

**Getting a job is only half the battle:  
Maternal job loss and child classroom behavior in low-income families**

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This study uses data from an experimental employment program and instrumental variables (IV) estimation to examine the effects of maternal job loss on child classroom behavior. Random assignment to the treatment at one of three program sites is an exogenous predictor of employment patterns. Cross-site variation in treatment-control differences is used to identify the effects of employment levels and transitions. Under certain assumptions, this method controls for unobserved correlates of job loss and child well-being, as well as measurement error and simultaneity. IV estimates suggest that maternal job loss sharply increases problem behavior, but has neutral effects on positive social behavior. Current employment programs concentrate primarily on job entry, but these findings point to the importance to workers and their children of promoting job stability.

Keywords: Job loss, welfare-to-work, child behavior

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In the decade since the passage of federal welfare reforms, job loss has become increasingly prevalent among less-educated single mothers. Parents who previously relied on cash assistance are now required to work and often enter the labor force by way of low-wage, short-tenure jobs, amplifying the odds of forced or voluntary job loss (Boushey, 2001). Even in the tight labor market of the late 1990s, studies indicate that single mothers commonly experienced unstable employment and long periods of unemployment between jobs (Johnson & Corcoran, 2003; Pavetti & Acs, 2001; Wood, Moore, & Rangarajan, 2008), patterns associated with job instability, rather than mobility and earnings growth (Johnson, 2007; Royalty, 1998; Topel & Ward, 1992). In the recent recessionary economy, there is every reason to believe that employment instability is increasing, particularly for low-wage workers.

In this period of high unemployment, the services that most job seekers encounter at welfare offices or Workforce Investment Act centers will focus primarily on promoting job entry, not stability. The now conventional “work first” model proliferated in the 1990s partially in response to the perceived success of experimental welfare programs that emphasized job search assistance over training or education at increasing participants’ short-term employment and earnings (Greenberg, Mandell, & Onstott, 2000; Haskins, 2006). It has since been argued widely that long-term employment stability and advancement is better promoted by some combination of job-specific training, careful job matching, and “post-employment” services (e.g. Haskins, 2006; Holzer, 2004; Strawn, Greenberg & Savner, 2001). A few proven models of such services exist, and more are being tested, but none has been widely adopted, leaving job loss as a critical and largely unaddressed concern for low-wage workers.

The implications of job instability in the low-wage labor market extend beyond workers to their children. Developmental theory and research suggest that parental job loss can be detrimental to low-income children’s well-being through both economic and family process pathways. Job loss and subsequent economic hardship are associated with increased behavior problems, decreased emotional well-being, and, in some cases, reduced educational attainment among low-income teenagers (Conger, Ge, Elder, Lorenz, & Simons, 1994; Elder, VanNguyen, & Caspi, 1985; Kalil & Ziol-Guest, 2005, 2008; McLoyd, Epstein, Ceballo, & Borquez, 1994; Randolph, Rose, Fraser, & Orthner, 2004). The few studies of job loss and younger children’s behavior (parent-reported) find effects ranging from neutral (Chase-Lansdale, Moffitt, Lohman, Cherlin, Coley, Pittman, et al., 2003) to moderately large and adverse (Johnson, Kalil, & Dunifon, 2010). Although the existing nonexperimental studies offer important insights into the ways that job loss may influence children’s socioemotional development, even with careful statistical controls, their findings are confounded with unmeasured characteristics of children and families or the contemporaneous timing of events.

This is the first study to use experimental data to examine relations between maternal job loss and children’s behavior in the classroom. The empirical technique uses exogenous variation in maternal employment patterns produced by an experimental welfare-to-work program to reduce sources of bias present in other nonexperimental studies. The combination of experimental data and instrumental variables (IV) estimation has been used in several prior studies (Gennetian,

Hill, London, & Lopoo, forthcoming; Gennetian, Magnuson, & Morris, 2008; Kling, Leibman, & Katz, 2007; Ludwig & Kling, 2007; Mamum, 2008) and is recognized as a promising approach to understanding the causal pathways of experimental impacts (Angrist, Imbens, & Rubin, 1996; Robins & Greenland, 1992; Shadish, Cook, & Campbell, 2002). The experiment in this case, the National Evaluation of Welfare-to-Work Strategies (NEWWS), was implemented during the recession of the early 1990s. It provided a model for the eventual adoption of “work-first” policies across the country, and as such, offers currently relevant insight into the potential effects of programs designed to promote employment among less-educated workers.

## BACKGROUND

Less-skilled workers experience disproportionately high rates of job loss. Holzer and LaLonde (2000) estimate that, in the first 18 months of a job, a female high school dropout is 50 percent more likely to quit or be fired than more educated workers of the same age. Single mothers moving from welfare to work in the 1990s exhibited relatively high initial rates of employment—generally over 50 percent and frequently between 65 and 80 percent (Brauner & Loprest, 1999)—but short average job tenures. Data from a 1997 survey indicated that three quarters of welfare leavers had occupied their jobs for less than one year and that one-third had tenure of less than six months (Loprest, 1999). In a study that followed welfare recipients in New Jersey for five years after federal welfare reform, most workers experienced at least one job loss and the average period of nonemployment was eight months (Wood, Moore, & Rangarajan, 2008). These findings highlight the success of programs focused on promoting job entry and increasing employment among low-income single mothers, but also the vulnerability of this group to job loss.

The higher risk of job loss among less-educated workers is partially attributable to lower-than-average skill levels and inconsistent employment histories, but it is also compounded by the economic vulnerability of the industries they occupy. Among welfare leavers, a “last hired, first fired” phenomenon has been ascribed to the more economically vulnerable industries they tend to occupy, namely retail, food services, personnel supply, and hotel/lodging (Boushey, 2001). A combination of low job quality—characterized by low pay, lack of benefits, and inflexible scheduling—the demands of single parenting, and the low education and experience levels of workers themselves may operate in concert to precipitate job loss and make stable employment more difficult to achieve among low-income single mothers (Lee & Vinokur, 2007).

In economically disadvantaged families, involuntary interruptions in maternal employment are most consistently associated with adverse school outcomes among children, including increased likelihood of grade repetition and higher rates of high-school dropout (Kalil & Ziolk-Guest, 2005; Randolph et al., 2004; Stevens & Schaller, 2009). The handful of studies that ask similar questions about child behavior find less consistent evidence (Chase-Lansdale et al., 2003; Johnson et al., 2010; Kalil, Dunifon, & Danziger, 2001). In Chase-Lansdale and colleagues (2003), maternal transitions out of employment are associated with increased internalizing and externalizing behavior problems among adolescents, whereas preschoolers appeared to be unaffected. Several studies of low-income children in early to middle childhood find that maternal employment transitions, particularly involuntary job loss, lead to higher levels of behavior problems (Johnson et al., 2010; Kalil et al., 2001). Johnson and colleagues (2010) estimate that each additional year in which a low-income mother has an involuntary job loss is

associated with a 0.15 standard deviation increase in child problem behavior, relative to working stably all year. As the authors point out, the cumulative negative effects of many years of unstable employment are potentially quite large.

Job-to-nonemployment transitions have the potential to impede child development primarily through changes in family income, child care arrangements, and parents' psychological well-being. If family income decreases due to a maternal job interruption or becomes less predictable over a period of unstable employment, families may curb spending on everything from food and housing to books and children's extracurricular activities. In addition, job loss is associated with volatility in non-parental child care arrangements, a particularly important context for the development of young children (Lowe, Weisner, & Geis, 2003; Miller, 2005). Young children who experience changes in child care arrangements tend to exhibit lower cognitive and language skills, more behavior problems, and less attachment security than those in stable care (Morrissey, 2009; NICHD ECCRN, 1999, 2001; Tran & Weinraub, 2006; Youngblade, 2003). Finally, there is substantial evidence that parental stress and depression associated with economic hardship can lead to less nurturing and harsher parenting, which, in turn, negatively affect child adjustment and behavior (Conger, Conger, Elder, Lorenz, Simons, & Whitbeck, 1992, 1993; Conger et al., 1994; Elder, 1974; McLoyd et al., 1994; Mistry, Vandewater, Huston, & McLoyd, 2002; Mistry, Lowe, Benner, & Chien, 2008; Parke, Coltrane, Duffy, Buriel, Dennis, Powers, et al., 2004). The negative economic and psychological consequences of job loss are likely to be pronounced among families headed by low-income single mothers because they tend to populate the bottom of the income spectrum and usually lack savings and other financial assets to buffer against economic crises.

The present study examines maternal job-to-nonemployment transitions in a sample of low-income preschool-aged children, contributing two primary innovations to the existing literature on this topic. First, unlike prior studies on this topic, the present study examines job loss induced by an experimental welfare-to-work program. We estimate the effect of job loss using only individuals for whom random assignment either led to one or more job losses that would not have happened otherwise (treatment group) or forestalled job losses that would have happened in the context of the treatment (control group). The advantage of this approach is that we can isolate the effects of job loss from other characteristics of families that may be associated with both job loss and outcomes for children.

Second, we use arguably more reliable measures of both maternal employment and child behavior relative to prior analysis of survey data. Maternal employment status is determined by state Unemployment Insurance (UI) earnings records and child behavior is assessed by a teacher relative to other children in the classroom. Unlike self-reported employment histories, UI records have the advantage of being free from recall bias and thus may provide more complete data relative to that from surveys (depending on survey methods used). For their part, teacher reports of child behavior are less likely to be biased by the effects of changes in the family environment on parenting stress and depression (which may be changing perceptions of behavior rather than behavior per se). They also capture children's behavior in a different and critical context for long-term success, and provide insight into the interactions between children's home and school environments.

The relationship between job loss among low-income single parents and children’s development has implications for current and future employment programs. The subject takes on particular significance during an economic downturn when the odds of job loss and unemployment are high. Although the economic context has changed in many ways since the early 1990s, NEWWS was implemented in a recessionary period with unemployment rates around 10 percent. In addition, although the broader policy environment has changed dramatically for low-income families since the time of NEWWS, the model of most employment services has not. By examining the influence of sustained maternal job loss on young children’s social behavior among low-income, single-mother families, we hope to provide insight into the potential intergenerational effects of those programs and the importance of services designed to support job stability.

