INSIDE THIS RESEARCH BRIEF

Different components of transitional jobs (TJ) programs may improve employment and recidivism among former prisoners. Using data from the Transitional Jobs Reentry Demonstration evaluation, we found that former prisoners who spent 30 days or more in a TJ over a six-month period were 14 percent more likely to obtain an unsubsidized job in the subsequent six months (45% vs. 31%). No other TJ program components (e.g., job development assistance, case management, retention bonuses) were found to individually affect employment or recidivism outcomes. Although these analyses were non-experimental, we incorporated regression-based adjustments for selection bias common to analyses of this sort. Future research evaluating different components of TJ programs via random assignment design would benefit practitioners interested in obtaining the most "bang" for their reentry programming "bucks."

Which Components of Transitional Jobs Programs Work Best?

Analysis of Programs for Former Prisoners in the Transitional Jobs Reentry Demonstration

Jennifer Yahner and Janine M. Zweig

Transitional jobs (TJ) models have emerged as a promising approach for increasing employment and reducing recidivism among the growing population of former prisoners. Since 2008, TJs have been the center of considerable policy interest by the current Obama administration and through the Second Chance Act, which passed with overwhelming bipartisan support.

Results from the first rigorous evaluation of the TJ model—that of the Center for Employment Opportunities in New York—showed short-term effects on employment followed by long-lasting reductions in recidivism for formerly incarcerated program participants (Redcross et al., 2009). Further analysis of the recidivism effects showed them to be strongest for highest-risk offenders (i.e., younger individuals with lengthier criminal histories) (Zweig, Yahner, and Redcross, 2011).

The Transitional Jobs Reentry Demonstration (TJRD) was another rigorous evaluation of the TJ model, designed to test its effectiveness compared to a standard set of job search (JS) activities for recently released prisoners

(Redcross et al., 2010). More than 1,800 male former prisoners were randomly assigned to either a TJ program or JS program in each of four states, and their employment and recidivism outcomes were followed two years after random assignment. Analyses in this brief are based on the first year of TJRD follow-up data.

One-year follow-up of TJRD study participants showed that the program generated early increases in employment driven by TJ participation, and that the program in one study site had some short-term impacts on recidivism. However, across all four sites the TJ programs reduced unsubsidized employment and earnings and did not affect the percentage of the sample that was arrested, convicted, or admitted to prison (Redcross et al. 2010).

Despite this somewhat discouraging picture of the first-year follow-up results from the overall TJRD evaluation, it is still possible for specific TJ program strategies to have functioned successfully as mediators affecting client outcomes—and practitioners in the field are hungry for such information. Program implementers want to know

about the relative contribution that different program strategies within the TJ model—such as length of time in a transitional job, receipt of job readiness/development assistance, case management services, and supportive payments—make to participant outcomes. The TJRD study provides a unique opportunity for us to explore the comparative effectiveness of these specific TJ program components.

For example, imagine that certain TJ program strategies were successful at improving long-term employment outcomes, but that these strategies were coupled with others that had no effects (or even detrimental effects). Under this scenario, any positive effects of the successful mediators would have been diluted or canceled out by the countering effects of those with no effects. Alternatively, imagine that some TJ strategies were successful for certain subgroups of participants, but were ineffective for others (e.g., job development might work well for younger offenders but have no effect among older offenders). In this case, the net effect might be that of no overall program impact if such mediational pathways were not explored across differing subgroups. Both of these scenarios are entirely possible in any program evaluation, including that of the TJRD study (see MacKinnon and Fairchild, 2009, for further details on these and other examples).

What is a transitional job?

A transitional job is generally defined as temporary, paid work experience intended to improve participants' employability, earnings, and longer-term success in the unsubsidized labor market. Transitional jobs promote self-sufficiency in a prosocial (noncriminal) working environment.

In this research brief, we attempt to explore just these ideas—by going beyond the overall comparison of TJ and JS groups in the TJRD study's first-year follow-up results to examine whether and which program features of the TJ model *were* successful in promoting unsubsidized work or in reducing recidivism. Our goal, quite simply, is to help give practitioners the best

"bang" for their TJ programming "bucks." If our results can be confirmed through future research specifically testing the effects of various TJ program strategies via experimental methods, they will be made that much stronger. Policymakers can then target limited TJ program resources toward those strategies most likely to generate successful outcomes.

This research brief is organized into the following sections: First, we present a more detailed description of the TJRD study and its various TJ program components. Second, we more formally state the research questions that are the focus of this brief. Third, we identify the methods used to answer those questions. Fourth, we present results and identify some of their limitations. Last, we discuss which TJ program component seemed to work best for this sample of recently released male prisoners, and the policy implications of this finding.

Transitional Jobs Reentry Demonstration

TJRD was a rigorous, experimental study designed to evaluate the overall effectiveness of providing transitional jobs and employment support services to former prisoners, compared to basic job search assistance with no subsidized jobs. From 2007 to 2008, more than 1,800 recently released prisoners were randomly assigned[1] to either the TJ program (n = 912) or job search (JS) program (n = 901) in each of four sites: Chicago, Illinois; Detroit, Michigan; St. Paul, Minnesota; and Milwaukee, Wisconsin.

TJRD was based on the assumption that increasing former prisoners' employment levels (both subsidized and unsubsidized) would result in lower recidivism rates. Consequently, the evaluation tracked employment and recidivism outcomes through one year after random assignment (RA), using official state data (Redcross et al., 2010).

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[1] Random assignment ensures that any significant differences in outcomes between the two groups can be attributed with confidence to the different sets of employment services each group received.

In Michigan, Minnesota, and Wisconsin, separate organizations provided the TJ and JS program services, while in Illinois, the same organization provided both the TJ and JS services. There were also important differences in labor market conditions, population characteristics, and criminal justice practices across the four sites, as discussed in Redcross et al. (2010).

TRANSITIONAL JOBS PROGRAMS

The purpose of the TJRD transitional jobs programs was to provide immediate employment to individuals in settings where performance and workplace issues could be identified and addressed, and not necessarily to build skills in any particular occupation. Through this process, the TJ programs aimed to increase participants' long-term ability to succeed in subsequent, unsubsidized employment settings.

