources of income below the income eligibility thresholds, it includes savings disincentives given rules about assets, and it also might induce fertility or family formation distortions for those who are otherwise eligible. Medicaid could also lead to reductions in bankruptcy to the extent it actually has insurance value.

Like the Medicaid research already discussed, the literature on unintended consequences for employment and earnings, for savings, and for other outcomes has examined effects associated with the introduction of the program and with later expansions. Across these studies, evidence regarding the expected work disincentive effect of Medicaid is decidedly mixed, and the same is true for other outcomes. For example, Strumpf (2011) uses a design like Almond, Decker, and Simon (2012a, 2012b) and finds no effect of Medicaid adoption on women's labor supply. Decker and Selck (2012) also use a similar design to consider women's welfare use and estimate that Medicaid adoption led to an increase in AFDC caseloads.

Yelowitz (1995) was the first to consider labor supply effects of the decoupling of eligibility for Medicaid from AFDC participation induced by the late 1980s and early 1990s Medicaid expansions. His early finding of large impacts has been questioned in work by Ham and Shore-Sheppard (2005) and others using more flexible functional forms that produce a smaller estimated impact of the program on labor supply. A recent study exploiting a reduction in Medicaid generosity in Tennessee in 2005, by contrast, demonstrates large increases in employment for those who lost coverage (Garthwaite, Gross, & Notowidigdo, 2014). And Hamersma (2013) finds that having lower eligibility limits for working parents compared with nonworking

parents led to increased labor supply in the post-PRWORA period. More recently, there has been little evidence in the Oregon Medicaid expansion of effects of access to Medicaid on earnings, employment, or participation in transfer programs. One exception is Food Stamps participation, which increased for those in the treatment group relative to the controls (Baicker et al., 2013a).

In terms of other outcomes, there is suggestive evidence that the Medicaid expansions depressed savings (e.g., Gruber & Yelowitz, 1999). At the same time, Gross and Notowidigdo (2011) find that the Medicaid expansions led to a nontrivial reduction in personal bankruptcies.

Housing Assistance

The U.S. government provides federal rental assistance to around five million low-income households through programs administered by the Department of HUD.[16] While there are a number of other smaller programs at the state and federal level, the major programs are Section 8 Housing Choice Vouchers, Section 8 Project Based Programs, and Public Housing.[17] Federal funding for housing assistance began with the Housing Act of 1937, which created the low-rent Public Housing program implemented through state-created local public housing authorities. Much of the other legislation related to low-income housing in the 1950s and 1960s, including the 1965 HUD Act, was aimed at subsidizing developers. The Housing Act of 1974 enacted the Section 8 program, which provided rental subsidies for units in privately owned properties. Private owners would receive payments in return for renting to low-income individuals who would pay a share of their income as rent.

Current eligibility for Section 8 Housing Vouchers is primarily conditioned on having income below 50 percent of the median income in an area, with more funding targeted to those below 30 percent of the area's median income and some spending aimed at those between 50 percent and 80 percent of the median. The Section 8 Housing Voucher program is administered by local public housing authorities, and the bulk of the funding is used to provide families with vouchers to make up the difference between the local fair market rent (the maximum subsidy) and the family contribution of 30 percent of their adjusted income. In addition, the Project-Based Section 8 Rental Assistance program provides slots in privately owned buildings. Finally, there are public housing buildings, run by public housing authorities. Over time, the trend has been toward providing more assistance through housing vouchers and less via slots in federally subsidized projects. In fact, a number of the large public housing projects have been demolished, while some of the privately administered programs had their contracts with HUD expire.

Because housing assistance is not an entitlement, families often spend a lengthy period on a waiting list for public housing, Section 8 vouchers, or Section 8 project-based rental assistance. Once a family gets a voucher or slot, they retain it as long as they are income eligible. Unlike many of the other programs we cover, where one can cycle on and off the program as income or other circumstances vary, the incentive to stay on these housing programs is large, given the lengthy waiting lists.

[16] The discussion of the program rules and history draws on Spar (2011) and McCarty, Perl, and Jones (2014).

[17] In addition, HUD administers the Low Income Housing Tax Credit program, which provides tax credits to developers who set aside a share of units to be affordable to low-income individuals. Through the HOME Investment Partnerships program, HUD provides states with other funding to increase access to housing for low-income individuals.

Intended Effects on Housing and Other Aspects of Well-Being

The goal of housing assistance is to provide families with access to affordable high-quality housing. Olsen (2003) reviews the evidence of the effects of housing programs based on research through the early 2000s. The research reviewed demonstrates that housing programs do improve the quantity and quality of housing consumption for participants and also increase consumption of other goods. Sinai and Waldfogel (2005) estimate that housing markets with higher levels of subsidized housing have more housing units, after controlling for demand. At the same time, they find substantial crowd-out, with more crowd-out where there is less unmet demand.

Some of the best evidence we have about the broader impacts of housing subsidies comes from the Moving to Opportunity (MTO) experiment, which assigned families with children living in public housing projects in five cities randomly to three treatments. The first treatment was a MTO Low-Poverty Voucher, which was a Section 8 rental assistance certificate that could only be used in a census tract with a low poverty rate. Families had to stay in a low poverty neighborhood for one year, and then could use their voucher wherever they chose, and otherwise had to comply with the normal rules for voucher access. The second and third treatments were a regular Section 8 voucher or no voucher (remaining in a project). These families have been followed over time, and a host of papers have been published about effects of the program (e.g., Kling, Liebman, & Katz, 2007). The program resulted in a neighborhood poverty rate reduction for the Low-Poverty Voucher group of 17.1 percentage points at the end of the first year. Interestingly, most of those in the Low-Poverty Voucher group eventually end up in neighborhoods with similar poverty levels to those in the regular Section 8 group. Note that the MTO experiment tells us about the effects of changing neighborhood poverty and participating in a voucher relative to public housing, and not the effects of public housing per se.

The final impact report for the MTO project finds that families in either voucher group were more satisfied with their neighborhood and had better neighborhood characteristics than those in the control group (Sanbomatsu et al., 2011). The Low-Poverty Voucher group reported statistically better overall ratings of housing quality compared to the control group. At the same time, Sanbomatsu et al. (2011) document small or no impacts on risky behavior and crime for children or adults, no impacts on use of social assistance, and no impacts on school achievement.

Researchers have also examined the longer-term effects of MTO on other aspects of well-being. Ludwig et al. (2011) estimate the effect of receiving a Low-Poverty Voucher on body mass index (BMI) and diabetes 10 to 15 years after randomization. They find a reduction in extreme obesity and diabetes for the Low-Poverty Voucher group compared to the control group, and a reduction in diabetes for the regular Section 8 group compared to the control group. Ludwig et al. (2012) consider the long-run effects of MTO on adults, pooling the two voucher groups together and comparing them to the group that obtained no voucher offer. Their estimates show that there were no long-term effects on self-sufficiency but some beneficial effects on mental health and large positive effects on subjective well-being. Kessler et al. (2014) examine the long-run impacts on children's mental health, finding boys in the Low-Poverty Voucher group and the regular Section 8 group had worse mental health in adolescence compared to the control group, while girls in the regular Section 8 group had better mental health than those in the control group.

While the MTO experiment suggests that housing vouchers may confer some short- and long-run benefits compared with traditional public housing, other studies that have exploited quasi-experimental methods to determine if housing subsidies more generally have their intended effects have generated equally mixed evidence.