## THE EXPERIMENT

In this study, we rely on data from the National Evaluation of Welfare-to-Work Studies to address our questions about the effects of sustained job loss on children. NEWWS, conducted between 1991 and 1999 by MDRC, evaluated seven Job Opportunities and Basic Skills Training programs across the country, all designed to increase employment and reduce dependence on government assistance among welfare recipients. As part of the NEWWS Child Outcomes Study (COS), three study sites—Atlanta, Georgia; Grand Rapids, Michigan; and Riverside, California—collected more extensive information about the well-being of preschoolers five years after random assignment, including teacher reports of well-validated measures of social skills and behavior. In these sites, participants were randomly assigned to one of two program groups that were subject to a new set of welfare rules or to a control group that received the standard benefits available to recipients of cash assistance (the Aid to Families of Dependent Children program, at that time). Of interest in this study is the NEWWS Labor Force Attachment (LFA) program group, which required participants to begin searching for employment immediately and offered intensive assistance finding work through case management and organized group activities (for more information on NEWWS, see: Freedman, Friedlander, Hamilton, Rock, Mitchell, Nudelman, et al., 2001; Hamilton, Freedman, Gennetian, Michalopoulos, Walter, Adams-Ciardullo, et al., 2001).

Our use of NEWWS LFA is motivated largely by its relatively straightforward causal model (both intended *and* realized) of program impacts on families and children (shown in Figure 1). In particular, among welfare experiments conducted in the 1990s, NEWWS was unique in its emphasis on mandating employment and assisting with the job search process without offering complementary income or work supports. Other similarly designed programs, such as the Minnesota Family Investment Program and Florida’s Family Transition Program, offered expanded child care assistance and more generous earnings disregards (Bloom, Kemple, Morris, Scrivener, Verna, & Hendra, 2000; Gennetian & Miller, 2000). In NEWWS, particularly in the LFA program approach, the principal goal was to increase maternal employment and, in doing so, promote self-sufficiency. Other aspects of family economic well-being, such as welfare receipt, total income, and non-parental child care use, were not directly targeted by the NEWWS LFA program components but were seen as potential “derivatives of the targeted outcomes” (Zaslow, McGroder, & Moore, 2000, Figure SR-1). NEWWS evaluators also recognized that while the program components did not target parent mental health and parenting, those factors might also be influenced by changes in family economic circumstances engendered by the

program. In other words, any effects that the program ultimately had on children *operated through changes in employment* directly or indirectly through effects of employment on the family.

Impact analyses of the NEWS programs, conducted two and five years after random assignment, indicate that this program approach operated as intended: the LFA programs increased levels of employment and earnings among participants and consequently decreased welfare receipt, resulting in neutral effects on overall family income (Hamilton et al., 2001). The effects of the programs on child academic functioning and health and safety were generally neutral for preschool- and school-aged children and negative for adolescents (Freedman et al., 2000; Hamilton et al., 2001; McGroder, Zaslow, Moore, & LeMenestrel, 2000). Site-level differences are also documented: Riverside had the most beneficial impacts on maternal earnings (increase) and welfare receipt (decrease), but it was also the only site to increase teacher-reported problem behavior among the children of LFA participants. The LFA treatment had neutral or beneficial effects on children’s classroom behavior in Grand Rapids and Atlanta, respectively (Hamilton et al., 2001). There is some limited evidence from studies of these same programs that differences in site-level implementation of the LFA program partially explain differential program impacts (Bloom, Hill, & Riccio, 2003; Morris, 2008). Surveys of front-line and administrative staff indicate differences in the emphasis on “quick job entry” over more deliberative job matching, the degree of personalized attention, and caseload sizes, among other things. In prior studies, greater emphasis on quick job entry, more personalized attention, and smaller caseload sizes have been associated with larger positive program impacts on earnings (Bloom, Hill, & Riccio, 2003). However, programs with a greater emphasis on quick job entry also increased rates of depression among participating mothers with young children, an effect thought to be mediated by program impacts on employment and job transitions (Morris, 2008).

## ESTIMATION STRATEGY

We combine experimental data with a common econometric technique for addressing measurement and specification errors, the instrumental variables estimator. Strong and valid instruments, which are correlated with problematic endogenous variables (in this case employment and job loss) but uncorrelated with unobserved heterogeneity, produce less biased estimates than OLS. In addition, the pairing of IV estimation with experimental data is a recognized approach to modeling mediation in the context of social programs (Kling et al., 2007; Ludwig & Kling, 2007; Shadish et al., 2002). Most recently, this same approach was used by Gennetian et al. (forthcoming) to estimate the effects of maternal employment on the health status of low-income children in the NEWS study.

The IV strategy we use relies on two-stage least squares estimation with treatment-by-site interactions as instruments. The first-stage models are:

$$(1) J_i = \beta_1 + \sum \gamma_1(S_i * T_i) + \sum \lambda_1 X_i + \sum \delta_1 S_i + \varepsilon_i$$

$$(2) E_i = \beta_2 + \sum \gamma_2(S_i * T_i) + \sum \lambda_2 X_i + \sum \delta_2 S_i + \mu_i$$

$J_i$  is the number of sustained job losses experienced by child  $i$ ’s mother, and  $E_i$  is the number of quarters the mother was employed during the follow-up period.  $S_i$  is a set of three binary variables for program site, and  $T_i$  indicates assignment to the experimental treatment group. The

second-stage equation estimates children’s behavior as a function of the predicted values of job loss and employment based on equations (1) and (2):

$$(3) C_i = \beta_3 + \alpha_1 \hat{J}_i + \alpha_2 \hat{E}_i + \sum \lambda_3 X_i + \sum \delta_3 S_i + \nu_i$$

$C_i$  is a teacher report of either positive social behavior or problem behavior for child  $i$ . Equations at both levels include  $X_i$ , a set of baseline characteristics, including child’s age and gender, mother’s earnings in the year prior to study entry, earnings squared, length of time of welfare receipt prior to study entry, employment experience prior to study entry, whether mother had a high school degree or equivalent, marital status, number of children in the family, age of youngest child, race/ethnicity, and whether mother was less than 18 years old at time of child’s birth. Also included are a set of site dummies ( $S_i$ ), which control for time invariant demographic and economic differences between the sites. Unobserved heterogeneity is captured in  $\varepsilon_i$ ,  $\mu_i$ , and  $\nu_i$ .

The approach described above leverages differences in exogenous treatment effects on maternal employment and job loss across the three sites. In a second approach, we focus specifically on differences in NEWWS LFA program *implementation* at the three sites by drawing on data from staff surveys. We use two measures of office-level implementation: emphasis on quick job entry and caseload size. In these models, the site-treatment interactions ( $S_i * T_i$  in eq. [1] and [2]) are replaced with interactions between assignment to the treatment group and the average score of the office-level characteristics. All else equal, we hypothesize that the probability of losing a job obtained through participation in the LFA program would be higher at sites that placed greater emphasis on quick job entry and had higher average caseloads. Emphasis on quickly moving participants into the workforce may lead to job placements that are not well matched to workers’ skills or family circumstances, while higher caseloads make it more difficult for staff to monitor and support employed participants. An important assumption of the models pooling multiple sites is that job loss and other baseline covariates included in the model had similar effects on child behavior, regardless of site. Later in this paper, we provide evidence to support this assumption by comparing site-specific models of program impacts to the pooled IV first-stage results.

Randomization to the LFA program largely ensures the validity of our instruments. The identification strategy uses differences in program impacts across sites, which may result from variation in local economic contexts or demographic characteristics of the participants, as well as site-level variations in the implementation of the program. The reasons why site differences emerge has little bearing on the validity of the instruments, however, because they rely on comparisons between randomly-assigned treatment and control group participants within a single site. Note that even in the case of the use of implementation features as instruments, we rely on the interaction between the implementation feature and random assignment—not the implementation features alone. Therefore, to the extent that randomization was executed correctly, we can be confident that our instruments are not correlated with unobserved heterogeneity.

In the context of an experimental program, the exclusion restriction also assumes that the IV model captures all possible pathways of program impacts. The design of the LFA program is helpful in this regard because the treatment sought to (and did) affect maternal employment, but not family income, maternal education, or other potential mechanisms of program impacts on child behavior (Hamilton et al., 2001). In fact, among the set of welfare experiments conducted

by MDRC during the 1990s, the NEWWS LFA programs are unique in their singular focus on providing employment services without expanded work supports such as financial incentives, child care subsidies, or health insurance. Our models include instruments for both the level of employment (quarters employed) and job loss as the primary pathways of program impacts on children's behavior. To further examine the validity of the instruments, we explore program impacts on a large set of family outcomes that might relate to both maternal employment and child behavior, but that were not targeted directly by NEWWS LFA. Sensitivity tests were also conducted using alternative measures of employment and job loss.

The strength of the instruments is measured by the covariance between random assignment to the treatment group at specific offices and sustained job loss. The F-statistics from the first stage of the IV models range from approximately 4 to 12. In our preferred models, they are all above 10, the conventional threshold for acceptably strong instruments in just-identified models (Staiger & Stock, 1997). Still, the F-statistics are small enough to warrant concern that our coefficient and standard error estimates may be biased, particularly given the relatively small sample size (Bound, Jaeger, & Baker, 1995; Murray, 2006; Nelson & Startz, 1990; Staiger & Stock, 1997). We examined this issue empirically and provide results in the sensitivity tests section of this paper. The common concern that models with weak instruments are more vulnerable to omitted variables bias (Angrist et al., 1996) is less of an issue in this study because random assignment ensures that the average characteristics of treatment and control group members are the same.

