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Child Well-Being in an Era of Welfare Reform: The Sensitivity of Transitions in Development to Policy Change

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This study examined the age-specific pattern of effects of welfare policies on child achievement. Drawing from 7 random-assignment welfare and antipoverty evaluations that provided more than 30,000 observations of children's achievement, this study found that times of developmental transition are the only periods sensitive to the changes in families brought about by these policies. More specifically, small positive effects of welfare and antipoverty policies were found for children making the transition into middle childhood, and small negative effects of these same policies were found for children making the transition out of middle childhood and into early adolescence. Effects were robust across various program groupings and could not be attributed to family characteristics that differ for children of different ages. This research informs the understanding of how changes in employment and income for low-income parents affect development across childhood.

Keywords: poverty, school achievement, policy

As with nonexperimental research on maternal employment and income, recent research based on random-assignment experimental studies has found that welfare and employment policies appear to affect younger and older children differently. Programs that increase parents' employment produce either neutral or positive effects for preschool and early school-age children in poverty but negative effects for adolescents (Gennetian, Duncan, Knox, Clark-Kauffman, & Vargas, 2002; Gennetian & Miller, 2002; Huston et al., 2001; Morris, Huston, Duncan, Crosby, & Bos, 2001). Experimental data on the youngest children are scarce, but the available evidence has shown neutral effects (Morris & Michalopoulos, 2003).

We are far from understanding the precise pattern of policy effects across the ages and stages of childhood (i.e., from toddlerhood to preschool, from early middle childhood to later middle childhood, and from preadolescence to adolescence). Are there points in development that represent particular sensitivity, positive or negative, to welfare and employment policies? In this article, we address this question by pooling microdata from seven random-assignment welfare and antipoverty evaluations that together provide more than 30,000 observations of children's achievement.

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This study adds to the existing literature in several ways. First, all of the programs were evaluated using a rigorous random-assignment design, the most reliable methodology for determining their causal effects. Second, because the programs targeted parents' employment and sometimes family income, examining the effects on children can inform hypotheses about how changes in parents' employment and income affect children's well-being. Finally, prior research on the effects of welfare and employment policies has examined the effects of policies one at a time (cf. Gennetian & Miller, 2002; Huston et al., 2001), and across groups of children that include multiple developmental stages (Morris & Michalopoulos, 2003). In this study, we examine the generalizability of the findings to a broader set of samples and policies, and we do so in a way that allows us flexibility to explore alternative hypotheses.

Effects of Employment and Income by Child Age

The welfare and antipoverty policies we include here target parents' economic outcomes and are likely to affect children only indirectly through changes in their parents' economic circumstances. A wealth of developmental, economic, and policy research, relying on both nonexperimental and experimental data, suggests that changes in parents' employment and family income can affect children's development, at least under certain circumstances. Economic and psychological theories provide frameworks for understanding the mechanisms by which parents' employment and income can have such effects, by emphasizing both the role and distribution of material and social resources (Becker, 1981; Bergstrom, 1997; Coleman, 1988) and family psychological processes (Chase-Lansdale & Pittman, 2002; McLoyd, 1990, 1997, 1998; McLoyd, Jayartne, Ceballo, & Borquez, 1994). More specifically, economic theory suggests that employment and income may affect children's development by influencing the goods parents purchase for children (books, toys, and child care) and the time parents spend with children. Psychological theory emphasizes

the effects of employment and income on parental emotional well-being (stress and depression), and in turn on parenting behavior, as one pathway by which parents' employment and income affect children. Clearly these two theories need not be considered mutually exclusive.

For low-income families headed by single mothers, the associations between maternal employment and children's cognitive and social development tend to be positive, but much of this difference is a function of preexisting differences between mothers who are employed and mothers who are unemployed (Harvey, 1999; Huston, 2002; Vandell & Ramanan, 1992; Zaslow & Emig, 1997). The effects of maternal employment on children's development also depend on the characteristics of employment (its quality, extent, and timing) and on the child's age (Brooks-Gunn, Han, & Waldfogel, 2002; Harvey, 1999; Parcel & Menaghan, 1994). Highly routinized jobs that pay very low wages and afford little autonomy are associated with low levels of home environmental stimulation, which in turn has been found to affect children's development adversely (Moore & Driscoll, 1997; Parcel & Menaghan, 1994, 1997).

More relevant for the current study are differences in the effects of maternal employment by child age that suggest difficulties during both infancy and adolescence of maternal employment. Some evidence suggests that there may be negative impacts of employment during the child's first year of life (Brooks-Gunn et al., 2002) and some investigations suggest negative effects for adolescents (Huston, 2002). In one low-income sample, maternal employment has been found to be associated with reduced parental supervision and increased adolescent delinquency in low-income families (Sampson & Laub, 1994). But more recent findings suggest that maternal movements into employment may be beneficial for the mental health of adolescents, while having few effects on preschoolers (Chase-Lansdale et al., 2003).

Poverty has consistently negative associations with children's development, but there is considerable controversy about the causal role of income per se, as opposed to other correlates of poverty (Bradley & Corwyn, 2002; Duncan & Brooks-Gunn, 1997; Duncan, Yeung, Brooks-Gunn, & Smith, 1998; Mayer, 1997; McLoyd, 1998). Several studies suggest that income is more consistently related to cognitive performance than to behavior and health (Duncan & Brooks-Gunn, 1997; Duncan et al., 1998; Klerman, 1991; Korenman & Miller, 1997). Movements into and out of poverty appear to be more important than changes across higher levels of income, and chronic poverty appears more detrimental than transitory poverty (Bolger, Patterson, Thompson, & Kupersmidt, 1995; Bradley & Corwyn, 2002; Dearing, McCartney, & Taylor, 2001; Duncan & Brooks-Gunn, 1997; Duncan, Brooks-Gunn, & Klebanov, 1994; McLoyd, 1998). As with employment, however, the effects may differ by child age. One longitudinal analysis demonstrated that early childhood poverty (when the child is ages 0–5 years) was more strongly associated with children's failure to complete schooling in adolescence than was poverty from age 6 to 15 (Duncan et al., 1998).

Understanding the Source of the Variation in Effects

Age-specific patterns of effects of welfare programs may arise because of differences in the way child characteristics interact with microsystem changes brought about by policy initiatives targeting maternal employment and income. Prior theoretical work would suggest that differences in developmental tasks, sensitivity to change, and interactions with the environment across childhood may explain differences in the way younger, middle childhood, and adolescent children are affected by maternal employment.

First, responses by younger and older children to changes in parents' employment and income may reflect differences in the extent to which changes in employment and income affect the achievement of developmental tasks, that is, the goodness of the stage-environment fit (Eccles et al., 1993). Children across developmental periods may vary in their responses to policies that affect employment and income because of differences in the fit between these changes in children's proximal environments and the attainment of stage-salient developmental tasks (Sroufe, 1979; Waters & Sroufe, 1983). For example, researchers finding negative effects of maternal employment in the second and third quarter of the first year of life have argued that the parent–child separation that occurs with parental employment interferes with the child's formation of the representation of the parent at the critical moment when these representations are being established (Baydar & Brooks-Gunn, 1991). For older children, parents' transitions to employment that occur at the time of identity development may foster the increasing autonomy needed at this point in development.

