Taking a Chance on Workers: Evidence on the Effects and Mechanisms of Subsidized Employment from an RCT*

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Abstract

This paper uses a randomized controlled trial to provide comprehensive evidence of the impact and mechanisms of a broadly targeted enhanced transitional jobs program. We develop a model to illustrate the mechanisms through which a wage subsidy with wraparound employment services may affect long-term employment and estimate treatment effects on a wide set of outcomes. The paper estimates intent-to-treat effects over a four-year follow-up period. Case managers addressed employment barriers and provided targeted financial assistance while participants were eligible for 30 weeks of fully subsidized employment. Program access increased employment rates by 21 percent and earnings by 17 percent while participants were receiving services. Though gains attenuated after services stopped, treatment group members experienced lasting improvements in employment stability, job quality, and well-being, and we estimate the program's marginal value of public funds to be in line with other adult workforce programs. Post-program impacts are concentrated among participants who were hired by their host-site employer post-program, suggesting that encouraging employer learning about potential match quality is a key mechanism underlying the program's impact.

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Losing a job can negatively affect the trajectory of a worker's career and well-being. Displaced workers suffer substantial earnings losses, primarily through the destruction of valuable worker-employer matches, and re-employment is challenging as job seekers with recent spells of unemployment face lower call-back rates from potential employers.[1] Moreover, unemployed workers in the low-wage labor market often face myriad barriers that make it hard to get back to work: a lack of in-demand skills, intermittent work histories, and other observable characteristics that lead employers to believe that they are unlikely to become productive employees. In addition to providing passive income support, policymakers have supported these workers' re-entry to employment using Active Labor Market Programs (ALMPs)—e.g., job search assistance, training, and subsidized employment. While programs that train job seekers to work in specific high-growth sectors have shown promise, they typically employ rigorous screening criteria for ability and aptitude, making them a potentially poorer fit for many unemployed workers.[2] Subsidized employment programs, in contrast, have proven successful in quickly re-employing narrow groups of low-wage workers with significant barriers. Less is known, however, about whether these programs foster sustained post-program employment for the broader low-wage workforce, which participants enjoy lasting program impacts, and why.

In this paper, we use a randomized controlled trial (RCT) to determine the effectiveness of a supported work program that pairs subsidized employment with wraparound case management and employment services to accelerate participants' return to employment and, ideally, to improve their longer-run labor market outcomes. ReHire Colorado, administered by the Colorado Department of Human Services (CDHS), places participants in temporary jobs with local employers and pays the full cost of their wages for up to 30 weeks. Case managers help match participants to willing host sites and are encouraged to facilitate placements they believe are especially well-suited for the individual, with the explicit goal of having participants transition to unsubsidized employment with the host-site employer after program exit. ReHire also provides financial assistance to address barriers and provides additional job search assistance, such as coaching toward new career opportunities and preparation of job application materials. The program has operated

[1] Beginning with Ruhm (1991), other studies have measured the scarring effects of job dislocation (Jacobson, LaLonde and Sullivan, 1993; Stevens, 1997; Arulampalam, 2001; Gangl, 2006; Krolikowski, 2018), and Rose and Shem-Tov (2023) explicitly consider the consequences of losing lower-wage jobs. Lachowska, Mas and Woodbury (2020) use administrative earnings data with observable hours worked to show that most of the earnings losses can be attributed to valuable employer-employee matches. A number of audit studies experimentally vary the timing and length of unemployment spells and measure differences in call-back rates (Kroft, Lange and Notowidigdo, 2013; Eriksson and Rooth, 2014; Farber et al., 2019).

[2] Experimental evaluations of successful sectoral training programs like those from the WorkAdvance model Katz et al. (2022) and Year Up (Fein and Hamadyk, 2018; Fein and Dastrup, 2022) study programs that incorporate upfront screening. Hendren and Sprung-Keyser (2020) provide evidence of cost-effectiveness (large marginal value of public funds) of sectoral training programs like WorkAdvance, YearUp, and Project QUEST.

at scale in multiple counties since January 2014 and recruits and serves a diverse set of participants who are reasonably representative of the low-wage Colorado workforce who have recently experienced job loss. Eligibility requires Colorado residency, legal authorization to work, an ability to pass a drug test, monthly household income below 150 percent of the poverty line, and being unemployed or underemployed for at least four consecutive weeks.