For example, Jacob (2004) uses variation induced by public housing demolitions in Chicago to examine the effects of leaving a project, because it is being destroyed, on student outcomes. He finds no impacts on achievement and that the new neighborhoods chosen by those who leave projects facing demolition are very similar to their old neighborhoods. The waiting lists for access to assistance in many localities is another example of quasi-experimental variation in program access, allowing for clean identification of effects for those who apply. Jacob, Ludwig, and Miller (2013) use the opening of the waiting list in Chicago's Section 8 voucher program in 1997 to estimate the effects of being offered a voucher through a lottery on child mortality, finding a protective effect of a voucher offer. However, an analysis of the same natural experiment by Jacob, Kapustin, and Ludwig (2015), based on administrative data for outcomes measured up to 14 years after the voucher lottery, indicates no effect on several longer-term outcomes for the affected children, namely standardized test scores, high school graduation, earnings, social welfare receipt, arrests, and health outcomes.

Unintended Effects on Employment, Welfare Use, and Other Outcomes

Given the requirement to contribute 30 percent of income toward rent, there is obviously a concern about public housing discouraging labor supply. Olsen (2003) summarizes the earlier literature on this point, finding that subsidized housing has nontrivial labor supply disincentives. More recently, Jacob and Ludwig (2012) exploited the 1997 Chicago Section 8 expansion to estimate effects of the program on labor supply and earnings for individuals who were living in private-market housing at application. For working-age adults, they report that using a housing voucher reduced employment by about 4 percentage points, cut earnings by $328 per quarter, and increased TANF use. At the same time, they find little effect on the number of moves or the neighborhood chosen. Olsen (2003) considers another potential unintended consequence, namely the inefficiency of public subsidies of various types for providing housing, providing estimates that the costs of public provision are higher than the private costs.

Subsidized Child Care

A known barrier in the transition from welfare to work was the high cost of child care, a necessity especially for employed single parents with young children. For this reason, states initially had the option of exempting up to $200 in monthly child care expenses when determining AFDC eligibility and benefit levels. As efforts to promote greater work effort gained ground in the 1980s, the 1988 Family Support Act provided child care subsidies for AFDC recipients, created a transitional child care benefit for those leaving AFDC, and added a benefit for targeted individuals at risk of going on AFDC (Blau, 2003; Lynch, 2012). The addition of the 1990 Child Care and Development Block Grant further extended child care subsidies to low-income families not on or at risk of being on welfare.

The 1996 PRWORA legislation consolidated the AFDC and non-AFDC child care subsidy programs into a single block grant known as CCDF. With the increased emphasis on work requirements and time-limited welfare receipt, the amount of funding for subsidies increased substantially and states were granted flexibility to shift up to 30 percent of TANF funds to the CCDF block grant and to spend other TANF funds on child care subsidies. States further had flexibility to determine the amount to contribute from state funds; to set income eligibility thresholds (up to 85 percent of state median income [SMI]), provider reimbursement rates (which may be tied to program quality), and parent sliding-scale co-payments; and to establish

procedures for application and recertification. The CCDF federal program rules limit eligibility, with some exceptions, to parents who are employed, in training, or in school. Subsidies can be used for children up to age 13 and in most cases come in the form of a certificate or voucher to the family that can be used for license-exempt family, friend, and neighbor care (including in the child's home), as well as for licensed home- and center-based care. At the state level, four percent of CCDF funds must be used for activities designed to improve child care quality and to provide consumer education, a share that will increase to nine percent with the phase-in of the November 2014 reauthorization of CCDF.

Although there is considerable research examining child care subsidy receipt, the duration of subsidy use, and the relationship to employment outcomes, as well as child care stability and quality, much of the existing research is descriptive (Forry, Daneri, and Howarth, 2013; Gormley, 2007). In our review, we focus on a handful of studies that used more rigorous designs. These include studies that have exploited variation in state policies, although doubts about the exogeneity of state choices regarding the availability and generosity of subsidies are a perennial concern. Several experiments with state policy levers provide further evidence of program impacts, but these studies have limitations, as well. Several more recent studies have used other sources of exogenous variation to identify program impacts, especially on child well-being.

Intended Effects on Employment, Welfare Receipt, and Child Care Use

A primary goal of the CCDF subsidies is to support the employment of low-income families and the transition to self-sufficiency for those on welfare, while also giving parents flexibility in their choice of child care arrangements. Estimates from the literature on child care markets confirm the expectation that a reduction in the price of child care increases employment, with estimated elasticities that tend to be larger for lower income families (Blau, 2003; Blau & Tekin, 2001). Studies exploiting variation in CCDF policies across states in the post-PRWORA era to identify subsidy receipt generally find that child care subsidies increase employment rates of single mothers (Blau & Tekin, 2007; Tekin, 2005) and promote greater use of center-based care (Tekin, 2005), but have no effect on welfare receipt (Blau & Tekin, 2007). Herbst (2010) exploits variation in policies over time and across states, finding again that care subsidies promote employment. His estimates further suggest that the effect of subsidies on employment is largest for mothers facing higher child care costs. Blau and Tekin (2007) find no effect of subsidy receipt on maternal education. However, more recent estimates by Herbst and Tekin (2011b) use variation in families' distance from social service agencies that process subsidy applications to identify subsidy participation, suggesting a positive effect on human capital, measured as enrollment in higher education or participation in job training.

Although there has been no experimental evaluation of the CCDF program as currently implemented, two experimental studies shed some light on the role that child care subsidies play in employment and other outcomes.[18] In the first RCT conducted from 2005 to 2006, Illinois tested an expansion of its child care subsidy program, available at that time to families with income below 50 percent of SMI or about $2,000 a month for a single parent with one child—one of the lowest income thresholds among the states. In the evaluation, nearly 2000 families in Cook

[18] Many of the state welfare reform experiments in the 1990s included transitional child care subsidies as one of the reform elements (Grogger & Karoly, 2005). However, since the child care benefit was one of a number of policy changes, it is not possible to disentangle the effect of the child care subsidy itself on employment, welfare use, child well-being, and the other outcomes considered in those evaluations.

County, Illinois who were new applicants for child care subsidies or recertifying their eligibility and had income between 50 percent and 65 percent of SMI were randomly assigned to a treatment group approved for child care subsidy receipt or a control group subject to the state's current income eligibility rule (Michalopoulos, Lundquist, & Castells, 2010). Families were subject to co-payments and the subsidy was a larger share of the reimbursement for center-based care. At the two-year follow-up, the evaluation shows no effect on employment or earnings, largely because the families targeted for the expanded subsidy were already employed at high rates (85 percent were employed). Those in the treatment group did increase their use of center-based care and had more stable care arrangements and fewer child care related job disruptions. The study design, however, does not measure the impact of expanding subsidy eligibility more generally for low-income families who may not be currently working or with children in care.

A second RCT, conducted with about 5,100 newly approved subsidy recipient families in Washington state in 2005, cut co-payments for treatment group families relative to the status quo co-payments for control group families (Michalopoulos, 2010). The reduction in co-payments increased with family income, so those with incomes in the range 138 to 200 percent of poverty saw the largest reductions (about $75 per month on average), effectively through a lower benefit reduction rate. The alternative copayment schedule, by cutting the out-of-pocket cost of child care, increased months of subsidy use. (by 1.1 months at the two-year follow-up), but had no effect on employment, earnings, or the use of food stamps or TANF. (The study lacks a reliable measure of the type of child care used.) There is survey evidence that parents in the RCT may not have fully understood the co-payment schedule and thus how a change in earnings would alter the out-of-pocket cost of care. This may have tempered the behavioral effects.

Unintended Effects on Child and Parent Well-Being

Given the evidence that child care subsidies may affect the choice of care arrangements, it is important to understand whether children are better off as a result of having access to subsidized care, either in terms of school readiness or other developmental outcomes. On the one hand, evidence cited above suggests that subsidies may allow parents to shift to more formal care arrangements (e.g., center-based care) where quality may be higher. In addition, children may also benefit if subsidies allow greater stability in care arrangements. On the other hand, with the emphasis of CCDF on parental choice in support of work, there is no assurance that higher quality care will be selected and descriptive evidence suggests that home- and center-based providers willing to accept subsidies may have lower quality on average (Antle et al., 2008; Jones-Branch et al., 2004; Raikes, Raikes, & Wilcox, 2005).

