

Promoting the Positive Development of Boys in High-Poverty Neighborhoods: Evidence From Four Anti-Poverty Experiments

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This study uses geocoded address data and information about parents' economic behavior and children's development from four random-assignment welfare and anti-poverty experiments conducted during the 1990s. We find that the impacts of these welfare and anti-poverty programs on boys' and girls' developmental outcomes during the transition to early adolescence differ as a function of neighborhood poverty levels. The strongest positive impacts of these programs are among boys who lived in high-poverty neighborhoods at the time their parents enrolled in the studies, with smaller or nonstatistically significant effects for boys in lower-poverty neighborhoods and for girls across all neighborhoods. This research informs our understanding of how neighborhood context and child gender may interact with employment-based policies to affect children's well-being.

Not all places have gangs, but [my neighborhood] is infested with gangs and drugs and violence. My son, I worry about him. He may be veering in the wrong direction . . . it's different for girls. For boys, it's dangerous. [Gangs are] full of older men who want these young ones to do their dirty work. And they'll buy them things and give them money.

—A 35-year-old mother of four in the New Hope Project, an experimental work-support program operating in two of Milwaukee's poorest neighborhoods (Gibson-Davis & Duncan, 2005, p. 292).

Poor children growing up in neighborhoods of concentrated poverty face both the potential risks from growing up with low family incomes as well as growing up in neighborhoods that offer few job prospects for teens or young adults, lower-quality schools, and higher rates of crime (Brooks-Gunn, Duncan, & Aber, 1997; Clampet-Lundquist, Edin, Kling, & Duncan, 2011; Murry, Berkel, Gaylord-Hardne, Copeland-Linder, & Nation, 2011). As poignantly expressed by the quote above, these risks that face poor children in high-poverty neighborhoods may systematically differ by gender. Boys may be particularly vulnerable in neighborhoods of concentrated poverty because of the prevalence and proximity to gangs, drugs, and violence in these neighborhoods and the greater likelihood that they will engage in delinquency and violence as compared with girls (Cook & Laub, 2002). The precarious position of low-income adolescent boys in high-poverty neighborhoods is not lost on their parents. As a result, parents may marshal and funnel what resources they have toward protecting their sons. For some parents, this may mean taking up opportunities to relocate their families into better neighborhoods to reduce exposure to these negative extra-familial influences. Indeed, in the Moving to

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Opportunity (MTO) study, a large study of housing vouchers for families in poor neighborhoods, families' primary motivation to move was high concerns around safety, crime, gangs, and violence in their baseline neighborhoods, which was especially pronounced for the risks it posed to boys and their involvement in crime and gangs (Sanbonmatsu et al., 2011). When relocation is not possible, parents may engage in parenting strategies designed to manage their children's autonomy—including the involvement of their sons in outside activities—and exposure to risky environments, but their ability to do so effectively may depend, at least in part, on their economic resources (Furstenberg, Cook, Eccles, Elder, & Sameroff, 2000).

Studies of experimental employment-based anti-poverty programs in the late 1990s provide a unique opportunity to examine the influences of neighborhood poverty and parents' economic resources on youth developmental outcomes. One such anti-poverty program, the New Hope Project, offered a menu of work supports to poor individuals living in high-poverty neighborhoods in Milwaukee (Duncan, Huston, & Weisner, 2007). In return for working 30 hr a week, New Hope participants earned wage supplements, low-cost health insurance, child care subsidies, and access to transitional jobs. An experimental evaluation of the program found that young boys whose families participated in the New Hope treatment were rated by their teachers as better-behaved and higher-achieving than their control group counterparts. New Hope had fewer detectable effects on girls (Bos et al., 1999; Huston et al., 2001). Overall, the findings suggest that gender-based maternal responses to perceived neighborhood dangers may account for New Hope's more favorable effect on boys than girls (Bos et al., 1999).

This is consistent with findings from economists that have found marginally higher levels of parental investment in boys compared to girls more generally (Lundberg, 2005; Lundberg & Rose, 2002), and research that has found that parents allocate family resources to protect certain children from dangerous or low-quality neighborhoods (Jarrett, 1995; Romich, 2009). This would suggest that the impact of welfare and employment programs on children's development may depend on child gender, that is, changes in familial income, employment, routines, or functioning resulting from program participation may have differential effects on low-income boys versus girls.

Over the 1990s, several welfare reform and anti-poverty programs designed to improve the employment and incomes of low-income families

were fielded and evaluated, and these programs operated in a diverse set of neighborhood contexts. Like New Hope, these programs improved parents' income and employment, and also like New Hope, many of these programs showed impacts on youth developmental outcomes such as behavior and achievement (Gennetian & Miller, 2002; Huston et al., 2003). We take advantage of this body of experimental anti-poverty studies to better understand the extent to which neighborhood context may moderate, or shape, the way in which program-induced increases in parental employment and income may influence boys and girls as they transition to adolescence.

BACKGROUND: YOUTH DEVELOPMENT AND NEIGHBORHOODS

We ground our inquiry by considering the development of youth identity and its interaction with the broader societal and environmental context. From a developmental perspective, problem behavior is thought to occur in adolescence when there is a mismatch between developmental needs and the opportunities afforded by the social context (Jessor, 1993). Identity, characterized as a need for increased independence and autonomy, as well as a sense of individual competence, accomplishment, and purpose, is a key developmental task of adolescence (Erikson, 1968; Meeus, 2011). Under optimal circumstances, parents and adolescents gradually transform their relationship from a vertical one, in which all authority flows from parents to their children, to a more horizontal relationship, in which youths exert more agency and authority as well as assume more responsibility.

As proposed in bioecological models, development also is influenced by the interaction of individuals and their proximal and distal contexts, including neighborhood environments (Bronfenbrenner & Morris, 1998, 2006; Leventhal, Dupere, & Brooks-Gunn, 2009). Neighborhood is one important aspect of youth's social context and plays an important role in affording youth positive opportunities to be autonomous, accomplish socially valued goals, and develop a healthy identity. However, residing in a high-poverty neighborhood increases the likelihood of a developmental mismatch, in which the needed opportunities for developmental growth are fewer and more difficult to access. Thus, developmental theory suggests that residing in a high-poverty neighborhood is likely to compromise the development of adolescents, increase problem behavior, as well as decrease positive adjustment, such as academic achievement.

Sociological theory and research point to a variety of specific ways in which youth development may be especially compromised in high-poverty neighborhoods, through peers and adults' behavior (Jencks & Mayer, 1990). With regard to peers, behavior problems may spread in communities through the social control functions of peers. Such contagion effects can arise from learning from peers, pure preference externalities (individuals enjoy imitating their peers), stigma effects (negative signal from delinquent behavior declines when more people do them), and physical externalities (e.g., higher crime rates reduce the chances of getting arrested because of congestion effects in law enforcement [Cook & Goss, 1996; Glaeser, Laibson, Scheinkman, & Soutter, 1999; Manski, 2000]). Development may also be compromised by competition between neighbors for scarce resources like grades or jobs, or the psychological impact on individuals or self-evaluation based on relative standing in the community (Luttmer, 2005). The failure to successfully compete for prosocial rewards, as hypothesized in competition models, could in fact lead some people to reverse course and try competing for resources or social standing by engaging in antisocial behaviors. And even beyond peers, adults in a neighborhood can influence young people who are not their children by acting as role models or enforcers of public order (Sampson, Raudenbush, & Earls, 1997; Wilson, 1987).

