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Getting a Job is Only Half the Battle: Maternal Job Loss and Child Classroom Behavior in Low-Income Families

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Abstract

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 of promoting job stability for workers and their children.

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, 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). The recent recessionary economy has only exacerbated these patterns of employment instability.

The services that most job seekers encounter at welfare offices or Workforce Investment Act centers 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 at increasing short-term employment and earnings of experimental welfare programs that emphasized job search assistance over training or education (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.

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The implications of job instability 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 circumstances and family processes. Job loss and related economic hardships are associated with increased behavior problems, decreased emotional well-being, and reduced educational attainment among low-income teenagers (Conger et al., 1994; Kalil & Ziol-Guest, 2005, 2008; McLoyd et al., 1994; Randolph et al., 2004). The few studies of job loss and younger children's behavior find effects ranging from neutral (Chase-Lansdale et al., 2003) to moderately large and adverse (Johnson, Kalil, & Dunifon, 2010). These studies offer important insights into the influence of job loss on children's socio-emotional development, but even with careful statistical controls, these effects may be 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 child behavior in school. We use exogenous variation in maternal employment patterns produced by an experimental welfare-to-work program to reduce sources of bias present in other studies. The combination of experimental data and instrumental variables (IV) estimation has been used in several prior studies (Duncan, Morris, & Rodrigues, in press; Gennetian et al., 2010; Gennetian, Magnuson, & Morris, 2008; Kling, Liebman, & 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). 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 offers 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 between 65 and 80 percent (Brauner & Loprest, 1999)—but short average job tenures (Loprest, 1999; Wood, Moore, & Rangarajan, 2008). These patterns highlight the success of programs focused on promoting job entry among low-income single mothers, but also the vulnerability of this group to job loss.

Parental job losses have the potential to affect child development primarily through changes in family income, child care arrangements, and parents' psychological well-being. If family income decreases or becomes less predictable due to a maternal job interruption or unstable employment, families may curb spending on everything from food and housing to books and extracurricular activities. In addition, job loss is associated with volatility in 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 emotional security than those in stable care (Morrissey, 2009; National Institute of Child Health and Human Development [NICHD], 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 et al., 1992, 1993, 1994; McLoyd et al., 1994; Mistry et al., 2002; Parke et al., 2004). Families

headed by low-income single mothers are particularlyvulnerable to the negative consequences of job loss because they lack the resources to buffer against economic crises.

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 & Ziol-Guest, 2005; Randolph et al., 2004; Stevens & Schaller, 2009), but there is less evidence about the effects on young children's behavior. Chase-Lansdale et al. (2003) found that maternal transitions out of employment were associated with increased internalizing and externalizing behavior problems among adolescents, but not preschoolers. Other studies have linked involuntary job loss to higher levels of behavior problems among low-income children in early to middle childhood (Johnson, Kalil, & Dunifon, 2010; Kalil, Dunifon, & Danziger, 2001). Johnson and colleagues estimate that each additional year of maternal involuntary job loss is associated with a 0.15 standard deviation increase in low-income children's problem behavior. As the authors point out, the cumulative negative effects of many years of unstable employment are potentially quite large.

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, this 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 fore-stalled 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 classroom teachers. Unlike self-reported employment histories, UI records have the advantage of being free from recall bias and may provide more complete data relative to self-reports in surveys. Similarly, teacher reports of child behavior are less likely than parent reports to be biased by the effects of changes in the family environment on parenting stress and depression (which may change *perceptions* of behavior along with actual behavior). Teacher reports also capture children's behavior at school, a critical context for long-term success, and provide insight into the interactions between children's home and school environments.

The effects of parental job loss on children take 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 aim to provide insight into the potential intergenerational effects of work-first programs.