Differences across developmental responses to policy changes may also reflect differential sensitivity of children of different ages to changes in development. A number of researchers have argued that early childhood is a sensitive period in development, amenable to intervention (Shonkoff & Phillips, 2000; Sroufe, 1979). Waddington (1957) has described development as proceeding along the branches of a tree; whereas changes in developmental trajectories can occur at any point at which a new branch is formed, the ability of the individual to substantially alter his or her developmental course becomes increasingly difficult over time.

Transitions in development may also serve as important periods in which change may be most likely to occur. Because developmental transitions involve a qualitative reorganization of an individual's organizing systems, these transitions are points at which development is most in flux and open to change in direction (Graber & Brooks-Gunn, 1996). For example, some researchers have found that the transition to adolescence is a time of an increased number of stressful life events (Brooks-Gunn, 1991; Ge, Lorenz, Conger, Elder, & Simons, 1994). Experiencing changes in maternal employment along with the pubertal and life-events stresses that may accompany the transition to adolescence may have negative implications for the development of young adolescents.

Also important is the extent to which the interaction between the child and the environment may change with development. Bronfenbrenner has described the development of the individual as occurring within a nested and interactive set of systems (Bronfenbrenner & Morris, 1998). However, the nature of the interactions among these systems is affected by the developmental stage of the individual. For example, young children are primarily affected by their family context and only indirectly, through interactions with their family, by community or neighborhood contexts. In contrast, older children may be more directly affected by their own interactions within their neighborhoods. Because welfare and employment policy interventions target changes in the family (rather than

school or community contexts), they may play a relatively stronger role for younger children's developmental outcomes (McCall, 1981; Yoshikawa, 1994).

Confounds of Family Ecology

In addition to developmental differences between younger and older children, the family ecology across childhood may also differ considerably, and may account for differences between older and younger children in their responses to increases in maternal employment and income among single-parent welfare recipients. Family differences associated with child age may include the demographic characteristics of parents, family composition, welfare and employment history, and the presence of varying employment barriers. For example, younger children are more likely to have younger mothers, and parents' age, rather than child's age, may be the more important factor in how employment policies affect children's development.

Method

Studies

Our analysis utilizes seven random-assignment studies that together evaluate the effects of 13 welfare and employment programs in the United States and two provinces in Canada. All were begun in the early to mid 1990s and designed to estimate the effects on low-income families and children of programs designed to increase parental employment. Many were pilot programs tested by individual states under waivers of the rules governing Aid to Families With Dependent Children (AFDC), the welfare system that was replaced in 1996 by Temporary Assistance for Needy Families (TANF). All were designed as employment treatments of one form or another; all were intended to reduce welfare and increase employment without direct intervention components targeted at parents' mental health, parenting, or outcomes for children directly. Although many policies were tested, the approaches can be grouped into two categories: *generous earnings-supplement policies* that are designed to encourage work and increase income via make-work-pay strategies, and *mandatory employment services* and *time-limited programs*, which attempt to encourage work via sanctions and benefit termination strategies.

More specifically, generous earnings supplements are designed to make work more financially rewarding, usually by increasing the earnings disregard (the amount of earnings that is not counted as income in calculating a family's welfare benefit) so that families can keep part of their welfare dollars when they go to work. Mandatory employment services are requirements that recipients participate in employment-related activities as a condition of receiving their welfare benefits. The primary tool used to enforce participation mandates is sanctioning, whereby a recipient's welfare grant is reduced if she or he does not comply with program requirements. Finally, time limits intended to reduce welfare dependence were a feature of two studies, although the limits were bundled with other program features like mandatory services and earnings supplements.

The following studies were included in this analysis: Connecticut Jobs-First (CT Jobs First; D. Bloom et al., 2002); Florida Family Transition Program (FTP; D. Bloom et al., 2000); Los Angeles Jobs First Greater Avenues for Independence (LA GAIN; Freedman, Knab, Gennetian, & Navarro, 2000); Minnesota Family Investment Program (MFIP; testing the effects of two programs, Full MFIP and MFIP Incentives Only; Gennetian & Miller, 2000); National Evaluation of Welfare to Work Strategies (NEWWS; testing the effects of six programs in three sites across two follow-up points; Hamilton et al., 2001; McGroder, Zaslow, Moore, & LeMenestrel, 2000); New Hope (testing the effects of one program at two follow-up points; Bos et al., 1999; Huston et al., 2003); and the Canadian Self-Sufficiency Project (SSP; testing the effects of one program at two follow-up points; Michalopoulos et al., 2002; Morris & Michalopoulos, 2000). Notably, the inclusion of the Canadian study (SSP) is appropriate given that prior work has shown very similar effects across generous-supplement policies in the U.S. and Canada (Berlin, 2000), despite any differences in the policy context (e.g., that Canada has national health insurance).

All of these studies had a common design. In each study, sample members were randomly assigned at baseline to one or more program groups that were subject to a new set of welfare rules and benefits or to a control group that received the prevailing AFDC welfare benefits package and rules (or other benefits largely available to low-income families). In all but one of the studies, parents were applying for welfare or renewing eligibility when they were randomly assigned. (In the case of the New Hope study, all geographically eligible low-income parents were eligible to participate.) Extensive analyses conducted on each sample as part of the original studies establish that on a variety of baseline parental and family characteristics, differences between program and control groups were extremely rare, suggesting that random assignment was implemented properly, and therefore that any differences in outcomes between the groups after baseline can be attributed to the program and not to any other differences between families.

Sample

Our data consist of 33,732 child observations taken from 25,088 children living in 13,302 families in the seven studies (see Table 1). Children range in age from 0–15 years at the point of random assignment. These children were living in primarily single-parent families, most of whom were receiving welfare at the beginning of the studies. In all but one study, families entered the studies at the time of their application or re-determination application for welfare (the only exception to this is the New Hope study, which recruited both welfare recipient and nonwelfare recipient families that met income and geographic residence requirements). This means that these samples, unlike those in many other studies, do not consist of study volunteers. That is, application to the welfare system or receipt of welfare in almost all of these studies (with the exception of New Hope) required participation in the study effort (i.e., random assignment to the program and control groups and the administrative sources of data collection). Of course, parents could opt out of the survey effort, although response rates in all studies were quite high (between 71% and 90%), and extensive nonresponse bias analysis conducted as part of the original studies confirmed the equivalence of program and control groups in these respondent samples (suggesting that any bias due to nonresponse is minimal; D. Bloom et al., 2000, 2002; Bos et al., 1999; Freedman et al., 2000; Gennetian & Miller, 2000; Hamilton et al., 2001; McGroder et al., 2000; Morris & Michalopoulos, 2000).

As shown in Table 1, though the children in our sample span the entire childhood age range, our largest samples of children were ages 2–5 years at the beginning of the study period, with smaller samples of children under age 2 and over 12. In some cases, individual children were followed at multiple intervals.