## SAMPLE AND MEASURES

The NEWWS-COS included 3,018 children across three sites who were between the ages of three and five years at the time of random assignment. We narrowed that sample to the 970 LFA treatment or control group families with a teacher report of problem and/or positive social behavior for the focal child. Table 1 presents pre-random assignment family characteristics separately for parents assigned to the LFA program group and the control group. Appendix Table 1 provides these statistics separately for each of the three sites included in this analysis.<sup>1</sup> Sample sizes are roughly equivalent across the three sites, as they are between the total number of control and treatment group members (427 and 543, respectively). As expected with randomization, there are few differences between the two groups on baseline characteristics. Our analysis uses the following measures:

### Sustained job loss

The explanatory variable of interest is the number of quarterly transitions out of employment during the five-year follow-up period. This variable was created with state administrative data on quarterly earnings from the unemployment insurance (UI) program. We considered a sample member employed in a given quarter if she had any positive earnings. A transition out of employment was defined as having positive earnings in quarter  $t$  and zero earnings in the subsequent quarter,  $t+1$ . The two primary measures of transitions out of employment are: *total job losses during the follow-up period* (ranges from 0-6; sample mean is

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<sup>1</sup> NEWWS was implemented in four offices in Riverside County: Riverside, Rancho Mirage, Hemmeter, and Lake Elsinor. Due to small sample sizes at the office-level, we pooled all four Riverside offices for these analyses.

1.22) and a dichotomous variable for having had *any job losses in the follow-up period* (sample proportion is .64). We also control for mother’s level of employment using the total number of quarters with positive earnings during the follow-up period.

What we define as *sustained job loss* is comparable to what prior studies using survey data have defined as job-to-nonemployment transitions or job instability (Johnson & Corcoran, 2003; Johnson et al., 2010; Royalty, 1998). While we cannot know whether a job loss documented in administrative data was voluntary or not, our definition of sustained job loss requires a long period of nonemployment, a characteristic more commonly associated with involuntary job instability rather than voluntary job mobility. Data from the Michigan Women’s Employment Survey suggests that nonemployment for more than one month is less likely than shorter periods of nonemployment to be voluntary or related to job searching (Johnson et al., 2010). Our data requires that the period of nonemployment be three months or more (i.e., one quarter).

The administrative data on employment that we use in this study have advantages over self-reported employment data used in many studies on this topic. Administrative data does not rely on a sample member’s participation in a survey or accurate recall of employment status, making it largely free of the response and non-response bias common to survey data. There are limitations, however, to the coverage of UI data, which could also introduce bias. UI data covers formal employment in a specific state, but excludes earnings from self-employment, “off-the-books” work, and jobs in other states. In addition, UI data does not provide start and end dates of a job or capture job losses that are recovered within a quarter. Error in UI records can also occur with misreported social security numbers (Kornfeld & Bloom, 1999). Despite these limitations, several papers indicate that UI records track employment and earnings at least as well as self-reports in surveys (Kornfeld & Bloom, 1999; Wallace & Haveman, 2007).

To explore these tradeoffs in the NEWWS data, we used survey information collected from treatment and control members two years after random assignment to compare employment patterns in UI and survey data directly. We learned from the survey data that those who become unemployed for a full calendar month typically stay unemployed for a full quarter, providing greater confidence in the use of administrative data for capturing the full number of job losses in the follow-up period. In addition, our analysis of cases with a discrepancy between the total number of job losses reported in the survey and administrative data showed that the administrative data often captured *more* employment (contrary to expectations). Some sample members had Unemployment Insurance earnings recorded in periods when they did not report working on the survey. This suggests that recall problems may bias survey measures of employment more than the missing sources of earnings (like informal employment) bias administrative sources of data. Moreover, we found that discrepancies between administrative and survey data were particularly common in the first year of follow-up, consistent with the notion that survey respondents may have had trouble recalling employment that occurred two years prior to the survey. We were particularly concerned that it was the workers with more job instability who would be most likely to have trouble recalling past employment histories. In sum, our comparison of these two sources of information on employment and job loss suggested that, for the purposes of this study, the administrative records provided more complete and reliable data.

### Child behavior in the classroom

We estimate models predicting measures of child social development, specifically problem behavior and positive social behavior in the classroom. In a survey conducted five years after random assignment, the focal child's current school teacher (who was blind to a child's experimental group) completed items from the Behavior Problems Index (BPI; Peterson & Zill, 1986) and the Positive Child Behavior Scale, Social Competence Subscale, (PCBS/SCS; Polit, 1996). At the time of assessment, children (who were three to five years old at random assignment) were eight to ten years of age. The total problem behavior measure used in this study summed percentage scores on three BPI sub-scales: internalizing behaviors, such as being sad or anxious; externalizing behaviors, such as fighting; and hyperactivity, including disrupting others and acting without thinking. In the sample, this problem behavior measure ranged from 0 to 2.83, with a mean of 0.83 (sd=0.69). The PCBS/SCS evaluates child cooperation, positive assertion, self-control, and interpersonal skills. The total positive social behavior scale used in this study, which sums the scores on four sub-scales, ranged from 5 to 84, with a mean of 50.87 (sd=19.73). We standardized total problem behavior and positive social behavior scores by dividing the difference between each child's score and the sample mean by the standard deviation of the control group. This transformation of the measures into standard deviation units eases the interpretation of regression coefficients. For additional information on these measures, see Appendix J.1 in Hamilton et al. (2001).

### Program implementation features

Our IV estimation strategy takes advantage of differences in the implementation of the NEWWS LFA program at the site level, as reported by frontline staff. As part of the program evaluation, administrators and line staff at each of the six offices were surveyed about their approach to serving clients. These survey measures have been used in prior studies of the factors associated with impacts on earnings and depression in NEWWS and other similarly designed welfare-to-work programs (Bloom, Hill, & Riccio, 2003; Morris, 2008).

The surveys of frontline workers were conducted two years after random assignment and had completion rates of 90 percent or higher. The number of case managers that participated in the survey by site were 23 in Grand Rapids, 27 in Atlanta, and 71 in Riverside (Hamilton, Brock, Farrell, Friedlander, Harknett, et al., 1997). For the purposes of this analysis, we selected two measures collected in the survey that are conceptually linked to the process of finding and keeping employment: caseworker/program emphasis on *quick job entry* (as opposed to education, training, and waiting for "better" jobs) and *caseload size*. We averaged the responses from all staff respondents to the survey in a given office, creating two site-level measures of program implementation.<sup>2</sup> Table 2 describes the two measures and provides site-specific means, as well as the overall study means and standard deviations. The quick job entry scores were -0.57, -.08, and 2.39, in Atlanta, Grand Rapids, and Riverside, respectively. Grand Rapids had the highest average caseload at 120 clients per staff; the averages in Riverside and Atlanta were 102 and 95, respectively.

### Demographic characteristics

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<sup>2</sup> In the case of Riverside, we used the average of all 4 offices in Riverside County. Our results were not sensitive to a model that used variation between these offices.

Baseline parent and family characteristics were collected on a pre-random assignment information form completed by the participants and through interviews of participants by program staff. Our models include the following baseline control variables: family earnings in the year prior to random assignment (in \$1,000 units); family earnings in the year prior to random assignment squared; whether mother was employed in the year before random assignment (0 or 1); whether mother has a high school diploma (0 or 1); mother’s marital status (never married, married, separated); child gender; number of children in the family; age of youngest child in the family; mother’s race (Black, white, Latino, other); and whether mother was under 18 years of age when the focal child was born (0 or 1). Because the timing of random assignment and the fielding of the five-year surveys varied by child, we also control for follow-up length in months. Finally, all models include indicator variables for two of the three welfare offices: Atlanta and Riverside (Grand Rapids is the omitted category).

## RESULTS

### Program impacts

The instrumental variables approach used in this analysis leverages the variation in the number of job losses due to being randomly assigned to the experimental group in one of three NEWWS LFA sites. This identification strategy warrants careful attention to the site-level program impacts on job loss and employment, as well as the intended and unintended derivatives of employment impacts. This analysis of NEWWS LFA program impacts across the five full years of follow-up tests the causal model for shown in Figure 1 and highlights variation in impacts by site that would explain differential effects of the program on child behavior. Table 3 shows the regression-adjusted mean differences between the NEWWS LFA treatment and control groups by site on dimensions of employment, the intended derivatives of employment, the nontargeted parent outcomes, and child behavior. The final column in Table 3 indicates whether the differences between site-level program impacts were statistically significant according to the nonparametric Kruskal-Wallis H test.

If maternal job loss is associated with heightened child problem behavior, we would expect sites with program impacts on job loss over the follow-up to also have impacts on child behavior. This appears to be the case, particularly for problem behavior. In Atlanta, random assignment decreased the probability of experiencing any sustained job loss by nine percentage points and reduced child problem behavior in the classroom by one quarter of a standard deviation. Children’s positive behavior was also higher, on average, for treatment group members in Atlanta, but this difference was only marginally significant. In contrast, the Riverside LFA treatment increased maternal job loss—a one half of a job loss increase and a 17-percentage-point increase in the probability of any job loss—and also increased children’s problem behavior. There were no statistically significant differences in job loss or children’s behavior between the treatment and control groups in Grand Rapids. The cross-site differences in program impacts on number of job losses and child problem behavior in the classroom are statistically significant at the  $p < .01$  level.

The patterns of the coefficients shown under “employment and earnings” in Table 3 do not suggest that the changes we see in child behavior are driven by dimensions of employment other than job loss. Here again, we are focusing on *variation* across sites rather than merely experimental impacts within sites. Across all three sites, LFA treatment group members worked

and earned more (although the earnings impact was not statistically significant in Atlanta). In all sites, the length of the longest employment spell in quarters was longer for treatment- than control-group members, although the difference did not reach statistical significance in Riverside. While there is variation in the magnitude and statistical significance of program impacts on these measures across sites, none of the differences across sites are statistically significant.