THE EXPERIMENT

This study relies on data from the National Evaluation of Welfare-to-Work Studies (NEWWS) to estimate the effects of sustained maternal job loss on children. NEWWS was conducted between 1991 and 1999 by MDRC, and it evaluated seven Job Opportunities and

Basic Skills Training programs across the country, alldesigned to increase employment and reduce dependence on government assistance among welfare recipients. As part of the NEWWS Child Outcomes Study, three sites—Atlanta, Georgia; Grand Rapids, Michigan; and Riverside, California— collected more extensive information about the well-being of preschoolers five years after random assignment. 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 with Dependent Children program, at that time).[1]

Germane to 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. Among its contemporary welfare experiments, NEWWS LFA was unique in its emphasis on mandating employment and assisting with the job search process without offering complementary income or work supports. The principal goal of the program 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 nonparental child care use, were not directly targeted by the program components but were seen as potential "derivatives of the targeted outcomes" (Zaslow, McGroder, & Moore, 2000, Figure SR-1). Similarly, parent mental health and parenting were not direct targets of the intervention, but might have been influenced by program-induced changes in family economic circumstances. In other words, any effects that the program ultimately had on children were expected to operate directly or indirectly through the effects of changes in maternal employment on the family. This relatively straightforward causal model of program impacts on families and children (shown in Figure 1) facilitates our use ofrandom assignment to the program as an instrumental variable for estimating the effects of employment processes among single parents.

Impact analyses of the NEWWS programs indicate that the program operated as intended: 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 et al., 2000). These effects varied somewhat by site: 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 (Hamilton et al., 2001). Surveys of frontline and administrative staff indicate that greater emphasis on quick job entry, more personalized attention, and smaller caseload sizes were 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 (Morris, 2008).

While the central focus of NEWWS was to increase maternal employment and reduce welfare receipt, we will show that it had some unintended consequences on job loss, as well. By encouraging employment among mothers with little prior labor force experience, NEWWS appears to have increased employment "churning" among treatment group

[1]In two of the sites, Atlanta and Grand Rapids, some control group members became eligible to receive "welfare-to-work" services three years into the evaluation of NEWWS. Subsequent analysis of administrative and survey data indicates, however, that there were low rates of cash assistance and welfare-to-work service receipt among control group members (Hamilton et al., 2001).

mothers, particularly in one site (Riverside). This variation may also relate to the approach of the welfare caseworkers implementing the mandatory requirements and the local economic conditions at each site.

ESTIMATION STRATEGY

We combine experimental data with a common econometric technique for addressing measurement and specification errors, the instrumental variables (IV) estimator. Strong and valid instruments, variables that are correlated with problematic endogenous variables (in this case employment and job loss) but uncorrelated with unobserved heterogeneity, produce less biased estimates than ordinary least squares (OLS) estimates. The pairing of IV estimation with experimental data is also emerging as an approach to modeling mediation in the context of social programs (Gennetian et al., 2010; Kling, Liebman, & Katz, 2007; Ludwig & Kling, 2007).

We estimate two-stage least squares models with treatment-by-site interactions as instruments. The first-stage models are:

$$J_i=\beta_1+\sum\gamma_1(S_i \quad * \quad T_i)+\sum\gamma_1 X_i+\sum\delta_1 S_1+\varepsilon_i \tag{1}$$

$$E_i=\beta_2+\sum\gamma_2(S_i \quad * \quad T_i)+\sum\lambda_2 X_i+\sum\delta_2 S_1+\mu_i \tag{2}$$

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 the 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):

$$C_i=\beta_3+\alpha_1\widehat{J_i}+\alpha_2\widehat{E_i}+\sum\lambda_3 X_i+\sum\delta_3 S_i+\nu_i \tag{3}$$

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 and S_i, site dummies controlling for time-invariant demographic and economic differences between the sites. Unobserved heterogeneity is captured in ϵ_i, μ_i, and $[H9263]_i$. Our interest is in estimating α_1, the average marginal treatment effect of job loss on children's behavior, controlling for employment participation.

This approach leverages differences in exogenous treatment effects on maternal employment and job loss across the three sites. In a second set of IV models, we focus specifically on differences in NEWWS LFA program *implementation* at the three sites by drawing on data from staff surveys. In these models, the site-treatment interactions—$S_i * T_i$ in Equations (1) and (2)—are replaced with interactions between assignment to the treatment group and the average score on two measures of site-level implementation: emphasis on quick job entry and caseload size. 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. 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.

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 validity of our instruments rests on two primary assumptions: Assignment to the treatment is uncorrelated with any unobserved family and child characteristics (independence assumption), and maternal employment patterns are the only pathway of treatment effects on child behavior (exclusion restriction; Angrist & Pischke, 2009). Randomization to the LFA program provides a strong basis for assuming independence of our instruments. The identification strategy uses differences in program impacts across sites, which may result from variation in local economic contexts, demographic characteristics of the participants, or the implementation of the program. The reasons why site differences emerge does not threaten the independence assumption, however, because the estimates rely on comparisons between randomly assigned treatment and control group participants *within* a single site.