Procedures

Data in each study were compiled from a variety of sources. Basic demographic information is available for all sample members from a background information form completed just prior to random assignment. Staff in the financial offices interviewed each sample member and collected important demographic information, such as the sample member's age, educational attainment, work history prior to random assignment, and prior welfare receipt.

Data from administrative records are used to track families' benefit receipt and employment prior to baseline and during the follow-up periods.

Table 1
Sample Sizes by Child's Age at Baseline and by Program

Program	Age at baseline (years)								
	0–1	2–3	4–5	6–7	8–9	10–11	12–13	14–15	Total
All programs	1,964	9,284	10,132	4,169	3,595	2,607	1,442	539	33,732
Earnings supplement programs									
MFIP	129	1,003	922	785	704	510	384	149	4,586
New Hope: 2-year follow-up	3	171	295	259	245	179	68	53	1,273
New Hope: 5-year follow-up	377	761	524	533	470	218	0	0	2,883
CT Jobs-First	129	754	767	758	639	427	313	92	3,879
SSP: 3-year follow-up	424	1,154	1,617	1,228	1,020	862	369	132	6,806
SSP: 4.5-year follow-up	578	1,217	505	0	0	0	0	0	2,300
Total	1,640	5,060	4,630	3,563	3,078	2,196	1,134	426	21,727
Non-earnings supplement programs									
Los Angeles Jobs-First GAIN	0	0	169	229	193	171	155	113	1,030
NEWWS: 2-year follow-up	0	1,260	1,607	0	0	0	0	0	2,867
NEWWS: 5-year follow-up	0	2,374	3,206	0	0	0	0	0	5,580
FTP	324	590	520	377	324	240	153	0	2,528
Total	324	4,224	5,502	606	517	411	308	113	12,005

Note. Because of their small sizes, age groups 12–13 and 14–15 years were combined for the purposes of the analyses in this article. MFIP = Minnesota Family Investment Program; CT Jobs-First = Connecticut Jobs-First; SSP = Canadian Self-Sufficiency Project; GAIN = Greater Avenues for Independence; NEWWS = National Evaluation of Welfare to Work Strategies; FTP = Florida's Family Transition Program.

Public assistance benefit records include monthly information on cash welfare and food stamp benefits provided to each member of the research sample. Records from two other programs provide information on cash supplements given to working families outside of the welfare system. Unemployment insurance earnings records (for the studies in the U.S.) provide quarterly earnings information for every sample member, as reported by employers to the Unemployment Insurance (UI) system. (Although the system is labeled *unemployment*, the earnings records are compiled for all workers who work for any employer reporting earnings to the Social Security system.)

Information on children and families was collected from a parent survey administered to each family 2 to 5 years after baseline, depending on the study. Most of these surveys were conducted in the home, although some telephone surveys were conducted (if the family lived outside the interviewing area or refused an in-home interview). The survey in each study had many common questions and was approximately 1 hr and 15 min long. Information was collected on parents' employment, family circumstances (such as household composition, sources of income, and material hardship), and children's development.

Finally, in some of the studies, measures of children's development were also collected via surveys conducted directly with children, tests administered to children, and/or surveys conducted with children's teachers 2 to 5 years after baseline, depending on the study.

Measures

Child age at baseline. Each child's age at the point of random assignment was computed from birthdate information provided on the parent surveys and information regarding the date of random assignment of the family. Eight age categories were created: ages 0–1, 2–3, 4–5, 6–7, 8–9, 10–11, 12–13, and 14–15 years. Examination of the sample sizes suggested that the 12- to 13- and 14- to 15-year-old age groups were more limited in size, and analyses indicated that these latter two groups needed to be combined for sufficient power to detect effects for this older age group. (The pattern of effects was similar across these two age groups, however.)

School achievement or cognitive performance. Children's cognitive performance or school achievement was measured using a parent or teacher report or test scores, with some studies including multiple sources per child, and three of the studies (testing eight programs) assessing children at multiple times. (Socioemotional behavior measures were not collected consistently across the studies.) All but one study included parent reports of children's achievement, typically measured with a single-item 5-point rating of how well the child was doing in school. (Data in SSP are based on an average across three core academic subjects instead of an overall rating, but they are still based on the same 5-point scale.) Teacher reports of achievement (collected in three of the studies) were based on items from the Academic subscale of the Social Skills Rating System (Gresham & Elliott, 1990). On this 10-item measure, the teacher compares the child's performance with that of other students in the same classroom on reading skill, math skill, intellectual functioning, motivation, oral communication, classroom behavior, and parental encouragement (internal consistency α = .94). Test scores include the Peabody Picture Vocabulary Test, a math skills test containing a subset of items from the Canadian Achievement Tests, 2nd ed. (CAT/2), the Bracken Basic Concepts Scale, and the math and reading scores from the Woodcock Johnson (WJ) tests of achievement, all well validated and reliable tests of children's cognitive performance. To provide comparability in outcomes across studies, we have standardized the cognitive performance and achievement outcomes using study-specific control-group standard deviations and centered the scores using study-specific full-sample means.

A reliability and validity analysis of these data shows that parent and teacher reports of children's functioning are modestly associated (r = .37). Although teacher reports are slightly more highly associated with standardized test scores than are parents' ratings, both show moderate associations (r = .38 between parent reports and scores on the CAT/2 math skills test, and r = .49 and .54 between teacher reports and scores on the WJ test of math and reading, respectively). It is important to note that the relation between parent and teacher reports of child achievement does not differ from early elementary school age (ages 6–9 years at the time of the assessment) to pre- and early adolescence (ages 10–14 years at the time of the assessment).

Earnings. The pooled data set includes quarterly earnings from UI records for all years of the follow-up period for each study conducted in the U.S. These data provide information about earnings of sample members in the state in which the study took place, but do not include earnings from other states nor from jobs not reported to the state's UI system. For the

study of SSP conducted in Canada, earnings were collected via the parent surveys, computed from reports on hourly wages, hours worked per week, and weeks worked per month. Canadian dollars were converted to American dollars. Average earnings per year were computed for the first 2 years of follow-up using earnings, adjusted for 2001 prices using the Consumer Price Index (CPI) annual average figures.

Income. For all sample members in the studies conducted in the U.S., administrative records provided data on monthly cash assistance and food stamp benefits, cash supplement payments provided by the earnings supplement programs, and quarterly earnings in jobs covered by the UI system. For SSP, administrative records provided information on receipt of Income Assistance (IA; Canada's welfare program) and receipt of SSP supplement payments, whereas the parent survey collected data on earnings from employment. The pooled data include information for each year following random assignment on (a) average annual parent income, based on the sum of earnings; (b) AFDC, TANF, IA, and supplement payments; and (c) food stamp payments. Note that this income measure omits self-employment and informal earnings, other public transfers, private transfers, and earnings from family members other than the sample member. All income amounts have been inflation-adjusted for 2001 prices using the CPI annual average figures. In the case of the SSP, Canadian dollars are converted to American dollars before being adjusted for inflation. From this information, average annual income per year was computed over the first 2 years of the follow-up.

