Can Post-Employment Services Combined with Financial Incentives Improve Employment Retention for Welfare Recipients? Evidence from the Texas Employment Retention and Advancement Evaluation

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Abstract

Data from a recently-completed experimental program for out-of-work welfare recipients in Texas are used to examine the effects of a time-limited financial incentive coupled with post-employment services on recipients' rates of entering and leaving employment. While there is strong evidence that such programs can increase overall employment, the crucial question of how these increases arise is not well-understood. This paper presents a rigorous analysis of employment entry and exit effects, using a fully-specified dynamic model of employment duration that accounts for non-random sorting into employment statuses through flexible specifications for duration dependence and unobserved heterogeneity. The results indicate that for the Corpus Christi site, short-term effects were due to both employment retention and employment entry but, over time (as the program ceased operation), the retention effects faded out but the employment entry effects persisted and grew. For the Fort Worth site, there were smaller effects overall and less evidence of impacts that lasted much beyond the program operation period.

1. Introduction

In recent years, many social programs have sought to encourage out-of-work welfare recipients to seek and retain employment through the use of earnings supplements conditioned on work. Typically, such programs pay workers a financial incentive for each month of employment. Some programs condition the financial incentives on full time employment. Others combine financial incentives with enhanced pre- and post-employment services intended to help recipients obtain and retain jobs.

This paper uses data from a recently-completed experimental program for out-of-work welfare recipients in Texas (the Texas Employment Retention and Advancement program - Texas ERA) to examine the effects of a time-limited financial incentive (available for up to 12 months) coupled with post-employment services on recipients' rates of entering and leaving employment. While there is strong evidence that such programs can increase overall employment, the crucial question of how these increases arise is not well-understood even in the context of well-designed random assignment studies. If the employment effects are due solely to more people *entering* work, this would indicate that these programs did not achieve their aim of improving employment retention, and the longer-term benefits that might flow from remaining employed longer – increased employment stability, skill acquisition, earnings growth, career advancement, etc. – are unlikely to be realized. If, on the other hand, the increases are largely attributable to more people *remaining* employed (e.g., greater employment retention), this would point to the effectiveness of the programs in supporting individuals in the early months of new employment when the risk of job loss is highest and would suggest that such policies might have the potential to break the oft-cited 'low-pay no-pay' cycle, thereby improving upward mobility in the labor market.

Many programs tested in recent years have utilized random assignment on samples of welfare recipients in various locations throughout North America and Europe. The evaluations of these programs have exploited the experimental data to obtain unbiased estimates of program effects on a variety of employment outcomes, using the full sample of treatment and control group members. Well-designed random assignment studies produce unbiased estimates of the overall effect on employment. Using experimental data to examine program effects on the rates of entering and leaving employment, however, can be problematic. This is because obtaining such effects requires separate analysis of spells of employment and non-employment and the experimental design does not guarantee that treatment-control comparisons within these subgroups provide unbiased estimates. The problem arises because randomization at the baseline (the point in time when persons are randomly assigned to the treatment and control groups) does not insure that the treatment incidence is independent of unobservable variables in subsequent employment and non-employment spells. For example, when the program is successful, it helps persons not working at baseline to become employed and these persons may have characteristics that are different from control persons who become employed for reasons other than the experimental program. Comparing employment exit rates of treatment group and control group members employed after the program has been operating for a while may suffer from a selection bias that arises because of the differential sorting of treatments and controls into the employment spell. In this paper, we attempt to overcome these difficulties by adapting the methodology presented in Eberwein, Ham and LaLonde (1997) to estimate, non-experimentally, the effects of the Texas ERA program on job-finding rates and employment retention rates during the program operation period.

Another important question concerning these programs is whether effects on employment retention can last beyond the program period. In many past evaluations of such programs, it was found that employment effects fade towards the end of the incentive period or shortly afterwards (Michalopolous, 2005; Berlin, 2000). This paper will examine whether a program combining financial incentives with pre- and post-employment services can produce effects that last beyond the program operation period.

2. Findings from Previous Programs for Welfare Recipients

Many previous experimental programs targeting out-of-work welfare recipients provided financial incentives to encourage employment (Martinson and Hamilton, 2011; Gennetian et al., 2005; Huston et al., 2003; Michalopoulos et al., 2002; Riccio et al., 2008). In some cases, the financial incentives were designed to encourage work by providing a cash reward if a job was found. Some programs also offered incentives to promote employment retention by providing earnings supplements upon the achievement of designated employment milestones, such as 90 days of continuous employment (overall or in a specific job).[2] Still other programs offered incentives to encourage full time employment, with receipt contingent upon working a certain number of hours in a given time period (Hendra et al., 2011).

Several studies have shown that provision of financial incentives can promote employment among low-wage workers. Rigorous evaluations, using random assignment experiments, of several financial incentive programs, including the Minnesota Family Investment Program (MFIP), The New Hope Project, and the Canadian Self-Sufficiency Project (SSP), are remarkably consistent in demonstrating positive effects on economic outcomes (Michalopolous & Card, 2005). This research shows that individuals offered financial incentives were more likely to work, earned more, and had more income than those in a control group. While all of these programs produced positive effects on employment during the operational period, these effects subsequently faded soon before or after the financial incentive ended.

More recently, programs targeting out-of-work welfare recipients have combined financial incentives with a variety of employment related services aimed at helping the recipients find and retain jobs. The services provided to these persons ranged from simple job-search assistance to more extensive services prior to and after jobs were found. The Employment Retention and Advancement demonstration in the United Kingdom (UK ERA), for example, provided both financial incentives and a rich assortment of pre- and post-employment services to encourage stable employment. UK ERA showed some significant and sustained increases in employment and reductions in benefit receipt in the short term. These effects persisted for one group (long-term unemployed) included in the experiment, but faded in the

[2] The intuition behind such time-limited financial incentives is that the transition from benefits into work is often difficult and can give rise to hysteresis effects whereby the risk of employment exit is particularly high in the period immediately following employment entry. By providing financial support for a fixed period of time, the intention is to help individuals complete the transition successfully and, with time, become established workers. This should increase long-term employment and earnings for former welfare recipients. Such interventions are distinct from more traditional policies in the sense that they aim explicitly to support employment retention as opposed to employment entry.

long run for the single parent welfare recipient target groups (Hendra et al., 2011). SSP Plus, an experimental program providing financial incentives and limited employment services to single-parent families on welfare in Canada found sustained effects that exceeded those from a program that provided financial incentive alone (Robins, Michalopoulos, and Foley, 2008). The Texas ERA program, which included both pre- and post-employment services, produced long-term increases in earnings in one site (Corpus Christi), but in another site (Fort Worth), the pattern of effects was more typical of a traditional incentive program in which effects faded shortly after the program period (Hendra et al., 2010).

While the ability of these interventions to increase employment has been demonstrated, precisely how the effects arose is not known. As already noted, knowing whether they were due to effects on employment entry or to effects on employment retention is important and findings in either direction potentially could provide guidance for policy makers in allocating funds to run the programs. A very small number of studies distinguish between these two effects. Card and Hyslop (2005), for example, attribute the overall effect found in the Canadian SSP evaluation primarily to faster exits from welfare, with only one-quarter due to reduced rates of welfare re-entry (i.e. employment retention). In the UK, Brewer et al. (2009) found positive and sustained effects on maintaining employment of the In-Work Credit, a financial incentive payable to single parents leaving welfare to start a job of 16 or more hours per week.