In Michigan and Minnesota, Goodwill Industries operated the TJ programs, and participants worked at Goodwill enterprises, such as in retail stores or in a light manufacturing plant. In Illinois, Safer Foundation established a staffing agency that employed transitional jobs workers. The staffing agency contracted with a waste management firm working for the City of Chicago to operate garbage recycling plants where program clients worked. In Wisconsin, the New Hope program employed and paid program clients and placed them in local nonprofit organizations and small businesses throughout the area, using a scattered site TJ model.

Although the TJ programs were structured in differing ways, each provided participants with temporary, minimum-wage jobs offering 30 to 40 hours of paid work each week. The actual **number of days worked in a TJ** varied from 0 days to over four months, with the average participant spending 35 workdays in a transitional job (typically, at a frequency of four workdays per week) during the first six months after random assignment.[2]

In addition to subsidized transitional jobs, TJ program participants were offered a number of ancillary services and supports, including the following:

- *Job readiness/development assistance*, which involved individual or group meetings with program staff to discuss job search preparation, to identify job openings at local agencies, and to develop soft skills. Exactly half of the TJ program participants took part in job readiness/development activities during the first six months after random assignment, yielding an average rate of activities of 2.4 per participant overall.[3]

Who provided TJ program participation data?

Each site tracked client participation in various TJ program components using a management information system. For this brief, we focus on participation during the first six months after random assignment (months 1–6). We then compare these data to outcomes during the subsequent six months (months 7–12).

- *Case management* services that varied across sites, but at a minimum included assistance with identification recovery (e.g., helping recently released prisoners obtain new Social Security cards or driver's licenses). Over two-thirds (69%) of the TJ program participants met with a case manager at least once during the first six months after random assignment, with the average participant having two meetings with his case manager.

- *Vocational training* opportunities that were offered primarily in Minnesota (on-site) and Wisconsin

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[2] Participation numbers reported herein correspond to the specific analytic scheme of this brief (i.e., analysis of the effects of

participation during the first six months after random assignment on outcomes during subsequent months 7–12). These participation numbers differ from those reported in the full TJRD evaluation report (Redcross et al., 2010), which focused on one-year participation data for the entire TJ program group and, in some instances, combined different components together to present a more comprehensive story.

[3] As indicated in Appendix A, information on whether a participant took part in any job readiness/development activity was provided by all four sites, but information on the exact number of activities was only available for Illinois and Michigan TJ program participants.

(referrals to off-site providers), and that included skills training in forklift operation, automotive repair, and construction, with participants earning certificates upon completion. Nearly a fifth (18%) of program participants received vocational training during the first six months after random assignment.

- **Support payments**, averaging $93, that were provided to program participants for things like transportation costs, photo identification fees, housing rent/deposits, and emergency food cards. Program participants received an average of two support payments during the first six months after random assignment.

- **Retention bonuses** that were offered only in Minnesota and Wisconsin, and that consisted of incremental payments to program participants who obtained and maintained unsubsidized jobs. During the first six months after random assignment, 8 percent of eligible participants received a retention bonus.[4]

JOB SEARCH PROGRAMS

The JS programs of the TJRD study provided basic assistance designed to help participants learn how to prepare a résumé, fill out job applications, interview for jobs (including how to answer questions about their convictions), and identify job leads. Some of the JS programs also provided referrals to housing, child support, and other assistance programs.

ONE-YEAR EVALUATION RESULTS

As reported in Redcross et al. (2010), one-year evaluation results indicated that, overall, the TJ programs generated early increases in employment, driven by subsidized participation in the TJs, and that the Minnesota TJ program had some short-term

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[4] In Minnesota, retention bonuses were offered partway through the TJRD's enrollment period; thus, TJ participants who enrolled prior to that date were excluded from analyses as ineligible for evaluation of retention bonus effects.

impacts on recidivism. However, across all four sites the TJ programs reduced unsubsidized employment and earnings and did not affect the percentage of the sample that was arrested, convicted, or admitted to prison (Redcross et al., 2010).

Yet, despite this lack of overall program effects, specific TJ program components may have been successful at increasing employment and/or reducing recidivism outcomes (and their effects simply washed out by other unsuccessful components). We examine just these issues in the next sections.

Research Questions

In this research brief, we attempt to answer the following questions for the sample of former prisoners who participated in TJ programs:

1) Was participation in any specific TJ program component, as measured during the first six months after random assignment, associated with successful unsubsidized employment or recidivism outcomes, as measured during the subsequent six months (months 7–12)?

2) Which specific TJ program component(s), if any, had the strongest associations with positive outcomes?

Methods

Although TJRD was a rigorous experimental study, our methodological approach was not experimental in nature, because participants were not randomly assigned to different TJ program strategies (but rather, to the entire TJ program as a whole). Typically, analyses of this sort are flawed to the extent that individuals self-select into certain program components for the same reasons that affect their ultimate outcomes. For example, suppose we found that lengthier participation in a TJ was associated with lower subsequent recidivism. It would be very difficult to tell whether this finding resulted from the *experience* of working more days in a TJ or was simply indicative of *individual*

differences (selection bias) among participants in, for example, motivation to succeed.

CONTROLLING FOR SELECTION BIAS

To address the issue of selection bias, we capitalized on information known about the non-TJ program participants (i.e., the JS program group) at the time of random assignment to measure and statistically control for initial differences in outcome propensity. In other words, typical analyses of this sort lack information about what employment and recidivism outcomes TJ program participants might have had if they had *not* participated in any TJ program components. Yet, in this case, we had a very good proxy for those outcomes in the JS participants, who represented a similar group of recently released male prisoners exposed to basic job search assistance but nothing more.[5]

That said, using methods described in Appendix B, we estimated an employment propensity and a recidivism propensity, at the time of random assignment, for each TJ program participant. We then controlled for these proxy measures of "propensity to succeed or fail" in each relevant model analyzing the effects of TJ program strategies on employment and recidivism outcomes.