Baseline control variables. Baseline surveys and administrative data sources provided a set of comparable pre-random-assignment parental and family control variables: length of time cash assistance was received in the year prior to baseline, average earnings in the year prior to baseline, whether the parent was employed in the year prior to baseline, whether the parent had a high school degree or GED, whether the parent was a teenager at the time of the child's birth, the marital status of the parent, the number of children in the family, the age of the youngest child in the family, and the race or ethnicity of the parent.

Control variables in pooled sample models. When conducting models across the pooled dataset of welfare and employment experiments, we include a set of control variables in addition to the sample member-specific control variables listed above. These include length of follow-up, type of assessment, and 10 variables representing site or study controls; these 10 include 1 variable for each of the five studies that took place in a limited geographic region and 1 for each site in the two studies taking place across diverse regions (SSP in New Brunswick and British Columbia, Canada; NEWWS in Atlanta, Georgia, Grand Rapids, Michigan, and Riverside, California).

Analysis Strategy

We examine individual study estimates and aggregated results using pooled study estimates. Our aggregation across studies is appropriate if each study is considered to be a realization of a larger population of policy experiments, the general properties of which we seek to explain (Cooper & Hedges, 1994). In this view, pooling reduces sampling variation inherent in any individual study and enables us to make comparisons across program types.

Individual study impacts by child age. We test our hypotheses by estimating ordinary least squares (OLS) models in which treatment status (e.g., whether in the experimental or control group) varies with children's age, within each study, and separately by length of follow-up for those studies with multiple follow-up waves. We accomplish this by constructing a set of interaction terms of child age group (ages 0–1, 2–3, 4–5, 6–7, 8–9, 10–11, and 12–15 years at baseline) with the experimental dummy for each program model tested. Our regression models include child age (the seven groups listed above), interactions of experimental dummy and age, and controls for baseline family and child-level characteristics. More explicitly, our regression model is:

$$Y = a + b_1A_1 + b_2A_2 + b_4A_4 + b_5A_5 + b_6A_6 + b_7A_7 + c_1A_1 \times \mathrm{EXP} + c_2A_2 \times \mathrm{EXP} + c_3A_3 \times \mathrm{EXP} + c_4A_4 \times \mathrm{EXP} + c_5A_5 \times \mathrm{EXP} + c_6A_6 \times \mathrm{EXP} + c_7A_7 \times \mathrm{EXP} + d_1\mathrm{control\ variables} + \varepsilon_1, \quad (1)$$

where Y is child achievement, mother's earnings, or family income, $A_1 - A_7$ are dummy variables for the seven age groups, EXP is the experimental–control group dummy, and control variables are our set of baseline control variables.

Note that A_3 is omitted from the age dummy group but not from the age-by-treatment interaction groups (A_3 was chosen as the reference category because all of our data sets provide observations on children who were ages 4 or 5 at baseline), and there is no main effect term for EXP. With this formulation, coefficients c_1 to c_7 show experimental and control group differences by age, controlling for baseline characteristics. (This model is equivalent to that using the more typical specification with a main effect of experimental status and the interaction with all but one of the age group terms, but has the distinct advantage of formally deriving estimates for, and testing the significance of, the experimental–control contrast for each age group rather than deriving estimates of the difference in impacts between a particular age group and the randomly selected omitted age group.)

Huber-White methods are employed to adjust standard errors for nonindependence of multiple reports per child and multiple children per family, clustering the findings at the family level. Therefore, standard errors adjust for the nonindependence of siblings and of multiple reports on a given child's achievement. Sensitivity tests were conducted to examine the extent to which clustering by child rather than family changed the results; results were very similar with either approach.

Pooled data regression analysis. As noted earlier, this set of studies can be clustered into two groups: those that were aimed at moving parents from welfare into employment through mandates and time limits, and those that had the added objective of increasing family income through the use of financial incentives. Our pooled data models take the form of Equation 1 above, but also includes cross-study control variables as well, such as a measure of the time between baseline and the given achievement assessment and a study/site indicator. To enhance interpretation, all variables representing parent and family characteristics, source or reporter, follow-up length, and country were centered with mean zero before interacting with the experimental dummy.

By including study/site dummy variables, our regression estimates of treatment and control differences are based exclusively on within-site variability. The fact that we pooled data across sites but included site/study dummies provides what amounts to an average treatment effect across the sites. Although analogous to meta-analytic techniques that average impact estimates across studies, our pooled estimates differ slightly from meta-analytic averages because they are based on a model that assumes that baseline covariates have the same effects across sites. To test the sensitivity of our key findings to this assumption, average effects across studies were also computed using conventional meta-analytic techniques that allow the effect of baseline covariates to differ across sites (findings available from the authors). These analyses show a very similar pattern of effects as those derived from the pooled sample estimates, providing confidence in the specification of the model we estimate here.

Our pooled impact estimates include only a variance component that is, in effect, the weighted average estimation error for each site. As such, our estimates assume homogeneity of impacts across this set of sites and studies (referred to as a "constant-effects estimator"; H. Bloom, 2003), although perhaps more commonly referred to as a "fixed-effect estimator" (Lipsey & Wilson, 1996). We do not assess impacts under conditions of cross-site variation in true impacts by including the necessary additional variance component that would have been included had we estimated a random-effect estimator through such techniques as hierarchical linear modeling. This choice reflects, in part, our limited power with the current

number of studies to test this second-level variance component. With regard to the interpretation of our findings, this means our estimates provide efficient impact estimates under the assumption of homogeneous impacts across sites, but they do not allow for inference to a broader population of experiments under the assumption of variation in the true impacts across sites.

Results

Descriptive Statistics

Means and standard deviations of all of the baseline demographic variables and our three outcome measures (earnings, income and child achievement) are presented in Table 2, across all studies and by experimental group status. (Note that some of the measures represent dichotomous characteristics and therefore can be interpreted as percent of the sample with that particular characteristic.)

The pooled sample experimental and control groups are well matched. Although significance tests conducted on the individual variables find that mean values on 8 of the 12 baseline variables are significantly different at the $p < .01$ level (not shown), the magnitude of the differences observed between the experimental and control groups is quite small. Moreover, these differences did not indicate systematic imbalance of the experimental and control groups. All of these baseline characteristics were included as

Table 2
Means and Standard Deviations of Baseline Sample Characteristics and Outcome Measures

Variable	Total sample	Experimental group	Control group
	Parent characteristics		
Age (years)	30.1 (6.3)	30.1 (6.3)	30.0 (6.3)
Teenager at time of child's birth	.102	.097	.108
Race			
Black	.366	.381	.347
White	.440	.437	.444
Latino	.121	.111	.134
Other	.074	.072	.076
Marital status			
Never married	.557	.566	.547
Separated or divorced	.413	.407	.420
Married	.026	.023	.030
	Parent education, employment, and income		
High school graduate	.569	.579	.557
Employed in year prior to random assignment	.423	.414	.434
Earnings in year prior to random assignment	$2,313 ($5,119)	$2,198 ($5,075)	$2,457 ($5,170)
Years on AFDC prior to random assignment			
0–2	.058	.056	.060
2–5	.216	.209	.224
5+	.726	.735	.716
	Child characteristics		
Child's age (years)	5.89 (3.18)	5.90 (3.17)	5.88 (3.21)
Child has younger sibling	.384	.376	.396
	Family composition		
Age of youngest child (years)	4.09 (2.80)	4.11 (2.78)	4.06 (2.84)
No. of children in family	2.39 (1.22)	2.39 (1.22)	2.39 (1.21)
	Outcome measures		
Child achievement scores[a]	.000 (1.00)	.008 (1.00)	−.010 (1.00)
Average yearly earnings[b]	$4,353 ($6,072)	$4,620 ($6,093)	$4,019 ($6,029)
Average yearly income[b]	$11,829 ($5,756)	$12,269 ($5,992)	$11,278 ($5,397)

Note. All numbers between 0 and 1 (except child achievement scores) indicate proportions of the group to which the characteristic applies. Standard deviations are in parentheses. AFDC = Aid to Families With Dependent Children.