3. The Texas ERA Program

The Texas ERA (hereafter "ERA") program was designed to provide both job search assistance and post-employment services (which could include employer site visits and re-employment assistance) to individuals applying for or receiving cash assistance under the Temporary Assistance for Needy Families (TANF) program, most of whom were not working when they entered the program.[3] To encourage employment retention and advancement, the program offered a monthly supplement of $200 for TANF leavers working at least 30 hours a week after receiving an earned income disregard for four months. It

[3] TANF is the main cash assistance welfare program in the U.S., having replaced the Aid to Families With Dependent Children (AFDC) program in 1997.

also offered post-employment services provided by local workforce development boards. Both treatment and control group members received pre-employment services focused on quick job entry; the difference between the treatment and control groups was the financial incentive and the provision of post-employment services.

Random assignment began at the end of 2000; shortly before the economic recession in 2001.[4] The program operated in the cities of Corpus Christi, Fort Worth, and Houston. Due to implementation problems, particularly the lack of post-employment services for most of the program operating period, the program in Houston is excluded from the analysis in this paper. Changing economic conditions throughout the program and follow-up period resulted in sample members facing different economic conditions depending on the time at which they were randomly assigned.[5] Sample members who were randomly assigned at the beginning of the enrollment period contended with rising unemployment rates for much of the program and follow-up period; the increase in unemployment rates was particularly noteworthy for Fort Worth. In contrast, sample members who were randomly assigned towards the end of the enrollment period encountered more stable, or even declining unemployment rates. Examination of the unemployment rate trends for the two sites indicates that Fort Worth's labor market was generally more volatile during the program and follow-up period. Wage rates are generally higher in the Fort Worth area which made the financial incentive somewhat more valuable in Corpus Christi (Hendra et al., 2010).

As shown in Table 1, at the time they entered the ERA evaluation most of the sample members were young (less than 30 years of age), with fairly low educational attainment (only around half had a high school diploma), and a majority had a child under the age of two years. Most had a history of

[4] Individuals were assigned to the ERA (treatment group) or control group immediately following an eligibility or recertification interview for TANF (in the case of applicants, before they were approved for cash assistance). Only those applicants and recipients who were subsequently approved for TANF could receive ERA services. Thus, a small proportion of the sample (11 to 16 percent across the sites) was never eligible for program services since they were not approved to receive TANF. These sample members were included in the evaluation analyses and are included in the analyses reported here as well because the determination of eligibility was made after random assignment. An analysis conducted as part of the MDRC evaluation found that the inclusion of this group of ineligibles did not substantively affect the analysis (Hendra et al., 2010).

[5] Random assignment (program enrollment) was from October 2000 to December 2002.

previous cash assistance receipt of two years or less, with Corpus Christi containing more "long-term" recipients. Very few of the sample members had no previous employment, with about a quarter having worked more than 24 months in the three years prior to enrollment in the study.[6] The largest difference between the two sites is the racial/ethnic composition of the sample members; in Corpus Christi, nearly 75 percent of the sample members were Hispanic, while the majority of the Fort Worth sample members were black. Less than half of the sample in both sites worked in a UI-covered job in the quarter of random assignment though most people reported that they were not employed at the time of random assignment.[7]

The ERA program was rigorously implemented in Corpus Christi, with key features put in place relatively quickly, including a developed strategy for marketing the supplement and a strong post-employment service component featuring site visits to employers to address job-related issues and job advancement (Martinson & Hendra, 2006). Overall, Corpus Christi implemented the post-employment component of the ERA program most smoothly, with the other sites adopting some of their strategies over time. Thirty percent of all treatment group members received a supplement over the course of program operations; almost half of those who received a supplement received 11 or more supplements (the maximum number of supplements a participant could receive was 12).

The ERA program in Fort Worth struggled during the early portion of the study period, but made significant improvements over time. Initially, the program had a strong emphasis on assessment and barrier removal that sometimes delayed treatment group members' movement into employment and post-employment services. The ERA program in Fort Worth made significant improvements over time, however, including adding more structured job search services and stronger post-employment services that included regular employer site visits. Accordingly, supplement receipt rate was lower in this site, with about 20 percent of treatment group members ever receiving one. However, like Corpus Christi,

[6] The large proportion of sample members with recent employment most likely reflects the stringent nature of the TANF program in Texas, including low grant amounts and cessation of benefits even for minor infractions.

[7] Because UI data are reported quarterly, it is unclear whether any employment in that quarter was just before, at, or just after random assignment. According to self reports, very few were employed on the day of random assignment.

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As indicated earlier, the ERA program was formally evaluated in Hendra et al. (2010). These results indicated that the ERA program in Corpus Christi positively affected a number of employment measures, including the quarterly employment rate, the proportion of the sample having an employment spell of at least one year, and the length of the longest employment spell. The effects on the quarterly employment are shown in Figure 1. The program in Corpus Christi produced increases in employment in several quarters from the end of Year 2 through the end of the five-year follow-up period. Employment effects were only statistically significant in one quarter in Fort Worth, although the effects were in the same direction as in Corpus Christi in several quarters.

4. Methods and Data Sources

While suggestive, none of the effects presented in Hendra et al. (2010) directly address the question of how the program affected the length of employment spells *among those who became employed*, which is the key measure of employment retention. To address this question, we adopt a methodology similar to the one used by Eberwein, Ham, and LaLonde (1997) in their study of the Job Training Partnership Act (JTPA) experimental evaluation. Our analysis is based on data from the experimental evaluation of the ERA and estimates its separate effects on employment entry and employment retention among a TANF population. In addition, persistence of treatment effects on these duration outcomes past the program time period is also considered. This complements the analysis presented in the main ERA evaluation report (Hendra, 2010) by providing more detailed information on the mechanisms behind the overall effects on employment and earnings reported in that study.

4.1 Duration Models of Employment Entry and Exit

The research questions identified above will be examined using duration models to estimate the effect of being in the treatment group – that is, the effect of ERA – on the hazard rates of employment entry and exit. The models reported here control for individual characteristics, time period, and unobservable characteristics, and allow the baseline hazard to vary in a flexible way over the course of a spell in order to capture possible duration dependence. The effects of ERA are also allowed to change after the program formally ended.

As was indicated earlier, random assignment alone is not sufficient to ensure that the approach outlined above can provide estimates of the causal effect of ERA on retention. Random assignment creates a treatment and control groups that are statistically equivalent, so that issues of non-random selection are avoided and treatment-control contrasts have a causal interpretation for outcomes that are observed for the sample as whole, such as total months in employment for the follow-up period. However, employment retention is only observed for those who enter employment, which is not necessarily a random subgroup of the population as a whole. For example, if ERA had the effect of inducing less motivated treatment group members to enter employment, treatment-control group differences in the length of employment spells would be biased because less motivated control group members would not be in the employed sample used to estimate effects (they were not induced by the program to enter employment). Allowing for flexibility in these unobserved differences (heterogeneity) between treatment and control group members can help overcome any potential biases in the estimated effects using these non-randomly sorted samples.

The analysis presented in this paper follows the methodology pursued in other studies (Ham and Lalonde, 1996; Dolton and O'Neill, 2002; Kalwij, 2004) to address this complication. The empirical models simultaneously estimate two duration processes – the time to enter employment and the time to exit employment (referred to as employment retention). Each duration process is specified to include an unobserved heterogeneity term that can influence its hazard rate. The unobserved heterogeneity term of the employment entry hazard is allowed to be freely correlated with the unobserved heterogeneity term of the employment exit hazard and in this way controls for selection into subgroups on the basis of unobserved characteristics. Following Heckman and Singer (1984a), the unobserved heterogeneity joint distribution will be approximated by a specified number of discrete mass points. Such an approach has become dominant in the literature and is appealing as it avoids arbitrary distributional restrictions. Van den Berg (2001) discusses identification issues for this bivariate mixed proportional hazards (BMPH) model and notes that this is made more straightforward where multiple spells are observed for the same individual (as in this case).