Program access was allocated randomly among applicants on a rolling basis from July 2015 through December 2018. Applicants assigned to the control group maintained access to standard job search assistance offered by the service providers, including resume assistance, mock interviews, and job coaching. Applicants assigned to the treatment group were additionally offered access to the ReHire-funded bundle of services, including the wage subsidy, job placement, and financial assistance to address employment barriers.

Our analysis leverages the random assignment to estimate intent-to-treat (ITT) impacts of program access on labor market outcomes and well-being over the four years following program application. We track employment and earnings, benefit receipt, and credit outcomes in high-frequency administrative data, and we measure impacts on job quality and well-being using an 18-month follow-up survey. We estimate treatment effects separately for in-program and post-program time periods. Because the evaluation period overlapped in part with the COVID-19 pandemic, we consider two post-program periods—a pre-COVID period where less than half of the sample was affected by the pandemic and a post-COVID period where the majority of the sample experienced pandemic-related disruptions.

As expected, ReHire increased formal-sector employment and earnings during the in-program period. The quarterly employment rate improved by 11.2 percentage points (21 percent) and quarterly earnings rose by $301 (17 percent). However, there were no effects on SNAP or TANF receipt or credit outcomes.

In the first two years following program exit, program effects on these outcomes were more modest, with estimated ITT effects on employment and quarterly earnings of 2.5 percentage points (5 percent) and $154 (7 percent), respectively. However, we find that ReHire access led to improvements in other aspects of workers' lives including employment stability, job quality, and well-being. Treatment group members were 3.8 percentage points (16 percent) more likely to work in every quarter and 3.4 percentage points (35 percent) more likely to work for their first post-randomization unsubsidized employer during the pre-COVID post-program period. In addition, the 18 month follow-up survey reveals that the treatment group experienced meaningful improvements in job quality at their first unsubsidized job (0.13 SD) and well-being (0.17 SD), though bounds adjusting for attrition are wide. During this period, the program did not affect other aspects of participants' lives including benefit receipt, employment barriers, soft skills, or credit outcomes.

In the post-program COVID-affected period—roughly the fourth year after application—we find no lasting effects on employment and earnings, but a greater share of the treatment group continue to experience employment with their employer from the quarter following program application. The null effect on overall employment and earnings, however, may have resulted from the pandemic.[3]

Beyond establishing ReHire's impacts, we use a model-guided decomposition analysis to determine which mechanisms contribute to the estimated treatment effects. We develop a simple two-period model in which firms must choose whether to hire a worker in the first period without knowing their true productivity, which they learn over time as they observe job performance (Jovanovic, 1979; Altonji and Pierret, 2001; Pries and Rogerson, 2005). If the firm hires the worker in the first period, their true productivity (for the second period) is revealed, and the employer makes a retention decision based on full information. In this framework, ReHire program access lowers the effective cost of hiring by eliminating wage costs and reducing other administrative costs, which encourages firms to take chances on workers they might otherwise deem risky. This structure yields three distinct pathways through which the subsidy and additional supportive services can affect post-program employment: an experimentation and information revelation channel, as firms are incentivized to hire workers and learn their quality; a human-capital channel, as the period of supported and subsidized employment can raise future productivity through skill accumulation, coaching, or barrier removal; and an employer beliefs channel, as work experience or resume assistance may improve employers' assessment of the worker's productivity.

Motivated by the model, we provide descriptive analysis to understand the quantitative importance of these mechanisms. We conclude that information revelation is a key mechanism through which subsidized employment programs like ReHire create persistent impacts, potentially by interacting with improvements in human capital. The core of this analysis is a decomposition that shows that the group hired by their transitional job (TJ) host site continue to experience employment and earnings gains relative to the control group years after program completion. In contrast, the post-program labor market outcomes of those who complete a TJ but are not hired by their host site or who leave the program having received at most supportive services closely match the control group's. Importantly, the information revelation mechanism

[3] We use a surrogate approach that leverages data on pre-RCT participants (Athey et al., 2019) to provide suggestive evidence that impacts on employment and earnings would have been small but durable in the absence of the pandemic. We use these estimates as an input into a Marginal Value of Public Funds calculation.

is operative only among workers who persisted at their TJ host site, whereas portable human capital gains or improvements to subsequent employers' beliefs from the additional work experience could have improved future hiring outcomes with subsequent employers but seemingly did not. Because the subgroups in this decomposition were not randomly assigned, we provide evidence against four alternative explanations of the strong persistent post-randomization increase in employment among those hired by their host site—program effects through human capital improvements among participants who would have been hired soon after randomization regardless of their treatment status, selection bias on job-readiness including the possibility of cream skimming, selection on time-varying productivity shocks, and differences in placement types.