Notably, neighborhood effects may vary by child gender. Boys and girls residing in the same neighborhoods may experience them differently, with boys facing greater neighborhood risks than girls. Boys spend more time outdoors in the community playing with friends and in casual and organized activities, while in low-income families in particular, girls may be asked to take on greater family responsibilities, such as taking care of siblings and other household chores, and these responsibilities may naturally limit their exposure to neighborhood conditions compared with boys (Hofferth & Sandberg, 2001; McHale, Crouter, & Tucker, 2001). We note, however, there is no consistent empirical evidence of strong neighborhood effects on youth developmental outcomes (e.g., see Evans, 2006; Leventhal & Brooks-Gunn, 2000; Sampson, Morenoff, & Gannon-Rowley, 2002), despite these strong theories regarding its influence.

Although less often studied, the negative effects of high-poverty neighborhood may be buffered by parents (Jarrett & Jefferson, 2003). Parents do this by actively monitoring their children's whereabouts as well as limiting and restricting their activities and independence in neighborhood contexts, as

well as connecting youth to positive activities outside of their neighborhood of residence. Parents residing in high-poverty neighborhoods might be especially active in managing their children's experiences and allocating family resources to protect children from dangerous neighborhood (Dearing et al., 2009; Furstenberg et al., 2000; Romich, 2009).

In summary, adolescents are particularly vulnerable to developing problem behavior when the developmental needs of adolescence are not met by their surrounding social contexts. Indeed, high levels of neighborhood risks and the low levels of positive opportunities found in high-poverty communities are strongly linked to poor adolescent outcomes—problem behavior, poor achievement, and involvement with the criminal justice system. Moreover, these risks are especially pronounced for young boys compared with young girls. Finally, parents may work hard to counter the harmful effects of high-poverty neighborhoods, both by limiting their children's exposure to perceived negative influences as well connecting children to other positive network and opportunities, sometimes in more affluent communities.

EXPERIMENTAL EMPLOYMENT-BASED ANTI-POVERTY POLICIES

The effects of neighborhood context may be mitigated when parents are able to increase their resources. That is, when single parents go to work and increase their income, they may select to use their increased resources to invest in youth schooling, activities, or other resources as a way to protect their children in the context of dangerous environments. To date, nonexperimental research on the effects of parents' employment has shown small and mixed effects on children of increased parental employment per se (Han & Waldfogel, 2009), and positive effects of increased parental income have been observed for middle-childhood-age children (Dahl & Lochner, 2008; Duncan & Brooks-Gunn, 2000; Milligan & Stabile, 2008). As noted previously, New Hope had positive effects on boys; however, differences in program effects on outcomes by gender are not consistently found in reported results from other experimental welfare policy programs, such as Connecticut Jobs First (Bloom et al., 2002), Family Transition Program (Bloom et al., 2000), or the Minnesota Family Investment Program (Gennetian & Miller, 2000).

Evaluations of welfare reform and anti-poverty programs during the 1990s, during a time of relatively low unemployment, provide a unique

opportunity to better understand the intersections of neighborhood risk, parental income and employment, and adolescent development. These studies found positive effects on children overall when parents' employment and income increased simultaneously as a result of these programs (Duncan, Morris, & Rodrigues, 2011; Morris, Duncan, & Clark-Kauffman, 2005). Whether the impact on children of the additional economic resources experienced by families in these programs differs depending on neighborhood poverty and gender, however, has not been explored.

THE CURRENT STUDY

This study uses geocoded address data from four random-assignment welfare and employment anti-poverty experiments to examine within-study neighborhood variation in experimental impacts on boys' and girls' behavior and school achievement. Our study focuses on children who, at baseline, were in middle childhood (ages 6–9 years), and at follow-up were in or entering early adolescence (ages 9–14). We investigate two questions: First, do these anti-poverty programs have different effects on youth development outcomes for those who live in high- versus low-poverty neighborhoods at study enrollment? Based on prior studies, we expect that experimental program impacts on children in high-poverty neighborhoods will be larger (more positive) than those in low-poverty neighborhoods. Second, we examine whether these program impacts further differ by child gender: Do anti-poverty programs (that increase parental employment and income) affect boys in high-poverty neighborhoods differently than girls in high-poverty neighborhoods? Based on evidence suggesting that parents in high-poverty neighborhoods may differentially respond to the developmental needs and risks of boys as compared to girls, we hypothesize that boys will experience larger positive experimental impacts in high-poverty neighborhoods compared with boys in low-poverty neighborhoods, as well as compared with girls in either neighborhood context.

METHOD

Design and Sample

We use four random-assignment studies from the Next Generation Project, a collaborative research project that utilized data from several random-assignment evaluations of welfare, anti-poverty,

and employment policies. The four studies are Connecticut Jobs First (abbreviated CT Jobs First in subsequent discussion; Bloom et al., 2002), Florida's Family Transition Program (FTP; Bloom et al., 2000), Minnesota Family Investment Program (MFIP; testing the effects of two programs, Full MFIP and MFIP Incentives Only, at multiple follow-up points; Gennetian & Miller, 2000), and New Hope (testing the effects of one program at two follow-up points; Bos et al., 1999; Huston et al., 2003). These four studies were chosen due to their combination of employment and income benefits, as well as having address data that could be geocoded. Although most of the studies were underway by 1996, they were designed to test the effects of many program features that have been implemented by the states since the federal welfare law of 1996 was passed. Table 1 summarizes key features of the four studies, including study design, type of policy tested, target sample, year of random assignment, and response rates.

All but one of the programs (FTP) offered generous earnings supplements designed to make work more financially rewarding by providing families with monthly cash supplements or by increasing the amount welfare recipients could keep when they went to work. Three programs (the Full MFIP program, CT Jobs First, and FTP) provided mandatory employment services, such as education, training, or immediate job search in which parents were required to participate a minimum number of hours in an approved work-related activity to be eligible to receive cash welfare benefits. Two of the programs under study put time limits on families' eligibility for welfare benefits (CT Jobs First and FTP), restricting eligibility to a certain number of months in a specified period.

As described above, we examine children who were in middle childhood (aged 6–9 years) at baseline data collection for each study. The racial and ethnic background of the sample was diverse: 44% of parents were African American, 40% were non-Hispanic White, and 12% were of Latino ethnicity. Most (94%) of the children were between the ages of 9 and 13 years at the time of follow-up assessments identified as our outcomes of interest for this paper. In some cases, children were assessed several times after baseline; our analysis includes all available follow-up data. All follow-up measures have been used extensively in prior large-scale studies or evaluations of child and adolescent well-being (Duncan et al., 2011; Yoshikawa et al., 2006). The 3,249 child-year observations are based on 2,980 children.