4.2 The Econometric Model

Our basic econometric model is in the spirit of Eberwein et al. (1997). In common with the analysis in that paper, we address the initial conditions problem (Heckman, 1981) using the solution suggested by Heckman and Singer (1984b), treating interrupted spells (that is, spells ongoing at the time of randomization) separately from those 'fresh' spells beginning after randomization. As described in the next section, employment status is observed on a discrete (quarterly) basis so we write the hazard rate for individual *i* in conditional log-log form:

$$\theta_{ijk}\left(t|x_{i\tau}, \upsilon_{ijk}\right) = \text{ 1 - exp}\left(-\text{exp}\left(\gamma_{jk}(t) + x'_{i\tau}\beta_{jk} + \upsilon_{ijk}\right)\right)$$

where $j \in \{1,2\}$ distinguishes between interrupted spells and those that started after the quarter of randomization, $k \in$ {u, e} distinguishes between non-employment spells and employment spells, *t* is the duration of the spell and τ is calendar time. The specification allows duration dependence to be captured by the contribution of the baseline hazard, where $\gamma_{jk}(t)$ has a flexible piecewise constant form and the effects of other observed characteristics are captured by the term $x'_{i\tau}\beta_{jk}$. Included in $x_{i\tau}$ are a dummy variable indicating whether the individual is in the treatment group or the control group, a set of personal characteristics (educational attainment, age, race/ethnicity, number of children, and age of youngest child), calendar time trend terms, the local unemployment rate, and a dummy variable indicating whether ERA eligibility had expired. Several of these variables are interacted with each other. Unobserved heterogeneity is represented by υ_{ijk}.

Estimation proceeds through maximum likelihood. The nature of each individual's contribution to the likelihood depends on how many transitions they have experienced. Someone who was out of work at the time of randomization and who did not enter work during the follow-up period *T* quarters later will contribute the following amount:

$$L_i(\upsilon) = \prod_{t=1}^{T}\big(1-\theta_{i1u}(t|x_{i\tau},\upsilon_{i1u})\big).$$

If, instead, that initial spell had completed in *t+d* quarters, the individual's contribution would have been the product of that completed spell and any further spells. Assume for the purpose of exposition that the initial spell of non-employment ends after *d* quarters and is followed by an employment spell that is censored at the end of the observation period. The overall contribution to the likelihood for this individual would then be

$$L_i(\upsilon) = \theta_{i1u}(t+d|x_{i\tau},\upsilon_{i1u}) \prod_{t=1}^{t+d-1}\big(1-\theta_{i1u}(t|x_{i\tau},\upsilon_{i1u})\big) \prod_{t=1}^{T-(t+d)}\big(1-\theta_{i2e}(t|x_{i\tau},\upsilon_{i2e})\big).$$

The contributions of individuals with different patterns of transitions can be derived analogously. To derive the marginal likelihood, we must integrate out the unobserved heterogeneity term, υ. We approximate the distribution of unobserved heterogeneity through a non-parametric mass point approach (Heckman and Singer, 1984a). There are four possible transition types: from initial employment into a fresh non-employment spell; from initial non-employment into a fresh employment spell; from a fresh employment spell into a fresh non-employment spell and from a fresh non-employment spell into a fresh employment spell. We allow an unobserved heterogeneity term for each transition type and so introduce mass points as (4×1) vectors, υ^m, $m = 1, 2,...,M$, where M is the number of mass points (defined on the joint distribution – see, for example, Røed and Raaum, 2006). With M mass points, the unobserved heterogeneity joint distribution is represented by the $\{\upsilon^m, p^m\}$, where p^m is the probability attached to υ^m and $\sum_{m=1}^{M} p^m = 1$. Across all individuals, the likelihood becomes:

$$L = \prod_{i=1}^{N}\sum_{m=1}^{M} p^m L_i(\upsilon^m).$$

The estimation of a mixed proportional hazards (MPH) model of this type is standard in the empirical literature (for a survey, see van den Berg, 2001).

It should be noted that, although the proposed analysis is non-experimental, the experimental design of the ERA evaluation still provides an important advantage. Specifically, to do an analysis of the type proposed without experimental data would require that we model both selection into employment (as described above) and selection into ERA. Since selection into ERA is taken care of through the random assignment design, only selection into employment needs to be modeled using non-experimental techniques.

4.3 Data

The analysis uses data collected at enrollment and over a four-year follow-up period for the ERA sample members in the ERA Corpus Christi and Fort Worth sites.[8] Data on clients' characteristics, such as educational background and welfare history, were collected by welfare staff during eligibility interviews (i.e., at program enrollment). Employment is measured using data from the Texas Unemployment Insurance (UI) system, which provides individual-level, quarterly wages for sample members employed in jobs covered by the State's UI system.[9] As the wage data only indicate employment, the employment histories used in the analysis consist of two types of spells – employment spells and non-employment spells.

Table 2 shows the sample size, in terms of the number of individuals and the number of spells for each site. This table highlights the highly unstable employment dynamics of this population. While the average number of spells is around 4 in each site, over 40 percent of the sample members in both sites had five or more spells in a five year period.[10] Table 3 shows the spell lengths for fresh spells. On average, both employment and non-employment spells lasted for about 4 quarters, with very little variation across

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[8] The analysis was restricted to female sample members, dropping the few sample members who were male or whose gender was unknown.

[9] UI records only cover employment that is reported to the UI system. It is estimated that UI data cover 90 percent of all jobs (Kornfeld & Bloom, 1999). UI coverage varies among states but generally excludes most federal, railroad, and agriculture employees, family workers, domestics, and independent contractors.

[10] Since UI wage data are only available in quarterly increments, this table presents a lower bound on the actual number of spells. For example, if someone worked and stopped working in the same quarter, only the employment spell would be observable.

the sites. There was, however, a great deal of variation underneath these averages--while about one third of spells lasted for just one quarter, nearly a third lasted for 5 or more quarters.

The Kaplan-Meier survival functions for spells beginning after random assignment are shown in Figure 2. This descriptive analysis indicates that duration dependence is fairly similar for both spell types across the sites, with the slope of the function decreasing over time. Treatment-control differences are evident for Corpus Christi, with control group members exhibiting slower entries into employment and faster exits from employment. There is little difference between the survival functions for treatment and control group members in Fort Worth. However, as noted above, these descriptive results do not account for differential selection into employment and non-employment post-random assignment.

5. Results from the Duration Analysis

5.1 *Model Estimates*

Tables 4 and 5 show the estimated coefficients for the duration models estimated for the Corpus Christi and Fort Worth samples. The coeffecients are exponentiated so that they represent hazard ratios. Coeffecients larger than 1 indicate an increased likelihood of transitioning out the employment state shown in the table and coeffecients less than 1 indicate a reduced likelihood of transitioning.[11]

Within each set of model results, the first two columns show the estimated effects of the treatment and the covariates on interrupted spell (that is, the spell in progress at the time of random assignment) while the third and fourth column show the effects on the fresh spells (that is, spells that began after random assignment and after the spell in progress at random assignment concluded).[12] The specification of unobserved heterogeneity allowed for three types of sample members with different propensities to experience transitions into employment and non-employment.[13]

[11] In addition, subtracting 1 from the coefficient provides an approximation of the effect expressed in percentage terms.