Finally, we consider whether it is possible to predict what types of workers or placements will lead to a successful transition to unsubsidized employment with their host site. We find that it is difficult to do so, even when using a rich set of covariates from a detailed baseline survey. In contrast, we find that employer characteristics, especially offering lower-wage positions, do predict which placements are likely to lead to a successful transition. This latter finding aligns with a prediction from the model that the option value of hiring a worker of unknown quality should be largest when the distribution of possible productivity is centered near the wage the firm would need to pay to retain the worker.

This paper contributes to our understanding of the effectiveness of ALMPs by evaluating an understudied and increasingly popular program model that addresses unemployment among low-wage workers without lengthy upfront investments in human capital.[4] Some alternative programs that provide intensive training lead to large long-term improvements in employment.[5] However, programs that train workers for careers in specific in-demand sectors typically have screening criteria for ability and aptitude that exclude many low-wage job seekers (Katz et al., 2022; Fein and Hamadyk, 2018; Fein and Dastrup, 2022). Re-Hire, in contrast, is a work-first intervention that welcomes nearly all job seekers and aims to get them back to work quickly. Our finding of a lasting program impact among participants who are hired by their host-site employer suggests that wage subsidies and wraparound services can also improve long-term outcomes among this hard-to-employ population even without substantially improving participants' human

[4]Several reviews and meta-analyses document the effectiveness of different types of ALMPs (Heckman, LaLonde and Smith, 1999; Greenberg, Michalopoulos and Robins, 2003; Card, Kluve and Weber, 2010; Barnow and Smith, 2015; Card, Kluve and Weber, 2018). Betcherman, Olivas and Dar (2004), Arbelaez et al. (2012), McKenzie (2017), and Carranza and McKenzie (2024) provide further evidence from middle- and low-income settings, many of which are dominated by small, informal firms and characterized by a scarcity of higher-wage formal sector jobs, which can limit the applicability of some results to higher-income contexts.

[5]Card, Kluve and Weber (2018) provide a meta-analysis of ALMP evaluations, including a comparison of the effectiveness of different program types. While subsidized employment programs tend to have larger short-term gains in employment, job training programs tend to lead to larger long-term gains.

capital. Programs that facilitate additional employment matches may be especially valuable for unemployed individuals for whom further investments in human capital have lower lifetime returns, such as older workers.

This paper also contributes to a closely related literature that studies the effects of transitional jobs programs targeting hard-to-employ workers in the US. Early experimental evidence found that gains in earnings and employment rates faded out once wage subsidies ended (Bloom, 2010).[6] More recent programs, including ReHire, enhanced the traditional transitional jobs model by providing more intensive case management, job training, financial support to address employment barriers, and by offering placements that are similar to typical positions at the same employer with the intent that some of these placements will lead to unsubsidized job offers. Evaluation reports on programs with similar enhancements targeted at specific sub-populations show stronger and more durable impacts compared to earlier program models (Barden et al., 2018; Anderson et al., 2019; Cummings and Bloom, 2020), and the results from this study are consistent with those findings.[7] The more positive impacts when including intensive case management are consistent with recent evaluations in other contexts, including education (Weiss et al., 2019; Azurdia and Galkin, 2020; Evans et al., 2020; Brough, Phillips and Turner, 2024), housing (Bergman et al., 2020), and anti-poverty programs (Evans et al., 2025).

Relative to other contemporaneously-developed evaluations of US transitional jobs programs, this paper makes two important contributions. First, this study deepens our understanding relative to the existing literature by providing the first evidence on the likely contribution of the multiple possible mechanisms through our model-motivated decomposition analysis. The finding that information revelation is a key mechanism both provides an explanation for the fade out seen in prior studies and suggests that TJ programs are successful to the extent that temporary placements have the possibility of becoming unsubsidized positions at the same employer. Second, we provide an evaluation of a subsidized employment program serving a broad segment of the low-wage workforce. Other programs either serve specific sub-populations—non-custodial parents or recently incarcerated job-seekers (Barden et al., 2018; Foley, Farrell and Webster, 2018), TANF recipients (Glosser, Barden and Williams, 2016), individuals at high risk of gun

[6] Prior to the current wave of subsidized employment programs in the U.S., the National Job Training Partnership Act (JTPA) included an on-the-job training (OJT) services stream that included a 50 percent wage subsidy for up to 6 months at a private employer (Orr et al., 1996; Bloom et al., 1997; Plesca and Smith, 2007). Although access to the three JTPA service streams was not randomly assigned, Orr et al. (1996), for example, found that adult female participants who were selected by caseworkers for the OJT saw significant earnings gains.