DETERMINING WHICH TJ PROGRAM COMPONENTS WORKED BEST

To determine which TJ program components worked best, we estimated a series of regression models and assessed the significance of each component's effect on (or association with) subsequent employment and recidivism outcomes.[6] In each model, we statistically

controlled for a number of other factors that might have affected the outcomes, so that we could narrow down the specific effect of the TJ program component being assessed. These control variables measured participants' age, race, time between prison release and random assignment, site/state, criminal history (three or more prior convictions, or fewer), employment history (full-time employment in the six months preceding incarceration, or not), and propensity to succeed or fail (as described previously and in Appendix B).[7] We also controlled for early employment and recidivism outcomes in the first six months after random assignment, as these likely affected participants' outcomes during the subsequent months as well.

Results

WERE ANY TJ PROGRAM COMPONENTS ASSOCIATED WITH SUCCESSFUL EMPLOYMENT OR RECIDIVISM OUTCOMES?

Associations with Employment Outcomes

To answer our first research question with regard to employment outcomes, we conducted a series of regression models with the following three dependent variables: any unsubsidized employment in months 7–12 after random assignment (38% obtained unsubsidized employment during that time); number of quarters of unsubsidized employment (ranging from 0 to 2); and earnings from unsubsidized employment

[5] Additional services that the JS participants might have received outside of the TJRD study cannot be definitively ascertained, because no participant survey was conducted; however, it is unlikely that they received any of the enhanced employment services provided to TJ program participants and none were offered a transitional job (see Redcross et al., 2010).

[6] See Appendixes A and C for tables of descriptives and correlations on the TJRD program components we examined. The degree of information available for respondents' participation in each component differed across the TJRD sites. For example, information

on support payments was provided by TJRD programs in Michigan, Minnesota, and Wisconsin, but not Illinois. Regression models testing each component were estimated on the subsample of respondents for whom such data were provided.

[7] Notably, collinearity diagnostics indicated that multicollinearity was not a serious problem in any employment outcome model. The only variables with a mild degree of collinearity with one another (variance inflation factors just over 2.0) were those representing employment propensity and state variables. Respondents from Michigan had a lower propensity of employment than of random assignment, while those from Minnesota had a higher initial propensity of employment. Collinearity diagnostics indicated that multicollinearity was not a problem in any recidivism outcome model.

Table 1. TJ Program Components' Effects on Employment Outcomes in Months 7-12 after RA[a]

TJ Program Components in Months 1–6 after RA	Employment Outcomes in Months 7–12 after RA		
	Any Unsubsidized Employment	Number of Quarters of Unsubsidized Employment	Earnings from Unsubsidized Employment
	(*odds ratios*[b])	(*coefficients*)	(*coefficients*)
Number of days worked in a TJ	1.009 **	0.002 **	$ 8.45 *
Ever participated in job readiness/development activities	1.551 *	0.162 **	541.99 *
Number of times participated in job readiness/development	1.023	0.012	11.77
Ever had individual case management meeting	1.260	0.082	306.86
Number of times had individual case management meeting	1.069 †	0.018	36.74
Ever participated in vocational training	1.103	0.110	252.74
Number of times received support payment	1.126 **	0.042 ***	130.56 **
Amount received in support payments	1.002 *	0.001 **	2.55 **
Ever received a retention bonus	1.639	0.417 *	871.34
Number of times received retention bonus	1.345	0.190 *	731.14 *
Amount received in retention bonuses	1.000	0.001 †	3.91 †

SOURCE: Data from the MDRC baseline information form, TJRD programs themselves, and TJRD state employment agencies.

NOTES: [a] Not shown but included in each model were the following control variables: propensity for employment in quarters 3 and 4 post-RA, age, race, TJRD site/state, time from release to random assignment, criminal history and employment history as of random assignment, and early employment and/or recidivism in quarters 1 and 2 post-RA. [b] Odds ratios above 1.0 indicate a positive relationship and below 1.0 a negative relationship. Regression models were estimated on up to 843 program group participants (actual *N* varied by model depending on how many TJRD sites provided participation data for that program component). Significance levels are indicated as † $p < .10$; * $p < .05$;

(ranging from $0 to $29,302).[8] Each of these outcomes was assessed using official state employment records.

In Table 1, we identify which TJ program strategies were significantly associated with each of these employment outcomes when tested individually (one by one) in models controlling for age, race, site/state, propensity to succeed/fail, etc. In other words, Table 1 synthesizes the results of 33 separate regression models, or 11 individual tests of each TJ program component's effect on the three employment outcomes shown. Numbers in the table represent the effect (odds ratio or regression coefficient) associated only with the TJ program component tested in each model.

Importantly, when examining the results in Table 1, we want to note whether any numbers are statistically significant (indicating a significant, nonzero effect of a TJ program component on that employment outcome) and—where there are significant effects—whether that association is positive or negative. Further, because we have tested so many regression models at once, we are especially looking for *trends* of positive effects, rather than a single, significant effect that might appear simply by chance (Curran-Everett, 2000).

The general conclusion from reviewing Table 1 is that several TJ program components showed trends of significant and positive associations with subsequent unsubsidized employment outcomes. These components included the following: ***number of days worked in a TJ***, receipt of ***job readiness/development*** assistance, ***support payments***, and, to some extent, ***retention bonuses***.[9] All four of these TJ program

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[8] In Illinois, some of the TJRD staff considered participants who worked 90 days or more in their transitional job as being "permanently placed" in that position and thereafter provided benefits and raises. However, payroll data did not support these claims; thus, we followed the decision of the main TJRD impact analysts (Redcross et al., 2010) in refraining from counting these lengthy transitional jobs as "unsubsidized employment."

[9] Given that Minnesota's TJ program had some effects that other sites did not in the overall analysis (Redcross et al., 2010), we

components had significantly positive associations with the unsubsidized employment outcomes we examined, even after controlling for participants' age, race, site/state, propensity to succeed/fail, etc.