[12] As discussed earlier, separate consideration of the spells in progress at random assignment is included in the model specification as more or less nuisance terms and so the discussion of the model results focuses on the estimates pertaining to the "fresh" spells.

[13] Specification tests were conducted to determine the number of unobserved heterogeneity terms included in the models. We were able to estimate models with up to $M=3$ mass points. For both, Corpus Christi and Forth Worth

The estimated mass points indicate the relative propensity to experience transitions; the distribution of the sample among the three types is reported in the section "Mass Points Sample Distribution" in Tables 4 and 5. For example, in Corpus Christi the largest group is the third group – which contains 57 percent of sample members. We can attempt a characterization of the different groups by considering the estimated mass points. We focus on fresh spells since these provide a better indication of transitions in the longer-term. This group has weaker employment outcomes relative to the other groups. For example, they are less likely to experience a transition into employment (the coefficient for the third mass point is less than 1 which indicates a decreased likelihood for transitioning out of fresh non-employment spells). The third group is also more likely to experience a transition out of employment (the coefficient for the third mass point is greater than 1 for fresh employment spells which implies a negative effect on employment retention). The second group, by contrast, has stronger employment outcomes than the other groups, arising both from more rapid exits from non-employment and slower exits from employment. The largest group in the Fort Worth sample is the second group. Like the largest group in Corpus Christi, the second group in Fort Worth is less likely to transition into employment and more likely to experience a transition out of employment (though only the latter effect is statistically significant). While the second group in Fort Worth shows the strongest tendency towards non-employment, the third group shows the strongest tendency towards employment.

For both sites and both types of spells (non-employment and employment), the coefficient estimates for the baseline hazard terms indicate strong negative duration dependence – the longer a spell lasts, the less likely a transition will occur.[14] Note that the inclusion of the heterogeneity terms allows interpretation of these effects as causal duration dependence (i.e., the estimated coefficients show that exit rates decline because of the length of the spell). In the absence of the heterogeneity terms, the baseline

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the Akaike Information Criterion (AIC) supported the inclusion of $M=3$ mass points. Gaure et al. (2007) suggest selecting the number of mass points that minimises the AIC.

[14] Note that the comparison category for the baseline hazard terms is spell quarter seven or later, so the positive coefficients for the baseline hazard terms indicate that transition probabilities are higher for earlier quarters in the spells compared to later quarters.

hazard coefficients would reflect the combined effects of both duration dependence and sorting (e.g., those with the least favorable labor market characteristics differentially sort into longer-term nonemployment) and so could only be interpreted as showing that exit rates decline with the length of the spell.

Among the covariates describing the observed personal characteristics of the sample members, education level has the most consistent effects. All else being equal, not having a high school diploma or GED decreases the probability of a transition into employment and increases the probability of a transition into non-employment (i.e., increases the duration of non-employment spells and decreases the duration of employment spells). The other personal characteristic covariates – age of youngest child, race, and age – have less consistent effects but were retained in the models as control variables.[15]

Including unobserved heterogeneity components in the models allows more precise estimation of the treatment effects. For Corpus Christi, treatment effects for fresh spells are statistically significant for transitions both into and out of employment – that is, the treatment increased both re-entry into employment and retention of employment.[16] For Fort Worth, the treatment coefficients are not significant for either type of transition. These results are consistent with the full follow-up effects reported for the ERA programs in these two sites (Hendra et al., 2010).

5.2 Distinguishing During and Post-Program Treatment Effects

Although there are no significant treatment effects on the hazard rates in Fort Worth over the full five-year follow-up period, examination of the upper panel of Figure 1 indicates a modest treatment effect on employment in the early years the program was in operation, although statistical significance is only achieved in one quarter. Because part of the five-year follow-up period occurs after the program formally ended, it might be informative to differentiate treatment effects on employment entry and exit before and

[15] Covariates representing environmental factors – seasonality, calendar time and the unemployment rate – were also included in the models as control variables.

[16] As mentioned, in order to get a feel for the magnitude of these effects, one can subtract 1 from the coeffecients. For example, the effect of the ERA program in Corpus Christi on transitioning into employment is shown as 1.143 in Table 4. Subtracting by 1 yields an estimate of .143, implying that the program led to a 14.3 percent increase in the probability of transitioning into employment.

after the program ended. In particular, as one of the primary program components directed towards increasing employment retention was the offer of a financial incentive for treatment group members who met certain employment criteria, the program's effect on employment retention may be stronger or only exist during the time period in which the financial incentive was available. Thus, we specified a second model in which we allowed for two treatment effects – one during the program treatment period and one after the program formally ended.

The estimation results for this second model are shown in Table 6. Only the treatment effects are shown in this table (results for the control variables are similar to those in Tables 4 and 5 and are not shown to conserve space). For Corpus Christi, there are statistically significant treatment effects on transitions into employment both during the program operation period and afterwards. However, the treatment effects on employment retention (i.e., transitions out of employment) are only statistically significant during the period of program operation. There are also treatment effects in Fort Worth for transitions out of employment (i.e., retention). During the program operation period, treatment group members in Fort Worth were slower to leave employment.

5.3 Using the Results to Simulate Employment Histories

To further explore the sources of the treatment effects, the results from the duration models were used to simulate employment histories for four scenarios: no treatment effects, both treatment effects (re-employment and retention), only retention treatment effects, and only employment entry treatment effects. Employment histories were generated for a random sample of 1,000 women involved in the experiment. Twenty quarters of post-randomization employment were simulated. Each simulation was generated using a random draw from the distribution of the estimated coefficients from the model that distinguishes between the operational and post-operational periods of the ERA programs. A total of 1,000 simulations were run.

Figure 3 shows the simulated effects on the quarterly employment rate (percentage of sample members employed). Table 7 shows summary measures describing the simulated employment histories for each site. For Corpus Christi, the treatment-control difference in the quarterly employment rate grew over time as the program operated (see Figure 3). In the early quarters of program operation, this difference was mostly due to retention effects. For example, in quarter 10, the overall effect on employment was 5 percentage points of which over 60 percent was due to effects on employment retention and slightly less than 40 percent was due to effects on employment entry. Because both treatment and control group members were participating in a welfare-to-work program, the main difference early on was the retention incentive and post employment services which encouraged participants to stay employed longer. Over time, as fewer individuals remained eligible for ERA, the employment entry effects of the program grew to be similar to and then to exceed the retention effects. Note that the size of the program's effects on employment entry grew steadily over time and did not reduce over time.

Overall, the simulations show significant effects on retention, but that these faded somewhat over time. Over the longer term, the most significant impact appears to have been an improved resilience; women who found themselves out of work, were now better-placed, as a result of ERA, to find a new job. Averaging over the entire follow-up period, Table 7 shows that the effects on employment in Corpus Christi were nearly evenly distributed between employment entry and retention effects. The overall effect on quarterly employment was 4.5 percentage points with 2.2 percentage points of this effect attributable to employment entry effects and the remainder attributable to retention effects.

For Fort Worth, the very small effects of the program were due to a mix of the programs retention and initial employment effects. As with Corpus Christi, the effect on employment entry grew over time to exceed the retention effect.

6. Conclusions and Implications

In recent years, a number of experimental programs in the US and elsewhere have tested the use of financial incentives together with employment retention and advancement services to encourage self-sufficiency among disadvantaged persons on welfare. These programs were the outgrowth of a long history of programs that provided financial incentives and only pre-employment services and produced moderate effects that tended to fade out either shortly before or after the program was terminated. It was thought that extending services to persons while they were employed might extend their employment and produce more lasting effects.