[a] Child achievement reports from parents, teachers, and achievement tests were standardized.

[b] Average yearly earnings and income were calculated over the first 2 years of program participation.

control variables in the model's testing effects on children's school achievement.

Impacts Across Age by Individual Program and Follow-Up Length

We used OLS to estimate the model in Equation 1, using child achievement as the dependent variable, by study. Presented in the table are the raw score coefficients from the regression analyses. Because the dependent variable is standardized, these regression coefficients can be interpreted as effect sizes (proportion of standard deviation change) associated with assignment to the experimental group for each age group of children. For example, the first entry (0.106) in Table 3 can be interpreted as the fraction of a standard deviation difference in achievement associated with family assignment to the treatment as opposed to control group for MFIP children who were ages 0–1 years at baseline. Given the 0.166 standard error, this difference is not significantly different from zero at conventional levels.

As shown in Table 3, significant experimental impacts for any age group are not common. Coefficients are generally positive (some statistically significant) for the 2- to 3- and 4- to 5-year-olds. Effects are generally negative for the children older than 10 years of age, and for the 0- to 1-year-olds. Effects are generally weaker and more variable for the children in middle childhood at parents' entry into the programs. Note that these models may differ from those in prior published reports on these data in that they combine across measures (parents report and test scores), report effects by child age at study entry (rather than at follow-up), and do not divide the sample by subgroups emphasized in the individual studies (e.g., child gender in the case of New Hope).

Pooled Sample Estimates

We then conducted the OLS model described in Equation 1, but this time pooling the microdata collected about individual study-sample children and their families (see Table 4). For example, the initial entry (−0.042) can be interpreted as the fraction of a standard deviation difference in achievement associated with family assignment to the treatment as opposed to the control group for children across all seven studies who were ages 0–1 at baseline.

A joint F test that tested whether the age-related experimental impacts were jointly equal to zero was significant, $F(7, 13301) = 2.81$, $p < .01$. Program impacts are statistically significant for two of the seven child age groups examined: positive for the 4- to 5-year-olds and negative for the 10- to 11-year-olds, with marginally significant positive effects for the 2- to 3-year-olds. The effects observed are small: a 0.07 standard deviation increase in school achievement for the 4- to 5-year-olds and a 0.11 standard deviation decrease in school achievement for the 10- to 11-year-olds, brought about by assignment to the experimental program. Models were also conducted to test the extent to which effects

Table 3
Raw Score Coefficients From Models Testing Age-Related Experimental Impacts on Child Achievement by Study

	Earnings supplement programs						Non-earnings supplement programs			
Age-related interaction	MFIP	New Hope 2-year follow-up	New Hope 5-year follow-up	CT Jobs-First	SSP 3-year follow-up	SSP 4.5-year follow-up	Los Angeles Jobs-First GAIN	NEWWS 2-year follow-up	NEWWS 5-year follow-up	FTP
Exp × Ages 0–1	0.106	—[a]	−0.005	−0.131	−0.051	0.019	—	—	—	−0.190
	(0.166)		(0.152)	(0.145)	(0.095)	(0.080)				(0.099)
Exp × Ages 2–3	−0.005	0.297*	0.060	0.089	0.056	0.035	—	0.081	−0.020	0.178*
	(0.057)	(0.138)	(0.104)	(0.074)	(0.055)	(0.059)		(0.059)	(0.061)	(0.084)
Exp × Ages 4–5	0.019	0.033	−0.041	−0.030	0.194**	0.264**	0.015	0.071	0.045	0.039
	(0.064)	(0.140)	(0.117)	(0.072)	(0.056)	(0.092)	(0.160)	(0.048)	(0.049)	(0.092)
Exp × Ages 6–7	−0.074	0.282*	0.165	−0.073	0.065	—	0.051	—	—	−0.131
	(0.073)	(0.141)	(0.113)	(0.075)	(0.063)		(0.128)			(0.100)
Exp × Ages 8–9	0.061	−0.107	0.104	0.108	−0.090	—	0.082	—	—	0.047
	(0.081)	(0.160)	(0.118)	(0.087)	(0.071)		(0.142)			(0.120)
Exp × Ages 10–11	−0.197	−0.257	−0.015	−0.172	−0.036	—	0.018	—	—	−0.148
	(0.104)	(0.150)	(0.183)	(0.103)	(0.072)		(0.152)			(0.133)
Exp × Ages 12–15	0.030	−0.178	—	−0.295**	0.083	—	−0.053	—	—	−0.376*
	(0.104)	(0.196)		(0.112)	(0.092)		(0.120)			(0.168)
R^2	.065	.080	.083	.052	.045	.027	.046	.078	.056	.055
F	10.36**	4.16**	5.60**	6.79**	9.02**	3.29**	2.20**	14.50**	9.38**	5.51**
n	4,586	1,270	2,883	3,879	6,806	2,300	1,030	2,867	5,580	2,528

Note. Standard errors are in parentheses. Ages are reported in years. Dummy variables for six of the seven age groups were included in all the models. Models also control for source of achievement report (where appropriate) and the following parent/family baseline characteristics: follow-up length, prior earnings, prior earnings squared, prior Aid to Families With Dependent Children receipt, prior years of employment, high school degree, teen parent, marital status, number of children, age of youngest child in family, and race. Dashes indicate that data are not available. MFIP = Minnesota Family Investment Program; CT Jobs-First = Connecticut Jobs-First; SSP = Canadian Self-Sufficiency Project; GAIN = Greater Avenues for Independence; NEWWS = National Evaluation of Welfare to Work Strategies; FTP = Florida's Family Transition Program; Exp = experimental dummy.

[a] The number of New Hope observations in age group 0–1 was too small for this analysis.

* $p < .05$. ** $p < .01$.

