



**Colorado Evaluation & Action Lab**

UNIVERSITY OF DENVER

Using data to drive action

# Cash for Coloradans: Center for Employment Opportunities

## Cash Assistance for Justice-Impacted Citizens Preliminary Findings

### REPORT HIGHLIGHTS:

- The study did not find statistically significant effects. The study uncovers suggestive evidence that justice-impacted citizens in the Returning Cash Stimulus (RCS) payment group were less likely to be employed five months post enrollment compared to the control group.
- Differences in participant engagement between the RCS payment and control groups were not statistically significant and mostly small in magnitude.
- Justice-impacted citizens in the RCS group and earlier cohort of control group participants display similar average earnings prior to enrollment. Prior to enrollment, the two groups had similar earnings trajectories.

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## Executive Summary

The workforce development system consists of an array of programs that aim to prepare individuals for meaningful jobs, improve their economic stability, and meet critical industry needs. Some of these programs have robust evidence bases to demonstrate their effectiveness. The purpose of this study is to examine the impact of adding cash assistance to a workforce readiness program for people who recently returned from incarceration.

Cash assistance in the form of \$500 a month for up to six months (for a possible \$3,000 in total) was made available to justice-impacted citizens while they participated in the Center for Employment Opportunities' (CEO) program.

The comparison group for the study consisted of two cohorts of justice-impacted citizens participating in CEO programming before and after the time period when the cash assistance was offered.

### Key Finding: Employment

**No statistically significant differences were detected, but the accumulated evidence suggests that justice-impacted citizens in the Returning Cash Stimulus (RCS) payment group were less likely to be employed five months post enrollment compared to the combined control group. Controlling for baseline characteristics, the difference ranged from 7 to 9 percentage points.**

**The difference indicates that the control group's employment level was approximately 19.7% higher.**

**These findings align with the hypothesis that justice-impacted citizens receiving RCS payments spend more time searching for employment.**

The lower employment rate among RCS recipients, while aligned with the hypothesis that RCS payments afford recipients more time to search for suitable employment, was not statistically significant. The finding leaves open the hypothesis that individuals receiving RCS payments may be more selective in their job search, waiting for better-fitting opportunities rather than taking the first available job. Alternatively, this finding also aligns with the hypothesis that cash assistance allows participants to delay what would have been their normal search process. Furthermore, there was not a statistically significant difference in CEO program engagement between the RCS and control groups. Thus, more research is needed to understand if and how cash assistance is affecting justice-impacted citizens' employment.

## Key Finding: Pre-Enrollment Earnings

**Justice-impacted citizens in both the RCS group and earlier control group display similar average earnings prior to enrollment.**

**A comparison of average earnings finds no statistically significant differences in all pre-enrollment quarters, suggesting that the earlier control group may be a valid comparison for the RCS group in future analyses.**

Given that the long-term goal of this project is to assess the impact of RCS payments on justice-impacted citizens' employment and earnings, it was important to determine if the study control group was comparable to the RCS (treatment) group prior to enrolling in CEO. Preliminary trends in earnings pre-enrollment align with the narrative that participants seek CEO services due to challenges in maintaining employment and earnings. Furthermore, the absence of significant earnings differences prior to enrollment supports a future Difference-in-Differences or event-study analysis.

## Recommendations for Ongoing Evidence Building

The current evaluation does not provide causal evidence on the effectiveness of cash assistance as implemented by the RCS program. To strengthen future evaluations of RCS payments, the study authors recommend that CEO work with evaluation partners to solidify a theory of change to guide data collection in alignment with program goals, maintain efforts to increase sample sizes to improve the ability to detect impacts and refine analysis for subgroups, and focus on collecting additional measures of job satisfaction and stability. In addition to a longer evaluation horizon, these steps would support a more comprehensive understanding of the effects of cash assistance on justice-impacted citizens' post-incarceration employment and earnings.

# Table of Contents

<b>Executive Summary</b> .....	<b>i</b>
Key Finding: Pre-Enrollment Earnings .....	ii
Recommendations for Ongoing Evidence Building .....	ii
<b>Table of Contents</b> .....	<b>iii</b>
<b>Acknowledgements</b> .....	<b>iv</b>
<b>Data Sources</b> .....	<b>iv</b>
<b>Suggested Citation</b> .....	<b>iv</b>
<b>Note on the Term “Justice-Impacted Citizen”</b> .....	<b>v</b>
<b>Introduction</b> .....	<b>1</b>
About the Center for Employment Opportunities .....	1
Evaluation Objectives .....	3
<b>Description of the Study</b> .....	<b>3</b>
Research Questions .....	3
Literature Review: Cash Assistance and Workforce Development .....	5
<b>Key Findings</b> .....	<b>6</b>
Program Engagement .....	7
Earnings.....	7
<b>Recommendations for Ongoing Evidence Building</b> .....	<b>8</b>
<b>Methods</b> .....	<b>9</b>
Analytic Sample.....	9
Data Elements.....	9
<b>Results</b> .....	<b>19</b>
Limitations .....	26
Ongoing Evidence Building .....	27
Discussion of Implications.....	28
<b>Conclusion</b> .....	<b>28</b>
<b>Appendix A: Data Glossary</b> .....	<b>30</b>
<b>Appendix B: Average Earnings and Sample Sizes</b> .....	<b>32</b>
<b>Endnotes</b> .....	<b>33</b>

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The opinions expressed are those of the authors and do not represent the views of the State of Colorado, Center for Employment Opportunities, CEEMI, or the University of Denver. Policy and budget recommendations do not represent the budget or legislative agendas of state agencies, the Governor's Office, or other partners. Any requests for funding or statutory changes will be developed in collaboration with the Governor's Office and communicated to the legislature through the regular budget and legislative processes.

*Thank you to our partners who provided subject matter expertise and guidance on this project: Roger Low and David Anderson at CEEMI, and Ahmed Whitt and Noely Drummond at Center for Employment Opportunities (CEO).*

## Data Sources

This study uses data from two primary sources:

1. CEO.
2. Colorado Department of Labor and Employment (CDLE).

The Colorado Evaluation and Action Lab at the University of Denver (Colorado Lab) received intake and programmatic data directly from CEO. CDLE earnings records were accessed through the Linked Information Network of Colorado (LINC).

LINC is a collaborative effort of the Colorado Lab and the Colorado Governor's Office of Information Technology that safely and securely connects and anonymizes data across state agencies and systems to fully inform solutions to specific societal challenges. The data used for this report came from a LINC project approved by participating data partners. The findings do not necessarily reflect the opinions of the Colorado Governor's Office of Information Technology, the Colorado Lab, or the organizations contributing data.

## Suggested Citation

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## **Note on the Term “Justice-Impacted Citizen”**

The Colorado Lab affirms our commitment to the use of person-first language. Throughout this report, we refer to formerly incarcerated individuals as “justice-impacted citizens.” This is the preferred language used by CEO and is part of a larger effort to reduce the stigma surrounding formerly incarcerated individuals.

## Introduction

The workforce development system consists of an array of programs that aim to prepare individuals for meaningful jobs, improve their economic stability, and meet critical industry needs. Some of these programs have strong evidence bases to demonstrate their effectiveness. The purpose of this study is to examine the impact of adding cash assistance to a workforce readiness program for people who recently returned home from incarceration. Cash assistance in the form of up to \$500 a month for up to six months (for a possible \$3,000 in total) was made available to justice-impacted citizens while they participated in the Center for Employment Opportunities' (CEO) evidence-based program. The comparison group for the study consisted of two cohorts of justice-impacted citizens participating in CEO before and after the time period when the cash assistance was offered.

The evaluation of CEO's Returning Cash Stimulus (RCS) pilot program (i.e., up to \$3,000 cash assistance) is part of a larger WorkRise funded project titled *Cash for Coloradans*. The Cash for Coloradans project aims to understand the impacts of financial assistance on learners' participation in evidence-based training programs, and ultimately their labor market outcomes. In a [companion report](#), the Colorado Evaluation and Action Lab (Colorado Lab) evaluated the impact of offering no-interest loans alongside a verified program model.

## About the Center for Employment Opportunities

CEO works to reduce recidivism and increase employment by providing people returning from prison, referred to as "justice-impacted citizens," with immediate paid employment, skills training, and ongoing career support. Through its numerous sites in Colorado and across the U.S., CEO supports economic stability and positive social outcomes for justice-impacted individuals

CEO guarantees every justice-impacted citizen participant who completes their paid job-readiness orientation a spot on their transitional work crews and daily pay. CEO operated transitional work crews provide supplemental indoor and outdoor maintenance and neighborhood beautification services to more than 40 companies across the United States. CEO also provides a robust set of wraparound vocational support services, including job coaching to find full-time employment and a year of retention services once participants find a

*"Once you're involved with the justice system, a lot of opportunities get taken away. CEO gives the opportunity to gain confidence and make a living, which goes hand in hand. To know that there's places like CEO that can level the playing field is valuable—that's the difference between a person committing crimes and not."*

- Justice-impacted citizen and program participant Demetrius

job to ensure that they have the support they need to grow in their careers.<sup>i</sup> According to CEO, justice-impacted citizens generally have been out of prison for one year or less.