The strongest justification for the exclusion restriction—assuming maternal employment processes (including job loss) are the only pathways by which NEWWS LFA affected child behavior—is the LFA program's singular focus on increasing maternal labor force participation. Unlike other welfare-to-work programs in the 1990s, NEWWS LFA did not supplement income, subsidize child care or health insurance, or offer educational opportunities; maternal employment processes are the primary plausible pathways of NEWWS LFA program impacts on children (Hamilton et al., 2001). Similar to Ludwig & Kling (2007), we use site-by-treatment interactions to instrument for multiple pathways simultaneously, in this case both level of employment and job loss. Within the constraints of the three available instruments, we also conduct sensitivity analyses using alternative measures of employment and welfare receipt.

Program implementation and impacts varied by site, but our pooled models assume that the effects of job loss on child behavior are constant across sites. This is an untestable, yet critical, assumption of our estimation strategy (and of similar studies by Gennetian et al., 2010, and Kling, Liebman, & Katz, 2007). It is reasonable when one considers the broad similarities between participants at all three sites: Most were single mothers with low education and income who had lengthy histories of Aid to Families with Dependent Children (AFDC) receipt prior to entering the program. The children at the three sites were all in the same age range, a critical factor in predicting how job loss might affect behavior at school. Our assumption of homogeneous effects of job loss on child behavior is also supported by the similarity of estimates from site-specific models of program impacts and pooled IV first-stage models (shown later).

The conventional interpretation of IV estimates as local average treatment effects requires a monotonic relationship between the instrument and the endogenous variable of interest (Angrist & Imbens, 1995; Angrist, Imbens, & Rubin, 1996). This assumption is unlikely to hold in this study, where, as we show later, the NEWWS LFA program increased job loss at one site and decreased it at another. In the words of the monotonicity assumption, we have both compliers (individuals who increased job loss in response to the treatment) and defiers (individuals who decreased job loss in response to the treatment). The implication is that our

estimates may not be a simple average of the individual causal effects of maternal job loss on child behavior. Our estimates could be biased if either the proportion of defiers is large or the treatment effect of job loss on child behavior differs substantially for compliers and defiers (Angrist & Pischke, 2009). Our relatively weak instruments make the estimates more vulnerable to violations of this assumption. Notably, however, we think it is plausible that the effect of job loss on children might not differ in programs that increased or decreased job loss—that is, an increase in job loss undermines children's behavior to the same extent that a decrease in job loss benefits children's behavior. Under this assumption, our estimates would not be biased by the violation of monotonicity.

SAMPLE AND MEASURES

The NEWWS Child Outcomes Study included 3,018 children across three sites who were between the ages of 3 and 5 years at the time of random assignment. We narrowed that sample to the 970 LFA treatment or control group children with a teacher report of problem 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.[2] 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 measures described as follows.

Sustained Job Loss

The explanatory variable of interest is the number of quarterly transitions out of employment during the five-year follow-up period based on state administrativedata on quarterly earnings from the unemployment insurance (UI) program.[3] 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 a quarter and zero earnings in the subsequent quarter. The two measures used in the analysis are: *total job losses during the follow-up period* (range = 0-6; mean = 1.33; *SD* = 1.27) and a dichotomous variable for having had *any job losses in the follow-up period* (mean = 0.68). We also control for the mother's level of employment using the *total number of quarters with positive earnings* during the follow-up period (range = 0-20; mean = 9.08; *SD* = 6.67).

What we define as sustained job loss is comparable to what prior studies have defined as job-to-nonemployment transitions or job instability (Johnson & Corcoran, 2003; Johnson, Kalil, & Dunifon, 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 (Johnson, Kalil, & Dunifon, 2010).

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

[3]The NEWWS study collected employment data through surveys and administrative records. Our extensive comparisons of these two sources, which have documented trade-offs for the purposes of studying employment (Kornfield & Bloom, 1999; Wallace & Havemen, 2007), suggested that the administrative records provided more complete and reliable data on job loss.