The relationship between child care subsidy receipt and child outcomes is the subject of a series of papers by Herbst and Tekin (2010a, 2010b, 2011a, 2012) using a sample of children of single mothers in the Early Childhood Longitudinal Study, Kindergarten Cohort (ECLS-K). These papers use two identification strategies to make causal inferences, county-level policy variation in subsidy rationing and instrumenting with the distance to the nearest social service agency. They find that subsidy receipt in the year before kindergarten is associated with lower reading and math scores and increased behavior problems at kindergarten entry (Herbst & Tekin, 2010a, 2010b), although the negative effects appear to fadeout by third grade (Herbst & Tekin, 2010b). Moreover, subsidy use is associated with higher BMI throughout the kindergarten year and a greater incidence of being obese or overweight (Herbst & Tekin, 2011a, 2012), with effects concentrated at the upper

end of the BMI distribution (Herbst & Tekin, 2011a). The authors point to the use of lower quality care as one possible explanation for the findings. One drawback with using a cohort study like the ECLS-K, of course, is that much of the variation is cross-sectional, or reflects changes that affect otherwise similar children at different ages.

New research by Herbst and Tekin (2014) indicates that there may be other detrimental effects of subsidy receipt, specifically on maternal health and the parent–child relationship. They augment the ECLS-K with another cohort study—Fragile Families—and a repeated cross-section, all of which have comparable outcome measures. They use several identification strategies within and across data sources—namely longitudinal data with a lagged-dependent variable, IV with the same distance variable used in their other work (see above), and DD estimation based on the presence and ages of children conferring subsidy receipt eligibility. The analyses demonstrate remarkably consistent findings showing that subsidy receipt diminishes mother's overall self-reported health and also increases symptoms associated with anxiety, depression, and parenting stress. Measures of mothers' psychological and physical aggression toward their children are also elevated with subsidy receipt. Further research is needed to confirm these results and to understand the mechanisms through which subsidy receipt may contribute to these indicators of maternal and family well-being.

EVIDENCE OF IMPACTS FOR HUMAN CAPITAL INVESTMENTS IN CHILDREN AND YOUTH

As noted earlier, the War on Poverty also included a set of programs designed to have longer-term impacts on well-being through investments in human capital, primarily through education and training. We now review evidence regarding intended and unintended consequences for four human capital investment programs that have their origins in the War on Poverty and continue to the present: Head Start, Title I funding for elementary and secondary education, targeted higher education financial aid, and Job Corps. These programs either provide direct services by providers that are grantees (Head Start and Early Head Start) or contractors (Job Corps) to the federal government, or federal funds that are used to subsidize public schools (Title I) or individuals seeking higher education (student financial assistance). Although a review of all publicly funded human capital programs is beyond the scope of our paper, we point to where there is a broader literature examining the impacts of related programs.

Head Start and Early Head Start

Motivated by the large share of children among the poor, as well as evidence from several model programs showing that structured early learning programs could boost the IQs of disadvantaged children, Head Start was initiated in 1965 as part of the War on Poverty. From its inception, Head Start was conceived as a program to provide comprehensive services to poor children to help prepare them academically and socially for school, while also engaging, and even employing, their parents (Zigler & Muenchow, 1992). Much as it did in the early years, Head Start currently serves three- and four-year-olds whose family income falls below poverty, for up to two years before kindergarten entry. Programs are offered in center and home settings, for part- or full-day schedules during the academic or calendar year. When Head Start was reauthorized in 1994, Early Head Start was added to serve poor pregnant women, infants, and toddlers under age 3. From a base of nearly 600,000 children served in the first summer-only program in 1965, Head Start and Early Head Start combined had funded enrollment of nearly 1 million children in FY

2012 with federal funding of $7.6 billion (Office of Head Start, 2014).[19] In its 50-year history, Head Start has served over 30 million children, aged birth to 5 (Office of Head Start, 2014).

Whether or not Head Start achieves its intended effect of promoting the school readiness and healthy development of participating children has been an ongoing subject of inquiry starting with the pilot summer program in 1965, with researchers employing both nonexperimental and experimental methods.[20] Efforts to measure the impacts of Head Start have been stymied by a number of evaluation challenges: (1) the difficulty of identifying a valid comparison group with nonexperimental designs; (2) the limits of extrapolating findings of large impacts from rigorous evaluations of model programs such as Perry Preschool (e.g., Heckman et al., 2010) or from effective local programs to a national program that is far from uniform in its implementation and quality; (3) the potential for Head Start to have effects on a broad array of short- and long-term outcomes, thereby requiring measurement of multiple outcomes and long-term follow-up; (4) the potential for varied impacts for different subgroups of children; and, (5) as early learning programs have expanded over time, the difficulty of finding a "no program" control group. With these issues in mind, we highlight the current state of knowledge about Head Start impacts.

Intended Effects on School Readiness and Later Outcomes

Despite the broader goals of Head Start and the comprehensive nature of the program's services, much of the early research using both experimental and quasi-experimental designs focused on the impact of Head Start on IQ. An early quasi-experimental study (Cicirelli, 1969) did not find any cognitive benefits for participants in the first summer program, while a meta-analysis published 20 years later (McKey et al., 1985) concluded that although Head Start appeared to produce gains in IQ and other cognitive measures as of kindergarten entry, any gains experienced by Head Start children soon disappeared, the "fade-out" effect.

Two decades later, the concerns about cognitive fade-out were reinforced in the findings from the national Head Start Impact Study (HSIS), an evaluation that randomly assigned nearly 4,700 new Head Start applicants to 84 Head Start programs and their associated 353 centers in 2002 to either have access to Head Start or to be in a control group (U.S. DHHS, 2005). Although estimates from the national evaluation indicated modest impacts of Head Start participation on measures of language and literacy, early mathematics, and socio-emotional readiness at kindergarten entry for children entering Head Start at age 3 or age 4, almost all treatment-control differences were no longer evident when children were assessed at the end of kindergarten, first grade, and third grade (Puma et al., 2010, 2012).

Further analysis of the HSIS by Bitler, Hoynes, and Domina (2014) demonstrates that the estimated average impacts of Head Start mask stronger favorable effects for some groups of children, namely Spanish-speaking children and children with larger cognitive deficits at entry. Given the high rate of preschool participation by control-group children (from 2/5 to half were in center-based care, often another

[19] In FY 2012, Early Head Start had funded enrollment of 104,000 children and nearly 7,000 pregnant women in just over 1,000 programs. In contrast, Head Start had funded enrollment of more than 800,000 children. Thus, the bulk of the funded enrollment and funding is for Head Start.

[20] The research literature is indeed voluminous. A 1997 General Accounting Office (GAO) review (GAO, 1997) identified close to 600 citations for studies of Head Start and the research base has continued to expand in the years that followed that review.

Head Start program), it is important to keep in mind that HSIS taken as a whole does not measure the causal impact of participation in Head Start versus parental care, but rather the efficacy of Head Start relative to the full range of public and private programs, as well as parental care, available to low-income children at the time of the study. Two further studies attempt to examine various counterfactuals. Feller et al. (2014) find that positive effects are concentrated among those who would counterfactually have been in home care (compared to counterfactually in non-Head Start center care). Kline and Walters (2014) also focus on substitution across different counterfactuals.