Table 4
Raw Score Coefficients From Models Testing Age-Related Experimental Impacts on Child Achievement for All Programs and by Program Type

Age-related interaction	All programs	Earnings supplement programs	Other programs
Exp × Ages 0–1	−0.042	−0.020	−0.184
	(0.050)	(0.056)	(0.098)
Exp × Ages 2–3	0.048	0.070*	0.031
	(0.027)	(0.032)	(0.046)
Exp × Ages 4–5	0.067*	0.100**	0.039
	(0.026)	(0.036)	(0.037)
Exp × Ages 6–7	0.025	0.039	−0.074
	(0.036)	(0.040)	(0.080)
Exp × Ages 8–9	0.024	0.019	0.043
	(0.040)	(0.044)	(0.093)
Exp × Ages 10–11	−0.105*	−0.112*	−0.059
	(0.045)	(0.049)	(0.101)
Exp × Ages 12–15	−0.083	−0.049	−0.188
	(0.049)	(0.056)	(0.101)
R^2	.033	.037	.043
F	20.53**	19.43**	9.85**
n	33,732	21,727	12,005

Note. Standard errors are in parentheses. Ages are reported in years. Dummy variables for 6 of the 7 age groups and for 9 of the 10 studies/sites were included in all models. Models control for source of achievement report and the following baseline characteristics: follow-up length, prior earnings, prior earnings squared, prior Aid to Families With Dependent Children receipt, prior years of employment, high school degree, teen parent, marital status, number of children, age of youngest child in family, and race. Exp = experimental dummy.
* $p < .05$. ** $p < .01$.

differed for boys and girls. These results (not shown) find a similar pattern of effects for both genders.

Differences in the coefficients between the age groups were tested. An overall F test finds significant differences across the age groups, $F(6, 13301) = 2.98$, $p < .01$. Tests of differences between each pair of age groups find the following significant differences: (a) effects for the 2- to 3-year-olds and the 4- to 5-year-olds differ from the effects for the 10- to 11-year-olds ($p < .01$ for the 2- to 3-year-olds and $p < .001$ for the 4- to 5-year-olds) and the 12- to 15-year-olds ($p < .05$ for the 2- to 3-year-olds and $p < .01$ for the 4- to 5-year-olds); and (b) the effect for the 10- to 11-year-olds also differs from those for the 6- to 7-year-olds and the 8- to 9-year-olds ($p < .05$ for the 6- to 7-year-olds and $p < .05$ for the 8- to 9-year-olds).

Control variables produced predictable effects, with parents' educational attainment positively associated with children's achievement, and number of children in the family, the age of the youngest child, and length of time on AFDC prior to baseline negatively associated with children's achievement (these results are available from the author).

By Program Type

We next examined how age-related program effects on child achievement differed by program model (see the second and third columns of Table 4). We find statistically significant positive effects of generous earnings supplement policies on 2- to 3- and 4- to 5-year-old children. Effects in these generous earnings-supplement programs were in the range of a 0.07–0.10 standard-deviation change, resulting from assignment to the program group. For older children, a negative coefficient is observed in both program types, although the negative effect among 10- to 11-year-olds is most pronounced in the generous earnings-supplement programs (with a 0.11 standard-deviation change). For the programs without an earnings supplement, the effect for 10- to 11-year-old children is nonsignificant, although the effect for the 12- to 15-year-olds approaches statistical significance.

Split sample analyses comparisons were conducted to test for differences in the coefficients between the two program types using a test statistic that is the weighted sum of squares of the impact estimates for the subgroups and has a chi-squared distribution (Cooper & Hedges, 1994; Greenberg, Meyer, & Wiseman, 1993). Despite this different patterning of effects across the two program types, comparisons between the coefficients of the Experiment × Age interactions across the two program types finds no significant differences for any of the age groups.

Effects on Parents' Earnings and Income

In order to test whether impacts on achievement by child age are a result of differing responses of parents with different age children (e.g., whether parents of younger children increased their employment as much as those of older children to the same incentives and requirements), we examined the effects of these programs on parents' earnings and income, separately by program type (see Table 5). Consistent with the goals of the programs, both program models significantly increase earnings for the parents of children in many of the age groups (although effects were most likely to be found for the generous earnings-supplement programs). Earnings impacts per year over the first 2 years following random assignment ranged from about $500 to about $1,500 across the two sets of programs.

With regard to income, programs with generous earnings-supplements increased income by about $1,400 to $2,200 per year over the first 2 years, whereas programs without supplements did not increase income significantly for any age group. As with earnings, there were few differences in impacts on income across parents with varying ages of children.

Controlling for Differential Effects by Parent or Family Characteristics

Analyses were conducted to examine the extent to which interactions between the experimental group dummy and parent or family characteristics accounted for differences in the impacts across the age groups (see Table 6). We also examined in these models whether the inclusion of interactions involving the following components changed the pattern of effects across age: (a) interaction between source of test and program or control group indicator, (b) interaction between length of follow-up and program or control group indicator, and (c) interaction between country (Canada vs. United States) and program or control group indicator.

The overall pattern of age effects does not differ with the inclusion of baseline-control-by-experimental-dummy interaction terms. For all program models and for the generous earnings-

Table 5
Raw Score Coefficients From Models Testing Age-Related Experimental Impacts on Parents' Earnings and Income for All Programs and by Program Type

	Earnings (in $1,000s)			Income (in $1,000s)		
Age-related interaction	All programs	ES programs	Other programs	All programs	ES programs	Other programs
Exp × Ages 0–1	1.011**	1.093**	0.680	1.434**	1.604**	0.755
	(0.310)	(0.363)	(0.434)	(0.301)	(0.349)	(0.473)
Exp × Ages 2–3	0.861**	0.797**	0.882**	0.984**	1.478**	0.301
	(0.152)	(0.216)	(0.205)	(0.145)	(0.200)	(0.204)
Exp × Ages 4–5	0.772**	1.098**	0.466*	1.032**	1.947**	0.192
	(0.162)	(0.249)	(0.212)	(0.156)	(0.235)	(0.208)
Exp × Ages 6–7	0.832**	0.734*	1.460**	1.376**	1.597**	0.188
	(0.250)	(0.285)	(0.377)	(0.238)	(0.271)	(0.386)
Exp × Ages 8–9	1.106**	1.176**	0.788	1.900**	2.215**	0.197
	(0.284)	(0.320)	(0.503)	(0.267)	(0.300)	(0.488)
Exp × Ages 10–11	0.625*	0.626	0.643	1.170**	1.444**	−0.133
	(0.281)	(0.321)	(0.506)	(0.270)	(0.307)	(0.517)
Exp × Ages 12–15	0.855**	0.759*	1.161*	1.581**	1.981**	0.142
	(0.290)	(0.336)	(0.556)	(0.271)	(0.312)	(0.527)
R^2	.355	.364	.301	.326	.299	.337
F	80.13**	70.00**	26.36**	82.15**	65.31**	34.78**
n	33,673	21,668	12,005	33,673	21,668	12,005

Note. Standard errors are in parentheses. Ages are reported in years. Dummy variables for 6 of the 7 age groups and for 9 of the 10 studies/sites were included in all models. Models control for the following baseline characteristics: follow-up length, prior earnings, prior earnings squared, prior Aid to Families With Dependent Children receipt, prior years of employment, high school degree, teen parent, marital status, number of children, age of youngest child in family, and race. Exp = experimental dummy; ES = earnings supplement.
* $p < .05$. ** $p < .01$.

supplement programs in particular, significant positive effects are still observed for the 4- to 5-year-old children. The negative effects for the 10- to 11-year-old children fall short of statistical significance for all program models, but are statistically significant for the generous earnings-supplement programs. For the programs without these supplements, negative effects for the 0- to 1-year-olds emerge with the inclusion of the interaction effects (although it can be seen from Table 1 that all sample members for this analysis come from a single study; FTP), and the magnitude of the effects on other age groups is largely unchanged from the original models. None of the Demographic/Family Characteristic × Experimental Dummy interaction terms reached statistical significance at conventional levels, when considering all program models. The only interaction that reached statistical significance was the control for the interaction of experimental dummy and country of study (Canada vs. United States).