[7] Results from the US Department of Labor's Enhanced Transitional Jobs Demonstration (ETJD) find that treatment group members earned \$700 more than the control group and were 4 percentage points more likely to be working during the final year of a 30-month follow-up (Barden et al., 2018).

violence (Bhatt et al., 2023), or youth (Heller, 2014; Gelber, Isen and Kessler, 2016; Cummings, Farrell and Skemer, 2018; Modestino, 2019; Davis and Heller, 2020)—or complement subsidized employment with additional interventions such as cognitive behavioral therapy (Bhatt et al., 2023). We find positive post-program impacts among this broader target population, and, although our analysis is not powered to find small differences in effect sizes, point estimates of effects by subgroup reveal relatively little heterogeneity. This finding suggests that this type of program need not be narrowly targeted to a particular subset of lower-wage workers.

More broadly our paper relates to studies of the effects of subsidized wage interventions on worker outcomes in other contexts.[8] Examples include studies of the Canadian Self-Sufficiency Project (Michalopoulos et al., 2002; Card and Hyslop, 2005; Lise, Seitz and Smith, 2004), studies of the US Earned Income Tax Credit (e.g., Eissa and Liebman, 1996), and a Mexican wage subsidy designed to induce workers to work in the formal rather than the informal sector (Abel et al., 2022).[9] In these studies, the wage subsidies were designed to overcome high reservation wages in order to increase participants' labor force participation, and program designers hoped that the subsidies could have a lasting effect by improving workers' future earning capacity through additional job-based human capital.[10] Our study, in contrast, examines the effect of subsidized employment among participants who have revealed themselves to be willing and able to work by applying for a program that offers placements into minimum wage jobs. We provide a theoretical model that highlights three mechanisms through which subsidized employment can improve post-subsidy outcomes, and find that incentivizing employers to hire workers whom they otherwise would screen out and allowing them to retain those who are revealed to be productive after a trial period is a key pathway. Groh et al. (2016) explore the role of the same set of mechanisms and find that temporary wage subsidies were unable to provide a stepping stone to future employment for recent female college graduates in Jordan. In their setting, host-site employers failed to retain workers in part because their productivity was insufficient relative to the wages required. In our context, wages are sufficiently low (the Colorado minimum wage), and a meaningful share of subsidized placements transition to unsubsidized employment. Further, those who successfully transition experience lasting employment effects. This study therefore

[8]There are also experimental studies that examine the effect of subsidizing employment on firm-level outcomes, especially in developing countries. These studies, such as De Mel, McKenzie and Woodruff (2019) and Hardy and McCasland (2023), are related, but the focus is on understanding how to increase labor demand by encouraging small business owners to hire more workers.

[9]There are additional RCTs examining effects of wage subsidies in low- and middle-income countries on worker outcomes. The context in these settings differs markedly: many interventions target youth with little or no labor market experience, have low take-up of wage vouchers, and operate in labor markets with large informal sectors dominated by microenterprises. McKenzie (2017) and Carranza and McKenzie (2024) and provide comprehensive reviews of this literature.

[10]Notably, in the Canadian Self-Sufficiency project, the positive employment effects did not last once the subsidies stopped.

furthers our understanding of the potential for information revelation to improve worker's outcomes by providing direct empirical evidence that incentivizing employers to take a chance on workers can improve workers' post-subsidy employment outcomes, even among those with substantial prior work experience.

I The Intervention

In this section, we describe the programmatic details of ReHire and the program's target population. We then incorporate the key features of this subsidized employment program into a two-period model of hiring and retention decisions under incomplete information to understand potential mechanisms.

I.A Program Design

ReHire Colorado is a suite of workforce services designed to help the unemployed get back to work. The program began in January 2014 following the passage of the Colorado Careers Act of 2013 and continues to operate throughout the state.[11] ReHire was developed as part of a new wave of subsidized employment programs designed to address persistent unemployment following the Great Recession. Other examples include programs studied through the US Department of Labor Enhanced Transitional Jobs Demonstration (ETJD) and the US Department of Health and Human Services Subsidized Training and Employment Demonstration (STED) (Anderson et al., 2019; Barden et al., 2018; Cummings and Bloom, 2020). CDHS administers ReHire centrally at the state level, but services are provided locally by community organizations located in both urban and rural areas.[12] Workers at these agencies identify clients on a rolling basis for whom the program might be a good fit, assess eligibility, work with clients to submit the program application, and provide program services to ReHire participants.