Early Head Start was also evaluated in an RCT in the late 1990s (Love et al., 2002) and children have been followed to fifth grade. The program had significant moderate effects on all domains of children's outcomes (health, cognitive outcomes, language, social-emotional outcomes) and on parenting at age 3, as well as on various prekindergarten measures two years after the program had ended. By fifth grade, in a familiar story, almost all of these impacts had dissipated, although treatment group children still had marginally significantly higher levels on an index of social-emotional success defined by an "absence of risk" in five domains covering behavior problems, peer bullying, and delinquent behaviors (Vogel et al., 2010).

In the absence of long-term follow-up with experimental data, researchers have used a variety of quasi-experimental methods to look for possible effects of Head Start participation on outcomes at older ages.[21] A series of papers employ within family sibling comparisons that net out family fixed characteristics, producing causal estimates to the extent that shocks driving one child to participate while the other does not are plausibly random. Currie and Thomas (1995), using the 1979 National Longitudinal Survey of Youth (NLSY) Mother–Child (MC) sample, find persistent benefits of Head Start participation on test scores and school attainment into adolescence for white but not for black children. Likewise, Garces, Thomas, and Currie (2002) used similar methods with the PSID and estimate that Head Start increased high school graduation and college enrollment for whites but not blacks, yet reduced criminal behavior was evident for blacks and not whites. Deming (2009) applied the same approach to the NLSY MC sample to produce evidence that Head Start improves an index of favorable life course outcomes in terms of educational attainment, criminality, teen pregnancy, and health. Taking a different approach, Ludwig and Miller (2007) employed an RD design (exploiting geographic variation in funding in the early years of Head Start), finding evidence of a favorable effect of Head Start on child mortality, educational attainment, and college enrollment. At least for earlier cohorts of Head Start attendees, this body of evidence is suggestive that while cognitive benefits of Head Start may fade by the early elementary grades, there may be other short- and longer-term benefits for health, educational attainment, and other life outcomes, although the gains may not be realized for all subgroups.

The findings for Head Start are often contrasted with evaluation evidence for other preschool programs, including most recently, impact estimates for large-scale targeted and universal public preschool programs operated at the local and state level. Recent evaluations, primarily using RD-type approaches, for Boston's public preschool program for low-income children (Weiland & Yoshikawa, 2013), for targeted state preschool programs in Arkansas, Michigan, New Jersey, New Mexico, South Carolina, and Tennessee (Barnett et al., 2007; Hustedt et al., 2007, 2008; Lipsey et al., 2013), and for Oklahoma's universal preschool program (Gormley et al.,

[21] For recent reviews of much of the research on the long-term impacts of Head Start, see Currie and Rossin-Slater (2015) and Gibbs, Ludwig, and Miller (2013).

2005, 2011) consistently find positive effects of participation on measures of early language and literacy, early math skills, executive function, and socio-emotional behavior at kindergarten entry.[22] In some but not all cases, follow-up in the early elementary grades using other regression-based methods to control for selectivity shows sustained favorable impacts on academic subjects, as well as other educational gains such as reduced grade repetition and lower rates of special education use (e.g., Barnett et al., 2013, for New Jersey's targeted Abbott preschool programs; Hill, Gormley, & Adelstein, 2012, for Oklahoma's universal program). The strongest evidence for long-term preschool impacts in a larger scale publicly funded program comes from the Chicago Child-Parent Centers (CPC) program where a quasi-experimental evaluation, now with follow-up to age 28, has demonstrated significant favorable impacts of participation on educational performance (e.g., grade repetition, special education use), high school graduation, income, substance abuse, and criminality (Reynolds et al., 2011).

One explanation for the stronger impacts of these local and state preschool programs relative to Head Start may be their higher standards overall and higher quality practices. For example, lead teachers are required to have a bachelor's degree in the Chicago CPC program, in Boston's public preschool program, and in Oklahoma's universal preschool program. At the time of the HSIS, fewer than half of Head Start teachers nationwide would have met this requirement (Zill et al., 2006). Head Start programs also score lower on a commonly used measure of the quality of teacher–child interactions, the Classroom Assessment Scoring System (CLASS). The CLASS Instructional Support subscale—the component most strongly predictive of children's learning (Hamre & Pianta, 2005; Mashburn et al., 2008)—averaged just 1.9 (on a scale of 1 to 7) when Head Start programs were first assessed in the nationally representative 2006 Head Start Family and Child Experiences Survey (FACES). By comparison, scores on this CLASS subscale averaged 3.2 in the Tulsa classrooms in Oklahoma's successful universal preschool program and 4.3 in Boston's effective preschool program (Phillips, Gormley, & Lowenstein, 2009; Weiland & Yoshikawa, 2013). The use of specific curricula targeted at reading and math may also account for school readiness differences (e.g., Weiland & Yoshikawa, 2013).

Motivated in part by the relatively weak findings from the HSIS, a key policy focus for Head Start in the last decade has been improving program quality. Initiatives include increased educational requirements for teachers, the use of the CLASS to monitor program quality and identify programs that must compete for renewed funding, and technical assistance to programs and professional development for staff to improve quality. For the first time, Head Start grantees must recompete for funding if they do not meet the designated quality thresholds.

Much of the attention of Head Start research has centered on the impacts for participating children but parents may benefit as well, both through the provision of free child care and through the program components that are designed to engage parents and support their life choices. Sabol and Chase-Lansdale (2015) use the HSIS to document that parents of children who were randomly assigned to Head Start at age 3 (but not those who were randomized at age 4) were more likely to show increased educational attainment by the time the child reached age 6 compared to those randomly assigned to the control group, with effects that were more pronounced for those with some college education and for African Americans. There were no effects, however, on entry or return to the labor force. These findings

[22] Exceptions include the RD evaluation of Tennessee's targeted state preschool program (Lipsey et al., 2011, 2013), which finds positive effects that faded away by the end of kindergarten. There is also a challenge with much of this earlier research relying on age cutoffs: children who enter school at a later age are only tested at a later age.

support the renewed interest in early childhood programs as part of a two-generation strategy for poverty alleviation (Mosle, Patel, & Stedron, 2014).

Unintended Effects on Parental Investments

Aside from the intended impacts on children's developmental trajectories, Head Start may have unintended consequences, although these remain relatively unexplored. For example, the Becker and Tomes (1976) model would suggest that a publicly provided early childhood program would crowd out private parental investments in their children. Using data from the HSIS to instrument for Head Start participation, Gelber and Isen (2012) find the opposite—an increase in parental involvement, measured in multiple ways, when children are offered the opportunity to enroll in Head Start. This result may stem from Head Start's heavy emphasis on parental engagement and participation.

Title I of the Elementary and Secondary Education Act

Recognizing that educational deficits continued beyond early learning into the elementary and secondary grades, the War on Poverty also launched the 1965 ESEA, a major investment in K-12 education through the Title I grant program.[23] The Act initially doubled federal education funds and, as reauthorized in the 2001 No Child Left Behind (NCLB) Act, Title I remains the largest targeted federal investment in K-12 education at nearly $17 billion in FY 2013.

Title I funds are intended to provide compensatory academic programs for poor children as a mechanism to equalize opportunities and raise achievement. Title I funds are distributed to school districts, through their state education agencies, based on the number of children in poverty and the average level of spending per pupil in the state. Districts rank their schools by proxies for poverty, often using the free- and reduced-price lunch rate, in determining which schools are eligible and at what level of funding per eligible child. Within schools, funds are either allocated to the most educationally disadvantaged children (this is the traditional program model, now referred to as Targeted Assistance Schools) or used to support school-wide programs. Subject to compliance requirements meant to ensure that Title I does not serve as a substitute for state and local resources, districts have a fair amount of discretion in the activities they support with Title I funds, particularly within the school-wide program model.