We also note in Table 1 that two TJ program strategies were seemingly ineffective at improving subsequent employment outcomes. These strategies—case management services and vocational training—may have been responsible for "washing out" any overall positive effects of the TJ program in the original evaluation.

Readers should note that, at this stage, we have determined only that certain TJ program strategies were significantly associated with subsequent employment outcomes, but we have not yet determined which strategies were *most* successful. Before doing that analysis, we repeat this same initial examination with regard to recidivism outcomes.

Associations with Recidivism Outcomes

We examined the association of TJ program components with the following six recidivism outcomes, all measured during months 7–12 after random assignment and assessed using official state crime data:

1) Any rearrest (23% of the program group was rearrested);
2) Any reconviction (9% of the program group was reconvicted);
3) Any reincarceration in a state prison (19% of the program group was reincarcerated);
4) Any recidivism, including any rearrest, reconviction, or reincarceration (36% of the program group recidivated in quarters 3 and 4 post-random assignment);
5) Number of rearrests, which ranged from 0 to 5 for program group participants; and
6) Number of days of reincarceration in state prison, which ranged from 0 to 184 for program group participants.

As in our examination of employment outcomes, the results presented in Table 2 contain the odds ratio or linear regression coefficient associated only with the TJ program component whose effect is tested in each model. Numbers in the table, therefore, were extracted from 66 separate regression models, or 11 individual tests of each TJ program component's effect on the six recidivism outcomes.

> Several TJ program components showed trends of significant and positive associations with subsequent unsubsidized employment, including number of days worked in a TJ, receipt of job development assistance, support payments, and retention bonuses.

Again, our focus was primarily on identifying whether any TJ program components showed a *trend* of significant reduction effects on recidivism. Unlike the case for employment outcomes, we noted no trends of recidivism effects for any particular TJ program component. Thus, our results mirror the takeaway point from the full TJRD evaluation (Redcross et al., 2010)—namely, that the TJ programs had no significant overall effects on recidivism outcomes, across all four states, for the one-year follow-up period.

Rather, only 6 out of the 66 regressions conducted showed statistically significant associations. These associations were for number of days worked in a TJ on subsequent days of reincarceration; ever participated in job readiness/development activities on rearrest (effect in opposite direction); ever participated in job readiness/development activities and number of such activities on days of reincarceration (effects in opposite directions); and support payments (number of payments received and amount of payments) on number of rearrests. Although we would like to attribute importance to some of these effects, they are no more than might have occurred by chance given the large number of regression models we examined.

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retested all results omitting Minnesota participants from the regression models. The results remained substantively the same.

Table 2. TJRD Program Components' Odds Ratios and Unstandardized Coefficients from Regression Models Predicting Recidivism Outcomes in Quarters 3 and 4 after Random Assignment[a]

TJRD Program Components	Recidivism Outcome					
	Rearrested (*odds ratios*[b])	Reconvicted (*odds ratios*)	Reincarcerated (*odds ratios*)	Rearrested, Reconvicted, or Reincarcerated (*odds ratios*)	Number of Rearrests (*coefficients*)	Number of Days of Reincarceration (*coefficients*)
Number of days worked in a TJ	1.002	0.996	1.000	1.000	0.000	-0.123 *
Ever participated in job readiness/development activities	1.526 *	0.953	0.999	1.295	0.072	-9.247 *
Number of times participated in job readiness/development	1.011	0.940	0.952	1.003	0.009	-1.198 *
Ever had individual case management meeting	1.361	0.827	0.918	1.016	0.047	-5.084
Number of times had individual case management meeting	1.031	1.028	0.999	0.994	0.007	-1.099
Ever participated in vocational training	1.299	1.418	1.215	1.130	0.034	0.350
Number of times received support payment	0.937	0.889	1.008	0.958	-0.019 *	-1.186
Amount received in support payments	0.999	0.999	0.999	0.999	-0.000 †	-0.016
Ever received a retention bonus	1.318	N/A[c]	0.512	1.146	-0.061	-16.477
Number of times received retention bonus	1.028	N/A	0.663	0.977	-0.052	-7.403
Amount received in retention bonuses	1.000	N/A	0.997	0.999	-0.000	-0.046

SOURCE: Data from the MDRC baseline information form, TJRD programs themselves, and TJRD state law enforcement and correctional agencies.
NOTES: [a] Not shown but included in each model were the following control variables: propensity for recidivism in quarters 3 and 4 post-RA, early recidivism and/or employment earnings in quarters 1 and 2 post-RA, age, race, TJRD site/state, time from release to random assignment, criminal history, and employment history. [b] Odds ratios above 1.0 indicate a positive relationship and below 1.0 a negative relationship. [c] Model would not converge given quasi-complete separation of data points. Models were estimated on up to 854 program group participants (actual N varied by model depending on how many TJRD sites provided participation data for that program component). Significance levels are indicated as † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; **** $p < .0001$.

WHICH TJ PROGRAM COMPONENT SEEMED TO WORK BEST?

To answer our second research question, we conducted additional analyses of the four TJ program components that showed trends of positive associations with unsubsidized employment—namely, length of time in a TJ, job development, support payments, and retention bonuses. Nothing further was done with regard to recidivism outcomes, because no TJ program components showed similar trends on recidivism outcomes.

Most Significant Association with Employment Outcomes

To identify the TJ program strategies most strongly associated with improvements in subsequent employment among TJ program participants, we focused on the dependent variable measuring "any unsubsidized employment" in months 7–12 after random assignment. We then conducted four additional regression models—shown in Table 3—which we called Models A, B, C, and D. Each model tested two or more TJ program components' effects on employment simultaneously (in the same model at the same time), so as to parse out their individual contributions.

We note that the number of TJ program participants included in each model varied from a high of 842 in Model A to a low of 322 in Models C and D, because sites varied in the amount of participation data provided for each TJ program component. Also, some components were only offered in certain sites (e.g., retention bonuses were only offered in Minnesota and Wisconsin). We note these facts as important, because smaller model Ns can make it difficult to identify statistically significant, nonzero effects.