In order to accurately evaluate these programs, it is necessary to examine effects on employment retention as well as on entering employment. In this paper, we have used data from an experimental program in Texas (Texas ERA) to provide a rigorous analysis of employment entry and exit effects, using a fully-specified dynamic model of employment duration that accounts for non-random sorting into employment statuses through flexible specifications for duration dependence and unobserved heterogeneity. The analysis builds on the methodology used by Eberwein, Ham, and LaLonde (1997).

Our results extend the findings of Hendra et al. (2010) for the Texas ERA program. Specifically, for the Corpus Christi site, short-term effects were estimated to be due to both employment retention and employment entry but, over time (as the program ceased operation), the retention effects faded out but the employment entry effects persisted and grew. For the Fort Worth site, there were smaller effects overall and less evidence of impacts that lasted much beyond the program operation period. Difference in the overall program effects between the two sites may have been due to differences in the way the program was implemented in the two sites (Hendra et. al, 2010), differences in the characteristics of the samples or differences in the local environment.

While the effects of the ERA program are modest in size, the evidence presented here indicates that the program had some success in promoting employment retention. While it is not possible to determine to extent to which employment retention was due solely to the provision of financial incentives (versus the other post-employment supports incorporated into the program), these findings suggest that the provision of post-employment supports might promote employment retention, a key element in achieving economic self-sufficiency for cash assistance recipients. However, the dissipation of the retention effects after the program ceased operation suggests that disadvantaged workers may require stronger or longer-term supports.

The methodology utilized in this paper does not guarantee unbiased estimates of treatment effects, but does provide a unified statistical framework that probably minimizes any biases due to non-random sorting into employment statuses. In future evaluations, especially those that utilize both pre- and post- employment services as well as financial incentives, it is important to differentiate effects on employment entry from those on employment retention. The methodology presented here may prove useful to future evaluations of such programs.

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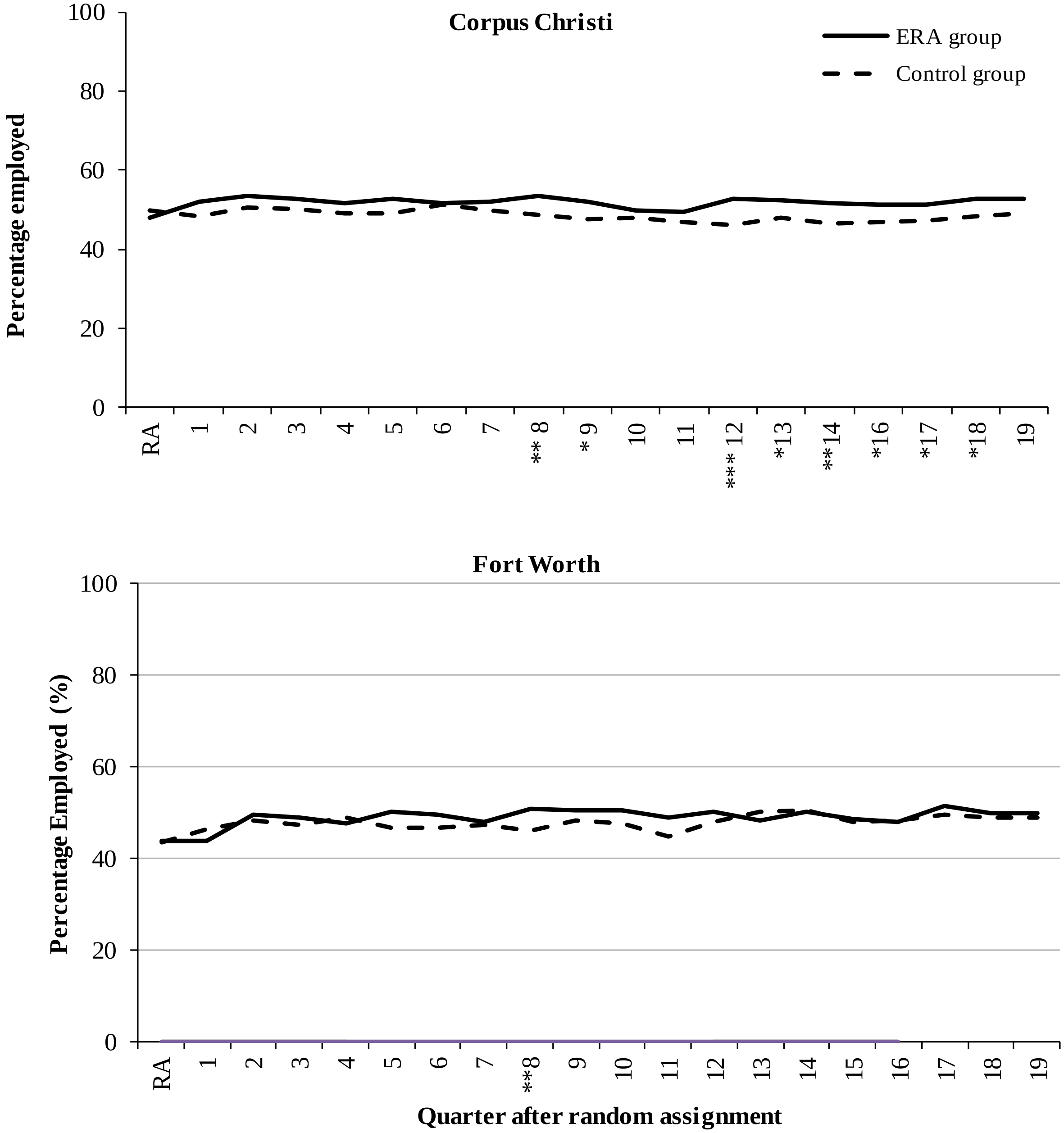
Figure 1
Impacts on Employment Over Time



SOURCE: MDRC calculations from administrative records for the State of Texas.

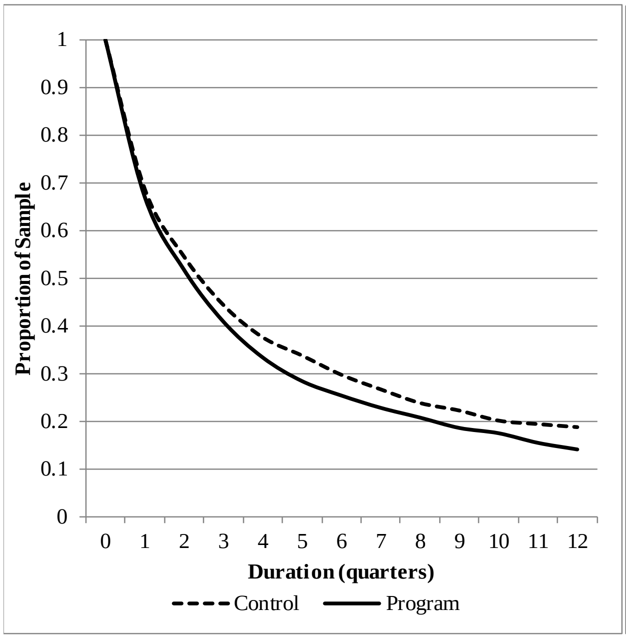
NOTES: "RA" refers to the quarter of random assignment.
Averages for dollar amounts include zero values for sample members who had no earnings.
Statistical significance levels are presented on the horizontal axis and indicated as follows: *** = 1 percent; ** = 5 percent; and * = 10 percent.