Title I has been the subject of evaluation almost from its inception, through both descriptive information on student achievement and more rigorous evaluation studies. Indeed, in a 1996 meta-analysis, Borman and D'Agostino (1996) identified 17 federal evaluations of Title I conducted between 1966 and 1993 with over 650 unique effect sizes. Each of these studies examined standardized achievement test results at the student level using a two-wave pretest–posttest design. Most studies had used a norm reference for comparison, while a handful used various methods to identify an otherwise similar non-Title I comparison group. Evaluations of Title I have the advantage of the routine measurement of student achievement through standardized tests, as well as administrative data on other education outcomes to measure student or school performance. However, the same challenge faced in

[23] Other components of the far-reaching 1965 ESEA provided federal support for textbooks, library books, and other instructional materials (Title II), for supplementary school centers and services (Title III), for improvement of educational research and training by institutions of higher education (Table IV), and for improvement of the functioning of state educational agencies (SEAs) (Title V) (Cascio & Reber, 2013b).

evaluation of other War on Poverty programs applies to Title I: how to identify a valid counterfactual in the absence of random assignment and with a program that is potentially universally available to the targeted population. Moreover, because Title I is a revenue stream rather than a specific intervention, the "treatment" to be evaluated varies across districts and schools.

Intended Effects on Student Achievement

As a whole, the evidence from the array of studies using various methods points to little impact from federal Title I investment. The Borman and D'Agostino (1996) meta-analysis of studies conducted through 1993 suggested, at best, modest effects of Title I on student achievement (the average effect size = 0.11). Additional studies produced after the meta-analysis using more rigorous designs provide even less support for Title I impacts. For example, van der Klaauw (2008) used school-level data and an RD design that compares schools with above-average district poverty counts that receive Title I funds with schools that fall below the average and are not designated Title I schools. Based on data for New York City, the study finds no impact of being a Title I school on a range of outcomes: reading and math test scores and gain scores, grade retention, suspensions, school attendance, and student mobility rates. Matsudaira, Hosek, and Walsh (2012) employ a similar RD approach for schools in a large urban district in the northeast and also fail to find significant effects of being a Title I school on school-level average scores or for scores of student subgroups that would be expected to benefit most.

One explanation for the absence of strong impacts on student achievement, explored in several of these studies, is that the increase in federal Title I funds to school districts may be offset by declines in state and local funds, leaving Title I schools with little change in real resources. Cascio and Reber (2013a, 2013b) demonstrate that at the time of implementation, Title I funds served to equalize school spending across states, although the total funding was too small to close the spending gap between states with higher versus lower shares of poor children, a feature that remains true 50 years later. Gordon (2004) exploits the sharp changes in funding that can occur when decennial census data provide new counts of poor children and thus reallocated funding across districts. The study estimates that new Title I dollars initially increase budgets dollar for dollar, but the new funds are completely offset within three years. Likewise, Matsudaira, Hosek, and Walsh (2012) document at least a partial offset of Title I funds in the urban school district they studied, while van der Klaauw (2008) estimates no significant increase in per pupil spending for Title I schools in New York City. Such expropriation is difficult to prevent, despite stringent guidelines regarding program implementation and various accountability provisions (Cascio & Reber, 2013b). Indeed, the very guidelines meant to prevent such expropriation may contribute to the ineffective allocation of funds: as schools and districts feel pressure to demonstrate that Title I funds support activities that otherwise would not be covered with state and local funds, they support more and more marginal interventions.

While there is little evidence to suggest that Title I, as currently structured, serves to boost student performance, Cascio and Reber (2013b) note that the funding stream has given the federal government leverage over states and school districts to achieve other policy objectives. For instance, Title I funds were tied to desegregation of schools in the South in the 1960s and beyond, and the threat of losing Title I funds has served as a stick to implement accountability reforms under NCLB.

Student Financial Assistance for Higher Education

The HEA of 1965 further extended the War on Poverty's focus on human capital investments to the realm of higher education, with the goal of making a college

education available to all academically qualified students regardless of income. The Act introduced the Educational Opportunity Grants (EOG) made available directly to colleges for purposes of recruiting qualifying low-income students, as well as subsidized student loans through the Guaranteed Student Loan Program. (The federal work study program, initially established in the 1964 Equal Opportunity Act, was the third leg of the War on Poverty's higher-education funding stool.) The structure of federal student aid grants, subsidized loans, and work study supports—with programs such as the Pell Grant (which largely supplanted the EOG, now called the Supplemental EOG or SEOG) accruing to students rather than higher education institutions—has continued to evolve through major reforms with reauthorization of the HEA in 1972, 1978, and beyond (see Long, 2013; and Dynarksi & Scott-Clayton, 2013, for historical background). As a result, the federal government continues to be a significant source of higher education financing, with an investment of more than $34 billion for grants and work study in FY 2013 reaching over 11 million students. Subsidized student loans further extend the federal investment in higher education.[24]

In contrast to the other policy areas discussed in this review, subsidies for higher education have become available to students from a wider range of circumstances: grants, subsidized loans, and work study, as well as tax credits (another mechanism added in the 1990s), are not just available to the poorest students, but have increasingly extended eligibility rules to address the education financing needs of students from a broader span of family incomes. In addition, financial aid is now available to a wider array of student types—full time and parttime, those entering in young adulthood and those returning mid-career—and to those attending qualifying public, private, and for-profit institutions (Dynarski & Scott-Clayton, 2013). Given our focus in this paper, we center our discussion on the research evidence regarding impacts for the most disadvantaged students. Regardless of the population of interest, in the absence of social experiments, researchers seeking to estimate the impact of student aid must contend with the same issues of selectivity affecting evaluations of other policy areas. Consequently, while some experimental studies have been conducted, researchers have also relied on the quasi-experimental methods discussed in other policy areas, including DD and RD.

Intended Effects on College Attendance and Persistence

Much of the research evidence on the impact of student financial aid on educational outcome focuses on grant-based programs rather than student loans, and the evidence often comes from policy variation in programs other than those funded through the HEA (e.g., the Social Security Student Benefit program in effect from to 1965 to 1982, the mid-century GI bills, and state grant aid programs). Both nonexperimental and quasi-experimental studies demonstrate the general finding, as expected, that lowering the price of college attendance through grant aid increases college attendance, with a 3 to 5 percentage point increase in college going for every $1,000 increase in grant aid (Dynarski & Scott-Clayton, 2013).

In contrast, quasi-experimental evidence for the Pell Grant program in particular shows little overall effect of these federal grants targeted to low-income students (Kane, 1996), although there is some evidence for favorable impacts of Pell Grants on persistence among students already in school (Bettinger, 2004) or on enrollment for older nontraditional students who have returned to school (Seftor & Turner,

[24] In FY 2013, the available loan funds reached nearly $130 billion, although this does not reflect the cost to the federal government (OMB, 2014).

2002). One explanation for the modest effect of grants and loans on college-going among poorer students may be a lack of information about the true price of a college education, given the complexity of the college financial aid system. A recent experimental evaluation providing low-income families with assistance in completing and submitting the Free Application for Federal Student Aid (FAFSA) (required to apply for a Pell grant and other aid) as part of receiving federal income tax preparation services shows that those receiving the FAFSA assistance (plus information on their likely net college costs) were 8 percentage points more likely to have completed two years of college during the three-year follow-up period (a 29 percent increase), compared with a control group that just received financial aid information (Bettinger et al., 2012). Another experiment further points to the importance of information. The Expanding College Opportunities project provided customized information on the application process and net college costs, as well as no-paperwork waivers for application fees for a population of high-achieving low-income students, with the result that students in the treatment group demonstrated significant improvements in the quality of the colleges they enrolled in (Hoxby & Turner, 2012).