Robustness of Model to Alternative Specifications

Analyses were conducted to examine the robustness of the findings to varying differences of model specification (results available from the authors upon request). First, we examined if results differed depending on whether the source of the achievement report was a parent report or test score. (There were too few studies that included teacher report measures to conduct a full model for this outcome measure.) We found a similar pattern of effects with those reported earlier, particularly with the parent report measure, the only exception being the negative effects for the 10- to 11-year-olds that were not replicated using test score data (although these results were based on a limited set of studies and sample children). Second, we examined the sensitivity of the results to the inclusion of the Canadian study, SSP. We found positive coefficients for the 2- to 3- and 4- to 5-year-olds, although neither of these coefficients reached statistical significance (although note that Table 6 found that the Canadian × Experiment interaction did not reduce the experimental effect for the young children). Third, we ran models using one randomly selected child from each family and again found similar results as reported earlier. Fourth, we examined the results by length of study follow-up; effects for the generous earnings-supplement programs are slightly more pronounced in the short (2–3-year follow-up) compared to the long term (4–5-year follow-up), although positive coefficients are found in both cases. Finally, we conducted a series of regressions in which we limited the sample of studies to those in which all age ranges examined were represented. In this way, we could assure that any differences across age groups would not be driven by the representation of a particular study for a particular age group. In all of these regressions, positive effects for the 4- to 5-year-olds and negative effects for the 10- to 11-year-olds emerged.

Discussion

Our analyses point to two sensitive periods of children's development for the effects of welfare and employment programs, both of which correspond to transition points in children's development. We find small positive effects of welfare and antipoverty policies for children making the transition into middle childhood, and small negative effects of these same policies for children making the transition out of middle child-

Table 6
Raw Score Coefficients From Models Testing Age-Related Experimental Impacts on Child Achievement, Controlling for Interactions Between Experimental Status and Baseline Characteristics

Treatment interaction	All programs	Earnings supplement programs	Other programs
Treatment Status × Age			
Exp × Ages 0–1	−0.064	−0.027	−0.293**
	(0.055)	(0.063)	(0.110)
Exp × Ages 2–3	0.050	0.073*	0.036
	(0.029)	(0.036)	(0.054)
Exp × Ages 4–5	0.070**	0.088*	0.074
	(0.026)	(0.037)	(0.045)
Exp × Ages 6–7	0.039	0.044	−0.082
	(0.037)	(0.041)	(0.084)
Exp × Ages 8–9	0.028	0.001	0.049
	(0.042)	(0.047)	(0.098)
Exp × Ages 10–11	−0.083	−0.114*	−0.034
	(0.048)	(0.055)	(0.108)
Exp × Ages 12–15	−0.057	−0.044	−0.147
	(0.056)	(0.065)	(0.112)
Treatment Status × Demographic Characteristics			
Exp × Parent Age	−0.004	−0.001	−0.008
	(0.003)	(0.003)	(0.005)
Exp × Parent Never Married	0.002	−0.011	0.021
	(0.032)	(0.038)	(0.056)
Exp × No. of Children in Family	0.019	0.029	0.002
	(0.014)	(0.018)	(0.024)
Exp × Younger Sibling Present	−0.006	0.030	−0.134*
	(0.034)	(0.039)	(0.067)
Exp × Years on AFDC	−0.023	−0.014	−0.016
	(0.025)	(0.029)	(0.049)
Exp × Parent Employed in Year Prior to Baseline	0.025	0.027	0.002
	(0.030)	(0.036)	(0.052)
Treatment Status × Study Characteristics			
Exp × Follow-Up Length	−0.011	0.001	−0.027
	(0.011)	(0.018)	(0.014)
Exp × Canadian Study	0.081*	0.081*	—
	(0.032)	(0.040)	
Treatment Status × Source of Achievement Report			
Exp × Teacher Report of Achievement	—	—	—
Exp × Parent Report of Achievement	−0.032	−0.068	0.093
	(0.042)	(0.058)	(0.082)
Exp × Achievement Test Score	−0.007	−0.012	0.002
	(0.040)	(0.061)	(0.052)
R^2	.034	.039	.046
F	16.26**	14.98**	7.99**
n	32,900	21,259	11,641

Note. Standard errors are in parentheses. Ages are reported in years. Dummy variables for 6 of the 7 age groups and for 9 of the 10 studies/sites were included in all models. Baseline demographic control variables included in all models: follow-up length, prior earnings, prior earnings squared, prior Aid to Families With Dependent Children (AFDC) receipt, prior years of employment, high school degree, teen parent, marital status, number of children, age of youngest child in family, race, younger sibling present, and parent age. All interacted control variables are mean-centered for this analysis. Dashes indicate that data were not included in the analysis. Exp = experimental dummy.
* $p < .05$. ** $p < .01$.

hood and into early adolescence, even though these policies have similar effects on parents' employment and income across age groups of children. Effects are more pronounced for generous earnings-supplement policies than for other welfare and employment strategies. Our effects are robust across various program groupings and cannot be attributed to family characteristics that differ for children of different ages.

Prior work has pointed to the importance of developmental transitions for changes in individual developmental trajectories (Graber & Brooks-Gunn, 1996). Here we have identified two

important developmental transition points for the effects of changes in maternal employment and increases in income brought about by welfare and antipoverty policies. Because of the reorganization that occurs with developmental transitions (Cicchetti, 1991; Sroufe, 1990), children's development may be most open to change during these periods.

But why are these two transition periods in development affected in particular? For young children, the increased plasticity of development during early childhood may have contributed to the positive effects observed. The finding that early childhood is affected by income-enhancing policies is consistent with prior nonexperimental work on the effects of poverty across childhood (Duncan et al., 1998). Moreover, the fact that the family system has such a strong direct effect on early childhood development (Bronfenbrenner & Morris, 1998) makes this period of development most sensitive to policy changes targeting the parent and family.

We suspect that the other systems affecting children in the sample may have interacted with changes in maternal employment in ways that affected individual development. For preschoolers, the transition to elementary school may have supported children's development during their parents' increased employment. Regardless of whether a parental transition to employment occurs, these children will make the transition to elementary school. In contrast, infants' and toddlers' child-care status may depend, at least in part, on parents' employment status. Unlike the low-income child-care environment, school may provide a stable context for young children's development as parents in poverty make the transition into employment. The finding that effects were more pronounced for younger children (2- to 3-year-olds at baseline) in the generous earnings-supplement programs may be a result of the parents' increased ability to purchase similarly stable child care and the increased benefit of these programs.