Results in Table 3 are quite straightforward to interpret. **Number of days in a TJ** was the only TJ program component that had significant, positive associations with unsubsidized employment among program participants, and this was true for all three of the models in which it was tested (Models A, B, and C). From these models, it is clear that the length of time participants spent working in a TJ mattered more than receipt of job readiness/development assistance, support payments, or retention bonuses. Even when the latter three components are tested by themselves without length of TJ in the model (see Model D), no single component emerges as having a stronger or statistically significant association with improved employment among TJ program participants.

SUPPLEMENTAL EXPLORATIONS

Last, we conducted two sets of supplemental explorations to refine the results presented so far. The first set examined the possibility that TJ program components' effects varied across subgroups of offenders, and the second set attempted to quantify the critical number of days in a TJ associated with improved employment outcomes.

Do TJ Program Components' Effects Vary across Subgroups of Offenders?

Given that prior research has shown that employment programs may be more effective for older offenders (Uggen, 2000) or that TJ programs, specifically, may be more effective for higher-risk offenders (Zweig, Yahner, and Redcross, 2011), we examined whether any TJ program strategies' associations with employment and recidivism outcomes varied across subgroups defined by age, criminal history, and employment history. We conducted these analyses by repeating the regressions described above while adding interaction terms, one by one, representing the products of each subgroup measure (for example, age) and the TJ program component whose effect was being tested.

Out of nearly 300 regression models testing these interactions (3 subgroups tested across 11 components' effects on 9 types of outcomes), the interaction term was *not* statistically significant 96 percent of the time. Thus, in only a negligible number of models (*n* = 11) did we find any evidence of variation across subgroups of offenders. Interestingly, there was indication in these few models that length of time in a TJ and job readiness/development reduced subsequent rearrest and reincarceration more for higher-risk offenders, as defined by those who were younger (under age 30) and not employed full-time before random assignment.

> Number of days in a TJ was the only TJ program component that had significant, positive associations with unsubsidized employment among program participants, and this was true in all three of the models in which it was tested (Models A, B, and C).

However, the overwhelming conclusion of these interaction analyses was that TJ program components' associations with subsequent employment and recidivism outcomes did not vary across subgroups of offenders. Notably, this finding accorded with those from subgroup analyses conducted on the full TJRD study sample (see Redcross et al., 2010).

How Many Days in a Transitional Job Were Best?

To assess how many days in a TJ mattered most toward improving unsubsidized employment outcomes among TJ program participants, we repeated the analyses presented in Table 3, with differing measures of the number of days worked in a TJ (i.e., any days, 15 days or more, 30 days or more, 60 days or more, and 90 days or more).

Results pointed to the importance of working 30 days or more in a transitional job.[10] As shown in Figure 1, TJ program participants who worked at least 30 days in a transitional job during the first six months after random

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[10] This was true for all three models in which length of time in a TJ was included (Models A, B, and C).

Table 3. Odds Ratios[a] from Logistic Regressions Predicting Any Unsubsidized Employment in Months 7–12 after Random Assignment

	Model A	Model B	Model C	Model D
N	842	669	322	322
TJRD Program Components in Months 1–6 after RA				
Number of days worked in a TJ	1.007 *	1.009 *	1.014 *	
Ever participated in job readiness/development activities	1.313	1.418	1.153	1.404
Number of times received support payment		1.062	1.013	1.059
Number of times received retention bonus[b]			1.421	1.328
Control Variables				
Employment propensity at random assignment	24.126 ****	36.895 ***	68.097 **	64.333 **
Age of the sample member	0.976 *	0.980 †	0.971 †	0.970 †
Black race	1.614 *	1.717 *	2.172 †	2.184 †
Illinois site	0.831			
Michigan site	0.709	0.738		
Minnesota site	1.120	0.887	1.072	1.272
Time from release to RA	0.998	1.001	0.998	0.999
More than three prior convictions	1.000	0.976	0.767	0.762
Employed full-time 6 mos. pre-prison	1.072	1.103	0.976	1.033
Any recidivism in months 1–6 after RA	0.367 ****	0.415 ****	0.323 ***	0.275 ****
Employment earnings in months 1–6 after RA	1.001 ****	1.001 ****	1.001 ****	1.001 ****
TJRD sites/states	Illinois; Michigan; Minnesota; Wisconsin	Michigan; Minnesota; Wisconsin	Minnesota; Wisconsin	Minnesota; Wisconsin

SOURCE: Data from the MDRC baseline information form, TJRD programs themselves, and TJRD state employment agencies.

NOTES: [a] Odds ratios above 1.0 indicate a positive relationship and below 1.0 a negative relationship. [b] Two-thirds of Minnesota participants were ineligible for evaluation of retention bonus effects. Collinearity diagnostics indicated that multicollinearity was not a serious problem in any employment outcome model; the only indication of mild collinearity (variance inflation factors just over 2.0) was respondents from Minnesota had a higher propensity of employment as of random assignment than those from Michigan. Significance levels are indicated as † $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$; **** $p < .0001$.

assignment were at least 14 percent more likely to obtain unsubsidized employment subsequently, compared to those who worked less than 30 days in a transitional job (45% vs. 31%, using the most conservative estimate from Model A). Notably, this measure is the actual number of days worked in a TJ and thus equates to almost two months of time (since participants typically worked in a TJ four days per week).

It is also notable from these additional regression results (not shown) that former prisoner participants who worked at least 15 days in a transitional job had significantly positive (but smaller) employment outcomes; however, working too many days (i.e., 90 workdays or more) yielded no significant benefits for TJ program participants. Because 90 workdays equates to almost six months of time, it is possible that any participant who remained in a TJ that long was either using his subsidized job as a crutch to help avoid "real-world" work or was the type of individual for whom unsubsidized work was for some reason unavailable (despite our attempts to control for such unmeasured individual heterogeneity).