The literature on the effect of loans on higher education outcomes is more limited, but provides some support to suggest that federal loan eligibility increases college going. For example, Dynarski (2003) exploits the change in loan eligibility following the 1992 Higher Education Amendments, which eliminated home equity from the assets that are counted in the federal financial aid formula and finds weak evidence of increased college attendance, particularly for four-year colleges. At the same time, the shift in the composition of aid packages from grants to loans over time appears to have reduced college enrollment, indicating that grant and loan aid are not interchangeable as would be expected (Savoca, 1991).

Potential Unintended Effects

The increased reliance on loans in aid packages has raised concerns that the burden of student debt may steer students away from pursuits with lower remuneration (e.g., teaching or other areas of public service) and could affect other life course decisions such as home ownership and family formation, but these issues remain to be explored in a rigorous way (Long, 2013). Cellini and Goldin (2014) examine the so-called Bennett hypothesis, assessing whether more aid simply leads to higher tuition, comparing for-profit schools that do and do not receive federal student aid, using several complementary designs. They find evidence in support of the hypothesized relationship.

Job Corps

Job Corps, established as part of the Economic Opportunity Act of 1964, was a workforce development program for disadvantaged youth modeled on the depression-era Civilian Conservation Corps and remains the largest such federal job-training program. In the past 50 years, the Job Corp model remains much the same: an intensive mix of education and employment-related services provided in a residential environment (or nonresidential variant) to low-income youth aged 16 to 24 who meet at least one of several criteria: being a foster child; a parent; or in need of additional education, vocational training, or intensive counseling and related assistance in order to participate successfully in regular schoolwork or to secure and hold employment. As presently configured, the intensive services include academic education; vocational training; courses in workplace skills, social skills, and independent living; and job placement delivered in 125 residential campuses located in 48 states, Washington, DC, and Puerto Rico. The goal is for participants to leave the program and enter the workforce, enroll in postsecondary education, or join the military. The program

outlays equate to about $30,000 per year per participant for the approximately 50,000 youth served annually.

Intended Effects on Education, Employment, and Crime

Job Corps has been the subject of a several rigorous evaluations, including a quasi-experimental evaluation of the 1977 cohort and a national RCT of the 1994 to 1996 cohorts (Schochet, Burghardt, & Glazerman, 2001; Schochet, Burghardt, & McConnell, 2006, 2008; Schochet, McConnell, & Burghardt, 2003). The latter study compares outcomes for just over 9,400 eligible applicants who were randomly assigned to Job Corps with outcomes for just under 6,000 eligible controls. On average, participants received eight months of services and almost 1,200 hours of instruction. Members of the control group could not enroll in Job Corps for three years (and just 1.4 percent did so), but about 70 percent enrolled in other job training programs for which they qualified. Outcomes were examined at 12 months, 30 months, and 48 months. Tax data provided further analysis of earnings impacts nine years after randomization.

At 48 months, intent-to-treat estimates show Job Corps participants received 711 more hours of education and training and were more likely to have a GED (15 percentage points) or vocational, technical, or trade certificate (22 percentage points). Although earnings impacts were negative in the first year when Job Corps participants were in the program, by 30 months, participants' earnings were higher by 12 percent (about $1,200 per year in 1995 dollars). The earnings differential is equivalent to the expected change from an additional year of schooling, a magnitude consistent with the hours of training received by program participants. However, by the time of the 48-month follow-up, the earnings gain, on average, had been erased (although it remained significant for youth age 20 to 24) and there were no subsequent earnings impacts based on tax data. Job Corps participation had a favorable impact on arrests, convictions, and incarcerations as of the 48-month follow-up. And receipt of public assistance was smaller by a modest amount ($460).

Not all outcomes were favorably affected: at the 48-month follow-up there were no significant treatment-control differences in college attendance or completion, in the use of alcohol and drugs, and in childbearing. A benefit-cost analysis as of the 48-month follow-up indicates that participants enjoyed a positive return through higher earnings, but the benefits to taxpayers did not exceed the substantial program costs (Schochet, McConnell, & Burghardt, 2003). While not a focus of the national evaluation, anecdotal evidence suggests that there is considerable variation in program quality across sites (Fahrenthold, 2014). The Obama administration is therefore focused on improving quality in low-performing Job Corp centers, with programs subject to closure that cannot be turned around (Office of Management and Budget [OMB], 2014).

Job Corps is but one of a number of job training programs that have been made available over time to disadvantaged youth and adults (see Holzer, 2013, for a recent review), but most other programs have been or are currently far less intensive and consequently less costly. The widely held consensus regarding these programs, based on experimental and quasi-experimental evaluations, is that most programs, particularly those that are less intensive, have failed to produce economically meaningful and longer lasting improvements in employment or earnings, with little impact on poverty as a result (Karoly, 2001). Exceptions include the National Guard Youth ChalleNGe program, which, like Job Corps, is an intensive residential program for disadvantaged youth. An experimental evaluation of the program shows favorable impacts on education and labor market outcomes three years after randomization (Millenky et al., 2011), with benefit-cost estimates indicating a positive return to

society despite the high program cost (Perez-Arce et al., 2012). In addition, there is emerging evidence that training for disadvantaged youth and adults targeting specific sectors of the economy is more effective compared with more generalized training programs (Holzer, 2013).

BROADER LESSONS FOR POLICY

The implementation of the initial War on Poverty transfer and human capital investment programs in the 1960s, the addition of other means-tested safety-net programs in the 1970s and beyond, and the various program reforms along the way, have been accompanied by an extensive body of social science research that has aimed to understand whether these programs have achieved their intended effects and if there were any unintended consequences. The contributions to this rich literature have been especially pronounced in the last 10 to 15 years as researchers have gained access to new data and exploited statistical and econometric techniques to tease out, where possible, the causal impacts of programs when social experiments are not feasible. The addition of evidence from RCT demonstration projects and other policy experimentation further contributes to our understanding of the behavioral consequences of the major War on Poverty programs. In this concluding section, we seek to draw out some broader lessons for policy that emerge from our review of the program-by-program evidence base. We also call attention to key gaps in our knowledge of the behavioral effects of antipoverty programs.

Individuals Respond to Program Incentives

As a whole, the body of evidence examining the causal impacts of antipoverty programs confirms key predictions of economic theory, particularly with respect to the effect of means-tested cash and in-kind transfers on labor supply. Using a variety of experimental and quasi-experimental methods, the cumulative research findings point to the work disincentive effects associated with the original AFDC program, SSI, the flat and phase-out regions of the EITC, Food Stamps, SNAP (to a lesser extent), Medicaid (although evidence is somewhat mixed), and subsidized housing. The magnitudes of the impacts are often not large, and typically the effects are different for single-parent households versus two-parent households. At the same time, it is important to recognize that the move toward work-conditioned transfers, more generous financial work incentives, and transitional child care in the case of the TANF reforms, as well as the expansion of the EITC, have shifted the weight of the safety net for the nonelderly toward programs where work disincentives are mitigated.

Although the effects of antipoverty programs on work have probably received the most attention, there is evidence that safety-net programs can induce other unintended behaviors. For example, a handful of studies find evidence for reductions in savings as a result of the asset tests for programs such as SSI and Medicaid. For the human capital programs, studies find crowd-out in the case of Title I and increases in higher education tuition as a result of greater financial aid. On the other hand, despite popular belief, there is very little evidence that means-tested transfer programs increase childbearing, discourage marriage, or promote marriage dissolution. With the exception of one study finding very modest effects of welfare benefit levels in the AFDC era on marriage and fertility and another study suggesting that the introduction of Medicaid increased fertility among the nonwhite population, the bulk of the experimental and quasi-experimental evidence finds no support for an effect of reforms to AFDC on fertility or marriage, nor does it suggest impacts of the EITC on these outcomes, nor substantive impacts of other programs. These

findings suggest that while individuals may respond to incentives, the magnitude of the incentives need to be sufficiently large to induce a behavioral response. For instance, the fact that means-tested benefits increase with family size does not necessarily mean that childbearing will increase, because the additional income is simply too small to engender that response.