Table 3 also examines program impacts on what we refer to as intended derivatives of maternal employment and non-targeted parent outcomes. Intended derivatives include measures of family economic circumstances—including quarterly welfare receipt and income. Non-targeted outcomes include non-parental child care type, and maternal depression and parenting aggravation. No component of the NEWWS-LFA program was designed to affect these factors directly, but the program model did presume that increases in maternal employment might lead to changes in these areas. In two of the sites, Grand Rapids and Riverside, mothers randomly assigned to the NEWWS LFA program had slightly lower average quarterly welfare receipt (\$80 and \$60 less, respectively). The cross-site differences in program impacts on welfare receipt were statistically significant at the  $p < .05$  level (although, notably, the pattern of impacts does not line up with the site-level impacts on child behavior). There were few statistically significant program impacts on any other intended derivative of employment or non-targeted parent outcomes, and for no other measures were there statistically significant differences between sites. This analysis indicates that job loss is the only dimension or derivative of employment with a clear pattern of impacts that descriptively match the impacts on children’s behavior.

#### OLS and IV results

Table 4 presents the first-stage results from IV models estimating the effects of sustained job loss on child behavioral outcomes. We present the results of two different models—the first using site-by-treatment interactions as instruments and the second using implementation feature-by-treatment interactions as instruments. The first-stage results using site-by-treatment interactions (model 1) are very similar to the experimental impacts presented in Table 3, supporting the assumption of the IV models that the effects of baseline covariates on job loss were homogenous across sites.

In model 2, the instruments are interactions of treatment status and two site-level implementation features. The first stage results indicate that staff emphasis on quick job entry increased job loss, while caseload size increased the level of employment among treatment group members. An increase of one point on the quick job entry scale was associated with 0.21 job losses and an 8 percentage-point increase in the probability of any job loss among the treatment group relative to the control group. The relationship between caseload size and employment is relatively weak—each additional case predicting 0.02 more quarters of employment (less than 2 days) over five years—but also counterintuitive if we assume that caseworkers with smaller caseloads will be able to provide more assistance and supervision to each client and increase their likelihood of working. One speculative, but realistic, explanation for this result is that the caseworkers who were most successful at placing clients in jobs were given larger caseloads over time.

The first column in Table 5 presents the OLS estimates of the effect of sustained maternal job loss on children's behavior. According to these estimates, a child whose mother experienced at least one sustained job loss over the follow-up period has a problem behavior score that is, on average, 17 percent ( $p < .10$ ) of a standard deviation higher and a positive behavior score that is, on average, 23 percent ( $p < .01$ ) of a standard deviation lower than a child whose parent experienced no job loss at all. Each additional job loss is not associated with worse problem behavior but is associated with a small (.07 standard deviations;  $p < .05$ ) but statistically significant decrease in children's positive behavior.

The second and third columns of Table 5 show the second-stage estimates of IV models using a set of interactions between the three NEWS sites and treatment status as instruments. Compared to the OLS estimates, IV model 1a shows much larger positive effects of sustained maternal job loss on children's problem behavior and no statistically significant effects on children's positive behavior. The coefficients indicate that each sustained job loss increases a child's problem behavior score by more than half a standard deviation, and that a child whose parent experienced at least one job loss after random assignment is expected to have a problem behavior score that is over one standard deviation higher relative to a child whose parent did not experience any job loss. Controlling for number of quarters employed over the follow-up, in IV model 1b, the effects are even larger. Despite the reduced precision associated with IV estimation, the coefficients on job loss predicting problem behavior are all statistically significant at the 5 percent level. The IV estimates of the effect of sustained maternal job loss on positive behavior are all negative and larger than the OLS estimates, but none are statistically significant at conventional levels.

Table 6 presents IV results using implementation features as instruments in place of site-by-treatment-status interactions. These instruments are stronger (as measured by the F-stats of the instruments in the first stage), making them the most reliable of our IV results. The coefficients remain stable using this alternate set of instruments. IV model 2a, using the quick job entry implementation feature as the only instrument and sustained job loss as the only endogenous variable, provides estimates that are almost identical to those from the comparable model using site-treatment status interactions as instruments. IV model 2b, which controls for employment using quick job entry and caseload size also provides estimates that are very similar to those using site-treatment status interactions as instruments.

The definition of sustained job loss in these models, a quarter of some positive earnings followed by a quarter of zero earnings, is subject to two potential problems. First, this definition treats the loss of a one-week temporary job during a twelve-month period where the person is otherwise unemployed the same as the loss of a full-time job that the person may have held for a few years. Second, the definition of job loss does not capture a job loss if the person started working again within the next few months so that there is no full fiscal quarter of zero earnings. In short, our estimates may actually underestimate the effects of job loss by counting small changes as job losses and missing some large changes in earnings that are shorter-lived.

We tested the sensitivity of our results to two alternative definitions of job loss (results not shown). If these sources of measurement error are rare, then we should find similar effects as in our main models, and indeed we do. First, to ensure that the drop in employment associated with

job loss was consequential, we defined job loss as having \$340 or more in earnings in quarter  $t$  followed by zero earnings in quarter  $t+1$ . The threshold of \$340 is the equivalent of 80 hours of work at \$4.25/hour, the minimum wage throughout most of the 1990s. This definition excludes job losses preceded by a quarter of very low earnings, when a mother worked only briefly or very irregularly, as a transition that is less likely to affect family circumstances. Second, we altered the definition of job loss to a 75 percent or greater drop in earnings between quarters  $t$  and  $t+1$ . This approach counts large, but less than 100 percent, drops in earnings as transitions that may have consequences for family circumstances that are similar to a drop in employment that ends in a loss of any employment. The estimates from models with these variables are almost identical to our original estimate, indicating that our estimates are robust to varying definitions of job loss.

### Supplemental analysis

We conducted three additional analyses to better understand the identification of the estimate of large adverse effects on child problem behavior associated with maternal job loss. First, we recognize that job loss is only one dimension of a worker's overall employment pattern, and that job loss might be experienced very differently among parents with low and high levels of employment. Therefore, we examined the cross-site pattern of impacts on job loss and level of employment in order to determine whether the impacts on job loss were most pronounced for certain workers. The results of this analysis are shown in Table 7.

We divided the sample in each site into three main categories capturing the level of employment during follow-up: continuously unemployed; high employment; and low employment. High employment is defined as being employed in nine or more quarters during follow-up, the median in the sample. Low employment is less than nine quarters. Within the high and low employment groups, we show the probability that the control group members experienced no job losses, one job loss, or two or more job losses, and the program impact (treatment-control difference) on that probability. Based on these results, the increases in job loss at the Riverside site appear to be concentrated among those individuals with high levels of employment. By contrast, we observe effectively no impact on job loss for those in Riverside with low levels of employment. In addition, the increase in job loss that we observe in Riverside is concentrated in multiple and not single experiences with job losses (2 or more job losses). In short, the effects of job loss that we are leveraging in these models are probably among those with high levels of post-treatment employment and for those who experience multiple job losses. It is also important to note that the employment patterns of the control group in Riverside differed from other sites. Riverside control group members were more likely to be continuously unemployed and less likely to have high employment with multiple job losses.

Second, we examined the timing of job loss. One feature of this study is that we estimated the effects of program-induced changes in maternal employment patterns over a relatively long period, five years, on child behavior measured only at the end of that period. In order to clarify the timing of changes in maternal employment relative to child outcomes measured at the five-year follow-up, we estimated program impacts on job loss by site in each of the five years of the program. Published findings from the NEWWS evaluation indicate that the average duration of participation in the program was quite short, ranging from 3 to 6 months across sites (Hamilton et al. 1997). Not surprisingly, the results shown in Figure 2 indicate that the increase in job loss

associated with the Riverside program and the decrease associated with the Atlanta program were heavily concentrated in, but not limited to, the first two years. The Atlanta treatment group had fewer average job losses than the control group in four of the five years, although these differences did not reach statistical significance. Given this pattern, we would have liked to test the effects of maternal job losses experienced in the first two years after random assignment on shorter-term as well as longer-term measures of child behavior, but data were only collected from teachers during the survey conducted five years after random assignment. That said, this analysis suggests that differences across sites in effects on job loss are weaker but still present in the later years of the follow-up, more contemporaneous to the timing of the data collection on children.

Finally, although this study is not designed to examine the potential mechanisms of the effects of maternal job loss on child behavior at school, we conducted some exploratory analysis to understand intervening family and contextual changes that might mediate the relation between job loss and children's behavioral outcomes. This analysis was constrained by the limited number of potential mediators measured in the original NEWWS study, and our reluctance to introduce additional endogenous measures that would re-introduce the bias typically found in OLS models. Nonetheless, our hypothesis was that maternal job instability would be most likely to have large and persistent adverse effects on child behavior if it led to adverse effects on the home and child care environment. We examined two aspects of children's microsystems for which we have data and which the literature would suggest might be mediating mechanisms of job loss: maternal depression and child care arrangement type. In IV models using job losses in the first two years as a predictor of these two outcomes at the five-year follow-up (not shown, but available from the authors), the only statistically significant result is that job loss experienced early in the study period is associated with higher levels of depressive symptoms at the 5-year follow-up, as reported by mothers.

#### Tests of instrument validity and strength

The IV models produce large estimates of the increase in child behavior problems associated with maternal job loss. The size of the estimates reflects the assumptions and mechanics of the IV estimator, which uses the ratio of program impacts on child behavior to program impacts on maternal job loss to identify the relationship between the two. The key assumption in this context is that program impacts on maternal job loss explain all of the difference between treatment and control children's problem behavior in Atlanta and Riverside. In other words, random assignment to the NEWWS LFA program at a given site must not be correlated with preexisting characteristics of participants or sites or have directly engendered changes in family life other than the level and stability of maternal work. This is a strong assumption, but one that is well supported by the randomized assignment of participants to the treatment and control groups and by the narrow focus of the NEWWS-LFA program on maternal employment. Nonetheless, we examined two possible threats: 1) an alternative pathway of NEWWS-LFA program impacts on child behavior and 2) site-level implementation being driven by local differences in the characteristics of participants.