A 2012 randomized controlled trial of CEO's Transitional Jobs Program demonstrated that the program's treatment group was significantly less likely than the control group to be convicted of a crime and to be incarcerated over a three-year follow-up period. Specifically, when compared to the control group, the program treatment group experienced a 5-percentage-point reduction in measures of recidivism.<sup>ii, 1</sup> A later 2018 New York State Division of Criminal Justice Services evaluation found that justice-impacted citizens were 48% more likely to be employed and 19% less likely to be re-convicted or re-arrested for a felony three years post-enrollment than their counterparts who did not participate in the program.<sup>2</sup>

In April of 2020, CEO launched the RCS program in response to the COVID-19 pandemic. It provided monthly cash transfers to people who were recently released from incarceration in 28 cities across the United States to ease their transition into society at a time of social distress and high unemployment. An initial MDRC evaluation of the 2020 RCS pilot revealed that over 90% of RCS participants met program milestones. Participants shared that the financial assistance offered a crucial sense of stability, enabling them to focus on rebuilding their lives post-incarceration. The majority used their RCS funds for essential needs such as rent, groceries, and clothing, as well as resources to prepare for employment.

**A mixed methods evaluation of CEO's 2020 RCS program in four U.S. cities found that cash assistance recipients reported feeling more financially stable, and nearly two out of three recipients reached an employment milestone.<sup>3</sup>**

In a forthcoming follow-up study (early 2025), MDRC further notes reductions in parole violation, including violent infractions, within the first six months, highlighting the immediate impact of cash assistance in reducing reentry challenges.

In July of 2023, CEO began supplementing its model with cash assistance for justice-impacted citizens in Denver and Colorado Springs, Colorado. Enrollees were eligible for monthly payments if they met program benchmarks, such as preparing resumes or attending an employment workshop. The RCS model was rolled out quickly and all accepted enrollees were eligible. As such, a randomized controlled trial program evaluation of the RCS pilot was not possible at the time.

CEO administered the RCS pilot as a direct payment program that provided up to \$500 a month for up to six months. Recipients must have met ongoing CEO program participation benchmarks to continue receiving program funds. There was no expectation of repayment and participants were

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<sup>i</sup> For more information on CEO programming and what justice-impacted citizens experience as they progress through the program, visit <https://www.ceoworks.org/our-model>.

<sup>ii</sup> The authors (Redcross et al., 2012) found an immediate boost in overall employment levels driven entirely by the transitional jobs themselves. Impacts on earnings are mixed but are generally not statistically significant.



allowed to use the cash assistance as they sought fit (i.e., an “unconditional use” cash assistance program).

## Evaluation Objectives

The purpose of the RCS pilot evaluation is to assess the impact of adding direct cash payments to CEO’s package of services on justice-impacted citizens’ employment, program participation, and earnings. The evaluation consists of two phases, the first phase analyzing preliminary outcomes in the short term and the second phase outcomes in the long term. This current report documents the results of Phase I, specifically evaluating the impact of RCS receipt on employment outcomes five months after enrollment and CEO program participation metrics 10 months after enrollment. Previewing Phase II, this report explores preliminary evidence on earnings pre-enrollment.

## Description of the Study

### Research Questions

This study addresses the following three research questions.

**Research Question #1. What is the impact of receiving RCS payments on the employment of justice-impacted citizens five months after enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments spend more time searching for employment.**

CEO collects information on unsubsidized job start dates, enabling researchers to identify if and when a justice-impacted citizen began a new job within a specified period of time.

**Research Question #2. What is the impact of receiving RCS payments on justice-impacted citizen’s program engagement 10 months after enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments report greater engagement in CEO programming.**

Participation in the CEO program is measured using a set of program milestones and tracking the number of days a justice-impacted citizen works in a CEO-provided transitional job.

**Research Question #3. What is the impact of receiving RCS payments on the earnings of justice-impacted citizens post enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments experience a steeper earnings trajectory post enrollment.**

An early assessment of CEO wage data found that there was a significant missing data problem, and of the data that was available, there is concern regarding who reported wages and who did not. To overcome this challenge, the Colorado Lab created a pathway through its partnership with the Linked Information Network of Colorado (LINC) to access quarterly earnings data from the Colorado Department of Labor and Employment (CDLE). For the subset of justice-impacted citizens available in CDLE records, the Colorado Lab documents the average earnings trajectory of justice-impacted citizens pre- and post-enrollment.

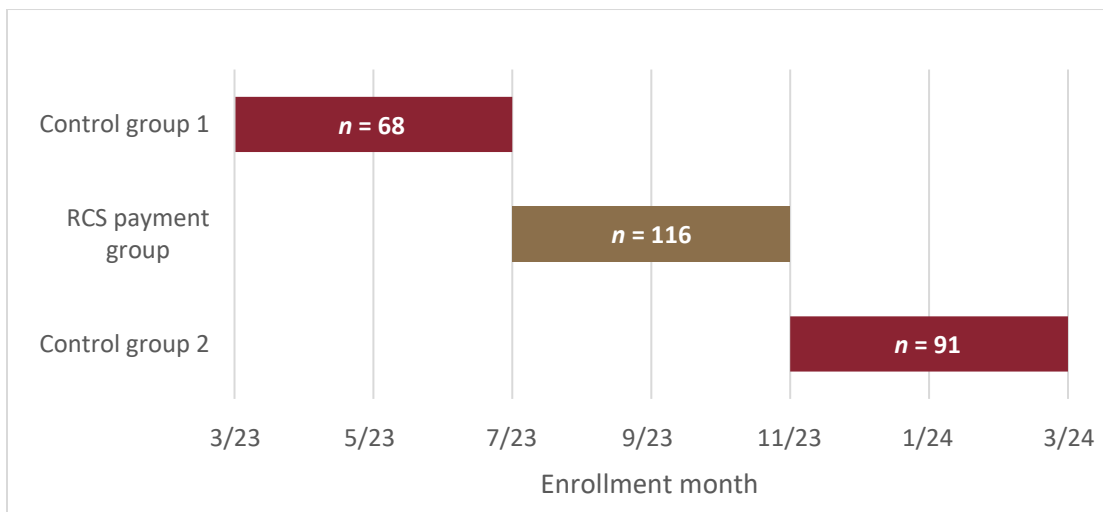
The evaluation methods include:

- Descriptive analyses
- Comparisons of outcomes between the RCS recipient cohort and two control cohorts
- Trend analyses
- Multivariate regression

The intuition behind the multivariate regression model specification is based on a canonical 2-by-2 difference-in-differences (DiD) design and previews a rigorous Phase II quasi-experimental design (QED). Analyses leverage three groups:

- RCS payment “treatment” group: Enrollees received CEO’s regular programming, including transitional employment, plus \$500 cash assistance per month for up to six months as described above (enrolled July through October 2023).
- Control group 1: Enrollees in the four months *prior* to the cash assistance period (March through June 2023). Control group 1 received CEO’s regular programming, including transitional employment.
- Control group 2: Enrollees in the four months *after* the cash assistance period (November through February 2024). Control group 2 received CEO’s regular programming including transitional employment.

**Figure 1. Timeline of Enrollment by Group**



## Difference-in-Differences Designs

A DiD design, or “controlled before and after” study, is a QED design that aims to estimate a causal effect by comparing two or more arguably similar groups over time. In the simplest case, key average outcomes are measured at two points in time, for example, pre and post a treatment or intervention. The name “difference-in-differences” comes from the fact that an initial difference at baseline is “differenced out” from the ending difference, effectively treating the comparison as what would have occurred absent the treatment or intervention.

## Literature Review: Cash Assistance and Workforce Development

Financial assistance, including conditional or unconditional cash payments, have been an integral part of upskilling and work training programs historically.<sup>4</sup> Financial assistance has included stipends aimed at alleviating childcare or transportation costs, loan forgiveness programs, individual training and incentive accounts, paid apprenticeships programs which combine on-the-job training with classroom instruction, and federal grants or loans for formal education or training.<sup>iii</sup>

When used to facilitate job search, cash assistance has been shown to increase earnings and the number of hours worked.<sup>5</sup> Cash assistance helps ease immediate financial pressures and reduces costs associated with finding a job. This is because when workers are unemployed, they may have difficulty meeting their basic needs, such as food and housing, thus making it difficult to focus on searching for a job. In addition, cash assistance enables job search by helping to pay for transportation costs to job interviews or for childcare costs while the individual is searching.<sup>iv, 6, 7, 8, 9, 10, 11</sup>

Participants in workforce training programs have different immediate and long-term financial needs, perceptions of debt, and future earnings potential, so the type of financial support offered may impact participation and completion rates. Cash assistance without the burden of repayment reduces stress and enables participants to fully engage with training by covering transportation, childcare, or forgone earnings. The simplicity, immediacy, and lack of future obligation make cash transfers attractive, so the hypothesis for this evaluation was that CEO’s RCS program would

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<sup>iii</sup> Barnow and Smith (2015) provide an in-depth review of means-tested U.S. federal training programs, including the Job Training Partnership Act, the Workforce Investment Act, the Workforce Innovation and Opportunity Act, the Job Corps program, and the Trade Adjustment Assistance program.