It is important to acknowledge that much of our understanding of the impacts of safety-net programs on behavior stems from research examining one program at a time. In reality, low-income individuals often participate in multiple programs and we have very little understanding of the combined effects of benefit structures, disregards, benefit reduction rates, eligibility cliffs, asset limits, and so forth on behavior. Thus, it is very challenging to go beyond the static comparisons of the poverty rate with and without cash and in-kind transfers that we discussed at the outset of this paper and instead predict the poverty rate (or any other outcomes) under the counterfactual of no safety-net programs, accounting for all the behavioral responses. That said, some recent work has attempted to create such a multiprogram calculator and use it to assess the effect of more spending on food insecurity (Schmidt, Shore-Sheppard, & Watson, forthcoming).

Policymakers May Face Trade-Offs Between Policy Goals

Our review of the empirical evidence indicates that many cash and in-kind means-tested transfer programs achieve their intended goal of raising incomes or boosting resources more generally, accounting for the value of in-kind transfers. In some cases, where the gains are large enough, individuals or families may also rise above the official poverty line, thereby achieving the goal of reducing poverty. However, these favorable outcomes are achieved because individuals participate in some transfer program, which can be viewed as a form of dependency, and because the eligibility thresholds for some programs are near the poverty line. One lesson from the welfare reform experiments of the 1990s was that the reform policies that were most successful in raising incomes and reducing poverty were those that provided generous financial work incentives tied to work requirements. Thus, although individuals increased their work effort, they continued to receive cash transfers longer than their counterparts who did not have access to the same enhanced benefits. Policies that produced larger income gains were also ones that tended to improve child well-being. In contrast, policies that placed an emphasis on a more rapid transition from welfare to work, without the more generous financial work incentives, produced gains in earned income that were offset by losses in welfare benefits, leaving families with little change in income or poverty status. Thus, the various experiments were not able to achieve income gains and poverty reduction while also reducing receipt of public benefits. By conditioning the receipt of transfers on work effort, however, the work disincentives are mitigated and there is the potential for improvements in self-sufficiency in the longer term.

Human capital investment strategies present another type of policy trade-off: providing short-term publicly funded benefits in return for longer-term gains in economic productivity and self-sufficiency. The evidence for longer-term benefits of programs such as Head Start in terms of favorable impacts on educational attainment, health status, and criminality illustrate the longer run payoff from investing public funds in early education. Subsidizing higher education costs for low-income students and providing job training through programs such as Job Corps, if they achieve their intended effects, also have the potential for long-term returns. Evidence presented earlier that in-kind transfer programs such as Food Stamps generated longer-term gains in self-sufficiency for girls exposed to the program from birth to age 5 further illustrates the potential for long-run gains from social

safety-net programs targeted to the poor. However, as we discuss further below, the ability of these programs to achieve these longer-term goals depends critically on program design features and the quality of the goods and services received.

Program Complexity May Attenuate Program Impacts

Even the casual observer of the structure of the antipoverty programs listed in Table 1 will quickly sense the complexity of each program's features, such as rules for eligibility, the application process, the approach for redetermining eligibility through time, and the structure of benefits and how they change with income, family structure, or other characteristics. Since these and other features vary across programs, the complexity is multiplied when programs are considered in tandem. Even accounting only for federal and state taxes and transfers yields an extremely complicated pattern of varying marginal tax rates at different earnings levels (CBO, 2012). These interactions are difficult for researchers to model and are likely not at all clear to potentially eligible individuals, who may not even be aware of their eligibility (e.g., Daponte, Sanders, & Taylor 1999). Further, a growing body of behavioral economics suggests that poverty itself can impede the ability to make good choices (Mani et al., 2013). This suggests that a world with complex program interactions may tax the poor even more than others.

A number of the studies we reviewed suggest a role for program complexity in attenuating the expected behavioral responses, including the intended effects. For example, an experiment to lessen the out-of-pocket costs for child care by altering the copayment schedule did not have the expected effect on employment, perhaps because families did not understand how changes in earnings would affect their copayments. An experiment with simplifying the process for applying for federal student loans led to higher college enrollments, indicating that the current process serves as a barrier to program take-up. These findings call attention to the need for more research to understand the role that program complexity plays in the measured impacts and the extent to which mitigating those barriers may allow programs to achieve their intended effects.

Achieving Intended Effects Requires Attention to Quality, Not Just Quantity

Two of the antipoverty programs we examined in Table 1 that have their origins in the War on Poverty—Head Start and Job Corps—involve the provision of direct services in the form of early education and job training, rather than transfers. A consistent theme from the evaluation research associated with those two programs is that the quality of the services is critical to the program's success. Both Head Start and Job Corps operate with federal funding and are subject to federal guidelines, but there is considerable variation in quality across program sites. Moreover, there is evidence that other early education or youth development programs serving similarly disadvantaged populations have been able to achieve even stronger impacts compared with the average effects of Head Start or Job Corps. Attention to quality requires addressing critical issues in program design and implementation such as the adequacy of funding per participant to reach the levels of quality required to achieve the intended impact, the need for incentives to achieve quality when programs are contracted out to private providers or community-based organizations, and the nature of the accountability measures in place to monitor and ensure quality. Notably, these are issues at the forefront of ongoing policy developments for Head Start and Job Corps.

Our review also points to the role that quality plays in the impact of in-kind transfer programs. Traditionally, these programs have been designed to provide

subsidized access to goods or services without regard to the nature of the goods or services eventually consumed by participants. Evidence that nutrition programs may be associated with undesirable outcomes such as overweight or obesity points to the need to address the quality of the food consumed and the potential for program reforms to influence dietary choices in the case of SNAP or the nutritional content of the meals provided as part of the various child nutrition programs. Likewise, considerations of housing or neighborhood quality factor into the potential impact of housing subsidies, while the quality of the subsidized child care received may have implications for unintended consequences of the CCDF program.

Antipoverty Policy Requires Coordination Across Federal, State, and Local Levels

Our review of the War on Poverty programs has focused on the major federal programs targeted to low-income children, youth, and families. In many cases, these programs give states the flexibility to set program features. In addition, state matching funds may be required or states may choose to supplement the federal spending. In addition, for some policy areas such as early education or K-12 education, states are actively engaged in funding and implementing their own policies targeted to the low-income population. And in many cases, there are local actors involved in spending the state and federal funding. For all these reasons, antipoverty policy typically involves the interaction of federal, state, and local policy. In some contexts, there are explicit efforts at policy coordination, but this is not always the case.

One issue is the potential for crowd-out of one form of public funding for another. As noted above, Title I illustrates this issue with evidence that Title I schools experience no net gain in funding because of offsetting funding from state and local sources, despite efforts to ensure that such displacement does not occur. Head Start provides another example of the coordination issues. When implemented in 1964, few states had anything akin to Head Start. As of 2013, 40 states funded early learning programs for three- or four-year-olds, either targeted to low-income children like Head Start or as universal preschool programs (Barnett et al., 2014). As these state investments in early education have grown, it has raised a host of issues for how to coordinate (or blend) Head Start, state, and local funding streams targeting the same population, as well as how to reconcile differences in program standards. Another challenge brought forth by the local nature of some of the programs funded by a combination of federal, state, and local money is the inherent tension between allowing local solutions in communities with successful models, while not allowing less-cohesive and less-competent communities to fall behind. These issues regarding the coordination (or lack thereof) of policy across the federal, state, and local levels merit more attention in terms of both research and program design.