TABLE 1
Description of Studies

<i>Study</i>	<i>Random-Assignment Study Design</i>	<i>Key Policy Features Tested</i>	<i>Target Sample and Site(s)</i>	<i>Start Date</i>	<i>Response Rates for Follow-up Surveys</i>	<i>Timing of Follow-up Surveys</i>	<i>Project Synopsis</i>
Connecticut Jobs First	2 Groups: Jobs First and control group	Mandatory employment services; generous earnings supplement; time limits	Single-parent welfare recipients; New Haven and Manchester, CT	1996	71–80	3 years after random assignment	Combined a generous earnings disregard (allowing families to retain their entire welfare grant as they worked) with requirements to participate in employment-related activity and a time limit on benefits.
Family Transition Program (FTP)	2 Groups: Family Transition Program and control group	Mandatory employment services; time limits; expanded child care resources	Single-parent welfare recipients; Escambia County, FL	1994	78–80	4 years after random assignment	Instituted a time limit on the receipt of welfare benefits along with requirements to participate in employment-related activities and increased resource and referral to child care.
Minnesota Family Investment Program (MFP)	3 Groups: Full MFIP; incentive only; and control group	Mandatory employment services; time limits; expanded child care resources	Single-parent welfare recipients; seven counties in Minnesota	1994	80–81	3 years after random assignment	Full program combined mandatory employment services and earnings disregards. Allowed families to receive a portion of welfare grant if they worked and paid for child care up front.
New Hope Project	2 Groups: New Hope Program and control group	Generous earnings supplement; expanded child care resources	Low-income parents looking for work; Milwaukee, WI	1994	71–79	2 years and 5 years after random assignment	Incentives Only included only the financial incentives. Offered cash supplement and health and child care subsidies contingent on full-time work.

Measures

Child outcomes variables. *Child behavioral problems.* CT Jobs First, FTP, and MFIP used the Behavior Problem Index (BPI), developed by Peterson and Zill (1986) and used in the National Longitudinal Survey of Youth, to measure children's behavioral problems at the time of the follow-up assessments. The 28-item index consists of a total score and two subscales (externalizing and internalizing) based on parents' reports of their children's behavior. The externalizing behavior subscale assesses the extent to which a child acts out or exhibits aggressive behavior (α ranged from .85 to .87 across the three studies). The internalizing behavior subscale assesses the extent to which a child exhibits social withdrawal or sadness (α ranged from .80 to .82 across the three studies). New Hope used a similar parent-report measure of behavior problems, the Problem Behavior Scale from the Social Skills Rating System (Gresham & Elliott, 1990), which also generates externalizing and internalizing subscales (α ranged from .61 to .81). To provide comparability in outcomes across studies, we standardized these behavior problem measures within each study, using the study-specific control group standard deviation, so effect sizes are comparable across studies.

Child positive behavior. The Positive Behavior Scale (PBS), a parent-report measure developed by Polit (1996) for the New Chance demonstration, was used in all four programs at the follow-up assessment and includes items on compliance and self-control (e.g., think before she or he acts, usually does what I tell him or her), social competence and sensitivity (e.g., gets along well with other children, shows concern for other people's feelings), and autonomy (e.g., tries to do things for himself or herself, is self-reliant). MFIP and New Hope used all 25 items to create their positive behavior measures (α ranged from .91 to .95), and CT Jobs First and FTP both used a subset of seven items α ranged from .90 to .91. Again, we standardized this measure by subtracting the study-specific mean from the score and dividing it by the study-specific control group standard deviation.

Child school achievement. Children's school achievement was measured using parent reports and teacher reports. All four programs collected parent reports of children's achievement. The measure is based on one item in the survey that asks parents to report how well the child is doing in school on a five-point scale. The CT and New

Hope studies also collected teachers' reports of the child's performance in school. Teacher reports of achievement 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 $\alpha = .94$). As we did with the child behavior measures, we standardized the cognitive performance and achievement outcomes within each study, using the study-specific control group standard deviations. We used a separate dataset to estimate program impacts on achievement, where each observation is an achievement measure. Therefore, if a child has both a parent and a teacher report of school achievement, the two different scores are included in the analysis for that child. This method is consistent with previous studies using these data (Morris, Huston, Duncan, Crosby, & Bos, 2001; Morris et al., 2005).

Grade retention and special education. In the follow-up surveys of all four programs, parents were asked whether their child received special education because of a physical, emotional, behavioral, or other problems and whether their child had repeated a grade since random assignment. Parents could respond "yes" or "no"; these measures are dichotomous.

Parental outcome variables. *Income.* Using data from administrative records, the follow-up income measure was created by summing monthly cash assistance (AFDC or TANF), food stamp benefits, cash supplement payments provided by the earnings supplement programs, and quarterly earnings in jobs covered by the unemployment insurance (UI) system. 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 (although most of the sample consists of single-parent families). Annual income was inflation-adjusted for 2001 prices using the CPI annual average figures and divided by \$1,000.

Employment. Our follow-up employment measure indicates the proportion of the follow-up period that the parent was employed. We used UI data to determine whether the parent was employed in each quarter of follow-up. A parent was considered employed if her UI earnings were greater than zero in a quarter. The total number of

quarters employed was then divided by the total number of quarters in the follow-up period.

Harshness. Parental harshness was measured using six survey items that asked the parent how many times in the past week she lost her temper; scolded or yelled at, spanked, or grounded the child; took away privileges from the child; or sent the child to his or her room. Items were rescaled to a 4-point scale ranging from 1 to 4, in which 1 corresponds to *0 times*, 2 to *1 time*, 3 to *2–6 times*, and 4 to *7 or more times*. For respondents who had answered 75% of the items, the total score was computed as the average across the nonmissing items. The scale demonstrated very high internal consistency, with $\alpha = .94$. This measure was standardized as above.

Aggravation. The parental aggravation used six items indicating the extent to which the mother felt that the child was hard to care for, the mother was angry with the child, the mother felt trapped by the child, or the child does things that really bother the mother. Responses to the items ranged from 1 (*all of the time*) to 4 (*none of the time*). Items were rescored so that high scores indicated greater parental aggravation. Total scores were computed by averaging the items on the scale (for parents with at least 75% of the items on the scale completed). The aggravation scale had good internal consistency, with $\alpha = .77$ internal consistency. As with the child outcome measures, we standardized by subtracting the study-specific mean and dividing by the study-specific control group standard deviation, so effect sizes are comparable across studies.