The program combines placement into temporary subsidized jobs—the program's key feature—with supportive services and case management. Job developers create a bank of local public and private employer sites willing to host program participants, and successfully placed participants can work up to 30 weeks with 100 percent of the cost of their wages (set at the state minimum wage) paid out of ReHire funds.[13] The host employers are often relatively small (roughly two-thirds have 50 or fewer employees),

[11] ReHire Colorado was modeled after Hire Colorado, an earlier program that used TANF emergency funds to place participants into subsidized work with private or public employers.

[12] Service providers have changed throughout the span of the program and through December 2018 have included Catholic Charities Pueblo, Discover Goodwill of Southern and Western Colorado (Colorado Springs), Goodwill Industries of Denver, Hilltop Community Resources (Grand Junction), Larimer County Workforce Center (Fort Collins), Rocky Mountain Human Services (Denver), Workforce Boulder County, and Colorado Coalition for the Homeless (Denver).

[13] Appendix Table A-1 reports the state minimum wage during the evaluation period, which increased from \$8.23 to \$12.00.

and placements occur across a variety of industries, with about half in Health and Social Assistance or Retail Trade.[14] Notably, job developers are explicitly encouraged to recruit host-site employers where a successful temporary employee has a strong possibility of being hired into an unsubsidized position.[15] This program feature distinguishes ReHire from some other transitional jobs programs that rely on public-sector positions or that provide temporary jobs with no direct pathway to or expectation of permanent employment. The local agency partner serves as the employer of record for the period of subsidized employment and is responsible for all other HR-related costs, such as worker's compensation insurance. The employer host site therefore has no direct monetary costs during a worker's transitional job, but they are responsible for reporting hours to the agency, evaluating the participant, and providing feedback and coaching.

Due to the population served, the program further includes supportive services and training to address barriers to work and to improve participants' reliability and productivity. Case managers work one-on-one with participants to develop an individualized service plan, which includes a minimum of one hour of coaching each month. Case managers have access to funds to support education and training (e.g., to cover the cost of a CDL or cosmetology training), which participants could pursue prior to or contemporaneously with their job placement. Financial assistance is also available to reduce employment barriers faced by the participant—for example, providing bus passes or gas vouchers; purchasing tools, equipment, or uniforms needed for work; or to incentivize positive workforce behaviors, such as consistent on-time attendance.

ReHire serves a broader population compared to similar subsidized employment programs that tend to focus on a single target population (e.g., recently-released inmates or TANF recipients). All Colorado adults with a family income lower than 150 percent of the federal poverty level and who have been unemployed or underemployed for at least four consecutive weeks are eligible.[16] The legislation authorizing the program identified three priority categories of participants: displaced older workers (aged 50+), non-custodial parents, and veterans. CDHS stipulates that local service agencies prioritize these groups when recruiting by requiring that 70 percent of applicants belong to at least one of the categories. Once applicants have been recruited, their membership in a priority group does not affect the likelihood that they are granted access to the program. Finally, applicants must meet at least five items from a standardized 10-item suitability screen to ensure their readiness for the program.[17]

[14] Table 5 includes a complete breakdown of firm size and industry for the subsidized job placements.

[15] Even prior to the RCT evaluation, ReHire administrators tracked the share of placements that led directly to permanent positions as a performance metric for the local agencies administering the program.

[16] The statutory eligibility specified underemployment as working less than 20 hours a week. To be eligible, an applicant needed to provide self-attestation that they were unemployed or underemployed for at least four consecutive weeks. During the evaluation period, individuals needed to self-attest that they were eligible to work in the United States.

[17] The 10-item list includes the following items: veteran, outstanding child support order, older worker, receiving SNAP or

Given the individualized nature of the ReHire program, a participant's timeline of service receipt can vary substantially depending on which program components they choose to use and for how long. Some participants receive only supportive services and exit the program fairly quickly. Among those who are placed in transitional jobs, program duration depends on both the time to placement and the length of the placement. In the end, most participants exit ReHire within six months of their application, and nearly all stop receiving services within one year.[18]

I.B Conceptual Framework

This subsection summarizes a model that describes how subsidized employment programs can influence longer-run employment outcomes. The program's wage subsidy and supportive services directly raise employment and earnings during participation. This model incorporates several mechanisms through which participation can also improve labor market outcomes even after services end: productivity improvements (e.g., through on-the-job learning or coaching); improving future employers' beliefs about productivity (i.e., "filling a hole" in the resume); and increased worker experimentation and employer learning.