Figure 2

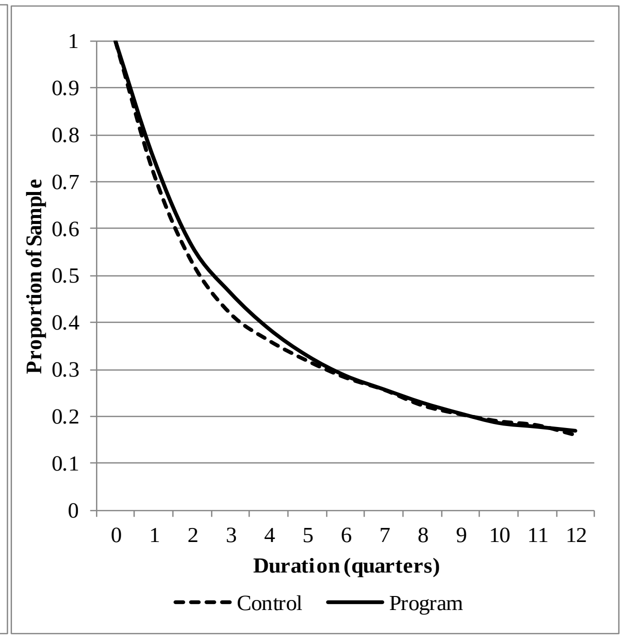
Kaplan Meier Survival Curves for Spells Starting After Random Assignment

Corpus Christi

Non-employment

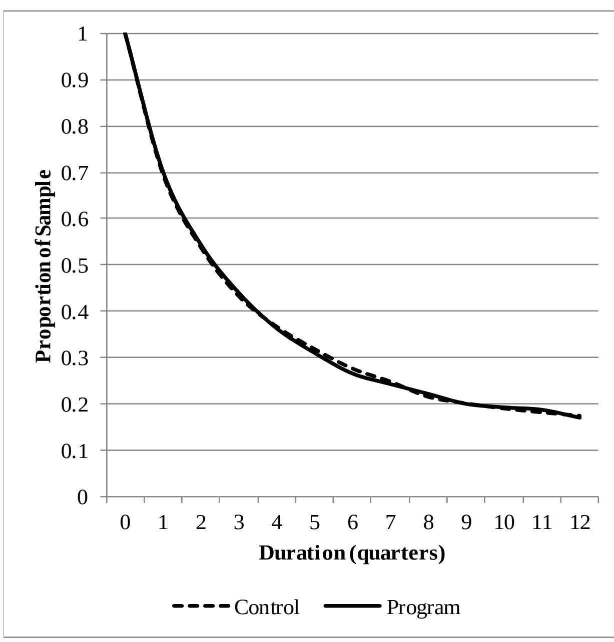


Employment

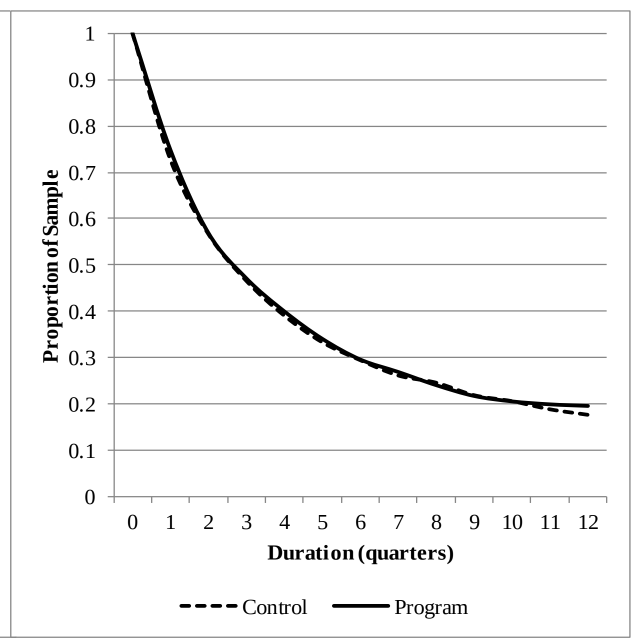


Fort Worth

Non-employment



Employment

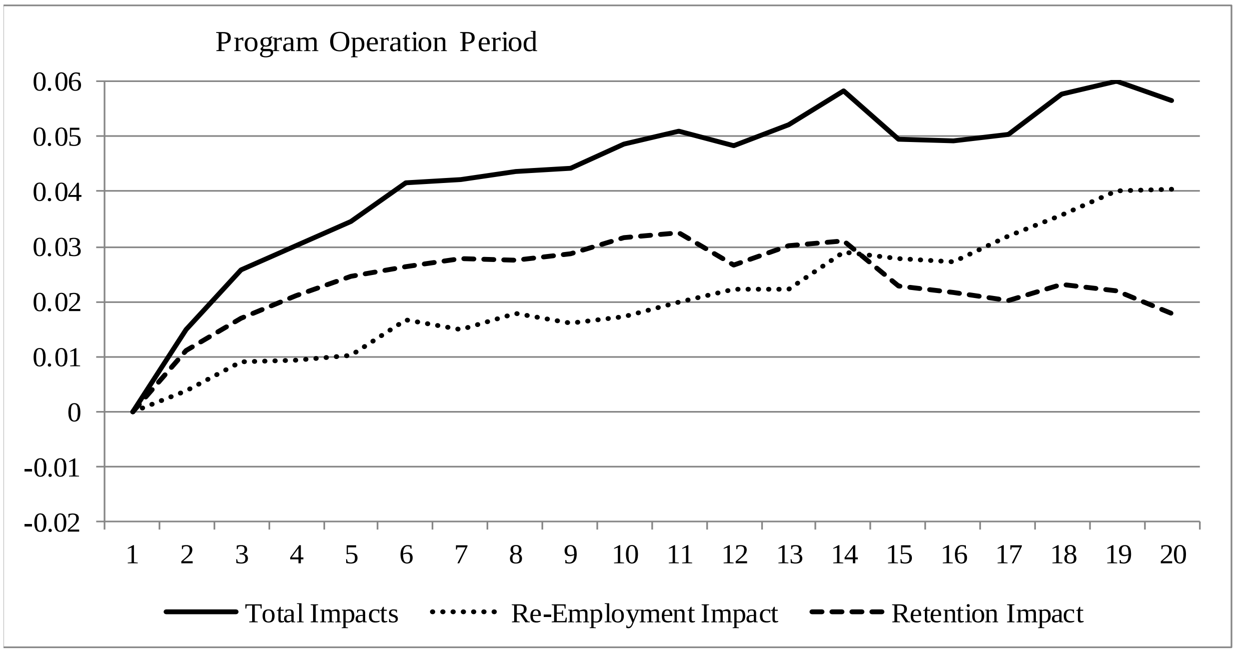


SOURCE: MDRC calculations from administrative records for the State of Texas.