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). Children were 8 to 10 years of age at the time of assessment. The total problem behavior measure used in this study (range = 0-2.83; mean = 0.83; *SD* = 0.69) summed percentage scores on three BPI subscales: internalizing behaviors, such as being sad or anxious; externalizing behaviors, such as fighting; and hyperactivity, including disrupting others and acting without thinking. The PCBS/SCS evaluates child cooperation, positive assertion, self-control, and interpersonal skills by summing the scores on four subscales (range = 5-84; mean = 50.87; *SD* = 19.73). To ease interpretation of regression coefficients, we transformed the child behavior measures into standard deviation units by dividing the difference between each child's score and the sample mean by the standard deviation of the control group.

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. Administrators and line staff at each of the six offices were surveyed two years after random assignment about their approach to serving clients. Survey completion rates were 90 percent or higher, for a total of 23 case managers in Grand Rapids, 27 in Atlanta, and 71 in Riverside (Hamilton et al., 1997). We selected two measures collected in the survey that are conceptually linked to the process of finding and keeping employment: caseworker and program emphasis on *quick job entry* (as opposed to education, training, and waiting for "better" jobs) and *caseload size*. The staff survey responses in a given office were averaged, creating two site-level measures of program implementation. The quick job entry scores were -0.57, -0.08, 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.[4]

Demographic Characteristics

Our models include the following baseline control variables collected on an information form completed by the participants prior to random assignment and through interviews with program staff: family earnings in the year prior to random assignment (in $1,000 units); family earnings squared; mother employed in the year before random assignment (0/1); mother has a high school diploma (0/1); mother less than 18 years old when focal child was born (0/1); mother's marital status (never married, married, separated); child gender (0/1); number of children in the family; age of youngest child in the family; and mother's race (black, white, Latino, other). Because the timing of random assignment and the fielding of the five-year surveys varied, we also control for follow-up length in months.

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 groupin one of three NEWWS LFA sites. This identification strategy warrants careful attention to the site-

[4]For more information on the NEWWS staff surveys, see Bloom, Hill, and Riccio (2003).

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 shown in Figure 1 and highlights variation in impacts by site that would explain differential effects of the program on child behavior. Table 2 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 2 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 impacts on job loss 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 9 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—by an increment of one-half of a job loss 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 $p < 0.01$ level.

The patterns of the coefficients shown under "Employment" in Table 2 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 for control group members, although the difference did not reach statistical significance in Riverside. While there is variation in the magnitude and statistical significance of impacts on these measures across sites, none of the differences are statistically significant.

Table 2 also examines program impacts on what we refer to as intended derivatives of maternal employment, including quarterly welfare receipt, income, and nonparental child care type, and non-targeted outcomes, including 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 $p < 0.05$ 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 Regression Results

We present the results of OLS models and two versions of the IV models—the first using site-by-treatment interactions as instruments and the second using implementation feature-

by-treatment interactions as instruments. The first stage IV results are shown in Appendix Table 2.[5] The first column in Table 3 presents the OLS estimates of the effect of sustained maternal job loss on children's behavior. According to theseestimates, 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 ($p < 0.05$) of a standard deviation higher and a positive behavior score that is, on average, 23 percent ($p < 0.01$) 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 (0.07 standard deviations; $p < 0.05$) but statistically significant decrease in children's positive behavior.

The second and third columns of Table 3 show the second-stage estimates of IV models using a set of interactions between the three NEWWS 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; a child whose parent experienced at least one job loss after random assignment is expected to have a problem behavior score that is over 1 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 in the IV models, the coefficients on job loss predicting problem behavior are all statistically significant at the $p < 0.05$ 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.

Table 3 also 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*-statistics 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. Finally, while not shown, models using alternative definitions of job loss (e.g., earnings decreases of 75 percent or more), produced nearly identical estimates (results are available from the authors).