<sup>iv</sup> Conditional and unconditional cash assistance has been shown to offer added protection against financial hardships and reduce negative health and education outcomes (National Academy of Sciences, 2019), reduce criminality and substance use (Marinescu, 2018), lower household food insecurity and homelessness (Dwyer et al., 2023; Shaefer et al., 2019), and child maltreatment and mortality (Bullinger et al., 2023). There are fewer representative long-term studies. Aizer et al. (2016) find that cash transfers to poor white mothers increase their son’s longevity and educational attainment.

increase participation.<sup>v</sup> That said, cash assistance can carry a social stigma, potentially deterring participation.<sup>12</sup>

A well-known behavioral concern with cash assistance is the potential for counterproductive labor supply effects. One way this manifests is in talent delaying entering or returning to the workforce. With permanent and semi-permanent cash transfer programs, economic theory suggests that no-strings attached payments can disincentivize formal employment (e.g., via reduced hours or levels of full-time employment). That said, empirical evidence from *temporary* workforce development programs finds that the reality is murky. Marinescu (2018) reviews evidence from the U.S. and finds many studies find no statistically significant effect of unconditional cash assistance on the probability of working.<sup>13</sup> In the studies that do find negative effects, the effects are small. When payments are permanent, as in the case of the small yearly payments studied by Jones and Marinescu (2022), there were no significant reductions in employment rates.<sup>14</sup> These findings suggest that the risk to the labor market is either small or inconsequential, and thus if there are benefits to employees, cash assistance is a viable tool for workforce development.

## Key Findings

### Employment

**No statistically significant differences were detected, but the accumulated evidence suggests that justice-impacted citizens in RCS payment group were less likely to be employed five months post enrollment compared to the combined control group. Controlling for baseline characteristics, the difference ranged from 7 to 9 percentage points.**

**The difference indicates that the control group's employment level was approximately 19.7% higher.**

**These findings align with the hypothesis that justice-impacted citizens receiving RCS payments spend more time searching for employment.**

Though not statistically significant, analysis of employment five months post enrollment points to RCS recipients being less likely to take an outside job within the first five months of enrollment compared to justice-impacted citizens who did not receive RCS payments. In line with economic theory, a subset of empirical studies, and limited anecdotal evidence, this possibility is not surprising as RCS group enrollees may not have felt the urgency to take the first job with which they were presented. In practice, one of the benefits of RCS payments is to afford justice-impacted citizens time to wait for a job that fits them best. Alternatively, this finding also aligns with the hypothesis that cash assistance allows participants to delay what would have been their normal

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<sup>v</sup> According to CEO staff, there is no documented evidence suggesting greater interest in CEO during the RCS period. One reason for this could be that enrollment is directly connected with the timing and rate at which justice-impacted citizens exit prison. That said, it could impact the perceptions of subsequent cohorts of justice-impacted citizens.

search process. Without additional information on job fit or turnover, we cannot disentangle the two possibilities.

## Program Engagement

**Differences in participant engagement between the RCS payment and control groups were not statistically significant and mostly small in magnitude.**

**Descriptive analysis suggests that RCS payment recipients were less engaged than their control group peers. This result should be interpreted with caution because participation measures were not recorded at the same point in time for the combined control group.**

**These findings do not support the hypothesis that justice-impacted citizens receiving RCS payments report greater program engagement. That said, they also do not provide definitive evidence against the hypothesis.**

The measures used to investigate program engagement include the percent of program completers, the percent of program non-completers, and the number of transitional job workdays. The combined control group had, overall, 3.4 percentage points more program completers, 7.6 percentage points fewer program non-completers, and justice-impacted citizens who work 1.2 more transitional job days compared to the RCS group. While not statistically significant and relatively small in magnitude, all three measures point in the direction of the RCS recipients having been less engaged.

## Earnings

**Justice-impacted citizens in both the RCS and control group 1 display similar average earnings prior to enrollment.**

**A comparison of average earnings finds no statistically significant differences in all pre-enrollment quarters, suggesting that control group 1 may be a valid comparison for the RCS group in future analyses.**

For both RCS and control group 1, inflation adjusted average total quarterly earnings displayed a prolonged period of stagnation prior to program enrollment. Nearer to enrollment, we saw a dip in the average total quarterly earnings. This dip is consistent with program participants experiencing a change in their financial situation and reflects the reasons justice-impacted citizens may choose to seek CEO's services, including trouble finding or keeping a job. After program enrollment, average total quarterly earnings for control group 1 recovered quickly, surpassing pre-enrollment levels.<sup>vi</sup>

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<sup>vi</sup> This is often referred to as "Ashenfelter's Dip" in the workforce development literature.

Statistical tests revealed no significant differences in average earnings prior to enrollment, establishing that control group 1 may be a valid comparison group for the RCS group when conducting a rigorous DiD or event-study design.

## Recommendations for Ongoing Evidence Building

The current evaluation does not provide causal evidence of the effectiveness of cash assistance in the form of RCS payments. This may be due in part to the following limitations:

- A compressed evaluation timeline.
- Incomplete wage and job satisfaction data.
- Insufficient measures of job turnover.
- A lack of timestamps on participation variables.
- Underenrollment in the control groups.

The goal of the recommendations listed below is to build evidence for pairing cash assistance with CEO's standard program to reach the goals of justice-impacted citizens securing stable, satisfying employment, and preventing re-involvement in the criminal justice system. These recommendations are likely best activated by CEO in partnership with an external evaluator.

The Colorado Lab recommends a two-year evaluation that includes:

- Solidifying a theory of change based on economic theory and participant experiences that aligns with CEO's programmatic goals. A formal theory of change can guide both the Colorado Lab and CEO in determining what data to collect and center.
- Data collection and processing safeguards to ensure data quality.
- Enlarging sample sizes for a QED so that there are enough observations to detect impacts and refine analyses by subpopulations of interest.
- Incorporating data on job satisfaction and stability to test the theory of change.
- Tracking of participant outcomes over multiple years, including earnings and criminal justice re-involvement.

A re-introduction of legislation that funds a statewide RCS program in a future legislative cycle is an opportunity to request resources to activate these recommendations and gain comprehensive information on the effects of cash assistance.

Through recent interviews, both CEO staff and participants have highlighted that cash assistance has contributed significantly to participants' sense of security and optimism during their reentry journey. Feedback suggests that the assistance could be even more effective if paired with a financial literacy requirement and, potentially, provided as a larger upfront lump sum. This approach bolsters participants' stability more immediately, allowing them to prioritize community

reintegration, rebuilding social connections with family and peers, and focusing on securing meaningful employment opportunities aligned with their long-term goals.

## Methods

### Analytic Sample

A total of 275 justice-impacted citizens were enrolled in the study period, beginning in March 2023 and ending in February 2024. Justice-impacted citizens were divided into three groups based on the date when they enrolled, as displayed in Figure 1.

*Control Group:* The motivation for creating two control groups stems from the implementation of the RCS pilot. All enrollees between July and October participated in the RCS pilot, thus there was no contemporaneous control group available. The collection of enrollees in the pre- and post-RCS period helped mitigate concerns that the control would not be comparable due to some specific feature of when justice-impacted citizens enrolled (e.g., seasonal trends in employment). A second control group can also be used to strengthen a DiD design by offering an additional comparison. Finally, control groups can be combined to increase sample sizes and estimate precision.<sup>vii</sup>

### Data Elements

Data came from CEO’s internal administrative records and CDLE. CDLE’s W-2 earnings records were matched and anonymized by LINC prior to analysis. Table 1 provides details on the data elements included in the report and their sources. All data is at the participant level. For additional information on the variables included in the analytic sample, see [Appendix A](#).

**Table 1. Data Elements Used in the Analysis and Their Sources**

Participant Characteristics	Data Source
Demographics: <ul style="list-style-type: none"> <li>● Enrolled through the Colorado Springs or Denver site</li> <li>● Age</li> <li>● Sex</li> <li>● Marital status</li> <li>● Race/ethnicity</li> <li>● Preferred language</li> <li>● Number of children under 18 years old</li> <li>● Highest level of educational attainment</li> </ul>	Intake form

<sup>vii</sup> Control group 2 can only be used in the research questions addressing employment and program engagement.

Participant Characteristics	Data Source
Program engagement <ul style="list-style-type: none"> <li>● Enrollment date</li> <li>● RCS benefit recipient</li> </ul>	Intake form
Employment: <ul style="list-style-type: none"> <li>● Previous work experience</li> <li>● Employment restrictions</li> <li>● Curfew status</li> <li>● Driver’s license status</li> </ul>	Intake form
Program engagement outcomes: <ul style="list-style-type: none"> <li>● Current program stage name</li> <li>● Total number of days worked in a CEO transitional job</li> </ul>	Follow-up progress reports or researcher generated
Employment outcomes: <ul style="list-style-type: none"> <li>● Start date of first job since program enrollment</li> <li>● Employment status at 90 and 150 days from enrollment</li> <li>● Number of days between program enrollment and first employment</li> </ul>	Follow-up progress reports or researcher generated
Employment outcomes: <ul style="list-style-type: none"> <li>● Start date of first job since program enrollment</li> </ul>	Follow-up progress reports or researcher generated
Earnings: <ul style="list-style-type: none"> <li>● Quarterly unemployment-insurance-qualifying earnings two years prior to enrollment and up to three quarters after enrollment from W-2 receipts</li> </ul>	Colorado Department of Labor and Employment

### Characteristics of Justice-Impacted Citizens at Intake

The justice-impacted citizens participating in this study were on average 40 years old. Over 84% were male, 38% were White, 24% were Black, and 26% were Hispanic/Latino. Over 21% of justice-impacted citizens did not have a high school diploma or GED.