Figure 3

Simulation Results: Quarterly Employment

Corpus Christi



Fort Worth

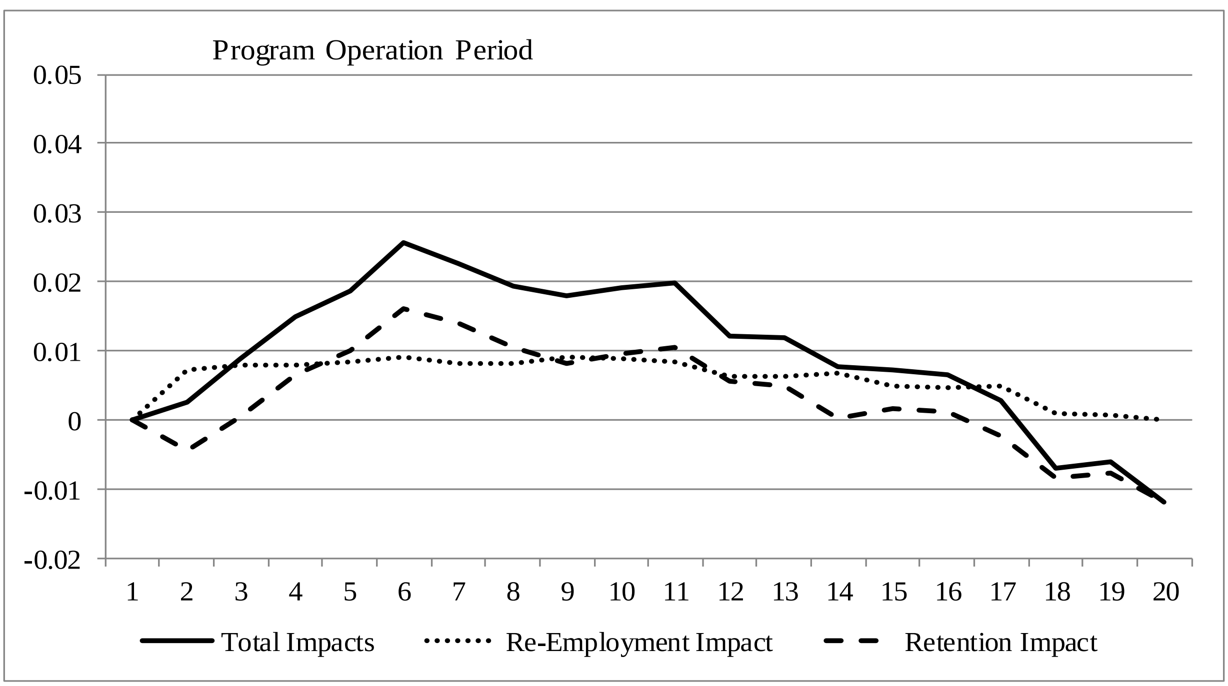


Table 1

Sample Characteristics at Time of Random Assignment

	Corpus Christi (%)	Fort Worth (%)
Education		
No High School Degree	52.5	44.9
Age		
18 to 29	62.5	64.4
30 to 45	34.2	32.7
Over 45	3.3	2.9
Race/Ethnicity		
Black	8.2	67.6
Hispanic	73.7	10.7
Other Race/Ethnicity	18.1	21.8
Number of Children		
None	1.5	0.3
One	44.8	41.5
Two	28.5	30.6
Three or more	25.3	27.6
Age of Youngest Child		
0 to 2	49.8	53.4
3 to 5	20.7	18.4
6 to 18	28.0	27.9
Previous Cash Assistance Receipt		
None	36.8	34.6
Two years or less	41.5	50.4
More than two years	21.7	15.0
Previous Employment		
None	14.9	13.1
Two years or less	57.8	61.1
More than two years	27.3	25.8
UI Employment Status in Quarter of Random Assignment		
Non-Employment	50.9	56.4
Employment	49.1	43.7
Sample size	1,620	1,503

Table 2

Spell Distribution

	Corpus Christi		Fort Worth	
	N	%	N	%
Sample Size				
Number of individuals	1,602		1,503	
Number of quarters observed				
19	182		206	
20	1,438		1,297	
Number of spells	6,878		6,247	
Number of spells per person				
Frequency Distribution				
	244	15.2	246	16.4
	174	10.9	178	11.8
	283	17.7	252	16.8
	200	12.5	191	12.7
5 or more	701	43.8	636	42.3
Mean	4.2		4.1	
Standard Deviation	2.5		2.4	

SOURCE: MDRC calculations from administrative records for the State of Texas.

Table 3

Spell Lengths For Spells Starting After Random Assignment

	Non-employment		Employment	
	Corpus Christi	Fort Worth	Corpus Christi	Fort Worth
Spell length				
(percentage of spells that lasted the indicated number of quarters)				
Quarters				
1	36.4	34.1	31.1	31.4
2	16.1	18.1	20.7	19.5
3	11.5	11.5	11.8	12.6
4	7.7	7.8	7.5	8.4
5+	28.2	28.5	28.9	32.0
Mean	4.0	4.0	4.0	4.2
(sd)	4.1	4.2	4.1	4.2
N (spells)	2,615	2,326	6,878	6,247

SOURCE: MDRC calculations from administrative records for the State of Texas.

Table 4

Duration Model EstimatedCoefficients

Corpus Christi

	Interrupted Spells				Fresh Spells			
	Not Employed		Employment		Not Employed		Employment	
Treatment	1.069		0.865		1.143	***	0.869	**
	(0.102)		(0.080)		(0.059)		(0.053)	
Baseline Hazard								
First Quarter	2.544	***	1.718	***	3.757	***	1.578	***
	(0.497)		(0.342)		(0.344)		(0.195)	
Second Quarter	1.869	***	2.755	***	2.597	***	1.708	***
	(0.337)		(0.460)		(0.247)		(0.205)	
Third Quarter	2.317	***	2.107	***	2.440	***	1.383	***
	(0.380)		(0.367)		(0.243)		(0.168)	
Fourth Quarter	1.642	***	2.187	***	2.130	***	1.194	
	(0.298)		(0.376)		(0.233)		0.151	
Fifth and Sixth Quarter	1.967	***	1.544	***	1.525	***	1.155	
	(0.271)		(0.227)		(0.165)		(0.132)	
Sample Member Characteristics								
No High School Diploma or GED	0.799	**	1.195	*	0.967		1.366	***
	(0.081)		(0.115)		(0.051)		(0.088)	
Age 18 to 29 years at RA	1.375	***	1.149		1.193	***	0.957	
	(0.162)		(0.131)		0.075		0.073	
Race/Ethnicity								
Black	1.592	**	1.037		1.412	***	0.911	
	(0.342)		(0.194)		(0.150)		(0.116)	
Hispanic	1.557	***	0.965		1.328	***	0.798	**
	(0.229)		(0.124)		(0.098)		(0.070)	
Age of Youngest Child								
0 to 2 years	1.213		1.006		1.065		1.134	
	(0.165)		(0.126)		(0.075)		(0.096)	
3 to 5 years	1.044		1.121		1.061		1.227	**
	(0.164)		(0.149)		(0.084)		(0.116)	
Quarter of the Year								
First Quarter	1.287	**	0.728	***	1.233	***	0.946	
	(0.150)		(0.084)		(0.081)		(0.063)	
Second Quarter	1.219		0.806	*	1.153	**	1.075	
	(0.147)		(0.090)		(0.079)		(0.069)	
Third Quarter	1.193		0.807	*	1.230	***	1.046	
	(0.145)		(0.091)		(0.082)		(0.068)	
Calendar Quarter (Time Period)	0.965		0.933		1.129	***	1.077	*
	(0.117)		(0.105)		(0.048)		(0.047)	
Calendar Quarter (Time Period) Squared	0.999		1.001		0.995	***	0.996	**
	(0.005)		(0.005)		(0.002)		(0.002)	
Unemployment rate	1.108		1.200		0.732	**	0.951	
	(0.390)		(0.402)		(0.098)		(0.126)	

(continued)

Table 4 (continued)

	RA Spells		Fresh Spells	
	Not Employed	Employment	Not Employed	Employment
Constant	0.329	0.442	0.230 **	0.121 ***
	(0.545)	(0.681)	0.156	(0.083)
Mass Points				
Second	0.062 ***	0.100 ***	2.468 ***	0.488 ***
	(0.021)	(0.031)	(0.425)	(0.101)
Third	0.126 ***	0.127 ***	0.745 **	2.620 ***
	(0.037)	(0.034)	(0.094)	(0.356)
Mass Points Sample Distribution Fractions				
Second Mass Point	0.250 ***			
	(0.032)			
Third Mass Point	0.570 ***			
	(0.037)			
Model Fit				
Log Likelihood	-13,186.413			
AIC	26,413			
BIC	26,560			
N (number of spells)	32,218			
* p<0.10, ** p<0.05, *** p<0.01				

SOURCE: MDRC calculations from administrative records for the State of Texas.