We also tested models in which child behavior was transformed into several dichotomous measures indicating different thresholds of child behavior to understand where job loss was having its effects in the distribution of child behavior. Estimates from IV probit models (not shown here) are positive and statistically significant, but suggest the effects are at moderate and not high levels of our outcome variable. 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 75th 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

[5]Note that the first-stage IV results (Appendix Table 2) using site-by-treatment interactions (model 1) are very similar to the experimental impacts presented in Table 2, supporting the assumption of the IV models that the effects of baseline covariates on job loss were homogeneous across sites. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

accurately described here as an increase in the probability that a child will exhibit more than the median level of problem behavior.[6]

Supplemental Analysis

We conducted three additional analyses to better understand the identification of the estimate of large adverse effects on child problem behavior associated withmaternal job loss. First, we recognize that job loss is only one dimension of a worker's overall employment pattern, and it 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. We defined three main categories of employment levels during follow-up: continuously unemployed, low employment, and high employment. High employment is defined as being employed in nine (the sample median) or more quarters during follow-up. Low employment is less than nine quarters. Notably, because both job loss and employment are post-baseline variables, they both represent impacts of the program. Table 4 provides joint impacts of being employed at a high level, for example, *and* experiencing one or more job losses based on site-specific OLS regression models.[7] Based on these results, Riverside NEWWS appears to have increased the probability of high employment paired with job loss. By contrast, we observe no higher probability of working less than nine quarters and experiencing a job loss among treatment than control group members in Riverside. In addition, the increase in job loss that we observe in Riverside is concentrated in multiple (two or more) and not single experiences with job losses. In short, the effects of job loss that we are leveraging in these models are probably among those with high levels of posttreatment employment and job loss, or "churning."[8]

Next, 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 three to six months across sites (Hamilton et al., 1997). 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 of follow-up. 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. Unfortunately, data were only collected from teachers during the survey conducted five years after random assignment. This limited analysis suggests that program impacts on job loss are weaker but still present in the later years of the follow-up, more contemporaneous to the observed effects on children.

[6]It is worth noting that the median problem behavior in this sample of low-income children is higher than for national samples (Hamilton, 2002).

[7]These are mutually exclusive categories; the predicted probabilities are coefficients on a dichotomous indicator of treatment status in 21 individual OLS regression models, with the same set of baseline controls used in our main models.

[8]Note that the employment patterns of the control group in Riverside differed from other sites. River-side control group members were more likely to be continuously unemployed and less likely to have high employment with multiple job losses. Controlling for site and examining program and control *differences* helps to mitigate some of the concerns that might be raised about differences between sites.

Finally, although this study is not designed to examine the mechanisms of the effects of maternal job loss on child behavior at school, we conducted some exploratory analysis (not shown here) to understand potentially mediating family and contextual changes. We were constrained by the limited number of potential mediators measured in the original NEWWS study and by our reluctance to add additional endogenous measures to our 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. In this vein, we examined two aspects of children's microsystemsfor which we have data and which the literature would suggest might be mediating mechanisms of job loss: maternal depression and type of child care. In IV models using job loss in the first two years as a predictor of these two outcomes at the five-year follow-up, job loss experienced early in the study period was statistically significantly associated with higher levels of depressive symptoms at the five-year follow-up, as reported by mothers, but not to the type of child care used.

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 be uncorrelated with preexisting characteristics of participants or sites and must not have directly engendered changes in family life other than the level and stability of maternal work. These are strong assumptions, but ones that are more likely given 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 2, 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 levelof employment and had the strongest relationship with random assignment relative to any employment during the follow-up and the maximum length of an employment spell. In sensitivity tests shown in Table 5, we estimated the IV 2b model, but sequentially replaced total quarters employed with measures of any employment (IV 3a), longest spell of employment (IV 3b), and average quarterly welfare receipt (IV 3c), the only other outcome with statistically significant program impacts that varied by site. The IV coefficient of 0.70 in our main models ranged from 0.59 to 0.93 ($p < 0.10$) when we substituted any employment, longest spell of employment, and welfare receipt (individually) for number of quarters employed in models predicting child problem behavior ($p < 0.10$). The first-stage *F*-statistics for the two alternative employment measures were 3.32 and 7.30, respectively, and 3.18 for welfare receipt, relative to 10.13 for total quarters employed. 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 somewhat at these sites as well (see Appendix Table 1[9]). Our models are strengthened if implementation differences are a function of factors outside of the clients' control,such as the approach of the manager of the welfare office, rather than client characteristics. 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. 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 in place of dichotomous variables 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, Jaeger, & Baker, 1995; Nelson & Startz, 1990; Staiger & Stock, 1997). Weak instruments generally bias IV estimates toward the OLS estimate (Bound, Jaeger, & Baker, 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 IV models 1a and 1b presented in Table 3 perform poorly on Stock–Yogo (2005) tests.[10] 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. 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 hypothesis that the instrument is weak (Staiger & Stock, 1997). 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 (but cannot eliminate) the concern that the significant relationship between maternal job loss and children's problem behavior is the result of bias in the standard errors.