Knowledge of children's activities. Parental knowledge of children's activities measures the extent to which the parent knew about the child's whereabouts and activities. The seven items used for this scale included "How often do you know who (CHILD) is with?" "How often do you know when to expect (CHILD) home?" "How often do you know where (CHILD) is when he or she is not at home?" "How often do you know which TV programs (CHILD) watched?" and "How often do you know whether (CHILD) finished any homework?" The scale for each item ranged from 1 (*almost never*) to 5 (*always*). The average of the seven items was computed for all cases with responses to at least 75% of the items for this scale. Higher scores indicated greater parental knowledge of the child's whereabouts. The scale had good internal consistency, with $\alpha = .80$. This measure was standardized as above.

Warmth. Parental warmth was measured using three survey items that asked the parent how many times the child was shown physical affection,

praised, and praised to other adults over the past week. The total score was computed as the average across the three items. The scale had good internal consistency, with $\alpha = .72$ for the three-item scale. This measure was standardized as above.

Depression. Parental depression was assessed using the Center for Epidemiological Studies Depression scale (Radloff, 1977); this is a screening instrument that has been used in many large-scale projects with low-income adults. High internal consistency has been reported, with Cronbach's alpha coefficients ranging from .85 to .90 (Radloff, 1977).

HOME. The quality of the home learning environment was assessed using items from the Home Measure of the Environment (Caldwell & Bradley, 1984); items included the presence of books and toys in the home and parental reading to children. Items were scored on a 3-point scale and summed ($\alpha = .72$).

Neighborhood poverty status and geocoding process. To characterize neighborhood conditions, we geocoded addresses and then matched the census tracts to data from both the 1990 and 2000 U.S. censuses, interpolated for the year the participant family entered the program. We choose the tract-level fraction of individuals with incomes below the official poverty thresholds as a measure of neighborhood disadvantage. While there are other measures of neighborhood disadvantage, including rates of high school dropout, teenage parenthood, and single-parent households, poverty correlates highly with all of them and is the most common single measure employed to describe levels of neighborhood deprivation (Brooks-Gunn et al., 1997).

Based on prior neighborhood poverty research (Kingsley & Pettit, 2003) and the differing neighborhood poverty levels of our samples, we created a cutoff score to distinguish children living at baseline in a neighborhood with relatively high levels of poverty, which we define as more than 30% of households living in poverty, from children living in a neighborhood with a poverty rate less than or equal to 30%. Although prior research from the neighborhood poverty literature has used lower and higher rates to delineate low from high poverty, the 30% threshold allowed for a substantial sample size from each study (i.e., CT, FTP, MFIP, and New Hope) on both sides of the threshold.

Neighborhood poverty status was generated by geocoding the baseline addresses in the sample. This task required accessing archived contact information of all families enrolled in the respective studies and cleaning and organizing the data to maximize the match rate. Not all families had provided complete or codable baseline addresses (for

example, some families provided P.O. boxes rather than street addresses). Families who were listed as living in a homeless shelter were dropped from the study, due to the transitory nature of their housing. Out of 6,529 total families in the four studies, we were able to successfully geocode 79% of their addresses. Table S1 displays the final match rates for the geocoded data set.

Baseline controls. To increase the precision of our estimates, we included a set of prerandom-assignment parental and family control measures from administrative data and surveys administered by program staff: length of time of prior AFDC or TANF receipt (no prior receipt, at least one month but <2 years, or more than 2 years); average family earnings in the 2 years prior to baseline, measured in thousands of 2001 dollars; average earnings in the 2 years prior to baseline squared; whether employed in the year prior to baseline; whether the parent has a high school degree or GED; whether the parent was a teenager (<18 years old) at the child's birth; the marital status of the parent (never married, married or separated); the number of children in the family; the age of the youngest child in the family; the race and ethnicity of the parent (Black, White, or Latino); and the length of the follow-up period in months. We also included program-specific dummy variables. These indicators control for site-level baseline differences in the samples across the studies that are not captured by the other covariates, such as site-wide average baseline behavior levels or local economic factors. By including these dummy variables, we ensure that impact estimates are based on variation within each program.

Treatment status. For all four welfare-to-work programs included in this study, program participants were assigned at random to the experimental group, where they received mandatory employment services, were subjected to time limits, or received work supports such as earning supplements or child care assistance, or to a control group, where they were not offered any additional program services. The treatment status variable equals 1 if the participant was randomly assigned to any experimental group and 0 if she was randomly assigned to the control group.

Analysis Procedures

We test our hypotheses by estimating ordinary least-squares (OLS) or logistic regression models in which treatment status is moderated by neighborhood poverty status and child gender. Results from our logit models are expressed as marginal effects—changes

in the probability of the outcome associated with unit changes in a given independent variable. To test our first hypothesis that experimental impacts would be larger for children in the highest poverty neighborhoods, we ran the models separately for high-poverty neighborhoods and low-poverty neighborhoods. Then, we used H-statistics to test whether the differences in program impacts between the two groups were statistically significant. The H-statistic is the weighted sum of squares of the impact estimates for the two groups and has a chi-squared distribution (Cooper & Hedges, 1994). The regression coefficients can be interpreted as effect sizes (proportion of standard deviation change) associated with assignment to the experimental group for each group of children. We opted for our separate-sample estimation of subgroup differences over a more conventional interaction formulation to facilitate presentation of the subgroup-specific treatment effects.

To test our second hypothesis that gender would further moderate this relationship and boys in high-poverty neighborhoods would experience larger experimental impacts than boys in less poor neighborhoods, we split the sample further, by gender and neighborhood poverty status. We ran separate program impacts for boys in low-poverty neighborhoods, boys in high-poverty neighborhoods, girls in low-poverty neighborhoods, and girls in high-poverty neighborhoods. We then used H-statistics to test whether the differences in program impacts between boys in high-poverty neighborhoods and boys in low-poverty neighborhoods were statistically significant, and whether the differences in program impacts between girls in high-poverty neighborhoods and girls in low-poverty neighborhoods were statistically significant. We also tested whether program impacts differed by gender within neighborhood poverty level.

All models also include baseline demographic control variables, program indicator dummies, and a measure of the time between baseline and the given assessment in all regression analyses. Huber-White methods are employed to adjust standard errors for nonindependence of multiple outcome measures per child and multiple children per family. All models were run using the Stata software.

RESULTS

Descriptive Statistics

In Table 2, we present means and standard deviations of all baseline demographic variables that serve as controls and independent variables in our

study. The first panel in this table shows considerable variation in neighborhood poverty levels across the studies, ranging from 16% on average in MFIP to 43% on average in the New Hope 5-year follow-up. The percent of families in our sample living in neighborhoods in which at least 30% of households fall below the poverty threshold also varies by study, ranging from 18% in MFIP to 89% in New Hope. The mean neighborhood poverty rate in the high-poverty neighborhoods is 43%, and the mean poverty rate for neighborhoods below the threshold is 13%. Table 3 presents the summary statistics for child outcomes for all sample children, including behavior and achievement outcomes.