Table 3, showing program impacts on a variety of measures of employment, income, child care, and parent well-being, provides the clearest evidence that NEWWS-LFA operated primarily through changes in the level and stability of maternal employment. In our main models, we included total quarters employed during follow-up as an endogenous variable

because it is a sensible measure of level of employment and had the strongest relationship with random assignment relative to any employment during the follow-up and maximum length of an employment spell. In sensitivity tests shown in Table 8, we estimated these same models using each of these other measures of employment in place of total quarters employed. Model IV 3a, including any employment during follow-up as an endogenous variable in models predicting child problem behavior, increased the IV coefficient on number of job losses from .70 (SE=.33;  $p < .05$ ) to 0.93 (SE=.52;  $p < .10$ ), while including longest spell of employment (model IV 3b) reduced the coefficient to .59 (SE=.30;  $p < .10$ ). The first-stage F statistics for the two alternative employment measures were 3.32 and 7.30, respectively, relative to 10.13 for total quarters employed.

The only other outcome with statistically significant program impacts that varied by site is average quarterly welfare receipt. It is our contention that these impacts were indirect, operating through changes in employment and earnings, but we nonetheless tested a model in which we controlled for welfare receipt as an endogenous variable. The results are shown in the final column of Table 8, model IV 3c. This substitution increased the IV estimate of the effect of number of job losses on child problem behavior only slightly to .73 (SE=.38;  $p < .10$ ). The F-statistic in the first-stage regression predicting average welfare receipt as a function of random assignment to NEWWS-LFA is 3.18. Despite the limitation of estimating just a few endogenous variables in each of these models (due to the number of available instruments), these results are supportive of the effects of the program on child behavior operating through job loss, rather than some other employment process or derivative of employment.

Our favored models, IV models 2a and 2b, rely on differences in program impacts on job loss associated with site-level implementation of the LFA program. It is important to note that the demographic composition and local economic context differed substantially at these sites as well (see Appendix Table 1). Ninety percent of NEWWS participants in Atlanta were African-American, while Grand Rapids served whites and African-Americans and Riverside served whites and Hispanics (Hamilton et al., 2001). In 1993, when most NEWWS participants were began the program, unemployment rates in Atlanta and Grand Rapids were in the 5 to 6 percent range, while nearly 12 percent of Riverside residents were unemployed (Hamilton et al., 2001, Table 1.2). While these are factors associated with rates of employment and job loss (and are therefore controlled for in our models using site indicators), we were concerned because implementation is not randomly assigned. Our models are strengthened if implementation differences are a function of factors outside of the individuals' control—like the ideas of the manager of the welfare office—than due to the characteristics of individuals being served in the offices.

In analyses not shown here (but available from the authors), we examined this issue using two composite measures of the site-level sample characteristics: proportion job-ready and proportion minority. We selected these two measures because both might be associated with emphasis on quick job entry, in that welfare caseworkers might be encouraged to push the most skilled workers and those who face the least discrimination in the labor market most quickly into employment. Job-ready was defined as having a high school diploma, two years or less of welfare receipt, and some work experience in the year prior to random assignment. Simply aligning these sample characteristics with the site's implementation scores suggests that these

two factors are not highly correlated. For instance, Riverside and Grand Rapids have quite similar proportions of job ready participants, but very different quick job entry scores and average caseload sizes. In addition, we estimated IV models that included controls for these site-level sample characteristics instead of indicators for site. In these models, the coefficient on number of job losses predicting problem behavior remained significant and similar in magnitude. Finally, we considered estimating IV models using sample characteristic by treatment status interactions as instruments, but, unlike the site-level implementation features, these characteristics are not strong predictors of job loss, providing support for the notion that it is variation in implementation features and not variation in characteristics of individuals that is driving differences across sites in impacts on job loss.

We also conducted sensitivity tests aimed at investigating the potential for bias in the IV estimates from finite sample bias and measurement error. In the context of IV models, the combination of small samples and weak instruments is associated with finite sample bias (Bound et al., 1995; Nelson & Startz, 1990; Staiger & Stock, 1997). The instruments used in this study range from weak to moderate in strength, and the concern about finite sample bias is amplified because the dependent variables, teacher-reported problem and positive social behavior, are not normally distributed. For instance, our unstandardized problem behavior scale ranges from 0 to 2.83, but 50 percent of the sample has a score below 0.7. This skewness is unlikely to cause bias in estimates using the full sample of more than 900 children. However, although the IV models use the entire sample in the analysis, they are leveraging differences for observations in which random assignment altered employment patterns (known as the “local average treatment effect”; Angrist, Imbens, Rubin, 1996).

Weak instruments generally bias IV estimates toward the OLS estimate (Bound et al., 1995), but that does not appear to be the case in this study where the IV coefficients are often 10 times as large as the OLS coefficients. However, standard errors can also be biased downward by finite sample bias, perhaps leading us to attribute statistical significance inappropriately (Staiger & Stock, 1997). The models presented in Table 4 have the weakest instruments and are overidentified, which allows for certain diagnostics on the potential for bias. Stock-Yogo critical values use the first-stage F-statistics to test two null hypotheses related to the instruments being weak (Stock & Yogo, 2005). The first is that the bias present in the IV estimates is less than 5, 10, 20, or 30 percent of the OLS estimates. The second indicates the level of sampling error (up to 25 percent) that would be acceptable in the rejection of the null if the true significance level was 5 percent. The models presented in Table 5 perform poorly on both tests. At a 5-percent significance level, we cannot reject the null hypothesis that the IV models have 30 percent or less of the bias present in OLS coefficients. In addition, we would have to accept a significance level over 25 percent (when the true level should be 5 percent) in order to consider the instruments F-statistics significantly different from zero.

If these were our only models, we would have reason to believe that we have inaccurately attributed statistical significance to the IV coefficients on job loss. However, our strongest model, IV model 2a, which is just-identified using quick job entry to instrument job loss, produces nearly identical estimates. The F-statistic for this model is approximately 12, larger than the conventional critical value (10) for rejecting the null that the instrument is weak (Staiger & Stock, 1997). In terms of the Stock-Yogo relative bias test, an F-statistic over 10 in a just-

identified model indicates that, at a 5-percent significance level, the *worst case* relative bias of the IV to OLS estimates is 10 percent (Stock & Yogo, 2005). The consistency of the coefficient estimates in this more robust model dampens the concern that the significant relationship between maternal job loss and children’s problem behavior is the result of bias in the standard errors.

We took one further step to address the potential of small sample bias, especially as it relates to the non-normality of our dependent variable. The outcome measures were transformed into several dichotomous measures indicating different thresholds of child behavior.<sup>3</sup> Table 9 presents the probit and IV probit results of models predicting child problem behavior, the measure that is associated with job loss in our main models. The transformation of our dependent variables does not alter the main pattern of findings, suggesting that finite sample bias is not a driving factor in the results. Consistent with our main models, the IV probit estimates are positive, statistically significant, and generally many times the size of the probit estimates. The effect sizes from the IV probit models are still large, but perhaps more realistic than our main results. For example, each additional job loss increases the probability of having a problem behavior score above the median by 25 percentage points, from a base of 46 percent. However, the coefficients on job loss predicting the probability that a child will have high levels of problem behavior—above the 75<sup>th</sup> percentile—is half the size and not statistically significant. What appeared in our main models to be a large marginal effect of each additional job loss on children’s problem behavior is more accurately described here as an increase in the probability that a child will exhibit more than the median level, but not extreme levels, of problem behavior.

## DISCUSSION

The goal of this study was to estimate the causal effect of sustained unemployment during children’s preschool years on their classroom behavior five years later. Using a sample of low-income, single-mother families receiving welfare, we find decidedly adverse consequences of job loss on child problem behavior but no statistically significant effects on child positive behavior. This study cannot speak to the effects of early job loss on child development in later childhood and adolescence, but it is possible that these large changes in behavior in the classroom would have implications for educational and behavioral outcomes in later years. We know that children who are viewed by teachers as having high levels of behavior problems go on to like school less and have fewer chances to learn from peers, both of which are critical to their long term academic progress (Ladd, Birch, & Buhs, 1999; McClelland, Morrison & Holmes, 2000; Raver, Garner, & Smith-Donald, in press). Furthermore, research suggests that adolescents’ behavior problems are predicted by problem behavior in earlier childhood (Campbell, Spieker, Burchinal, Poe, & NICHD ECCRN, 2006; Tremblay, Pihl, Vitaro, & Dobkin, 1994).

The estimates are noteworthy for their reliance on experimental data and an instrumental variables estimation strategy. Unlike other studies in which parental or child characteristics may lead to both job loss and children’s problem behavior, these analyses make use of an experiment

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<sup>3</sup> The conventional transformation of the dependent variable into its natural log did not produce a normal distribution in this case.

that manipulated employment behavior at random. In so doing, it allows us to be more confident that the effects we observe are indeed causal effects of job loss on the behavior of children. This approach convincingly addresses both nonrandom selection into employment and job loss and the potential for such factors as child health or child care to influence maternal work outcomes, something no prior study of maternal job loss and child behavior has done.

Our findings are consistent with growing evidence that parental job loss has adverse consequences on child behavior, academic achievement, and later employment outcomes, particularly in economically disadvantaged families (Johnson, Kalil, & Dunifon, 2010; Kalil & Ziol-Guest, 2005; Oreopoulos, Page, & Stevens, 2008; Randolph et al., 2004; Stevens & Schaller, 2009). The point estimates we observe are particularly large in magnitude relative to other studies: suggesting that a single job loss increases the problem behavior of children by about half of a standard deviation. The closest study to our own, Johnson et al. (2010), identifies the effects of involuntary job losses on child problem behavior using family-specific fixed-effects models and a sample of low-income families in Michigan. They estimate standard deviation-size increases in the *growth rate* of problem behavior associated with one job loss in a year, but the estimated effects on *the level* of behavior problems (most comparable to our own) are in the 0.20-0.40 standard deviations range. One possible explanation for the differences in magnitude is that the IV models in this paper control more thoroughly for measurement error and suppressing factors. However, the IV estimates could also be biased by weak instruments or violations of the exclusion restriction. That said, our estimated standard errors are large as well, showing a range of potential effects of job loss on children's behavior from these models. Perhaps the most appropriate conclusion to draw is that job loss does have a detrimental effect on low-income children's behavior, but the exact magnitude of that effect is uncertain.