NOTES: Statistical significance levels are based on the Wald test statistic and are indicated as follows: *** = 1 percent; ** = 5 percent; and * = 10 percent. the null hypothesis for the test is that the coefficient is equal to one.

"Interrupted spells" refer to the spells in progress at the time of random assignment.

"Fresh spells" refer to the spells that began after random assignment.

Coefficients in this table are exponentiated. Coeffecients larger than one indicate a positive effect, and coeffecients less than one indicate a negative effect.

Table 5

Duration Model Estimated Coefficients

Fort Worth

	Interrupted Spells		Fresh Spells	
	Not Employed	Employment	Not Employed	Employment
Treatment	1.022	1.036	1.031	0.968
	(0.096)	(0.113)	(0.062)	(0.058)
Baseline Hazard				
First Quarter	2.410 ***	1.676 *	3.183 ***	2.294 ***
	(0.498)	(0.494)	(0.366)	(0.266)
Second Quarter	2.388 ***	2.697 ***	2.569 ***	2.164 ***
	(0.446)	(0.537)	(0.294)	(0.247)
Third Quarter	2.323 ***	2.572 ***	2.339 ***	1.708 ***
	(0.398)	(0.465)	(0.274)	(0.201)
Fourth Quarter	1.685 ***	1.724 **	2.121 ***	1.637 ***
	(0.308)	(0.363)	(0.264)	(0.201)
Fifth and Sixth Quarter	1.576 ***	1.767 ***	1.900 ***	1.524 ***
	(0.231)	(0.300)	(0.220)	(0.169)
Sample Member Characteristics				
No High School Diploma or GED	0.763 ***	1.517 ***	0.893 *	1.408 ***
	(0.074)	(0.164)	(0.055)	(0.087)
Age 18 to 29 years at RA	2.050 ***	0.940	1.169 *	0.921
	(0.267)	(0.128)	(0.093)	(0.073)
Race/Ethnicity				
Black	1.643 ***	0.738 **	1.364 ***	0.859 **
	(0.212)	(0.105)	(0.108)	(0.066)
Hispanic	1.960 ***	0.771	1.138	0.805 *
	(0.354)	(0.151)	(0.134)	(0.093)
Age of Youngest Child				
0 to 2 years	1.079	1.066	1.204 **	1.017
	(0.145)	(0.160)	(0.105)	(0.090)
3 to 5 years	1.212	1.072	0.886	1.117
	(0.192)	(0.178)	(0.089)	(0.112)
Quarter of the Year				
First Quarter	1.341 **	0.834	1.299 ***	0.747 ***
	(0.157)	(0.115)	(0.093)	(0.052)
Second Quarter	1.187	1.036	1.279 ***	0.829 ***
	(0.145)	(0.137)	(0.095)	(0.055)
Third Quarter	1.406 ***	0.842	1.405 ***	0.872 **
	(0.168)	(0.118)	(0.102)	(0.057)
Calendar Quarter (Time Period)	1.004	1.034	1.094 ***	1.065 **
	(0.067)	(0.086)	(0.029)	(0.027)
Calendar Quarter (Time Period) Squared	0.999	0.996	0.996 ***	0.997 ***
	(0.003)	(0.003)	(0.001)	(0.001)
Unemployment rate	1.045	1.095	0.791 ***	0.949
	(0.105)	(0.134)	(0.051)	(0.062)

(continued)

Table 5 (continued)

	RA Spells		Fresh Spells	
	Not Employed	Employment	Not Employed	Employment
Constant	0.280 ***	1.837	0.135 ***	0.185 ***
	(0.128)	(1.307)	(0.052)	(0.075)
Mass Points				
Second	0.073 ***	0.031 ***	0.802	1.510 ***
	(0.026)	(0.017)	(0.117)	(0.235)
Third	0.145 ***	0.054 ***	2.531 ***	0.478 ***
	(0.055)	(0.034)	(0.355)	(0.077)
Mass Points Sample Distribution Fractions				
Second Mass Point	0.477 ***			
	(0.051)			
Third Mass Point	0.364 ***			
	(0.051)			
Model Fit				
Log Likelihood	-11,998.85			
AIC	24,034			
BIC	24,183			
N (number of spells)	29,854			
* p<0.10, ** p<0.05, *** p<0.01				

SOURCE: MDRC calculations from administrative records for the State of Texas.

NOTES: Statistical significance levels are based on the Wald test statistic and are indicated as follows: *** = 1 percent; ** = 5 percent; and * = 10 percent. the null hypothesis for the test is that the coefficient is equal to one.

"Interrupted spells" refer to the spells in progress at the time of random assignment.

"Fresh spells" refer to the spells that began after random assignment.

Coefficients in this table are exponentiated. Coeffecients larger than one indicate a positive effect, and coeffecients less than one indicate a negative effect.

Table 6

Effects of Program Treatment by Program Operation Period

	Interrupted Spells		Fresh Spells		Log Likelihood N (number of spells)
	Not Employed	Employment	Not Employed	Employment	
Corpus Christi					
During Program Operation	1.039	0.870	1.136 **	0.824 ***	-13,184
	(0.1029)	(0.084)	(0.070)	(0.058)	32,218
After Program Operation	1.473	0.832	1.149 *	0.951	
	(0.380)	(0.197)	(0.085)	(0.079)	
Fort Worth					
During Program Operation	1.078	1.061	1.022	0.879 *	-11,992
	(0.105)	(0.118)	(0.072)	(0.060)	29,854
After Program Operation	0.615 *	0.746	1.043	1.140	
	(0.166)	(0.248)	(0.088)	(0.094)	

SOURCE: MDRC calculations from administrative records for the State of Texas.

NOTES: Statistical significance levels are based on the Wald test statistic and are indicated as follows: *** = 1 percent; ** = 5 percent; and * = 10 percent. The null hypothesis for the test is that the coefficent is equal to one.

"Interrupted spells" refer to the spells in progress at the time of random assignment.

"Fresh spells" refer to the spells that began after random assignment.

Coefficients in this table are exponentiated. Coeffecients larger than one indicate a positive effect, and coeffecients less than one indicate a negative effect.

Table 7

Simulation Results: Employment Statistics

	Simulation Type					
	No Program	All Program		Employment entry only		Retention Only	
Mean across Simulations		Level	Impact	Level	Impact	Level	Impact
Corpus Christi							
Quarterly Employment Rate	0.468***	0.513***	0.045***	0.490***	0.022***	0.493***	0.024***
	(0.012)	(0.011)	(0.014)	(0.011)	(0.009)	(0.012)	(0.010)
Fort Worth							
Quarterly Employment Rate	0.480***	0.491***	0.010	0.487***	0.006	0.484***	0.003
	(0.012)	(0.013)	(0.016)	(0.011)	(0.010)	(0.012)	(0.010)

SOURCE: MDRC calculations from administrative records for the State of Texas.

NOTES: Statistical significance levels are indicated as follows: *** = 1 percent; ** = 5 percent; and * = 10 percent.
Quarterly Employment Rate represents the mean quarterly employment over the simulation period.