In Table 4, we describe the means for our outcomes of interest for children in the control group only. The outcomes are displayed by gender and poverty rate and girls by neighborhood poverty status, using control group means and noting where control mean differences vary by neighborhood poverty status and where overall levels vary by gender. The table shows much larger differences in behavior and achievement by gender than neighborhood poverty status. Compared with control group girls, control group boys at follow-up

have higher levels of externalizing behavioral problems, lower levels of positive behavior, and worse achievement and school outcomes, scoring about a quarter of a standard deviation worse in all of these areas. Even control group boys in low-poverty neighborhoods perform substantially worse academically, and have more behavioral problems, than girls in high-poverty neighborhoods.

Outcomes do not statistically differ between control group girls in poor vs. less poor neighborhoods, except for use of special education services, which is higher for girls in higher-poverty neighborhoods. Boys in poor neighborhoods have much lower achievement outcomes compared to boys in less poor neighborhoods. For example, the average achievement level at follow-up for control boys living in less poor neighborhoods is 0.15 *SD* below the overall sample average, while it is 0.40 *SD* lower for boys living in very high-poverty neighborhoods, a difference that is statistically significant at $p < .01$.

Neighborhood Poverty and Program Impacts

Our first research question concerned whether levels of neighborhood poverty moderated welfare

TABLE 2
Summary Statistics for Independent and Control Variables, by Study

	<i>All Studies</i> (<i>N</i> = 3,249)		<i>Connecticut</i> (<i>N</i> = 984)		<i>FTP</i> (<i>N</i> = 520)		<i>MFIP</i> (<i>N</i> = 1,152)		<i>New Hope</i> 2 Years (<i>N</i> = 315)		<i>New Hope</i> 5 Years (<i>N</i> = 278)	
	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>
Neighborhood poverty												
Lives in a neighborhood with a nonelderly poverty rate $\geq 30\%$ (%)	34		21		33		18		89		89	
Average% individuals in poverty	23		18		25		16		43		43	
Control variables, parent												
Avg earnings in year prior to random assignment (\$1000s)	3.38	6.33	2.49	5.40	2.49	5.35	3.80	7.20	4.78	6.32	4.85	6.47
No prior TANF receipt (%)	10		6		7		15		12		12	
At least 1 month but <2 years of TANF (%)	18		13		28		16		20		21	
More than 2 years of TANF receipt (%)	72		82		65		68		68		68	
Control variables												
Parent employed in the year prior to RA (%)	52		46		45		53		70		68	
Parent has high school degree (%)	65		59		55		77		59		60	
Parent was a teenage parent (%)	14		12		16		14		19		19	
Parent has never been married (%)	51		62		46		44		55		52	
Parent was separated or divorced (%)	45		37		52		56		34		34	
Number of children	2.84	1.27	2.79	1.25	2.93	1.32	2.66	1.27	3.17	1.24	3.15	1.14
Age of youngest child (in years)	4.91	2.64	5.32	2.73	4.54	2.29	4.95	2.65	4.48	2.71	4.51	2.62
Parent is Black (%)	44		44		57		30		59		57	
Parent is White (%)	40		34		40		58		14		14	
Parent is Latino (%)	12		22		1		1		25		27	
Follow-up length (years)	3.57	0.78	3.25	0.23	4.26	0.21	3.50	0.19	2.17	0.23	5.31	0.32

TABLE 3
Child Outcome Variables, by Study

	<i>All Studies</i>		<i>Connecticut</i>		<i>FTP</i>		<i>MFIP</i>		<i>New Hope 2 Years</i>		<i>New Hope 5 Years</i>	
	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>	<i>Mean</i>	<i>SD</i>
Child outcomes												
Behavioral Problem Index, standardized	0.00	1.02	0.00	1.05	0.00	0.98	0.00	0.95	0.00	0.99	0.00	1.12
Positive Behavior Scale, standardized	0.00	0.99	0.00	0.86	0.00	1.04	0.00	1.04	0.00	1.02	0.00	1.08
Special ed (%)	21		18		20		27		20		20	
Suspended (%)	21		16		22		18		29		38	
Grade repetition (%)	18		27		38		5		21		18	
Achievement, standardized	-0.05	1.00	0.02	1.01	-0.06	0.99	-0.07	0.99	-0.09	1.00	-0.14	1.01

TABLE 4
Control Group Means, by Neighborhood Poverty Status

<i>Outcome Measure</i>	<i>Boys</i>				<i>Girls</i>				<i>Sig of Mean Dif, Boys vs. Girls</i>
	<i>Poverty Rate ≤30%</i>	<i>Poverty Rate >30%</i>	<i>Sig of Mean Dif</i>	<i>Mean: All Boys</i>	<i>Poverty Rate ≤30%</i>	<i>Poverty Rate >30%</i>	<i>Sig of Mean Dif</i>	<i>Mean: All Girls</i>	
Behavior Problem Index	0.12	0.23		0.17	-0.07	-0.02		-0.04	**
Externalizing problems	0.15	0.26		0.20	-0.09	-0.04		-0.06	**
Internalizing problems	0.04	0.13		0.08	-0.02	0.05		0.02	
Positive Behavior Scale	-0.15	-0.21		-0.18	0.11	0.04		0.06	**
Achievement	-0.15	-0.40	**	-0.25	0.08	0.14		0.12	**
Special education	25.76	29.66		27.22	10.46	17.41	*	14.82	**
Grade repetition	19.31	25.23	†	21.41	19.53	17.93		18.49	

† $p < .10$; * $p < .05$; ** $p < .01$.

program impacts on children. To address this question, we estimated whether experimental impacts on child behavioral outcomes varied by neighborhood poverty status (Table 5). None of the differences is statistically significant, although there is a nonstatistically significant trend toward larger (positive) impacts in high-poverty neighborhoods.

Gender, Neighborhood, and Program Impacts

Our second research question concerned whether an interaction between neighborhood poverty and gender moderated the effect of welfare programs on children's outcomes. Here, we find that the nonstatistically significant trend toward larger impacts in high-poverty neighborhoods is driven completely by the moderation of program impacts by neighborhood poverty status for boys (Table 6). While there are no impacts on the overall Behavioral Problem Index (BPI) for boys in low-poverty neighborhoods, we find large reductions in behavioral problems ($-.33$ *SD*) for boys in high-poverty

neighborhoods, a difference that is statistically significant at $p < .01$. The same pattern was observed for both internalizing and externalizing behavioral problems, with much larger impacts for the boys in high-poverty neighborhoods, especially for externalizing problems. In contrast, in less poor neighborhoods, programs had larger effects on behavioral problems among girls than the boys in those neighborhoods. Girls in low-poverty neighborhoods experienced reductions of $.22$ *SD* ($p < .05$) on their overall BPI score (with similar size reductions for both internalizing and externalizing problems), as compared with boys who had no change, a difference that is statistically significant at $p < .05$. In contrast, within high-poverty neighborhoods, programs had large effects on externalizing behavioral problems among boys, and little detectable effects on girls; the difference between program impacts on boys vs. girls was statistically significant at $p < .05$.