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

[10]Stock-Yogo (2005) critical values use the first-stage *F*-statistics to test two null hypotheses related to instrument strength. 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 hypothesis if the true significance level was 5 percent.

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, we find decidedly adverse consequences of job loss on child problem behavior but no statistically significant effects on child positive behavior. Unlike other studies in which parental or child characteristicsmay account for both job loss and children's problem behavior, these analyses make use of an experiment 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 children's 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). Extended periods of unemployment, which we aimed to capture by examining quarterly unemployment, appear to be particularly detrimental. 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. Johnson, Kalil, and Dunifon (2010) estimate standard deviation-size increases in the *growth rate* of problem behavior associated with one job loss in a year, but 0.20-0.40 standard deviation effects on the *level* of behavior problems. One possible explanation for the differences in magnitude is that the IV models control more thoroughly for measurement error and other suppressing factors than did prior studies. However, the IV estimates and standard errors could also be biased by weak instruments or violations of the exclusion restriction and monotonicity assumptions. 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 remains uncertain.

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 five-year follow-up period, suggesting that the effects on children are due to job churning among those with some experience in the labor market. It is beyond the scope of this study to fully disentangle the interplay between child behavior and job losses over the follow-up period, but the fact that impacts on job loss were concentrated early in the follow-up period suggests that job losses may 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 exhibits higher than median, but not extreme, levels of problem behavior.

Several assumptions of our models warrant careful consideration and caution in generalizing the results to other populations or contexts. First, the estimates of the effects of job loss are valid for single-mother welfare recipients who responded to a labor force attachment program by becoming employed, thereby increasing their chances of job loss. Complicating matters, our estimates may be an average of the treatment effects among families for whom assignment to the treatment increased or decreased job loss. We think it is reasonable, however, to assume that the magnitude of the effect did not differ for these two groups (with beneficial effects on behavior for children of parents who decreased job loss and adverse

effects for children of parents who increased job loss). Second, our analysis 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. Our analysis of site-level differences indicates that implementation features are not associated with measured characteristics of the welfare recipients served at those sites. Nonetheless, we cannot say conclusively that unobserved differences in participants and children across the sites might not have moderated the effects of job loss on child behavior. Finally,our results are driven by differences primarily in two locales (Riverside and Atlanta), suggesting that further replication of these findings is indeed warranted.

There are also potential limitations to our measures of job loss and children's behavior. 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. If transitions to informal work are potentially less stressful to families than transitions to nonemployment, then this limitation of our data would likely 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.

Despite these limitations, our findings suggest that children could benefit if employment programs aimed at less-educated women emphasized job *retention*. This idea has been discussed and advocated for many years (see Hershey & Pavetti, 1997), yet job instability remains high in this group and few proven program models to promote employment stability exist. 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). The ongoing Employment Retention and Advancement evaluation is testing multiple models for promoting steady employment and career progression (Bloom et al., 2002), which focus primarily on addressing deficits or barriers faced by workers. 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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APPENDIX

Table A1

Selected baseline and demographic characteristics by site and research group.

	Atlanta		Grand Rapids		Riverside	
Baseline Characteristic	**Control**	**LFA**	**Control**	**LFA**	**Control**	**LFA**
Parent						

	Atlanta		Grand Rapids		Riverside	
Baseline Characteristic	**Control**	**LFA**	**Control**	**LFA**	**Control**	**LFA**
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 diploma 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†
Child and family						
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

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

† $p < 0.10$

* $p < 0.05$

** $p < 0.01$.

Table A2

First-stage results of IV models using site-by-treatment and implementation feature-by-treatment instruments for sustained job loss.

Instruments	Number of Sustained Job Losses	Any Sustained Job Loss	Number of Quarters Employed
Model 1: Program sites			
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)

Instruments	Number of Sustained Job Losses	Any Sustained Job Loss	Number of Quarters Employed
Riverside X treatment status	0.48**	0.18**	2.07**
	(0.15)	(0.06)	(0.72)
Model 2: Site-level implementation features			
Emphasis on quick job entry X treatment status	0.21**	0.08**	0.20
	(0.06)	(0.02)	(0.31)
Caseload size X Treatment status	0.00	0.00	0.02**
	(0.00)	(0.00)	(0.00)

Note: Standard errors shown in parentheses. The figures included in this table are from an IV models predicting problem behavior, but are consistent with figures from the model of positive behavior. Coefficients for "Number of quarters employed" come from the models using the continuous measure of job loss, but are consistent with the results from the models 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 diploma, 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.