The characteristics of justice-impacted citizens in the RCS group and combined control group are presented in Table 2. Given the staggered timing of the cohorts and CEO’s publicity efforts, participants in the later cohorts had the potential to be influenced by earlier cohorts. Analyses were conducted comparing each control group separately. We found no practical differences between control group 1 and control group 2.

When comparing the percentage distributions, a few noteworthy differences jump out. The RCS group tended to be more likely to identify as Black, but not Hispanic/Latino. The RCS payment



group was also less likely to be in possession of a driver’s license. These differences could be an artifact of CEO site location—RCS participants were more likely to be based out of the Colorado Springs site.

Table 2 shows sample sizes, observation counts and percentages (in parenthesis) for categorical variables, means and standard deviations (in parenthesis) for continuous variables,  $p$ -values from either a pooled  $t$ -test or Pearson’s chi-square test, and standardized effect sizes according to Hedges’  $g$  or Cox’s Index depending on if the variable is continuous or binary and categorical, respectively.

**Table 2. Comparison of RCS and Combined Control Groups**

Measures	RCS Group	Combined Control Group	$p$ -value	Hedges’ $g$ or Cox’s Index
Sample size	116	159		
CEO Site (%):			0.22	0.18
• Denver	49 (42.24%)	79 (49.69%)		
• Colorado Springs	67 (57.76%)	80 (50.31%)		
Age (years)	39.88 (11.40)	40.88 (12.66)	0.50	0.08
Sex (%):			0.63	0.094
• Male	96 (82.76%)	135 (84.91%)		
• Not male	20 (17.24%)	24 (15.09%)		
Race (%):			0.10	0.27
• Black	34 (29.31%)	33 (20.75%)		
• Not Black	82 (70.69%)	126 (79.25%)		
Ethnicity (%):			0.14	0.25
• Hispanic/Latino	25 (21.55%)	47 (29.56%)		
• Not Hispanic/Latino	91 (78.45%)	112 (70.44%)		
Children Under 18 years old (counts)	0.92 (1.50)	0.86 (1.35)	0.73	0.04
Marital Status (%)			0.70	0.07
• Single	76 (68.47%)	106 (71.14%)		
• Married	12 (10.81%)	10 (6.71%)		
• Divorced	19 (17.12%)	28 (18.79%)		

Measures	RCS Group	Combined Control Group	<i>p</i> -value	Hedges' <i>g</i> or Cox's Index
Education (%)			0.69	0.04
<ul style="list-style-type: none"> <li>No high school degree</li> </ul>	23 (19.83%)	32 (20.13%)		
<ul style="list-style-type: none"> <li>High school degree.</li> </ul>	63 (54.31%)	76 (47.80%)		
<ul style="list-style-type: none"> <li>Some college</li> </ul>	25 (21.55%)	39 (24.52%)		
Curfew Status (%)			0.87	0.03
<ul style="list-style-type: none"> <li>Has curfew</li> </ul>	51 (43.97%)	68 (42.77%)		
<ul style="list-style-type: none"> <li>Does not</li> </ul>	61 (52.59%)	78 (49.06%)		
Driver's License			0.31	0.16
<ul style="list-style-type: none"> <li>Has license</li> </ul>	37 (31.90%)	58 (36.48%)		
<ul style="list-style-type: none"> <li>Does not</li> </ul>	74 (63.79%)	89 (55.97%)		

**Notes:** With categorical variables, counts that do not add up to the total sample sizes indicate missing values. Hedge's *g* is calculated by taking the difference in group means divided by the pooled standard deviation assuming the variances of the groups are unequal, while Cox's Index is calculated by taking the difference in the proportion of stipend and control groups. Both Hedge's *g* and Cox's Index include adjustments for small sample sizes. For education, the index calculates the effect size for the binary indicator {No High School Degree/GED, Has High School Degree/GED}. For marital status, the index calculates the effect size for the binary indicator {Single, Not Single}.

## Baseline Equivalence

Assessing baseline equivalence provides information on whether the RCS and control groups differ meaningfully on observable characteristics. The assessment of baseline equivalence of the groups leveraged the data collected at intake (Table 2). The [What Works Clearinghouse \(WWC\) guide](#) was used to determine whether the differences between groups were large. Specifically, a standardized effect size of more than 0.05 indicates groups are moderately imbalanced and effect sizes of greater than 0.25 are strongly imbalanced.<sup>viii</sup>

For the two continuous variables, age and number of children under 18 years old, Table 2 reports means and standard deviations. The *p*-values are calculated from a pooled *t*-test without the assumption that there is similar variability in their distributions. For the remaining variables, the *p*-value is calculated from a Pearson's chi-square test regardless of whether the categorical variable is binary (e.g., male/not male) or not binary (e.g., single/married/divorced). A chi-squared test for equality of proportions is a non-parametric test (i.e., not assuming a distribution) used to test

<sup>viii</sup> According to WWC standards, a standardized mean difference value less than or equal to 0.05 satisfies baseline equivalence, a value of between 0.05 and 0.25 requires a statistical adjustment to satisfy baseline equivalence, and a value greater than 0.25 does not satisfy baseline equivalence.

whether the observed frequency distribution of the categorical variables is significantly different from another frequency distribution.

Standardized effect sizes are calculated using Hedges'  $g$  or Cox's Index.

- Hedge's  $g$  is calculated by taking the difference in group means divided by the pooled standard deviation, assuming the variances of the groups are unequal.
- Cox's Index is calculated by taking the difference in the logged prevalence of an outcome in RCS and the logged prevalence in the control group.

Both Hedge's  $g$  and Cox's Index include adjustments for small sample sizes.<sup>ix</sup>

As previewed earlier, identification as Black or Hispanic/Latino exhibited the strongest differences between RCS and control groups. Both  $p$ -values were at or near the 10% significance threshold and Cox's Index met the 0.25 cutoff, which suggests a strong imbalance. Upon closer inspection, the imbalance comes from control group 2. Control group 2 had a smaller proportion of Black enrollees and a higher proportion of Hispanic/Latino enrollees when compared to either the RCS group or control group 1. Both differences are statistically significant at the 5% level. Again, these demographic differences may have been a consequence of site location, as control group 2 had a lower proportion of participants based out of the Colorado Springs site.

None of the other characteristics displayed differences large enough to be considered close to statistically significant at the 10% significance threshold. Nonetheless, site location, age, sex, marital status, and possession of a driver's license all displayed varying degrees of moderate imbalance according to either their Hedge's  $g$  or Cox Index.<sup>x</sup>

Baseline equivalence can only be established on characteristics that can be measured. Assessing baseline equivalence provides insight into potential differences in the groups that may influence the analysis by confounding the impact of the RCS. Imbalances suggest that the groups are dissimilar with respect to *unobservable* individual characteristics too. Given the staggered rollout of the RCS program and inability to implement an experimental design that randomized RCS receipt, it is not surprising to see that the groups are not equivalent along all measures. Due to the imbalance, it will be important to make regression adjustments when estimating the impact of RCS receipt on employment.

## Missing Data

Justice-impacted citizens typically enroll within one year of incarceration. Participants who leave the program or stop communicating with CEO, but were observed at intake, will not have follow-up data. For program engagement, missing data is an outcome itself, as it is an indicator of whether a

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<sup>ix</sup> When interpreting Cox's Index, it is important to note that the index is insensitive to sample sizes, meaning that differences from both large and small samples will result in similar values.

<sup>x</sup> The Cox Index calculates a standardized effect size using proportions from a binary variable. Thus, for variables like education and marital status, we chose to calculate the index for binary indicators {No High School Degree/GED, Has High School Degree/GED} and {Single, Not Single}.

justice-impacted citizen completed CEO programming. For the employment analysis, missing data could indicate a loss of contact or that the justice-impacted citizen has not yet secured employment. While the analysis must be agnostic, CEO makes a concerted effort to confirm when justice-impacted citizens secure jobs. As a consequence, the employment analysis relies on instances of confirmed employment and missing data are assumed to be either unconfirmed or unemployed.

## Analytical Approach

The evaluation methods include:

- Descriptive analyses
- Comparisons of outcomes between the RCS recipient cohort and two control cohorts
- Trend analyses
- Multivariate regression

The intuition behind the multivariate regression model specification is based on a canonical 2-by-2 DiD design and previews a rigorous Phase II QED. When possible, control groups 1 and 2 will be combined to increase the possibility of detecting significant differences.<sup>xi</sup>

The following section outlines the analytic approach tailored to each of the three research questions. For clarity, research questions are re-stated prior to discussing the analytical methodologies.

**Research Question #1: What is the impact of access to the RCS program on the employment of justice-impacted citizens five months after enrollment?**

CEO collects information on unsubsidized job start dates, enabling researchers to identify if and when a justice-impacted citizen began a new job within a specified period of time.