Boys in high-poverty neighborhoods also experienced impacts of $.30$ *SD* ($p < .05$) on the Positive Behavior Scale (PBS), reflecting increases in positive behavior, whereas there was no impact on the

PBS for boys in low-poverty neighborhoods. In contrast, although there was an overall pattern of improved behavior for experimental girls as a result of the programs, there were no statistically significant differences for girls by neighborhood poverty status.

TABLE 5
Experimental Impacts on Child Outcomes by
Neighborhood Poverty

	Poverty Rate ≤30%	Poverty Rate >30%	Significance Level of Dif in Impacts
Behavioral	-0.10 (0.07)	-0.23* (0.10)	
Problem Index			
BPI—Externalizing	-0.12† (0.07)	-0.21* (0.10)	
BPI—Internalizing	-0.07 (0.07)	-0.20* (0.10)	
Positive behavior	0.06 (0.07)	0.23 (0.09)	
Achievement	-0.02 (0.05)	0.13 (0.07)	
Special education	0.02 (0.02)	-0.05† (0.03)	
Grade repetition	-0.02 (0.01)	-0.03 (0.03)	

Note. All regressions control for the following baseline, prerandom-assignment parental and family control measures: length of time of prior AFDC/TANF receipt; average earnings in the 2 years prior to baseline; average earnings in the two years prior to baseline squared; whether employed in the year prior to baseline; whether the parent has a high school degree or GED; whether the parent was a teenager at 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; the race/ethnicity of the parent; and the length of the follow-up period. We also included the dummies for the following program: FTP, MFIP Urban, MFIP Rural, and New Hope (Connecticut is the excluded category).

† $p < .10$; * $p < .05$; ** $p < .01$.

Table 6 also presents impacts on achievement outcomes. Here again, there are statistically significant impacts on achievement for experimental boys in high-poverty neighborhoods (.24 *SD* improvement in achievement), but no impacts for boys in low-poverty neighborhoods. The difference in impacts between boys in low- versus high-poverty neighborhoods was statistically significant at $p < .01$. In contrast, there were no statistically significant differences in impacts for girls by neighborhood context.

We also find that experimental group boys in low-poverty neighborhoods were somewhat more likely to be reported to be enrolled in special education classes compared with controls, while those boys in high-poverty neighborhoods were less likely to be reported to be enrolled in special education classes compared with controls; the difference in impacts on special education between boys in low versus high-poverty neighborhoods was statistically significant at $p < .05$. Impacts on grade repetition did not vary by neighborhood poverty.

In summary, Table 6 highlights the fact that boys in high-poverty neighborhoods experienced lower levels of behavioral problems, higher levels of positive behavior, and higher levels of achievement as a result of assignment to the experimental group, while the same is not true for boys in low-poverty neighborhoods. There were few statistical differences for girls by neighborhood poverty level, although the results show that girls in lower poverty neighborhoods experienced larger reductions in behavioral problems as compared to the boys in lower poverty neighborhoods.

TABLE 6
Experimental Impacts on Children's Outcomes by Neighborhood Poverty and Child Gender

	Boys		Girls		Test of Significance of Difference in Impacts			
	Poverty Rate ≤30%	Poverty Rate >30%	Poverty Rate ≤30%	Poverty Rate >30%	Boys: Low vs. High Poverty	Girls: Low vs. High Poverty	Low Poverty: Boys vs. Girls	High Poverty: Boys vs. Girls
BPI—Total	0.04 (0.10)	-0.33* (0.14)	-0.22* (0.09)	-0.12 (0.14)	**		*	
BPI—	-0.04 (0.11)	-0.39** (0.14)	-0.20* (0.09)	-0.04 (0.13)	**		†	*
externalizing								
BPI—	0.13 (0.11)	-0.18 (0.13)	-0.22* (0.09)	-0.20 (0.14)	*		*	
internalizing								
Positive	0.00 (0.10)	0.30* (0.13)	0.14 (0.09)	0.21† (0.12)	*			
behavior								
Achievement	-0.03 (0.06)	0.24* (0.10)	-0.02 (0.06)	0.08 (0.10)	**			
Special	0.05 (0.03)	-0.07 (0.05)	0.00 (0.03)	-0.04 (0.02)	*			
education								
Grade repetition	-0.01 (0.02)	-0.01 (0.04)	-0.02 (0.02)	-0.05 (0.03)				

Note. All regressions include controls listed in the Note in Table 5.

† $p < .10$; * $p < .05$; ** $p < .01$.

TABLE 7
Experimental Impacts on Parenting Outcomes by Neighborhood Poverty and Child Gender

	Boys		Girls		Test of Significance of Difference in Impacts			
	Poverty Rate $\leq 30\%$	Poverty Rate $> 30\%$	Poverty Rate $\leq 30\%$	Poverty Rate $> 30\%$	Boys: Low vs. High Poverty	Girls: Low vs. High Poverty	Low Poverty: Boys vs. Girls	High Poverty: Boys vs. Girls
Employment	0.08** (0.02)	0.15** (−0.04)	0.08** (0.02)	0.08* (0.04)	†			
Income	1.08* (0.43)	1.68** (0.59)	0.89* (0.42)	0.68 (0.67)				
Harsh parenting	0.01 (0.10)	−0.37** (0.14)	−0.11 (0.10)	0.08 (0.16)	*			*
Aggravation	0.10 (0.10)	−0.33* (0.13)	−0.02 (0.09)	−0.19† (0.11)	*			
Knowledge of activities	−0.03 (0.11)	0.09 (0.11)	0.06 (0.09)	0.05 (0.12)				
Warmth	−0.06 (0.10)	0.07 (0.13)	0.20* (0.10)	0.35** (0.12)				
CES-D	−0.11 (0.10)	−0.10 (0.12)	−0.18† (0.10)	−0.08 (0.12)				
HOME	0.02 (0.09)	−0.08 (0.22)	0.10 (0.10)	0.02 (0.20)				

Note. All regressions include controls listed in Table 5.

† $p < .10$; * $p < .05$; ** $p < .01$.

Post hoc Exploration of Experimental Effects on Other Ecological Systems by Neighborhood Poverty Level: Parental Income and Employment, Parenting, and After-School Participation

To explore pathways consistent with these differential impacts on child behavior and achievement by neighborhood and gender, we investigated program impacts on parents' economic and employment outcomes by neighborhood poverty. This analysis is afforded by the random-assignment design of the program evaluations. In our model of the effects of the treatment on child outcomes, with parent outcomes as a possible mediator, we can obtain experimental impacts of the total effects of the treatment on child outcomes as well as the total effects of the treatment on parent outcomes. We cannot generate experimental estimates of the mediational effects of parent outcomes on child outcomes or vice versa. However, a necessary condition for parent outcomes to be a significant mediator of child outcomes is for there to be experimental impacts on such parent outcomes, and we can test for this alignment. This approach has been previously used in other random-assignment evaluations of welfare reform programs (Gennetian & Miller, 2002).