†$p < 0.10$
*$p < 0.05$
**$p < 0.01$.

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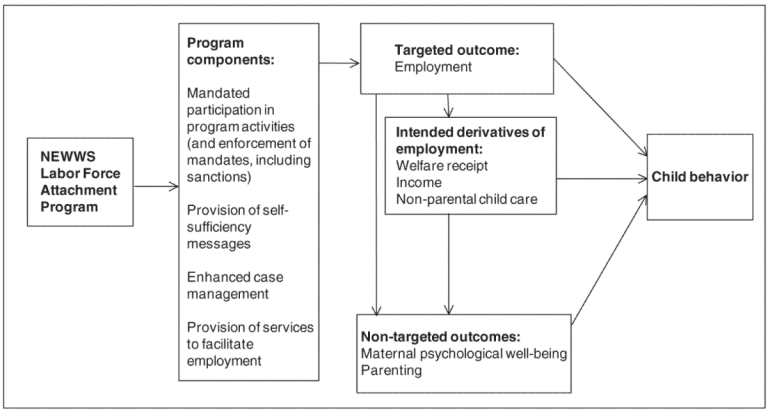

Figure 1.
Causal Model of NEWWS LFA Program Impacts on Child Behavior. *Source*: Adapted from Zaslow, McGroder, and Moore (2000, Figure SR-1).

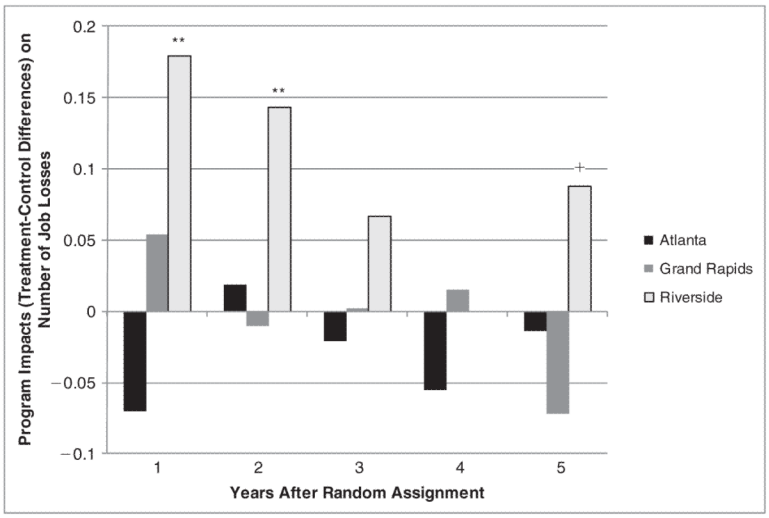

Figure 2.
The Timing of NEWWS LFA Program Impacts on Number of Job Losses, by Site.

Table 1

Selected baseline characteristics of *NEWWS LFA* participants, by research group.

Baseline Characteristic	Control Group	LFA Program Group
Parent		
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	41.44	34.89*
Married	1.66	2.81
Received high school diploma 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
2 or more years of AFDC receipt	72.19	74.94
Child and family		
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

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

† $p < 0.10$

* $p < 0.05$.

Table 2

NEWWS LFA program impacts on targeted and non-targeted outcomes, by site.

	Atlanta	Grand Rapids	Riverside	Significance of Cross-Site Differences[a]
Employment				
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)	**
Intended derivatives of employment				
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)	
Nonparental 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)	
Non-targeted outcomes				
Maternal depression	0.17 (0.67)	−0.50 (0.92)	1.92* (0.82)	
Maternal parenting aggravation	− 0.17 (0.24)	0.14 (0.27)	0.39 (0.27)	
Child behavior				
Problem behavior	− 0.27* (0.12)	−0.10 (0.11)	0.23* (0.10)	**
Positive 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 were 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 diploma, 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.

[a] 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 3

Results of OLS and IV models predicting child classroom behavior.