Figure 1. Predicted Probability of Unsubsidized Employment in Months 7–12 after RA, by Number of Days in a Transitional Job during Months 1–6
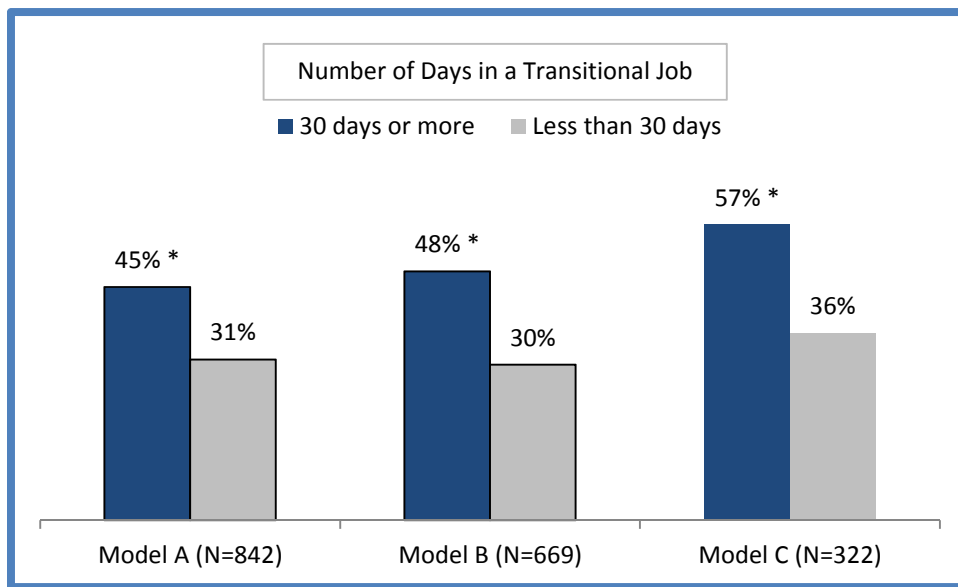
SOURCE: Data from the MDRC baseline information form, TJRD programs themselves, and TJRD state employment agencies.

NOTES: Significance levels are indicated as † p < .10; * p < .05; ** p < .01; *** p < .001; **** p < .0001.

Limitations

Because the analyses presented in this brief are non-experimental in nature, it is important to note several limitations to the results we have just presented. First, despite our best attempts to statistically control for the selection bias effect, it is impossible to know whether we have truly captured TJ program participants' individual differences in propensity to succeed or fail. Rather, we have created proxy measures for this propensity, based on what we observed to be true for the JS program group (see Appendix B for further explication of our approach). We can be certain that this regression-based adjustment has at least some degree of error in it due simply to the absence of predictor variables not available in this data set (e.g., information on participants' substance use and mental health histories). If results in this brief can be confirmed through future research focused specifically on testing various TJ program strategies' effects via

experimental or quasi-experimental methods, our findings will be made that much stronger.

Second, as is the case with any social science analysis, one can never truly establish a causal direction in effects. Although we have at least analyzed things in the proper temporal order (program participation measured in months 1–6 and outcomes measured in months 7–12), what we have presented are still the statistical *associations* observed between such participation and the employment and recidivism outcomes analyzed. We have also necessarily omitted information on other factors, unavailable in this data set, which may have been responsible for the observed outcomes among TJ program participants (e.g., substance abuse, family dynamics, mental health issues).

Last, as was apparent from the differing Ns in the models tested herein, not all TJ program components were offered across all four sites and, even when

offered, full information on participation in those components was not always available. Therefore, it is possible that some of what we have observed—or some of the associations that we did not observe, such as that of retention bonuses in Table 3—were the result of an absence or incomplete accounting of the participation data. Our main attempt to address this issue (beyond controlling for site/state in all models) was to repeat all analyses excluding results from the Minnesota TJRD site (which showed the best likelihood of success based on the full TJRD evaluation; Redcross et al., 2010). After excluding Minnesotan participants from all models, we still found that the results were substantively unchanged—namely, that the number of days in a TJ mattered more to employment outcomes than did other TJ program components.

Conclusion

In this brief, we examined the relative effectiveness of various components of transitional jobs programs for former prisoners participating in the TJRD. Although the TJ programs, as a whole, showed no positive impacts on outcomes in the year after program assignment (Redcross et al., 2010), it was possible that certain TJ program components *were* successful at improving outcomes, but had this success essentially washed out by other strategies that had no effect.

Unique to our analysis was incorporation of a proxy attempting to control for selection bias effects (e.g., the fact that greater participation in a particular component may have led to better outcomes only because more motivated individuals self-selected into both). Toward this end, we called the proxy a measure of TJ program participants' initial propensity to succeed and based it on what was observed among a highly similar group of recently released male prisoners who did not participate in the TJ program (namely, the JS group). We also controlled for other factors possibly related to outcomes, including participants' age, race, site/state, time between release and random assignment, employment and criminal history, and early outcomes during the follow-up period.

Overall, we observed a pattern of findings indicating that one TJ program component in particular was significantly associated with higher levels of subsequent unsubsidized employment among TJ program participants. That component measured the length of time that participants spent working in a transitional job. Specifically, we found that TJ program participants who spent 30 workdays or more in a transitional job during the first six months of the follow-up period (at a rate of four workdays per week, which equates to two months of time) were 14 percent more likely than other TJ program participants to obtain an unsubsidized job in the subsequent six months (45% vs. 31%; see Figure 1, Model A).

We also noted that—to a lesser extent—job readiness/development assistance, supportive payments, and retention bonuses provided to TJ program participants had some associations with improved employment outcomes later. However, neither of the other TJ program components we tested (namely, case management services and vocational training) showed consistent effects on the likelihood or amount of subsequent unsubsidized employment.

With regard to recidivism, we noted no overall trend or pattern of significant associations between any particular TJ program component and the measured recidivism outcomes. These findings mimic those for the larger study, which found no effect of the overall TJ program on recidivism during the first year after random assignment.