Consistent with past analyses, we find that the effects of welfare reform programs on parents' earnings are positive and statistically significant. Statistically significant positive program impacts on earnings of about \$1,000 were observed for mothers in both low- and high-poverty neighborhoods. Impacts on rates of employment for mothers in low-poverty neighborhoods were 8% points

compared with 12% points in high-poverty neighborhoods, a difference that was statistically significant at $p < .05$. When further broken down by gender (Table 7), there is a trend toward increased maternal employment for boys in high-poverty neighborhoods, with impacts on employment of 15% points for these mothers, as compared to 8% points for other mothers ($p < .10$). The estimate for the treatment impact on income was also higher for boys in high-poverty neighborhoods, but the differences in impacts did not reach standard levels of significance. Overall, this suggests a statistical trend toward larger program effects on work-related outcomes for mothers of boys in high-poverty neighborhoods compared with mothers of girls.

Our next set of analyses addressed whether impacts on parenting were aligned with the differential effects observed on child outcomes for boys in high- vs. low-poverty neighborhoods. Table 7 presents the results. Programs led to larger reductions (at statistically significant levels) in harsh parenting and aggravation for boys in high- as opposed to low-poverty neighborhoods.

We also performed similar analyses, examining whether there were differential patterns of effects on after-school participation. There were no differences by subgroups of children. Details of these results are available upon request.

Sensitivity Analyses

Poverty measure. We chose the neighborhood poverty threshold based on the distribution of this variable across our samples: A 30% threshold

provides a substantial sample size from each study on both sides of the threshold. Although this threshold has precedence in the literature (Galster, 2002; Kingsley & Pettit, 2003), we examined the sensitivity of our impacts to differing definitions of poverty, including cutoffs of 25% and 40%, as well as an SES factor (comprised of a larger set of variables from the census data, i.e., employment, education, marital status, etc.). We also wanted to ensure that the results we found were not solely driven by the New Hope sample, as its evaluation motivated the current study (Huston et al., 2003). Accordingly, we ran the core set of analyses excluding children in the New Hope study. Across these various specifications, the pattern of results was very similar, with boys in high-poverty or low SES neighborhoods accruing greater benefits than boys in low-poverty or high SES neighborhoods (tables available upon request).

We also examined neighborhood moderation using an interaction between experimental status and a continuous measure of neighborhood poverty. Although the pattern of results was similar, virtually none of the interaction terms was statistically significant at conventional levels. Due to the distribution of children by neighborhood poverty level, we were only able to examine thresholds between 25% and 40% poverty levels.

Family disadvantage. Our basic models control for a large number of baseline family characteristics. However, it remains possible that there are omitted variables or characteristics of the selection of families that are correlated with residing in poor neighborhoods that could account for our differential effects by neighborhood poverty.

To explore this question, we tested whether it is in fact family characteristics instead of neighborhood poverty that account for differential impacts by reestimating impacts by low versus high family disadvantage in place of neighborhood disadvantage. We defined low family economic disadvantage as having been employed in the year prior to random assignment and having received welfare for <2 years prior to random assignment, while high family economic disadvantage was defined as not having been employed in the year prior to random assignment and having received welfare for at least 2 years prior to random assignment. Using this definition, 37% of the families in our sample fell in the high economically disadvantaged group at baseline. In contrast to our neighborhood poverty interactions, we found that boys in less economically disadvantaged families experienced

larger program impacts on externalizing behavior (less externalizing behavior) and larger program impacts on positive behavior (more positive behavior) compared with boys in more economically disadvantaged families. Family socioeconomic status (at least as we have measured it here) does not appear to moderate program impacts in the same way as neighborhood poverty. Unfortunately, the studies we drew from did not all have a measure of household income at baseline, so we were unable to investigate the effect of this particular family characteristic.

Race and Ethnicity

Neighborhood poverty is often linked to neighborhood racial composition in many American communities. Because of this, we wished to test whether our findings further varied by children's racial and ethnic background. To do so, we ran the study's main set of analyses (welfare program impacts moderated by gender and neighborhood poverty level) further moderated by racial and ethnic subgroups (White, African American, and Hispanic). These analyses did not reveal any systematic or statistically significant patterning of effects by race or ethnicity.

Does Response Bias Affect Our Results?

Geocode-based results could be biased if families that provided valid addresses were different from families that did not in ways that influenced both where they lived and their children's outcomes. Notably, bias in our estimates would only result from an imbalance in the proportion or characteristics of the sample in the experimental or control group as a result of the geocoding. In Table S2, we present differences in baseline characteristics for geocoded and nongeocoded families in our sample. Although there are differences between families with and without geocoded addresses in each study, there are no systematic patterns of greater disadvantage in one group compared with the other group. Families with matched addresses had used welfare for a longer period of time and were more likely to be a teenage parent at baseline. However, a higher proportion of these families also had a high school degree and were employed in the year prior to baseline than of nongeocoded families. More importantly for our impact analyses, we found that the match rates for control group and experimental group families are almost identical (see Table S1).

In Table S3, we present differences in baseline characteristics for pooled experimental vs. control group geocoded families of children in our sample. The only consistent difference across studies is that control group members are more likely to have been employed in the year prior to random assignment, but otherwise there were few other differences, suggesting that random assignment “worked” in all the studies in this subsample of families. This is the most important test and suggests that our impact estimates on this group of families are unlikely to be biased by the requirement to have a geocoded address to be in our analysis sample.

DISCUSSION

During the transition from childhood to early adolescence, developmental contexts broaden from the immediate family to peer, school, and neighborhood environments (Magnuson, Duncan, & Kalil, 2006). Whether with their family or venturing out into the world on their own, children have increasing interaction with their broader community and neighborhood through playing and spending time with friends and siblings, walking to and from school, participating in after-school activities, or visiting the corner store. During this key time of developmental transition seeking independence from the family context, parents in poor neighborhoods face the challenge of restricting their children’s exposure to danger while still allowing their children to explore newfound skills, friendships, and identities (Chase-Lansdale & Gordon, 1996; Furstenberg et al., 2000; Gonzales, Cauce, Friedman, & Mason, 1996; Jarrett, 1995). It is critical to understand the interactions between poverty, neighborhood quality, and child gender during this important transitional period.

The results of our analysis show that the impacts of welfare reform and anti-poverty programs on child outcomes vary by child gender and neighborhood poverty. Without the additional income and employment made possible by participation in such anti-poverty programs, boys, especially in high-poverty neighborhoods, were significantly worse off academically and behaviorally than their female counterparts. Conversely, the four anti-poverty programs in this study had the largest beneficial impacts for boys in high-poverty neighborhoods, bringing the outcomes of boys in high-poverty neighborhoods about level to the average attainment in achievement and behavior in the overall sample. These results suggest that changes in income and employment may have

differential impacts on boys versus girls, depending on neighborhood context. This has implications for the understanding of the development of low-income boys and girls, our understanding of neighborhood effects, and policy and programs targeting low-income families with young adolescents.