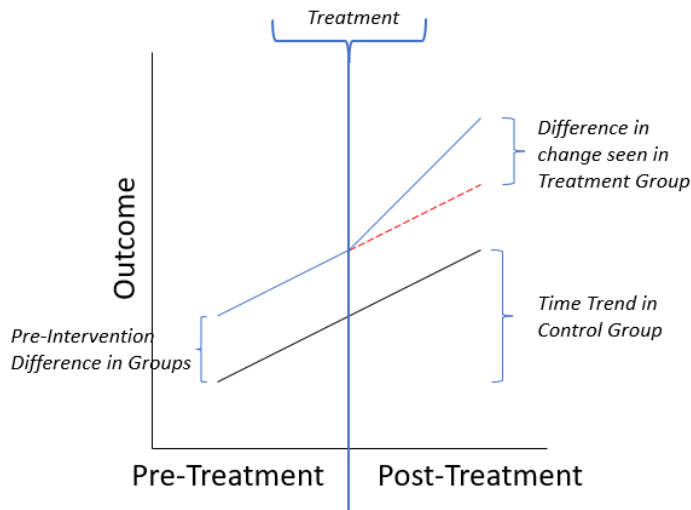
**Difference-in-Differences Designs.** The approach to this research question follows the intuition behind DiD designs. A DiD is a QED and are also known as “controlled before and after” studies. DiD analysis allowed CEO to implement the RCS funding for a complete cohort of justice-impacted citizens and compare outcomes to a different control cohort who did not receive RCS funds. Key to the design is the measurement of outcomes at two points in time; in other words, before and after enrollment.

The simplest of DiD designs is the 2-by-2, meaning two arguably comparable groups are measured at two points in time. In practice, this implies that outcomes for both the RCS and control groups are collected prior to program start and at some later point in time (e.g., five months after enrollment). Figure 2 provides the graphical intuition behind the 2-by-2 DiD design.

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<sup>xi</sup> Both groups did not reach the 100-enrollee threshold.

**Figure 2. 2-by-2 Difference-in-Differences Graphical Illustration**



Suppose the RCS group is represented by the blue line and a control group represented by the black line. In a 2-by-2 design, we measure an average outcome in the pre-treatment period and in the post-treatment period for both groups (i.e., the end points of each line). The name “difference-in-differences” comes from the fact that the initial vertical difference in outcomes is removed from the ending vertical difference. This result of “differencing out” the initial difference is labeled as the *difference in change seen in the treatment group* on the graphic and reflects the causal effect of the treatment.

As illustrated in Figure 2, in a DiD design, equivalence of an outcome at baseline is *not* required. What is required is that the difference initially observed does not change over time. The parallel trends assumption states that in the absence of RCS payments, average outcomes for the RCS and control groups would have evolved “in parallel” as illustrated by the red-dashed line in the graphic. If the treated group would have followed the same trend they were previously on, any break from that trend is attributed to the RCS payments.

**Strengths of the DiD Approach.** A DiD design can be executed using a linear or non-linear multivariate regression. Using multivariate regression analysis provides several practical advantages for the evaluation of the RCS pilot.

- Systematic differences between RCS and control groups can be accounted for explicitly by including them as controls in the model. Doing so would mitigate biases produced by the omission of those factors.
- Multiple time points can be incorporated into the model, allowing for progressive assessments of RCS payments over time.
- There can be flexibility in the outcomes assessed, so that binary outcomes like employment status can be similarly compared to continuous outcomes like earnings.

**Limitations of the DiD Approach.** As with all regression and matching-based approaches, systematic differences between RCS and control groups that are *not* observed can still bias estimates. This concern is addressed by assuming that the unobserved factors impacting both RCS enrollment and outcomes are adequately proxied for by the variables used as controls in the multivariate regression.

A second concern, also not specific to DiD, arises if there are large differences in attrition rates across groups. Estimates will be biased because they would be based on only remaining participants, who could be different in various ways from the original sample (e.g., more determined or likely to succeed and better resourced).<sup>xii, 15, 16</sup> In the employment analysis, we have no way to distinguish between a person who has a job but could not be contacted and a person who does not have a job. Therefore, we must assume attritors are similar across comparison groups.<sup>xiii</sup>

Within the context of employment, average outcomes at intake are the same since enrollees all start with the same employment status. Thus, it can be shown mathematically that the 2-by-2 DiD estimator collapses to a simpler post-outcomes comparison. That said, we can maintain some of the intuitive strengths of the design.

**Research Question #2: What is the impact of access to the RCS program on justice-impacted citizen program engagement 10 months after enrollment?**

Participation in the CEO program is measured using a series of program milestones and counting the number of days a justice-impacted citizen works in a CEO-provided transitional job. CEO is well aware that both are imperfect measures of participant success. For example, consider the number of days a justice-impacted citizen works in a transitional job first. Suppose a person enrolls, works one month in a transitional job, then finds a good paying full-time job. They would record fewer transitional workdays compared to a person who has worked for two months in the CEO-provided transitional job before finding a job. In this scenario, fewer transitional workdays would not be an indicator of program success or failure. From CEO's perspective, either outcome would be a successful outcome.

Turning to the program milestones variable, it too can be difficult to interpret. Consider the same example just outlined. A justice-impacted citizen who only stays connected to the program for one month jumps many of the program milestones when they gain full-time employment. Though they would not have progressed through CEO's programming necessarily, this would not indicate that the person did not make positive progress. Alternatively, a person who spends more time with the program meeting milestones consistently is also making positive progress.

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<sup>xii</sup> For more on the strengths and limitations of the DiD analysis, see Stock and Watson (2018), and Gelman, Hill, and Vehtari (2020).

<sup>xiii</sup> Knowing who attritors are in each group, we could compare them with non-attritors to determine if they were significantly different at baseline.

A feature of the participation data is that CEO can update justice-impacted citizen data based on their most recent reporting. For a research design that collects data over time, this implies that we need timestamps on variables to know what exact moment a piece of information represents (e.g., at 90 or 180 days). With participation measures, this was not possible.<sup>xiv</sup> To allow for a comparison, the analysis compares the RCS group 10 months after the end of the stipend period with control groups 1 and 2, who when combined, are measured at an *average* of 10 months after their enrollment periods. Due to this timing issue, control groups 1 and 2 must be combined if we are to make a defensible comparison.

From the perspective of CEO programming, stable full-time employment is considered a success regardless of their program engagement, and it may be not appropriate to treat participation as an outcome measure. Thus, to address the research question, we provide a descriptive analysis to potentially inform implementation and fidelity. Included in the descriptives are statistical tests of differences between control and RCS groups—two-sided pooled *t*-tests for continuous variables and chi-square tests for categorical variables.

### **Research Question #3: What is the impact of the RCS program on the earnings of justice-impacted citizens after enrollment?**

Earnings data in CEO administrative files are incomplete.<sup>xv</sup> An early assessment of the data found that of justice-impacted citizens who reported having a job within the first 90 days after program enrollment, 31% of control group 1 and 58% of the RCS group did not have a value for self-reported wage even though they reported working. Also an issue was the lack of concrete earnings data at or prior to intake. While the employment measures at intake are known, we cannot assume to know wages at intake.

In response to the challenges presented by incomplete earnings data, the Colorado Lab leveraged a pathway to access earnings data from CDLE through its partnership with LINC. LINC matched CEO participants with Colorado W-2 records pre- and post-enrollment, allowing the Colorado Lab to track earnings trajectories over time. That said, lags in the processing and availability of CDLE data means we do not have earnings data for the RCS group post enrollment or control group 2 yet. Nonetheless, the trends presented in the results are informative in establishing validity of the parallel trends assumption in preparation for a rigorous QED.

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<sup>xiv</sup> The research plan originally was designed to solicit data when each group reached six months from enrollment, but there were challenges in procuring finalized data, such that there were significant delays in receipt and processing. That said, employment and earnings measures can be dated, so this concern is specific to CEO participation measures only.

<sup>xv</sup> This could be due to justice-impacted citizens refusal to offer this information, CEO losing contact (e.g., moving away or re-incarceration), or justice-impacted citizens not having earnings to report. As researchers, we have no way to distinguish between these reasons in the data.



**Colorado Evaluation & Action Lab**  
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## Results





## Results

The following section reports the results of the analysis. The research questions and hypotheses are re-stated, along with a summary of the key findings.

**Research Question #1: What is the impact of access to the RCS program on the employment of justice-impacted citizens five months after enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments spend more time searching for employment.**

**No statistically significant differences were detected, but the accumulated evidence suggests that justice-impacted citizens in the RCS payment group were less likely to be employed five months post enrollment compared to the combined control group. Controlling for baseline characteristics, the difference ranged from 7 to 9 percentage points.**

CEO collects information on job start dates with timestamps that allow us to precisely know when a justice-impacted citizen begins a new job and whether it was within five months of enrollment.<sup>xvi</sup> For the employment analysis, we compared the RCS group with the combined control group.

Table 3 reports that five months post enrollment, participants in the combined control group had a higher percentage of justice-impacted citizens who started a non-CEO job. The difference is not statistically significant. The lack of significance is unsurprising if you consider that if only five more people in the RCS group had been employed, the percentages would be nearly equal.