Issues on the Horizon

Looking to the future, we can expect that the structure of U.S. safety-net programs—aspects such as eligibility requirements, redetermination processes, benefit levels, and services provided—will not remain static. In many respects, our knowledge of the behavioral effects of antipoverty programs is based on impacts measured when programs were first implemented (given variation in program roll-out), but that may tell us little about how programs affect decisionmaking and outcomes today. The SNAP program and the Head Start program are examples where the program and the relevant no-program counterfactual have likely changed since the era for which the roll-out impacts are valid. Even recent program reforms, in many cases, have yet to be fully investigated in terms of their behavioral impacts.

The various child nutrition programs, for example, illustrate the issues involved. As with other means-tested transfers, evaluating a mostly national program is difficult after the program has been uniformly implemented. Thus, researchers have turned to other approaches (see Table 1), such as exploiting the variation in program roll-out. As a result, much of what we know about school meal programs is from an era before states and localities had implemented direct certification methods and had experimented more broadly with universal offerings. One reason that there has been relatively little study of the universal free school meals programs and other recent reforms that go beyond immediate impacts on participation is the lack of data that can be linked about school-level school meals program details and child (and possible adult) outcomes of interest. Hopefully the new emphasis on measuring student outcomes in ways that can be linked across places and time will uncover new opportunities to learn about the consequences of these programs.

Likewise, there are still outstanding questions about the short- and long-run effects of the Medicaid program, especially in light of the PPACA, which itself has several implications for Medicaid and Medicaid research. First, PPACA included a substantial expansion of access to Medicaid for adults, to 133 percent of the FPG (which, combined with a five percent disregard, effectively means 138 percent of the FPG). The Supreme Court, while ruling that PPACA's other provisions were constitutional, also decided that states were free to refuse to expand their Medicaid program without losing access to other Medicaid funds. In practice, this has several implications. One is that there are states that have elected not to expand Medicaid. Adults in those states refusing the expansion whose income is under 100 percent of poverty (the point at which the subsidies in the exchanges kick in) have no access to subsidized insurance if their income is above the state's cutoff for Medicaid eligibility (typically quite low). Thus, there is an incentive for such individuals to work more to get access to subsidized insurance coverage, which is quite different from the typical incentives. At the same time, individuals near state borders may face similar labor markets but quite different public health insurance rules if one state has adopted an expansion and another has not, and this could have implications for health, health care use, and other outcomes such as migration.

Although federal policy with respect to antipoverty programs will often mean programs implemented on a national scale or nationwide policy reforms, it is important to continue to allow for policy experimentation at the state and local levels. Such variation may arise when states are given authority to determine program features, such as eligibility requirements or benefit levels subject to federal guidelines or when states are granted waivers to deviate from federal requirements. As we have seen in our review of prior research, this type of policy variation can support quasi-experimental research methods to evaluate policy impacts. Another critical source of policy experimentation is through smaller or larger scale demonstration projects accompanied by RCT evaluation. Such experimental evaluations provide a valuable complement for understanding the shorter- and longer-term impacts of distinct policy reforms. Together, both types of empirical evidence will serve to advance our understanding of the intended and unintended consequences of antipoverty programs and will support the design of more effective strategies for achieving the policy goals that motivated the War on Poverty.

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APPENDIX A

Table A1. Funding, participation, and benefit levels for major antipoverty programs serving the nonelderly population.

Program	Federal outlays FY 2013 (billions of dollars)	Participation 2013	Benefit amount 2013 (dollars)
AFDC/TANF	$17.107	1.7 M families 0.9 M adults 3.0 M children	Per month: $170–$923 for single adult with two children depending on state
SSI	$56.489[a]	9.0 M persons[a] 6.8 M ≤ age 64 1.4 M ≤ age 18	Per month: $710 for single recipient $1,066 for couple
EITC[b]	$57.513	26.7 M tax filing units	Per month: $195 per tax filing unit (avg.)[b]
Food Stamps/SNAP	$82.548[a]	23.1 M HHs[a] 47.8 M persons[a]	Per month: $132 per person (avg.)
Child Nutrition Programs	$19.325	11.0 M SBP 21.6 M SLP	Per month: $22.87 SBP per child (avg.) $30.63 SLP per child (avg.)
WIC	$6.557	2.1 M women 2.1 M infants 4.6 M children	Per month: $43 per person (avg.)
Medicaid	$265.392[a]	12.2 M nonelderly nondisabled adults 28.0 M nondisabled children	n.a.
SCHIP	$9.483	5.7 M children	n.a.
Housing Assistance[c]	$34.180[a]	2.1 M Section 8 vouchers 1.3 M Section 8 project-based units 1.1 M public housing units	n.a.
CCDF	$5.049	0.9 M families 1.5 M children	n.a.
Head Start/Early Head Start	$7.573	956K children	n.a.
Title I of ESEA	$16.795	21 M children	n.a.
Student Financial Assistance for Higher Education[d]	$34.037	8.9 M Pell Grants 1.5 M SEOG 0.7 M work study	Per year per student (avg.): $3,651 Pell Grants $866 SEOG $1,678 Work study
Job Corps	$1.577	~50 K youth	n.a.

Sources: Federal outlays: OMB (2014). Participation and benefits: DHHS (2014a, 2014b), Floyd and Schott (2013), SSA (2014), IRS (2014), USDA (2014a, 2014b), Kaiser Family Foundation (2014a, 2014b), Spar (2011), Office of Head Start (2014), U.S. Department of Education (DoE) (2014), and OMB (2014).
Notes: Participation and benefit data are for 2013 with the exception of the EITC (2012 tax year), Housing Assistance (FY 2009), CCDF (2012), and Title I (2009-10 school year). HH, household; M, million; K, thousand.
[a]Includes funding for the elderly or elderly recipients.
[b]Based on average EITC credit in 2012 of $2,335. The maximum tax credit in tax year 2013 for a single adult or couple with three or more children was $6,044, for two children was $5,372, for one child was $3,250, and for no children was $487. Monthly amount is total credit divided by 12.
[c]Figures are for Tenant-Based Section 8 Rental Assistance, Project-Based Section 8 Rental Assistance, the Housing Certificate Fund, the Public Housing Operating Fund, and the Public Housing Capital Fund.
[d]Figures are for Pell Grants, Supplemental Educational Opportunity Grants (SEOG), and work study.

APPENDIX B: RESEARCH ON CAUSAL IMPACTS OF ANTIPOVERTY PROGRAM

This appendix lists the research studies, reviewed in the body of the paper, that use experimental or quasi-experimental methods to examine the effects of the antipoverty programs summarized in Table 1. For each antipoverty program, the relevant studies reviewed are listed. Studies may be listed under more than one program.

Aid to Families with Dependent Children (AFDC) and Temporary Assistance for Needy Families (TANF)

Bansak, C., Mattson, H., & Rice, L. (2010). Cars, employment, and single mothers: The effect of welfare asset restrictions. Industrial Relations, 49, 321–345.

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Klerman, J., & Haider, S. (2004). A stock-flow analysis of the welfare caseload. Journal of Human Resources, 39, 865–886.

Matsudaira, J. D., & Blank, R. M. (2013). The impact of earnings disregards on the behavior of low-income families. Journal of Policy Analysis and Management, 33, 7–35.

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Moffitt, R. (1983). An economic model of welfare stigma. American Economic Review, 73, 1023–1035.
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Supplemental Security Income (SSI)

Black, D., Daniel, K., & Sanders, S. (2002). The impact of economic conditions on participation in disability programs: Evidence from the coal boom and bust. American Economic Review, 92, 27–50.
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