It is puzzling that we would find such large effects on only one of two measures of child behavior in the classroom. The existing literature on job loss and child behavior does not include measures of positive social behavior, so we had few *a priori* hypotheses about what we would find when we examined it as an outcome. The lack of an effect in this study is partially the result of our IV estimation strategy. Only one site, Atlanta, had a statistically significant program impacts on positive social behavior and it was smaller in size than the impacts in Atlanta and Riverside on problem behavior. In addition, while problem behavior and positive social behavior scale items capture some common constructs, such as controlling anger, it is quite consistent in the literature on welfare-to-work programs that they do not move in tandem. In a review of evidence from welfare experiments, Grogger & Karoly (2005) found eight programs with impacts on a behavior problems measure, only two of which also changed positive social behavior.

Several supplementary analyses shed light on the nature and timing of the effects estimated in this study. The experimentally-induced job losses occurred primarily among treatment group members in Riverside County, California who worked nine or more quarters during the 5-year follow-up period, suggesting that the effects on children relate not to discreet job losses, but to job churning. In addition, the fact that NEWS impacts on job loss were concentrated early in the follow-up period indicates that job losses can have sustained effects on child behavior or precipitate instability in family life that manifests as child behavior problems over an extended period of time. Finally, our analysis of percentile problem behavior scores showed that job loss

increased the probability that a child will exhibit more than the median level, but not extreme levels, of problem behavior (although it is worth noting that the median problem behavior in this sample of low-income children is higher than for national samples; Hamilton, 2001).

There are several assumptions of our models that warrant careful consideration. First, the estimates of the effects of job loss are local average treatment effects valid for single-mother welfare recipients who responded to a labor force attachment program by becoming employed, thereby increasing their chances of job loss. It is not at all clear, for instance, that the effects of maternal job loss would be similar in two-parent families, and, indeed, recent research suggests that, in two-parent families, loss of a job for fathers is considerably more detrimental for children's development than loss of a job for mothers (Kalil & Ziol-Guest 2008). In addition, the policy context surrounding single mothers' employment changed drastically in the years following NEWWS with the implementation of federally mandated work requirements and time limits for welfare receipt, as well as work incentives offered through the Earned Income Tax Credit. Speculatively, one could imagine that the psychological consequences associated with losing a job and returning to assistance may be even more pronounced now in a time-limited welfare environment with few supports available for nonworkers.

Second, our models assume similarity in the effects of job loss on child behavior across the three sites, or across the offices with differing levels of the implementation. That is, our analysis (like that of Ludwig & Kling, 2007) leverages cross-site variation in impacts and assumes that the same impact on job loss in each site would produce a similar impact on child behavior. Fortunately, our analysis of site-level differences indicates that implementation features are not associated with measured characteristics of the welfare recipients served at those sites. Third, due to random-assignment, our results do control for the effects of child behavior that precede any changes in job loss, but our models could be overestimated if the job loss leads to changes in children's behavior, *and* those changes in behavior then lead to further job losses on the part of parents and further detriments in children's behavior. To be clear, we can confidently attribute some of the changes in behavior problems to the initial job loss given the programs targeted job loss and not child behavior directly. As such, we can be reasonably sure that the *start* of the causal chain was maternal job loss and not child behavior or other characteristics of children and families—a statement few papers on this topic can assert. But the subsequent interplay between child behavior and subsequent job losses is impossible for us to address in these models.

There are also potential limitations to our measures of job loss and children's behavior. In this study, state unemployment insurance records provided information on earnings, which we used to identify transitions out of employment. UI data are limited to formal employment in a specific state and do not capture job losses recovered within a quarter, self-employment, employment in other states, or informal work. While prior studies (Wallace & Haveman, 2007) and our own analysis of the NEWWS survey data indicate that UI records track employment, earnings, and economic hardship at least as well as self-reports, it is the case that mothers in our study may have been working in some capacity without formal work earnings. If transitions to informal work are potentially less stressful to families than transitions to nonemployment, then this limitation of our data would lead to underestimates of the effects of job loss on children. In addition, the measure of child behavior in the classroom was collected only once, five years after

random assignment, limiting our ability to carefully align the program’s impacts on maternal job loss with impacts on child classroom behavior earlier in the follow-up period.

What might explain the negative effects of sustained job loss on children’s behavior? Among former welfare recipients, the emotional setback of job loss could be heightened if accompanied by a return to public assistance. Alternatively, if a single mother is prevented or discouraged from re-entering the rolls, she may experience added economic stress and/or forced dependency on financial support from family or friends. In sum, in a policy environment where work is mandatory, job loss, returns to welfare, and decreased economic self-sufficiency may represent a more marked failure than they might otherwise, especially by those who parents who were most “ready” to make the transition to employment. In our admittedly limited exploratory analysis of potential mediators, we found some support for this theory: Job loss in the first two years of follow-up sharply increased mothers’ depressive symptoms at the five-year follow-up.

The study suggests that employment programs aimed at former welfare recipients and other women with low educational attainment or limited work experience would do well to emphasize job *retention*, in the interest of both parents and children. This idea has been discussed and advocated for many years (see Hershey & Pavetti, 1997), yet job instability remains high among the least-educated workers, particularly among single mothers. Longer-run analysis of the GAIN program in California, a precursor to NEWWS, suggests that the initial benefits of a “work-first” approach over education and deliberative job matching fade and reverse over time (Hotz, Imbens, & Klerman, 2006). While the economic and policy context has shifted several times since these models were tested in the 1990s, most services for the unemployed still follow the “work-first” approach. Policies that promote job stability and mobility might include guided job matching, job-specific training, incentives to increase the availability of child care during nonstandard hours, and reemployment resources. Some of these supports exist in the current policy context, but do not cover as many workers as they might. One of the NEWWS sites not included in this study, Portland, Oregon, is the most often cited example of a welfare program that successfully combined education and job search assistance to improve participants’ long-term economic circumstances (Hamilton, 2002). The ongoing Employment Retention and Advancement evaluation is testing multiple models for promoting steady employment and career progression (Bloom, Anderson, Wavelet, Gardiner, & Fishman, 2002). These approaches focus almost exclusively on addressing deficits or barriers faced by workers, but additional research and program demonstrations are needed to identify the right combination of supply- and demand-side interventions to improve job stability in the low-wage labor market.

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**Table 1**  
 Selected Baseline Characteristics of NEWWS-LFA Participants, by Research Group

Baseline Characteristic	Control Group	LFA Program Group
<u>Parent</u>		
Under 18 at child's birth (%)	11.38	8.03
Race (%)		
Black	52.67	56.44
White	28.91	28.34
Latino	16.57	13.82
Other	1.84	1.41
Marital status (%)		
Never married	56.91	62.30 †
Separated/Divorced	0.41	0.35 *
Married	0.02	0.03
Received high school degree or GED (%)	57.64	59.48
Employed in year prior to random assignment (%)	40.15	39.11
Earnings in year prior to random assignment (\$1,000s)	1.82	1.63
Length of AFDC receipt prior to random assignment (%)		
No prior AFDC receipt	0.00	0.47
Less than 2 years of AFDC receipt	27.81	24.59
		74.94
<small>NOTES: LFA = Labor Force Attachment program. Two-tailed t-tests were applied to differences between the LFA program and control group statistics.</small>		
<small>† p &lt; 0.10; *p &lt; 0.05; ** p &lt; 0.01.</small>		
Child is male (%)	46.84	48.07
Child's age	4.26	4.37 *
Age of youngest child in family	3.50	3.45
Number of children in family	2.15	2.22
Sample size	427	543

**Table 2**  
Description of Office-Level Implementation Features Used as Instruments in IV Models

Variable	Description	Atlanta	Grand Rapids	Riverside	Mean	SD
Emphasis on quick job entry	<p>Average of responses to four 7-item questions (averaged and standardized by office):</p> <ol style="list-style-type: none"> <li>1. Based on the practices in your unit, what would you say is the more important goal of your unit: to help clients get jobs as quickly as possible or to raise the education or skill levels of clients so that they can get jobs in the future?</li> <li>2. In your opinion, which should be the more important goal of your unit: to help clients get jobs as quickly as possible or to raise the education or skill levels of clients so that they can get jobs in the future?</li> <li>3. After a short time in the program, an average welfare mother is offered a low-skill, low-paying job that would make her slightly better off financially. Assume she has two choices: either to take the job or to stay on welfare and wait for a better opportunity. If you were asked, what would your personal advice to this client be?</li> <li>4. What advice would your supervisor want you to give to a client of this type?</li> </ol>	-0.567	-0.083	2.391	0.00	1.00
Caseload size	Caseworker caseload of clients on day of survey. Averaged by office.	95.13	120.07	108.76	106.09	11.78

SOURCE: Survey items from Bloom, Hill, & Riccio (2005); statistics are author's calculations.

NOTES: SD = Standard deviation.