For children making the transition to adolescence, however, negative effects of work policies were observed. The transition to early adolescence has been characterized by significant biological as well as socioemotional growth (Brooks-Gunn & Petersen, 1983; Hamburg, 1974), and research has pointed to the challenge of this period for self-system development (Seidman, Allen, Aber, Mitchell, & Feinman, 1994; Simmons & Blythe, 1987). The impersonal nature of the junior high school context may not support the developmental tasks of the transition to adolescence, requiring a restructuring of social roles at the moment in development when adolescents are trying to establish their identity.

These difficulties may be exacerbated by increases in maternal employment accompanied, perhaps, by changes in the total amount and source of family income. First, maternal employment results in the absence of the parent at the moment when children are making a transition to a less attentive school environment, leaving children unsupported in navigating the transition to adolescence. Second, parents' employment may restructure children's own roles in the household by requiring them to take on greater responsibilities, for example (Gennetian et al., 2002), making it more difficult for a child to establish an identity just when identity development is so critical. Although identity development may be fostered by the availability of multiple roles, the fact that these adolescents may have little choice in taking on these responsibilities may interfere with, rather than promote, this identity development.

Surprisingly, the fact that these effects were somewhat more pronounced in the generous earnings-supplement programs rather than the other program models suggests that the increase in welfare income may also be playing a role here. A preliminary examination of this issue suggests that welfare income may indeed be negative for the school achievement of this group of adolescents (Morris, Duncan, & Rodrigues, 2005). Perhaps the establishment of identity makes this time particularly vulnerable to the source of parents' income; having to establish one's identity as a child in a welfare-recipient family may be particularly taxing emotionally and take a toll on children's school achievement.

Data limitations forced us to focus on chronological age rather than grade in school. Although most 10- to 11-year-olds were likely in 5th and 6th grade at study entry and made the transition to junior high school over the course of the follow-up period of these studies, a sizable proportion may have repeated an early grade in their school careers and made later transitions out of elementary school. (We suspect few children were ahead of their grade for their age in this high-risk sample.) This means that some of these children may have made this transition later in the follow-up period of these studies (but likely still within the 2–5 years of the follow-up we observed). Also, this means that some of the 12- to 15-year-old children may have made this difficult school transition, which would be consistent with the negative but weaker effects we observed for this older age group of children.

Other, nonexperimental research has also pointed to age differences in the effects of changes in maternal employment, although most of the work has focused on early childhood (Brooks-Gunn et al., 2002; Harvey, 1999; Waldfogel, Han, & Brooks-Gunn, 2002). A recent article that examines the effects on young children and adolescents finds benefits to mental health for the older age group of children, but neutral effects on other adolescent outcomes (Chase-Lansdale et al., 2003). Whereas the study by Chase-Lansdale et al. examined the effects of voluntary transitions to employment, the current study examines the effects of changes in welfare policies. Also, the positive effects observed in the study by Chase-Lansdale et al. were limited to mental health outcomes that were not examined here.

Magnitude of the Effects

How large are our estimated effects? In the case of our positive program effects on younger children, an important issue is whether effect sizes on the order of 0.07 of a standard deviation are economically meaningful. The answer depends on how permanent these achievement gains are. Even small effect sizes may be profitable if, as research suggests, small increases in achievement result in substantial increases in lifetime earnings. Krueger (2003), for example, estimated that the one-fifth standard deviation increase in test scores from the Tennessee Student Teacher Achievement Ratio class-size experiment could increase future earnings by between $5,000 and $50,000, depending on assumed discount and future earnings growth rates. Our 0.07 effect size, if permanent, would increase earnings by one third of this amount. Conversely, the negative effects for the older children may similarly decrease earnings.

We do not know how permanent the achievement gains and losses we estimate are, although evidence is emerging from several of our studies that positive effects for younger children may be

sustained to 5 years beyond the point of random assignment. For example, there is evidence that the positive achievement effects found in the New Hope program 2 years after baseline were just as strong 5 years after baseline (Huston et al., 2003). The effects of the SSP were also found to be sustained for young children in a 4.5-year follow-up (Michalopoulos et al., 2002). Longer-run follow-ups on other studies we include here are currently underway to provide additional information to answer this question.

Even with these potential benefits, these children continue to be at risk. In some programs, standardized scores on tests put children in these samples at about the 25th percentile in the absence of these programs, and probably only at about the 30th percentile with these improvements observed. Although these programs may have important effects on parents' employment and income, these programs are probably not the best way to help most low-income children. Past research indicates that some early education programs are cost-effective methods of promoting the achievement of young children (Shonkoff & Phillips, 2000). Our results suggest that certain kinds of welfare and employment programs may benefit certain subgroups of children, but the effect sizes are much smaller than those of direct interventions (although notably, these interventions are significantly less costly from a government perspective).

Limitations

Although policy experiments do a very good job of providing causal estimates of the effects of a particular policy approach, their designs are not as strong for determining the developmental nuances of how those effects occurred. Moreover, although our experimental design provides us with considerable confidence in attributing differences between program and control groups to policy, we note that these effects were observed in a particular economic context (the U.S. and Canada in the 1990s). Although the studies span a period of about 8 years, most of the regions during the study periods enjoyed economic vitality, and effects might well be different under other economic conditions. Also, these findings are applicable to single-parent welfare-recipient families, rather than to low-income families more generally.

The small size of these effects warrants caution in overinterpreting the results. Although the sampling variability is smaller in our pooled sample estimates than in any individual study, program effects are neither strong nor common across the age groups of children. Therefore, another possible interpretation of these results is that there are very few effects of these welfare interventions on children. Replication with other data sets may help determine the most appropriate interpretation of these results.

Finally, as mentioned earlier, our analysis assumes homogeneity of impacts across this set of sites and studies. As such, our estimates do not allow for inference to a broader population of experiments under the assumption of variation in the true impacts across sites.

Implications for Science and Policy

Because developmental transitions are critical periods for policy change, these periods are important for studying changes in developmental trajectories and for the focus of intervention efforts designed to alter such trajectories. Prior research has highlighted the importance of developmental transitions for exactly these kinds of effects; yet, most research into the effects of changes in maternal employment and income has focused on the effects on broad age groupings of children, rather than on children at the bridge between developmental periods.

For policy, interventions have focused more exclusively on groups of children at a distinct developmental stage or on families without regard to children's developmental stage. As our findings attest, developmental transitions appear key to understanding both the positive and negative impacts of family interventions on children's development. This may be a fruitful focus for policy interventions aimed directly at children as well as those focused on parents' economic outcomes.

Finally, these results show that welfare reform policies have surprisingly limited effects, positive or negative, on children's achievement. By and large, such policies support neither the soaring hopes of reform advocates nor the worst fears of reform critics. We do find that certain kinds of programs may provide benefits for young children, but only children at a particular point in their development. The results also show that attention in the context of welfare reform policies needs to be paid to adolescents, who seem to face increased difficulties when parents go to work.

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