Why Child Gender and Neighborhood Quality Might Moderate the Impact of Welfare Programs

The evidence we present in this study supports our second hypothesis that, with increases in maternal income and employment, boys in very high-poverty neighborhoods ($\geq 30\%$ poverty) may benefit more than boys in less poor neighborhoods ($< 30\%$ poverty), while there are few differences in impacts for girls by neighborhood poverty level. Supplemental analyses suggest that programs did not impact the use of out-of-school activities and that the differential impacts found by neighborhood condition are not a proxy for impacts that might be influenced by initial family economic disadvantage. So what could be driving these results?

The pattern of impacts on child and parental outcomes suggests a few plausible pathways. As suggested by prior research, one possibility is that parents of boys in high-poverty neighborhoods channel their increased income as a result of random-assignment social policy programs toward their boys because they perceive them to be most at risk or because they have less control over their boys’ whereabouts.

Another pathway is suggested by the pattern of impacts on parenting aggravation and harshness for mothers of experimental group boys in the highest poverty neighborhoods. Why would this have happened? One possibility is that if mothers of boys in high-poverty neighborhoods were most worried about the resources needed to provide for their sons, changes in income, employment, and other aspects of family dynamics may have led to differential decreases in parental aggravation, stress, and the need to be more restrictive and disciplinary for their preadolescent and young adolescent sons. The existing empirical and theoretical literature suggests that the associations between early adolescent behavior problems and harsh parenting and parenting aggravation are bidirectional and transactional (Lansford et al., 2011; Pardini, Fite, & Burke, 2008). That is, it is possible that as a result of an initial improvement in family income and employment, boys in high-poverty neighborhoods (who, as evidenced in the control group means, were most at risk for behavior and school

problems) experienced small improvements in behavior, further eliciting reductions in maternal aggravation and harsh parenting, which in turn meant fewer externalizing problems for those boys and more positive school experiences, and so on. This pattern of results departs from some prior research and theory suggesting that moderately harsh parenting can actually be beneficial for young males growing up in at-risk environments (Roche, Ensminger, & Cherline, 2007). In contrast, our results find associations between declines in harsh parenting (including losing one's temper, scolding and yelling, spanking, grounding, and taking away privileges) and declines in externalizing behavior problems for the most at-risk boys.

It is important to consider our results in light of the MTO findings, a study that directly estimated experimentally induced changes in neighborhood poverty through a housing voucher. MTO also found gender differences in impacts on youth outcomes. Although children's achievement was little affected by the MTO offer, their behavior was (Sanbonmatsu et al., 2011). Girls in the experimental group generally did better than their control group counterparts, becoming more engaged in school and exhibiting fewer behavior problems (Clampet-Lundquist et al., 2011; Popkin, Leventhal, & Weismann, 2008). Behavioral problems *worsened* for boys in the program relative to their control group counterparts, however, perhaps because boys remained tied to their old neighborhoods. These effects were similar though more muted—particularly in detecting few long-term impacts among male youth—for the MTO long-term follow-up study (Sanbonmatsu et al., 2011).

One key difference between MTO and the programs we examine here is their impact on parental income and employment. Unlike MTO, mothers in these programs have the opportunity to direct their increased economic resources to their children, and boys' developmental trajectories may be more sensitive to such changes in income family routines, and parenting. Leventhal and Brooks-Gunn (2011) and the present study suggest greater sensitivity of boys to the complex interactions of neighborhood poverty and the family environment. It is possible that this sensitivity is most relevant among children with the "most to gain," or the boys in the highest-poverty neighborhoods; that is, there is more room for the welfare reform programs to have an impact for these boys.

This explanation is also consistent with the MTO and welfare and anti-poverty experimental results, although it merely explains the differences between

boys and girls, not poor and nonpoor neighborhoods. In addition, the processes underlying results presented in this paper may differ from the gender results found in MTO and Leventhal and Brooks-Gunn (2011). Continued work to help elucidate why boys may be especially sensitive to deleterious neighborhood conditions, as well as to changes in maternal income or employment, is needed.

Limitations

This study was able to utilize a unique data set matching geocoded address data with several large, random-assignment policy experiments to examine differential impacts by gender and neighborhood poverty level. This design, however, does present some limitations. First, these studies, designed to determine the causal impact of various welfare and work programs, are less advantageous for determining the developmental processes or mediating pathways by which the effect occurred and do not allow for formal tests of transactional models of adolescent development encompassing neighborhood quality, family processes, and adolescent characteristics. Future work should attempt to replicate our findings, as well as formally test the potential mediating pathways described above.

In addition, our results may not generalize to all families of young adolescents in high-poverty neighborhoods, as our samples drew from families enrolled in welfare reform programs during the 1990s. It is possible that the results are limited to a policy and economic context when work was more readily available for low-income parents entering (or reentering) the work force under particular policy conditions. Clearly, more research is needed to determine and understand differential social or welfare program effects by neighborhood poverty and child gender. Future work should also employ more sensitive measures of parenting, children's time use and activities, family expenditures, and the differences in these by gender and neighborhood poverty level, as well as explore other youth characteristics that may interact with neighborhood characteristics, such as race or ethnic identity.

Implications

Our findings have implications for research and policy. As discussed above, this paper contributes to our understanding of the potentially heterogeneous nature of the ecological effects of income, employment, and neighborhood quality on child and adolescent development (Harding, Gennetian,

Winship, Sanbonmatsu, & Kling, 2011; Leventhal & Brooks-Gunn, 2011; Murry et al., 2011). With regard to policy, our findings highlight the potential of focusing on improving neighborhood quality while targeting income supplements to low-income families. The impacts on boys' behavior problems and achievement measures were much larger than what was achieved with the Moving to Opportunity (MTO) experiment, which provided families with vouchers to move to higher-quality neighborhoods (Kling, Liebman, & Katz, 2007; Sanbonmatsu et al., 2011). This work may further underscore the relative efficacy of targeted income and employment support for low-income parents, particularly those in high-poverty neighborhoods. Although all youth were able to enjoy the benefits of the program (both via benefits to their mothers, as well as their own behavior and achievement gains), our results suggest that the boys in the most at-risk neighborhoods benefited the most.

Could this translate into longer-term positive outcomes for these most at-risk boys? Boys are at greater risk for becoming involved in delinquency and crime, and current trends in educational achievement and labor force participation are troubling for many young men, especially those in the most disadvantaged neighborhoods (Murry et al., 2011). If targeting resources toward boys in under-resourced neighborhoods translates into large improvements in achievement and behavior for these children, this represents a promising approach to supporting the healthy development of a particularly at-risk population. With current rates of family poverty and job instability alarmingly high due to the worst recession in generations, and major changes in many neighborhoods due to declines in property values and numerous home foreclosures, understanding the dynamics of neighborhood poverty, gender, and family economic circumstance is a critical, policy-relevant, developmental issue.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Table S1. Match Rate.

Table S2. Family-Level Response Bias Analysis: Geocoded vs. Non-Geocoded.

Table S3. Family-Level Response Bias Analysis: Control vs. Experimental.

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