**Table 3**

**NEWWS LFA Program Impacts on Targeted and Non-Targeted Outcomes, by Site**

	Atlanta	Grand Rapids	Riverside	Significance of Cross-Site Differences <sup>a</sup>
<b>Employment</b>				
Any employment	0.01 † (0.03)	0.06 * (0.03)	0.09 † (0.05)	
Number of quarters employed	1.42 * (0.66)	2.49 ** (0.67)	2.11 ** (0.74)	
Longest spell of employment (quarters)	1.50 * (0.64)	2.24 ** (0.62)	1.12 0.70	
Quarterly earnings (\$1,000)	4.02 (2.68)	7.42 * (3.29)	5.84 † (3.26)	
Any sustained job loss	-0.09 † (0.05)	0.02 (0.05)	0.17 ** (0.06)	
Number of sustained job losses	-0.14 (0.13)	-0.01 (0.16)	0.48 ** (0.14)	**
<b>Intended Derivatives of Employment</b>				
Average quarterly welfare receipt (\$1,000)	-0.03 (0.03)	-0.08 * (0.04)	-0.06 ** (0.04)	*
Quarterly income (\$1,000)	2.68 (2.16)	2.52 (3.03)	-0.07 (3.32)	
Non-parental child care type				
Only center care	-0.98 (4.20)	0.06 (3.73)	-7.12 * (2.89)	
Only home-based	3.30 (4.59)	0.47 (5.75)	9.12 (5.71)	
<b>Non-Targeted Outcomes</b>				
Maternal depression	0.17 (0.67)	-0.50 (0.92)	1.92 (.82)	
Maternal parenting aggravation	-0.17 (0.24)	0.14 (0.27)	0.39 (0.27)	
<b>Child behavior</b>				
Problem behavior	-0.27 * (0.12)	-0.10 (0.11)	0.23 * (0.10)	**
Postive social behavior	0.19 † (0.11)	-0.11 (0.12)	-0.03 (0.11)	

NOTES: Standard errors shown in parentheses. NEWWS five-year follow-up data were used in the analysis. Administrative data was used to assess all employment and income variables. Behavioral outcomes are measured by teacher report. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.

<sup>a</sup>Two-tailed H tests used to test the equality of program impacts across the three sites.

† p < 0.10; \*p < 0.05; \*\* p < 0.01.

**Table 4**

First-Stage Results of IV Models Predicting Child Behavior Using Sites and Site-Level Implementation Features as Instruments for Sustained Job Loss

Instruments	Number of Sustained Job Losses	Any Sustained Job Loss	Number of Quarters Employed
<u>Model 1 - Program Sites</u>			
Atlanta X Treatment status	-0.19 (0.13)	-0.11 * (0.05)	0.94 (0.64)
Grand Rapids X Treatment status	0.00 (0.15)	0.04 (0.06)	2.40 ** (0.72)
Riverside X Treatment status	0.48 ** (0.15)	0.18 ** (0.06)	2.07 ** (0.72)
<u>Model 2 - Site-Level Implementation Features</u>			
Emphasis on quick job entry X Treatment status	0.21 ** (0.06)	0.08 ** (0.02)	0.20 (0.31)
Caseload size X Treatment status	0.00 (0.00)	0.00 (0.00)	0.02 ** (0.00)

NOTES: Standard errors shown in parentheses. The figures included in this table are from an IV model predicting problem behavior, but are consistent with figures from the model of positive behavior. Coefficients for "Number of Quarters Employed" come from the model using the continuous measure of job loss, but are consistent with the results from the model using the dichotomous measure. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.  
 † p < 0.10; \* p < 0.05; \*\* p < 0.01.

**Table 5**

OLS and IV Coefficients for Sustained Job Loss using Site-by-Treatment Status Interactions as Instruments

	Child Problem Behavior			Child Positive Behavior		
	OLS	IV 1a	IV 1b	OLS	IV 1a	IV 1b
Number of sustained job losses	0.02 (0.03)	0.55 * (0.26)	0.71 * (0.33)	-0.07 * (0.03)	-0.16 (0.23)	-0.20 (0.26)
<i>F-stat (first stage)</i>		4.00 **	4.15 **		4.00 **	4.00 **
<i>Partial R<sup>2</sup></i>		0.013	0.013		0.013	0.013
Number of quarters employed			-0.06 (0.05)			0.01 (0.04)
<i>F-stat (first stage)</i>			7.10 **			7.75 **
<i>Partial R<sup>2</sup></i>			0.023			0.024
Any sustained job losses	0.17 * (0.07)	1.36 * (0.61)	1.81 * (0.78)	-0.23 ** (0.07)	-0.65 (0.57)	-0.78 (0.64)
<i>F-stat (first stage)</i>		5.06 **	5.06 **		4.71 **	4.71 **
<i>Partial R<sup>2</sup></i>		0.016	0.016		0.015	0.015
Number of quarters employed			-0.06 (0.05)			0.02 (0.04)
<i>F-stat (first stage)</i>			7.10 **			7.75 **
<i>Partial R<sup>2</sup></i>			0.018			0.024
Instruments:						
Atlanta X Treatment status		X	X		X	X
Grand Rapids X Treatment status		X	X		X	X
Riverside X Treatment status		X	X		X	X

NOTES: Standard errors shown in parenthesis. F-stat (first stage) and partial R<sup>2</sup> are for the instruments in the first stage of IV models. IV Model 1a includes 1 endogenous variable (number of sustained job losses). IV Model 1b includes 2 endogenous variables (number of sustained job losses controlling for number of quarters employed). Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.

† p < 0.10; \* p < 0.05; \*\* p < 0.01.

**Table 6**

IV Coefficients for Sustained Job Loss using Implementation Feature-by-Treatment Status Interactions as Instruments

	Child Problem Behavior		Child Positive Behavior	
	IV2a	IV 2b	IV 2a	IV 2b
Number of sustained job losses	0.50 † (0.26)	0.70 * (0.33)	-0.11 (0.23)	-0.19 (0.26)
<i>F-stat (first stage)</i>	12.16 **	6.12 **	11.60 **	5.90 **
<i>Partial R<sup>2</sup></i>	0.013	0.013	0.012	0.013
Number of quarters employed		-0.06 (0.05)		0.03 (0.04)
<i>F-stat (first stage)</i>		10.13 **		10.91 **
<i>Partial R<sup>2</sup></i>		0.022		0.023
Any sustained job losses	1.31 * (0.65)	1.83 * (0.83)	-0.30 (0.61)	-0.47 (0.66)
<i>F-stat (first stage)</i>	12.78 **	6.43 **	11.79 **	5.92 **
<i>Partial R<sup>2</sup></i>	0.014	0.014	0.013	0.013
Number of quarters employed		-0.06 (0.05)		0.02 (0.04)
<i>F-stat (first stage)</i>		10.13 **		10.91 **
<i>Partial R<sup>2</sup></i>		0.022		0.023
Instruments:				
Emphasis on quick job entry X Treatment status	X	X	X	X
Caseload size X Treatment status		X		X

NOTES: Standard errors shown in parenthesis. F-statistics and partial R<sup>2</sup>s are for the instruments in the first stage. IV model 2a includes 1 endogenous variable (number of sustained job losses) and only the emphasis on quick job entry instrument. IV model 2b includes 2 endogenous variables (number of sustained job losses controlling for number of quarters employed) and 2 office level characteristics (emphasis on quick job entry, and caseload size) as instruments. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.

† p < 0.10; \* p < 0.05; \*\* p < 0.01.

**Table 7****NEWWS-LFA Program Impacts on the Probability of Experiencing Patterns of Employment and Job Loss**

	Continuously Unemployed		High employment (>9 quarters)						Low employment (<=9 quarters)					
	Control Group	Program Impact	No job loss		1 job loss		2+ job losses		No job loss		1 job loss		2+ job losses	
			CG	PI	CG	PI	CG	PI	CG	PI	CG	PI	CG	PI
Atlanta	0.09	-.009 (.031)	0.14	.075+ (.040)	0.17	-.016 (.039)	0.20	.016 (.042)	0.03	.026 (.022)	0.17	-.053 (.037)	0.19	-0.04 (.041)
Grand Rapids	0.09	-.065* (.028)	0.07	.088* (.038)	0.15	.001 (.041)	0.29	.043 (.056)	0.07	-.054* (.025)	0.09	-.010 (.035)	0.25	0.00 (.052)
Riverside	0.31	-.087 (.053)	0.12	-.007 (.038)	0.11	.004 (.038)	0.05	.176** (.036)	0.07	-.046 (.028)	0.20	-.040 (.049)	0.14	0.00 (.042)

NOTES: High versus low employment defined by median quarters worked. Standard errors shown in parentheses. NEWWS five-year follow-up data were used in the analysis. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.

† p < 0.10; \*p < 0.05; \*\* p < 0.01.

**Table 8**

IV Coefficients on Number of Job Losses and Alternative Pathways  
Predicting Child Problem Behavior

	Child Problem Behavior		
	IV 3a	IV 3b	IV 3c
Number of sustained job losses	0.93 † (0.52)	0.59 † (0.33)	0.73 † (0.38)
<i>F-stat (first stage)</i>	6.12 **	6.12 **	6.12 **
<i>Partial R<sup>2</sup></i>	0.013	0.013	0.013
Any employment	-2.66 (2.58)		
<i>F-stat (first stage)</i>	3.32 †		
<i>Partial R<sup>2</sup></i>	0.007		
Longest spell of employment (quarters)		-0.07 (-0.06)	
<i>F-stat (first stage)</i>		7.30 **	
<i>Partial R<sup>2</sup></i>		0.016	
Average quarterly welfare receipt (\$1000)			2.22 (2.11)
<i>F-stat (first stage)</i>			3.18 *
<i>Partial R<sup>2</sup></i>			0.007
<b>Instruments:</b>			
Emphasis on quick job entry X Treatment status	X	X	X
Caseload size X Treatment status	X	X	X

NOTES: Standard errors shown in parenthesis. F-statistics and partial R<sup>2</sup>s are for the instruments in the first stage. IV models 3a-c include 2 endogenous variables (number of sustained job losses controlling for any employment, longest spell of employment, or average quarterly welfare receipt) and 2 office level characteristics (emphasis on quick job entry, and caseload size) as instruments. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child, and child gender.

† p < 0.10; \* p < 0.05; \*\* p < 0.01.