While the samples were not large enough to detect whether the difference is significant, it is still noteworthy that the RCS group was less likely to be employed five months out. CEO and the Colorado Lab anticipated this result as it mirrors past internal assessments. Anecdotally, CEO hypothesizes that RCS group justice-impacted citizens did not feel the financial pressure to take the first job opportunity they were presented with. RCS payments may have allowed justice-impacted citizens to be more judicious and wait for a better fitting job. Alternatively, this finding also aligns with justice-impacted citizens delaying what would have otherwise been their normal job search process. Without additional information on job fit or turnover, we cannot disentangle the two possibilities.

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<sup>xvi</sup> The decision to use five months stems from not having outcomes data for July 2024 and a compressed evaluation timeline.

**Table 3. Employment Outcomes Five Months Post Enrollment**

Measures	RCS Group	Combined Control Group	Difference	p-value
Sample size	115	155		
Employed within five months (%)	39.13%	44.52%	-5.39 points	0.38

**Multivariate Regression Analysis.** Enrollees all begin with a paid transitional job. There is no variation in this measure at intake, so the 2-by-2 DiD estimator collapses into simpler multivariate regression.<sup>xvii</sup> The model specifies an indicator for RCS receipt, as well as variables listed in Table 2 as displaying imbalance across RCS and combined control groups based on their value of Hedges  $g$  or Cox’s Index.

Table 4 reports the estimate on the RCS group indicator (i.e., the “RCS payment effect”) for three models with progressively more controls:

- Specification (1) is an estimate for the linear probability model with only the RCS group indicator. This will reproduce the raw difference from Table 3.
- Specification (2) adds the variables for site location, age, race, and ethnicity. These controls displayed imbalance in Table 2 and are exogenous because they cannot be impacted by RCS payments.
- Specification (3) adds the behavioral characteristics that display imbalance including marital status and driver’s license status. These variables are assumed exogenous since they are measured at intake and not updated, and we assume the values of these variables were not influenced by potential receipt of RCS funds.
- As a check, Specification (4) adds the variables found to display balance in Table 2, educational attainment, the number of children under 18 years old, and curfew status.

In all specifications, standard errors are heteroskedasticity robust.<sup>xviii</sup>

The estimate for Specification (1) reproduces and confirms the raw difference from Table 3 (note the rounding). When multiplied by 100, estimates can be interpreted as percentage point differences. Thus, on average, the employment percentage within five months of enrollment for justice-impacted citizens in the RCS group report was 5.39 points smaller compared to the combined control groups. The difference is not statistically significant. The estimate from

<sup>xvii</sup> The regression model does not have a post-period indicator or the post-period and RCS group interaction term.

<sup>xviii</sup> Non-linear specifications, such as probit or logit, produce very similar effects as the linear probability model when computed at the means of the other variables. Linear probability model estimates are reported for ease of interpretability and because the variable of interest, enrollment in the RCS group, is binary and we are not interested in estimating marginal effects near the tails.

Specification (2) is larger in absolute terms but is still not statistically significant. The coefficients estimated from Specifications (3) and (4) do not display notable differences. All estimated coefficients were within each other’s 95% confidence intervals.

Following the WWC guidelines, our preferred specification, Specification (3), estimates an 8.38 percentage point difference. Though not statistically significant, analysis of employment 5 months post enrollment points to RCS recipients being less likely to take an outside job within the first five months of enrollment compared to justice-impacted citizens who did not receive RCS payments. The coefficients do not change very much across specifications despite different sets of control variables. The consistency suggests that bias from unmeasured factors may not be as concerning as suggested by the results of the baseline equivalence analysis. In other words, if we added more controls to the model, we would not expect the coefficient to change substantively.<sup>xix</sup>

**Table 4. Regression Coefficient Estimates for Employment Outcomes**

Measures	Specification (1)	Specification (2)	Specification (3)	Specification (4)
Sample size	270	266	254	254
RCS group	-0.05 (0.06)	-0.09 (0.06)	-0.08 (0.07)	-0.07 (0.07)

**Notes:** Sample sizes differ due to missing covariate values. The difference between Specifications (1) and (2) is due to a few people without a reported race/ethnicity. The difference between Specifications (2) and (3) is due to missing values for marital status and driver’s license status. None of the coefficients are statistically different from 0 at any conventional level. Heteroskedasticity-robust standard errors are reported in parenthesis below the coefficients.

While exploring balance between the control and RCS groups earlier in the methods section, it was noted that there were differences in enrollment patterns across the two CEO sites. Denver enrollment in control group 1 was 39.71%, 42.24% in the RCS group, and 57.14% in control group 2. Location matters because justice-impacted citizens in Denver and Colorado Springs experience distinct labor markets, thus they face distinct probabilities of finding employment. This consideration is why it was important to include an indicator for site location to the regression Specifications (2), (3), and (4). Building on this, we add a term to the model interacting location and RCS group enrollment to determine if the impact of RCS receipt on employment differed by location. This version of the model finds that, on average, justice-impacted citizens based outside of Denver have between a 14- and 17-point lower percentage employment rate five months after enrollment. This difference is statistically significant. That said, where a justice-impacted citizen received RCS payments did not seem to matter.

In summary, our analysis of justice-impacted citizens’ employment five months post enrollment provides suggestive evidence that RCS recipients were less likely to take an outside job within the

<sup>xix</sup> The estimated coefficients on the control variable are not reported in the table because they do not have a causal interpretation and are sensitive to the inclusion or exclusion of additional controls. Under certain conditions, like when all control variables are exogenous, interpretation is reasonable.

first five months of enrollment compared to justice-impacted citizens who did not receive RCS payments. For whatever reason, RCS group enrollees seem to have altered their job search process. That said, we also uncovered significant differences between justice-impacted citizens in Denver and Colorado Springs unrelated to RCS receipt and potential signs of larger macro trends that could bias our estimate of the impact of RCS receipt.

**Research Question #2: What is the impact of access to the RCS program on justice-impacted citizen program engagement 10 months after enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments report greater engagement in CEO programming.**

**Key Finding: Differences in participant engagement between the RCS payment and control groups were not statistically significant and mostly small in magnitude.**

Participation in the CEO program is measured using a series of program stages and counting the number of days a justice-impacted citizen works in a CEO-provided transitional job. As discussed in detail earlier, both are imperfect measures of participant success, so we need to be careful when drawing conclusions based on these results. Important caveats are noted throughout.

CEO program stage is the first way we approach program participation. Program completion can be defined as a justice-impacted citizen reaching the “employed/receiving retention services” or reaching the 180-day milestone. Importantly, participants who are employed, regardless of their program stage, are also considered completions.

The first row in Table 5 shows the sample size of the combined control and RCS groups. The second row reports the percentage of participants in the RCS and combined control groups that are program completers. Participants in the combined control group report a higher percentage, but the absolute difference of 3.37 percentage points is not statistically significant at any conventional level.

**Table 5. Comparison of Participation Outcomes**

Measures	RCS Group	Combined Control Group	Difference	p-Value
Sample size	116	159		
Program completed (%)	45.69%	49.06%	-3.37 points	0.58
Not completed (%)	62.93%	55.35%	7.58 points	0.21
Transitional job workdays	24.29 (19.69)	25.50 (18.69)	-1.21 days	0.61

An alternative way to look at the data is to investigate instances when CEO documented that a participant did *not* complete the program. We define non-completion as a justice-impacted citizen either being discharged without completion, requiring an intervention, or disengaged and requiring re-engagement. The third row of Table 5 reports non-completion percentages. The difference in non-completion percentages is 7.58 points. Though the difference is larger, it is still not statistically significant at any conventional level.

Not detecting statistically significant differences in either measure is a consequence of the percentages being very sensitive to the number of justice-impacted citizens in each group. For example, if just three more people were categorized as program completers in the RCS group, the completion percentage would be nearly identical to the combined control group.<sup>xx</sup> Note that program completion and non-completion percentages sum to more than 100%. This is because a justice-impacted citizen could be both employed and be recorded as being in one of the stages that fall under non-completion.

A third measure of program participation is the number of transitional job workdays a justice-impacted citizen worked. Transitional job workdays are paid workdays on a CEO work crew. The fourth row of Table 5 reports the average number of transitional job workdays. Standard deviations are provided in parenthesis underneath the averages. The difference between RCS and combined control groups is 1.2 days, which is not statistically significant at any conventional level. That said, even if a statistically significant difference was observed, justice-impacted citizens who spend more time working the transitional job could be considered more engaged but cannot be assumed to be more successful than their peers who worked fewer days.

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<sup>xx</sup> Recall that we are comparing the RCS group 10 months after the end of the RCS enrollment period with control groups 1 and 2, who together, are measured at an *average* of 10 months after their enrollment periods. Given that the compositions of the control groups are different along various dimensions, the average could be skewed by the idiosyncratic group differences.

The combined control group had, overall, 3.37 percentage points more completers, 7.58 percentage points fewer than non-completers, and justice-impacted citizens who work 1.21 more days compared to the RCS group. While not statistically significant and relatively small in absolute magnitudes, all three measures point in the direction of the RCS recipients being less engaged.