Future experimental research evaluating the effectiveness of specific TJ program components would likely benefit practitioners interested in obtaining the most "bang" for their reentry programming "bucks." If they confirm the findings in this brief, then practitioners might realize the greatest returns on their investments if they can somehow ensure that former prisoners participate in at least 30 workdays of transitional employment across several weeks or months. We acknowledge that ensuring such participation would not

be an easy feat to accomplish, but realizing its importance is the first step toward doing so.

The goal of this brief was to provide helpful information to TJ program practitioners and policymakers, so that they might begin to target limited resources toward the TJ program components most likely to lead to successful outcomes. We have at least accomplished that, but we have also noted several limitations to our analysis that preclude us from making any more definitive statements. It is vital for future research to compare the effectiveness of various TJ programming strategies, especially the ones highlighted in this brief, after program participants have been randomly assigned to receive varying amounts of each.

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APPENDICES

Appendix A. Participation in TJ Program Components, Months 1–6 after RA

TJ Program Components	N	Min	Max	Mean	SD	Data Available?
Number of days worked in a TJ	904	0	132	34.89	30.07	IL, MI, MN, and WI
Ever participated in job readiness/development activities	904	0	1	0.50	0.50	IL, MI, MN, and WI
Number of times participated in job readiness/development	395	0	21	2.38	3.60	IL and MI
Ever had individual case management meeting	904	0	1	0.69	0.46	IL, MI, MN, and WI
Number of times had individual case management meeting	651	0	15	1.80	2.70	IL, MI, and WI
Ever participated in vocational training	509	0	1	0.18	0.39	MN and WI
Number of times received support payment	715	0	17	2.24	2.79	MI, MN, and WI
Amount received in support payments	715	0	$996	$93	$161	MI, MN, and WI
Ever received a retention bonus	342	0	1	0.08	0.27	MN and WI
Number of times received retention bonus	342	0	4	0.15	0.55	MN and WI
Amount received in retention bonuses	342	0	$700	$23	$91	MN and WI

SOURCE: Data from the MDRC baseline information form and TJRD programs themselves.
NOTES: *N*s varied depending on how many TJRD sites provided participation data for that program component. For example, information on job development was provided by TJRD programs in Illinois, Minnesota, and Michigan, but not Wisconsin.

Appendix B. More Details on Methodological Approach

Our methodological approach was non-experimental in nature, in that participants were not randomly assigned to various TJ program components. However, we used regression-based techniques—based on work by Kemple and Snipes (2001) and Zweig, Yahner, and Redcross (2011)—to incorporate information from those in the non-TJ group (the JS group) in an effort to minimize the problem of selection bias common to other explorations of this sort.

In other words, we capitalized on the opportunity presented by the TJRD experimental data to estimate the probability of unsubsidized employment and the risk of recidivism for former prisoners in the TJ program group, as of the time of random assignment, based on our observations of such risk in the JS group and using pre-random assignment characteristics. We then incorporated these estimated probabilities, whose calculations are described in the next section, into our main outcome analyses as statistical controls.

CALCULATING PROPENSITIES FOR EMPLOYMENT AND RECIDIVISM

The logistic regression models used to estimate the propensities for unsubsidized employment and recidivism (assessed separately) as of the time of random assignment are shown in Table B-1 and Table B-2 below. To develop these models, we first examined the bivariate associations between all baseline characteristics and each outcome, within the JS group sample only. All of the variables with bivariate significance ($p < .10$) were then tested in one multivariate model that also included demographic controls for age, race, TJRD site/state, and time since release.[11] In the interest of parsimony, we removed from this model certain variables that showed up as the least statistically significant (e.g., $p > .40$), resulting in a final model for each outcome with the best remaining predictors[12] and the specified controls.

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[11] No controls for supervision status were included, because virtually all respondents (97%) were on parole, probation, or community supervision after their prison release.

[12] Although some predictors in the final models were not statistically significant, they were retained because they approached

The coefficients for the final model predicting propensity to be employed in an unsubsidized job during months 7–12 after random assignment are shown in Table B-1, and for the model predicting propensity to recidivate during the same months are shown in Table B-2. These coefficients represent, in theory, those that would have been found in any similar sample of former prisoners, and especially those that would have been observed in the TJ program group had they not participated in any TJ program components. Thus, we applied these very parameters to the TJ program group to develop estimated propensities of employment and recidivism as of the time of random assignment, based on the TJ program group's pre-random assignment characteristics.

Among those in the TJ program group, estimates of unsubsidized employment propensity ranged from a low of 0.06 to a high of 0.80, while estimates of recidivism propensity ranged from 0.05 to 0.87. As stated previously in the Methods section, these propensities were then used as statistical controls in the main outcome analyses focused on identifying the most successful TJ program components.

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significance and/or their removal would have affected the significance of other included variables, and we were more concerned with retaining predictive validity than reducing multicollinearity. However, the variance inflation factors for all variables in the final models were 2 or less, indicating that multicollinearity was minimally present.

Table B-1. Coefficients Predicting Employment Propensity at Random Assignment

Sample Characteristics at Random Assignment	Coefficient	P-value	Odds Ratio
Age at random assignment (RA)	0.009	0.267	1.009
Black race	-0.425 *	0.021	0.654
Illinois site *(reference is Wisconsin)*	-0.651 **	0.004	0.521
Minnesota site	0.110	0.579	1.117
Michigan site	-1.195 ****	<.0001	0.303
Time from release to RA	-0.001	0.776	0.999
Time served during most recent incarceration	0.003 †	0.095	1.003
High school graduate/GED	0.328 †	0.069	1.388
Worked 6+ mos. for same employer pre-RA	0.427 **	0.007	1.533
Living in same place as preprison	-0.375 *	0.042	0.687
Receiving food stamps	0.555 **	0.003	1.742
Number of prior incarcerations	-0.064 †	0.082	0.938
Number of prior misdemeanor convictions	-0.165 **	0.003	0.848
Intercept	-0.340	0.372	

SOURCE: Data from the MDRC baseline information form and TJRD state employment agencies.
NOTES: Coefficients come from one logistic regression model estimated on *n* = 881 control group participants. Collinearity diagnostics indicate that multicollinearity was not a problem in the model (variance inflation factors were all below 2.0). Significance levels are indicated as † *p* <. 10; * *p* <. 05; ** *p* <. 01; *** *p* <. 001; **** *p* <. 0001.