**Table 9**

Probit and IV Probit Coefficients and Marginal Effects (in brackets) Using Dichotomous Measures of Problem Behavior

	Child Problem Behavior					
	Any		Top 50th Percentile		Top 25th Percentile	
	Probit	IV Probit Model 2a	Probit	IV Probit Model 2a	Probit	IV Probit Model 2a
Number of sustained job losses	0.09 *	0.67 **	0.02	0.64 **	0.07	0.39 †
	(0.05)	(0.14)	(0.03)	(0.11)	(0.04)	(0.27)
	[0.02]	[0.19]	[0.01]	[0.25]	[0.02]	[0.12]
Any sustained job loss	0.29 *	1.77 **	0.13	1.72 **	0.30 **	1.03
	(0.12)	(0.39)	(0.09)	(0.29)	(0.10)	(0.73)
	[0.06]	[0.55]	[0.05]	[0.58]	[0.08]	[0.29]
Base probability	0.80		0.48		0.26	

NOTES: Standard errors shown in parentheses and marginal effects in brackets. IV probit model 2a is identical to IV model 2a in Table 5, with 1 endogenous variable (sustained job loss) and the emphasis on quick job entry as an excluded instrument. Baseline covariates included in all models are earnings in year prior to RA divided by 1,000, earnings squared in year prior to RA divided by 1,000, time on AFDC, high school degree, parent 18 or under at child's birth, never married, separated, number of kids, black, white, Latino, length of follow-up, employed in year prior to RA, age of youngest child.  
 † p < 0.10; \*p < 0.05; \*\* p < 0.01.

**Appendix Table 1**

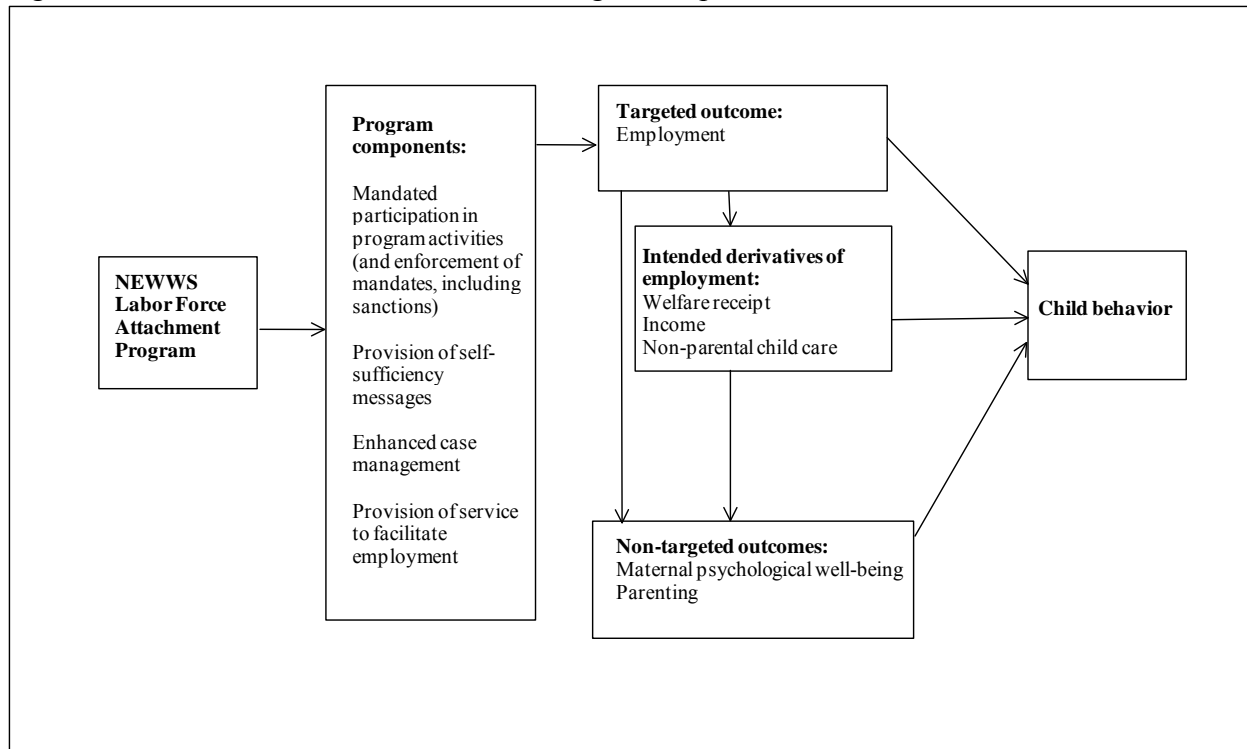
Selected Baseline and Demographic Characteristics by Site and Research Group

Baseline Characteristic	Atlanta		Grand Rapids		Riverside	
	Control	LFA	Control	LFA	Control	LFA
<b>Parent</b>						
Under 18 at child's birth (%)	8.60	4.49	16.31	13.89	9.26	5.71
Race (%)						
Black	95.70	94.38	42.55	40.28	22.22	14.29 †
White	3.23	3.93	46.81	51.39	39.35	38.10
Latino	0.54	1.12	8.51	6.94	35.65	44.76
Other	0.54	0.56	2.13	1.39	2.78	2.86
Marital status (%)						
Never married	74.19	76.40	60.28	58.33	39.81	43.81
Separated/Divorced	24.19	23.03	39.01	36.11	57.87	53.33
Married	1.61	0.56	0.71	5.56 *	2.31	2.86
Received high school degree or GED (%)	66.67	61.24	56.03	59.72	50.93	56.19
Employed in year prior to random assignment (%)	34.95	34.27	61.70	52.78	30.56	28.57
Earnings in year prior to random assignment (\$1,000s)	1.08	1.19	2.68	2.12	1.89	1.73
Length of AFDC receipt prior to random assignment (%)	2.78	2.79	2.74	2.75	2.66	2.67
No prior AFDC receipt	0.00	0.00	0.00	0.00	0.00	1.90 †
Less than 2 years of AFDC receipt	22.04	21.35	25.53	25.00	34.26	29.52 †
2 or more years of AFDC receipt	77.96	78.65	74.47	75.00	65.74	68.57 †
<b>Child and Family</b>						
Child is male (%)	48.39	47.75	51.77	44.44	45.37	48.57
Child's age	4.30	4.50 *	4.32	4.36	4.17	4.18
Average age of youngest child	3.75	3.85	3.05	2.97	3.59	3.45
Average number of children in family	2.15	2.22	2.16	2.18	2.14	2.29
Sample size	186	178	141	144	216	105

NOTES: LFA = Labor Force Attachment program. Two-tailed t-tests were applied to differences between the LFA program and control group statistics.

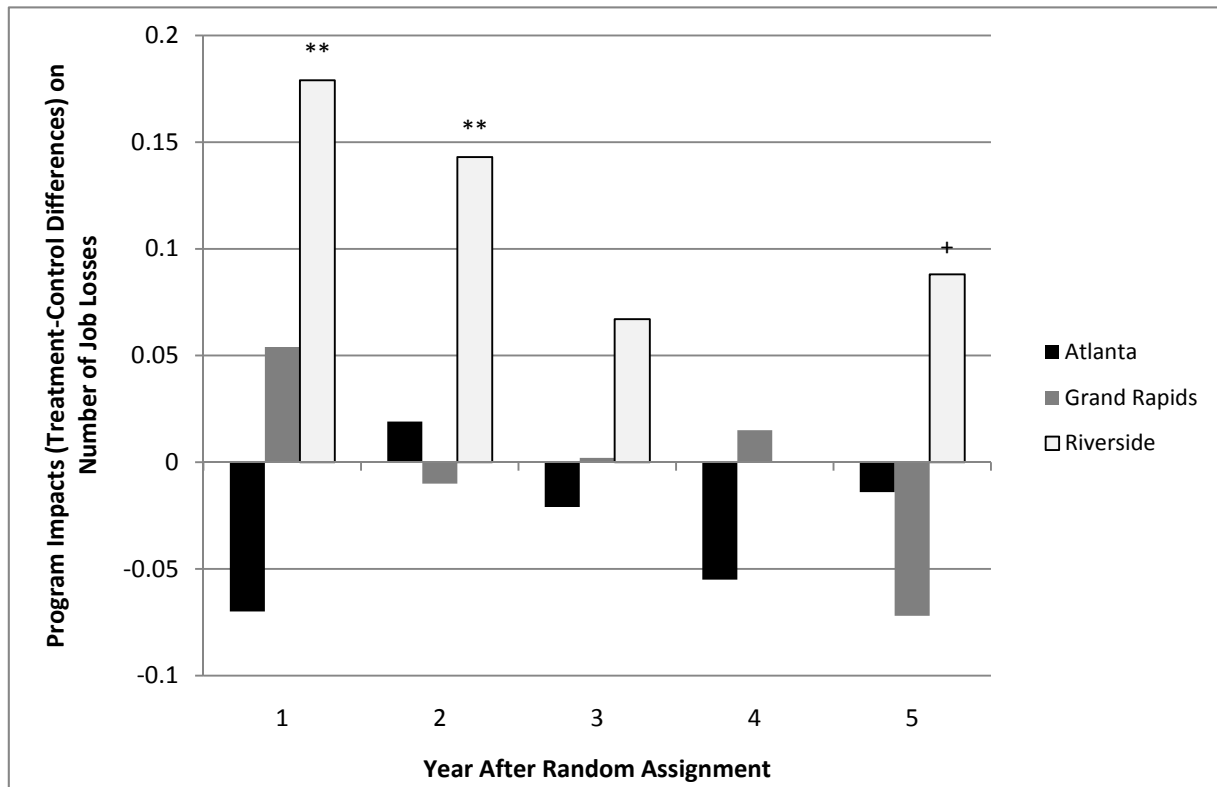
† p < 0.10; \*p < 0.05; \*\* p < 0.01.

Figure 1. Causal Model of NEWWS LFA Program Impacts on Child Behavior



Source: Adapted from Zaslow, McGroder, & Moore (2000; Figure SR-1)

Figure 2. The Timing of NEWWS LFA Program Impacts on Number of Job Losses, by Site



Notes: +p<.10; \*\*p<.01