**Research Question #3: What is the impact of the RCS program on earnings for justice-impacted citizens after enrollment?**

**Hypothesis: Justice-impacted citizens receiving RCS payments experience a steeper earnings trajectory post enrollment.**

**Key Finding: While post-enrollment earnings trajectories for justice-impacted citizens in the RCS group and control group 2 are not yet available, pre-enrollment trajectories for the RCS and control group 1 display similar average earnings.**

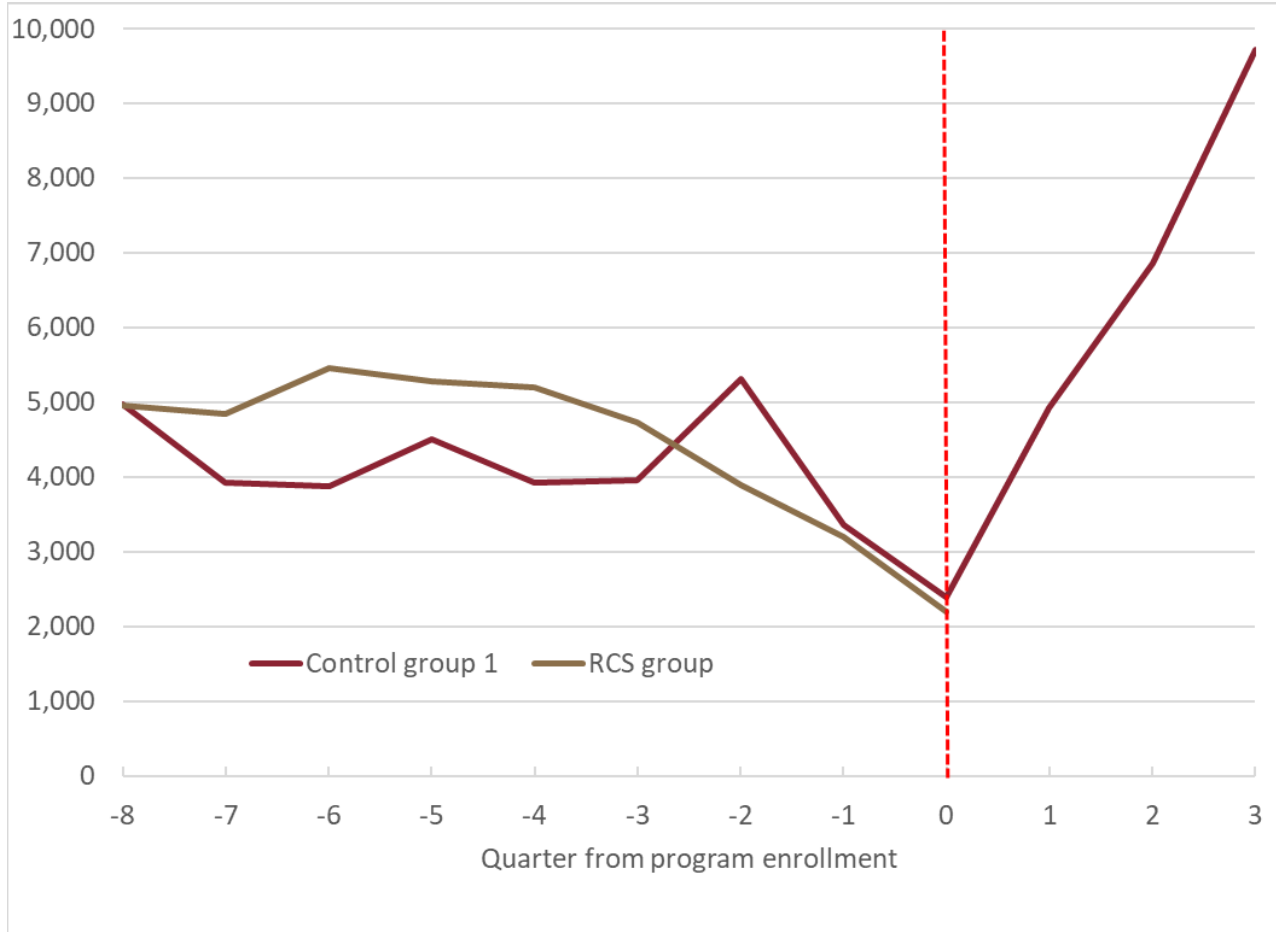
The Colorado Lab leveraged a pathway to access earnings data from CDLE through its partnership with LINC. CEO sent LINC their enrollment rosters, and LINC matched CEO enrollees with Colorado state W-2 records pre- and post-enrollment, allowing the Colorado Lab to track quarterly earnings over time. Earnings are defined as income from formal employment (e.g., an hourly wage rate times the number of hours worked), and earnings exclude sources of unearned income (e.g., rents, government transfers, and cash tips).

Earnings are gross (pre-tax and before any other deductions), and all earnings values are inflation adjusted to reflect purchasing power in 2023 dollars using the Denver-Aurora-Lakewood area Consumer Price Index.<sup>xxi</sup>

Figure 3 presents an event-study graph plotting the average total quarterly earnings for enrollees in control group 1 and the RCS group. Participants include all enrolled learners, whether they graduated from the program. Quarter 0 is the quarter when justice-impacted citizens started the program. Earnings histories are tracked as far back as eight quarters prior to program start (the pre-period) and up to three quarters after program start for enrollees in control group 1 (the post-period). Sample sizes and average earnings values are reported in [Appendix B](#).

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<sup>xxi</sup> This includes Adams, Arapahoe, Adams, Arapahoe, Broomfield, Clear Creek, Denver, Douglas, Elbert, Gilpin, Jefferson, and Park Counties.

**Figure 3. Average Quarterly Earnings Relative to Quarter of Enrollment**


Due to the lagged release of W-2 records by CDLE, control group 2 is not yet available and we also do not have post-enrollment quarters for the RCS group yet. Nonetheless, the trends presented in the results are informative in establishing the validity of control group 1 as a good comparison in accordance with the parallel trends assumption. The trends present a future opportunity to conduct a rigorous QED.

For both groups, the inflation-adjusted average total quarterly earnings display a prolonged period of stagnation, as earnings are just keeping up with inflation.<sup>xxii</sup> Nearer to enrollment, we see a dip in the average total quarterly earnings. This dip is consistent with program participants experiencing a change in their financial situation and reflects the reasons individuals may choose to seek CEO's services, including trouble finding or keeping a job. After program enrollment, the average total quarterly earnings for control group 1 recover quickly, surpassing pre-enrollment levels.

A comparison of quarterly earnings from quarters -8 to 0 reveals no statistically significant differences of the average values across the groups, establishing control group 1 as a potentially

<sup>xxii</sup> For both groups, the pre-enrollment period is a period in which a justice-impacted citizens may have been in prison.

valid comparison group for the RCS group in a DiD QED. Looking ahead to 2025, we will be tracking whether the RCS trend behaves similarly to control group 1 in the post-period, and whether we can detect statistically significant differences.

## Limitations

There were five primary limitations of this study:

- A compressed evaluation timeline.
- Incomplete wage and job satisfaction data.
- Insufficient measures of job turnover.
- A lack of timestamps on participation variables.
- Under-enrollment in the control groups.

CEO has expressed a desire to assess longer-term outcomes. Unfortunately, the compressed timeline of this evaluation and length of time it takes to recruit a cohort limited investigating outcomes like recidivism and employment one year from program enrollment. Relatedly, acquiring earnings data from CDLE is a slow process. We do expect additional earnings data until early 2025.

A central challenge throughout the evaluation was the execution of the planned QED. While rigorous post-outcomes comparisons can be informative, ideally, we aim for a design that can be used to estimate a causal effect. The research questions and analytic approach were built around a DiD design, but the limitations required researchers to take a step back and simplify the design.

Another challenge relates to sample size. Underenrollment limited the ability to detect significant effects. While the RCS group surpassed their 100-enrollee target, control group 1 (68 enrollees) and control group 2 (91 enrollees) fell short of the combined 200. We are confident that some of the employment differences calculated would have been statistically significant given larger samples.

Lastly, there was misalignment between the program goals and the evaluation outcomes. For example, one of CEO's primary goals is to reduce recidivism, but this was not measurable. Concerning employment goals, while having a job is indeed one important outcome we did assess, CEO was also interested in job stability and satisfaction. The job satisfaction measures available in the data were largely incomplete; using them would render results that were based on a very select sample of justice-impacted citizens.

An alternative measure of job satisfaction, and direct measure of stability, is the number of jobs a justice-impacted citizen held within a fixed period of time (i.e., turnover). If a justice-impacted citizen remains in the same job, it could be a sign that the job is a good fit and that they are content. Extending this reasoning to the current evaluation, if an RCS recipient takes longer to find employment compared to a justice-impacted citizen who does not receive RCS payment, but the financial stability affords them the opportunity to search and wait for a job that they feel good about, then the RCS program is meeting one of its most important goals.



## Ongoing Evidence Building

Phase II of the evaluation aims to assess outcomes one year after initial RCS fund disbursement, with three central research questions overlapping with some of what the current evaluation has discussed. There is no funding currently identified for this phase, and thus these steps and CEO's and Colorado Lab's roles in executing them are contingent on identification of additional resources.