Table B-2. Coefficients Predicting Recidivism Propensity at Random Assignment

Sample Characteristics at Random Assignment	Coefficient	P-value	Odds Ratio
Age at random assignment (RA)	-0.052 ****	<.0001	0.950
Black race	0.211	0.273	1.235
Illinois site *(reference is Wisconsin)*	-0.430 †	0.069	0.651
Minnesota site	-0.277	0.203	0.758
Michigan site	-0.257	0.259	0.773
Time from release to RA	-0.004	0.267	0.996
Time served during most recent incarceration	-0.003 †	0.065	0.997
Never worked 6 mos. same employer & no HS diploma	0.448 *	0.049	1.565
Had work release job in prison	-0.537 *	0.024	0.584
Living with parents	-0.281	0.138	0.755
Receiving food stamps	0.554 **	0.003	1.741
Family helping with finances	-0.205	0.196	0.814
Number of prior incarcerations	0.123 **	0.002	1.131
Number of prior public order arrests	0.067 *	0.039	1.069
Number of prior property felony convictions	0.138 *	0.019	1.148
Intercept	0.871 *	0.025	

SOURCE: Data from the MDRC baseline information form and TJRD state law enforcement and correctional agencies.
NOTES: Coefficients come from one logistic regression model estimated on *n* = 898 control group participants. Collinearity diagnostics indicate that multicollinearity was not a problem in the model (variance inflation factors were all below 2.0). Significance levels are indicated as † *p* < .10; * *p* < .05; ** *p* < .01; *** *p* < .001; **** *p* < .0001.

Appendix C. Correlations among TJRD Program Components in Months 1–6 after Random Assignment

		Days worked TJ	Job read./ development	# job read./ development	Case mgt.	# case mgt. mtgs.	Vocational training	$ support payments	# support payments	Retention bonus	$ retention bonus	# retention bonus
Days worked TJ	CORRELATION	1	0.29165	0.47492	0.20722	0.1312	0.3845	0.48456	0.52306	-0.0207	0.02849	0.01
	P-VALUE		<.0001	<.0001	<.0001	0.0008	<.0001	<.0001	<.0001	0.7029	0.5995	0.8538
	N	904	904	395	904	651	509	715	715	342	342	342
Job readiness/ development	CORRELATION	0.29165	1	0.38761	0.12447	0.24432	0.16624	0.13423	0.08499	0.15102	0.12796	0.1502
	P-VALUE	<.0001		<.0001	0.0002	<.0001	0.0002	0.0003	0.023	0.0051	0.0179	0.0054
	N	904	904	395	904	651	509	715	715	342	342	342
# job read./ development assistance	CORRELATION	0.47492	0.38761	1	0.03141	0.14819	.	0.31687	0.31687	.	.	.
	P-VALUE	<.0001	<.0001		0.5337	0.0032	.	<.0001	<.0001	.	.	.
	N	395	395	395	395	395	0	206	206	0	0	0
Case mgt.	CORRELATION	0.20722	0.12447	0.03141	1	0.55587	0.15802	0.25173	0.28088	0.10324	0.11975	0.12921
	P-VALUE	<.0001	0.0002	0.5337		<.0001	0.0003	<.0001	<.0001	0.0565	0.0268	0.0168
	N	904	904	395	904	651	509	715	715	342	342	342
# case mgt. mtgs.	CORRELATION	0.1312	0.24432	0.14819	0.55587	1	-0.00929	0.07334	0.34706	0.02124	0.004	0.01659
	P-VALUE	0.0008	<.0001	0.0032	<.0001		0.8824	0.1154	<.0001	0.7352	0.9492	0.7916
	N	651	651	395	651	651	256	462	462	256	256	256
Vocational training	CORRELATION	0.3845	0.16624	.	0.15802	-0.00929	1	0.29682	0.31548	0.01435	0.03596	0.05349
	P-VALUE	<.0001	0.0002	.	0.0003	0.8824		<.0001	<.0001	0.7914	0.5075	0.324
	N	509	509	0	509	256	509	509	509	342	342	342
$ support payments	CORRELATION	0.48456	0.13423	0.31687	0.25173	0.07334	0.29682	1	0.80562	0.21821	0.30747	0.27492
	P-VALUE	<.0001	0.0003	<.0001	<.0001	0.1154	<.0001		<.0001	<.0001	<.0001	<.0001
	N	715	715	206	715	462	509	715	715	342	342	342
# support payments	CORRELATION	0.52306	0.08499	0.31687	0.28088	0.34706	0.31548	0.80562	1	0.16164	0.20203	0.1778
	P-VALUE	<.0001	0.023	<.0001	<.0001	<.0001	<.0001	<.0001		0.0027	0.0002	0.001
	N	715	715	206	715	462	509	715	715	342	342	342
Retention bonus	CORRELATION	-0.0207	0.15102	.	0.10324	0.02124	0.01435	0.21821	0.16164	1	0.85626	0.90256
	P-VALUE	0.7029	0.0051	.	0.0565	0.7352	0.7914	<.0001	0.0027		<.0001	<.0001
	N	342	342	0	342	256	342	342	342	342	342	342
$ retention bonus	CORRELATION	0.02849	0.12796	.	0.11975	0.004	0.03596	0.30747	0.20203	0.85626	1	0.96962
	P-VALUE	0.5995	0.0179	.	0.0268	0.9492	0.5075	<.0001	0.0002	<.0001		<.0001
	N	342	342	0	342	256	342	342	342	342	342	342
# retention bonus	CORRELATION	0.01	0.1502	.	0.12921	0.01659	0.05349	0.27492	0.1778	0.90256	0.96962	1
	P-VALUE	0.8538	0.0054	.	0.0168	0.7916	0.324	<.0001	0.001	<.0001	<.0001	
	N	342	342	0	342	256	342	342	342	342	342	342