	Child Problem Behavior					Child Positive Behavior				
	OLS	**IV la**	**IV lb**	**IV 2a**	**IV 2b**	**OLS**	**IV la**	**IV lb**	**IV 2a**	**IV 2b**
Number of sustained job losses	0.02 (0.03)	0.55* (0.26)	0.71* (0.33)	0.50 (0.26)	0.70* (0.33)	−0.07* (0.03)	−0.16 (0.23)	−0.20 (0.26)	0.11 0.23)	−0.19 (0.26)
1st stage F-statistic		4.00**	4.15**	12.16**	6.12**		4.00**	4.00**	11.60**	5.90**
Number of quarters employed			−0.06 (0.05)		−0.06 (0.05)			0.01 (0.04)		0.03 (0.04)
1st stage F-statistic			7.10**		10.13**			7.75**		10.91**
Any sustained job losses	0.17* (0.07)	1.36* (0.61)	1.81* (0.78)	1.31* (0.65)	1.83* (0.83)	−0.23** (0.07)	−0.65 (0.57)	−0.78 (0.64)	−0.30 (0.61)	−0.47 (0.66)
1st stage F-statistic		5.06**	5.06**	12.78**	6.43**		4.71**	4.71**	11.79**	5.92**
Number of quarters employed			−0.06 (0.05)		−0.06 (0.05)			0.02 (0.04)		0.02 (0.04)
1st stage F-statistic			7.10**		10.13**			7.75**		10.91**
Instruments (#):										
Sites-by-treatment status (3)		X	X				X	X		
Implementation features-by-treatment status (2)				X	X				X	X

Note: Standard errors shown in parentheses; IV Models la and lb use site-by-treatment status interactions as instruments for number of sustained job losses and number of quarters employed. IV Models 2a and 2b use two implementation features, emphasis on quick job entry and caseload size, interacted with treatment status as instruments for the same endogenous variables. 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 diploma, 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 4

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)					
			No Job Loss		1 Job Loss		2 + Job Losses		No Job Loss		1 Job Loss		2 + Job Losses	
	Control Group (CG)	Program Impact (PI)	CG	PI	CG	PI	CG	PI	CG	PI	CG	PI	CG	PI
Atlanta	0.09	−0.009 (0.031)	0.14	0.075 † (0.040)	0.17	−0.016 (0.039)	0.20	0.016 (0.042)	0.03	0.026 (0.022)	0.17	−0.053 (0.037)	0.19	−0.04 (0.041)
Grand Rapids	0.09	−0.065 * (0.028)	0.07	0.088 * (0.038)	0.15	0.001 (0.041)	0.29	0.043 (0.056)	0.07	−0.054 * (0.025)	0.09	−0.010 (0.035)	0.25	0.00 (0.052)
Riverside	0.31	−0.087 (0.053)	0.12	−0.007 (0.038)	0.11	0.004 (0.038)	0.05	0.176 ** (0.036)	0.07	−0.046 (0.028)	0.20	−0.040 (0.049)	0.14	0.00 (0.042)

Note: Estimates are the coefficients on a dichotomous variable indicating treatment status in 21 separate OLS regression models predicting the probability of experiencing each of the seven employment/job loss pattern in each of the three sites. Standard errors shown in parentheses. High versus low employment is defined by median quarters worked. 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 diploma, 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.

†$p < 0.10$

*$p < 0.05$

**$p < 0.01$.

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Table 5

Results of IV models predicting child problem behavior using implementation feature-by-treatment instruments for sustained job loss and alternative pathways.

	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)
F-statistic (first stage)	6.12**	6.12**	6.12**
Any employment	−2.66 (2.58)		
F-statistic (first stage)	3.32†		
Longest spell of employment (quarters)		−0.07 (0.06)	
F-statistic (first stage)		7.30**	
Average quarterly welfare receipt ($1000)			2.22 (2.11)
F-statistic (first stage)			3.18*
Instruments (#):			
Implementation features-by-treatment status (2)	X	X	X

Note: Standard errors shown in parentheses. *F*-statistics and partial R^2s are for the instruments in the first stage. IV models 3a–c include two endogenous variables (number of sustained job losses controlling for any employment, longest spell of employment, or average quarterly welfare receipt) and two 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 diploma, 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.

† $p < 0.10$

* $p < 0.05$

** $p < 0.01$.