**Phase II Research Question #1: What is the impact of the RCS program on employment for justice-impacted citizens one year after enrollment?**

**Phase II Research Question #2: What is the impact of the RCS program on earnings for justice-impacted citizens one year after enrollment?**

**Phase II Research Question #3: What is the impact of the RCS program on criminal justice recidivism for justice-impacted citizens one year after enrollment?**

To meet CEO's desire to understand the impact of cash transfers over a longer period, we recommend a formalized theory of change that can guide both the Colorado Lab and CEO in determining what data to collect and how it helps meet CEO's programmatic goals. This could be accomplished via the creation of a logic model that ties program participation directly to recidivism and explicitly identifies the mechanisms by which this operates.<sup>xxiii, 17</sup>

For a successful Phase II, it will be imperative that the Colorado Lab and local CEO staff work closely to establish that data collection and processing safeguards are in place to ensure that the research questions can be adequately addressed. This will include reviewing the quality of baseline measures, modifications in how information is recorded, and doing as much as possible to stay connected with justice-impacted citizens to periodically collect outcome measures. Colorado Lab staff can be made available to assist CEO staff with technical assistance such as survey design, data recording and organization, and general troubleshooting.

Fortunately, we are already in the process of addressing Phase II Research Question #2. Through our partnership with LINC and CDLE, we expect to receive enough administrative earnings data to answer the earnings question. From the earnings trends presented in this report, there is reason to be optimistic about the possibility of conducting a more rigorous evaluation.

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<sup>xxiii</sup> For example, Burns and Dague (2023) find that Medicaid coverage upon release from prison yields a significant and meaningful reduction in the probability of reincarceration and improves employment outcomes. Their findings strongly indicate that Medicaid enrollment's effect on reincarceration likely operates through the provision of financial security—precisely the avenue through which CEO's cash assistance program is intended to operate.

## Discussion of Implications

The best available research evidence to inform the ongoing use of cash assistance in CEO's program continues to be based on national studies. The current study builds on previous work by evaluating the addition of cash assistance but falls short of reaching causal conclusions. Within the Colorado context, our understanding of the use of cash assistance is at Step 3 on Colorado's Steps to Building Evidence. That said, with the lessons learned during the current evaluation and increased partner capacity, a clear path to Step 4 has been established.

Redcross et al.'s (2011) analysis of CEO programming found that justice-impacted citizens randomized into a fuller set of wraparound services substantially increased employment early on, but that the impacts faded over time.<sup>18</sup> The results in that previous analysis of CEO programming showed some mixed results, but the causal effect of introducing cash assistance along with regular programming was not tested. A survey of participants in CEO's previous cash assistance program during COVID-19 in 2020 documented participants reporting greater financial stability and greater ease in achieving employment-related milestones.<sup>19</sup> Together with literature showing that job training programs offering wraparound services, like career counseling or access to childcare for working parents, increased participation and completion of upskill training, suggests cash assistance can promote financial stability and program engagement.

Relevant to the ongoing piloting of unconditional cash assistance is the potential for it to affect justice-impacted citizens' eligibility for public benefits. In the United States, the social safety net functions as a patchwork of federal, state, and sometimes local economic means-tested assistance programs that is further complicated by tax policies like the Earned Income Tax Credit and Child Tax Credit.<sup>20</sup> Households are expected to do the complicated math of weighing potential gains from increased earnings via workforce participation against possible declines in government assistance and changes in tax credits (Anderson et al., 2022; Chien & Macartney, 2019; Maag et al., 2012).<sup>21, 22, 23</sup> For example, a wage increase could disqualify a household from government aid and tax credits. In response, individuals may find it in their best interest to *reduce* the hours they work. Such situations where reductions in aid or tax credits effectively negate or reduce overall take-home income of household earners is referred to as a "benefits cliff".<sup>24</sup> Benefits cliffs create perverse incentives for households because they effectively create a disincentive for households to continue earning more, thus maintaining their reliance on safety net programs.

## Conclusion

Economic theory posits that cash assistance reduces the pressure to accept the first job a person is presented with, allowing them time to find a more suitable position. It also points to the possibility that financial security simply delays the search process.

The evidence suggests that cash assistance alters the job search process. Five months after entry into the program, RCS participants who received cash assistance were less likely to have accepted their first non-CEO job. This finding was not statistically significant, and the sample size was small, so additional research is needed. Future research should be based on a larger sample and aim to

collect complementary data on job satisfaction and job stability. Future research might also consider potential differences across sites, given the difference in demographics served and local economies.

Regarding participation, CEO's focus on long-term employment and recidivism outcomes makes participation metrics less relevant for future research.

The success of Phase II will hinge on collaboration between the Colorado Lab and CEO staff to ensure effective data collection and processing. To understand the long-term impact of cash assistance, we recommend developing a formal theory of change to guide data collection and the interpretation of the results.

With the assistance of LINC and access to CDLE W-2 records, an early analysis of earnings shows significant potential for a rigorous QED. Based on the trends, we can begin to establish the validity of using control group 1 as a comparison. For the two groups in the analysis, inflation-adjusted average total quarterly earnings display a prolonged period of stagnation as earnings are just keeping up with inflation. Nearer to enrollment, we see a dip in the average total quarterly earnings. This dip is consistent with program participants experiencing a change in their financial situation and reflects the reasons individuals may choose to seek CEO's services, including trouble finding or keeping a job. After program enrollment, average total quarterly earnings for control group 1 recover quickly, surpassing pre-enrollment levels. Looking ahead to 2025, we will be tracking whether the RCS trend behaves similarly to control group 1 in the post-enrollment period.

Overall, the findings in this study are not conclusive enough to determine whether they are in line with previous studies or not and should be discussed with caution. With respect to policy, the Colorado Lab recommends that this study be used only to inform evaluation proposals that contain additional evidence-building stipulations within them.

## Appendix A: Data Glossary

- **Age:** Age of the justice-impacted citizen at intake in years.
- **Actual Transitional Job Days Worked:** Number of days a justice-impacted citizen worked in a CEO transitional job. There is no maximum number of transitional jobs days possible.
- **CEO Code:** A unique six-digit number that identifies participants. Using an anonymous code ensures the privacy of participants.
- **Curfew:** Identifies whether a participant has any curfew restriction. There are various ways of reporting a curfew, so the variable is made binary for non-missing values.
- **Current Stage Name:** A measure of participant's programmatic progress. The stages are labeled {Not job start ready (job coaching), Pending assessment, Re-engagement pending, Job Search/match & potential placement (redev), Job search/match & potential placement, Intervention, Intervention (unexcused absence), Employed/retention services, 180-day milestone, 360-day milestone}.
- **Earnings:** Gross quarterly earnings from unemployment-insurance-qualifying employment as documented in W-2 records.
- **Education Level Combined:** Records the highest level of educational attainment a participant has achieved. The categories are {Grade 7, Grade 8, Grade 9, Grade 10, Grade 11, Grade 12, no diploma, GED or alternative, Regular high school diploma, Some college but less than 1 year of college credit, 1 year or more of college credit but no degree, AA or AS, BA or BS, and Refused to answer}. Categories are collapsed to {Less than high school/no diploma/no GED, High school diploma/GED, and Some college or more}.
- **Enrollment Date:** Day, month, and year when a participant enrolled in the program. The date of intake.
- **Gender:** Participant sex, including {Female, Male, Gender non-conforming, and Additional gender category/other}.
- **Has Driver's License:** Indicates whether the participant has a driver's license.
- **Job Start Date for First Placement:** The date a justice-impacted citizen says they start their first non-CEO job after enrollment.
- **Location:** The site where a participant was based during the sample period. The two possible sites are Denver and Colorado Springs.
- **Marital Status:** Participant marital status includes {Divorced, Single, Married, and Not available}.
- **Race/ethnicity:** Participants' identification of their race and ethnicity. Categories include {African American, Hispanic or Latino, Native Hawaiian/Pacific Islander, White, American Indian/Alaskan Native, Two or more races, and Other race}. Responses can also be a combination of the listed items.

- **Stipend:** Participant received the cash stimulus payment and was a member of the cash assistance treatment group.
- **Work Experience Combined:** A descriptive variable that combines a participant's past industry and occupation (e.g., "construction" or "welder;" "retail" or "stock;" "food service" or "waiter").
- **Work Environment Restrictions:** If a participant has a work restriction, this variable reports the industry a participant cannot work in.
- **Number of Children under 18:** Number of own children under the age of 18 years old.

## Appendix B: Average Earnings and Sample Sizes

Table B1. Average Earnings by Quarter Since Enrollment

Period	RCS Group Sample Size	RCS Group (\$)	Control Group 1 Sample Size	Control Group 1 (\$)	p-value
-8	15	4,960	15	4,980	0.9913
-7	18	4,844	16	3,929	0.5605
-6	16	5,451	16	3,872	0.3085
-5	17	5,287	15	4,513	0.6378
-4	21	5,200	18	3,919	0.3831
-3	19	4,732	19	3,951	0.5702
-2	12	3,894	22	5,318	0.3333
-1	12	3,201	26	3,358	0.9113
0	38	2,203	72	2,393	0.5887
1			69	4,932	
2			26	6,861	
3			10	9,717	

**Notes:** Earnings data for the RCS post-enrollment is not yet available. Most justice-impacted citizens do not have W-2 records in every quarter leading to small quarterly sample sizes. It is important to remind ourselves that justice-impacted citizens spent some time in prison so they would not show up in these numbers.

## Endnotes

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