In Appendix Section B, we adapt ideas from models of employer learning (Jovanovic, 1979, 1984; Altonji and Pierret, 2001; Pries and Rogerson, 2005, 2022) in which firms are initially uncertain about a worker's productivity and learn about productivity as they observe performance on the job. Specifically, we consider a partial equilibrium two-period search model with an initial hiring decision made under uncertainty and a subsequent retention decision made under full information. The model focuses directly on the riskiness of employer-side hiring, rather than on the labor supply and search effort of the worker. As a result, our theory of change stands in contrast to other models that consider how wage subsidies can help overcome high reservation wages and "make work pay" (e.g., Lise, Seitz and Smith, 2004; Card and Hyslop, 2005; Abel et al., 2022).

When an employer initially meets a potential employee, the employer sees limited information about the applicant. Employers use this limited information to form an expectation of the worker's productivity at that employer in that period. The employer observes two pieces of information: an application-quality group, which summarizes the strength of the worker's resume and work history, and an interview that provides an unbiased but noisy signal of the worker's true productivity in the job. The employer's expectation of the worker's productivity is a weighted average of the mean productivity for the worker's

other public assistance, safe/stable housing, reliable transportation, good health and able to work, able to pass a drug test, have GED or HS diploma, excited about getting back to work.

[18] Appendix Section A.2 provides additional details on service receipt and timing.

application-quality group and the signal, placing greater weight on the signal when it is relatively more precise.

Hiring is therefore risky because the employer is uncertain about whether the match will be profitable until after observing the worker's performance. When hiring, the firm considers both the expected payoff in an initial startup period and the option value of being able to retain a productive match in the subsequent period. If the worker is hired, the employer learns the worker's true productivity for subsequent periods, which can include any improvements due to training or on-the-job learning during the initial period. The firm thus makes retention decisions with full information for workers who were previously hired. Workers who are not hired in the initial period, or who are hired but not retained, have the opportunity to match with a new employer in subsequent periods. The subsidy and reduced startup costs available to members of the treated group in the initial period reduces risk and incentivizes experimentation, allowing the employer to hire workers whom they otherwise would have declined to hire.

The framework highlights three mechanisms through which program participation can affect post-program employment:

1. **Productivity improvements:** Members of the treated group will be more likely to form initial matches because they are subsidized. This additional time working raises their productivity through experience, training, and mentoring. Some of this increase in human capital is firm-specific—it improves the worker's productivity only as long as they stay with the same employer—while other learning is portable, improving productivity in future jobs with other employers. The program's supportive services can also raise productivity by providing lasting solutions to barriers to work, such as transportation or child care problems.

2. **Potential employers' beliefs:** Employment itself conveys information to future employers. Even if true productivity does not change, having recent work experience can shift how employers interpret a worker's potential. In the model, this mechanism corresponds to moving from a "low-quality" to a "high-quality" applicant group when searching for jobs after the program. Resume assistance provided by the program can further reinforce this channel. Improving employers' beliefs about the worker's productivity will increase the likelihood that they are hired the next time that they meet an employer.

3. **Information revelation:** By reducing the cost of experimentation, the wage subsidy encourages firms to hire workers they might otherwise consider too unlikely to yield a profitable match. When a

period of subsidized employment reveals that a worker-firm match is productive, the firm retains that worker after the subsidy ends. This opportunity to be evaluated based on true productivity rather than an unbiased but incorrect expectation can improve employment outcomes for some program participants.

The two-period setup of hiring under uncertainty and retention under full information generates two distinct pathways to improved post-program employment. Workers who are hired and retained by their initial employer because of the program may benefit from increased firm-specific or portable human capital, or through information revelation. Workers who leave but find new jobs elsewhere may benefit from increased portable human capital or from improving future employers' beliefs because they have more recent work experience.

These distinctions motivate our descriptive analysis by program experience in Section IV. "Program experience" refers to the participant's hiring and retention outcomes with the host-site employer. Comparing post-program employment outcomes across these groups, as well as to the control group, allows us to infer which mechanisms are most operative. Although program experience is not randomly assigned, we provide evidence of minimal selection into these categories. Descriptively, nearly all post-program gains in employment and earnings accrue to participants who remained employed at their host-site employer after the subsidy ended. This pattern suggests that information revelation is a key mechanism behind the program's effects, potentially in combination with human capital improvements gained during the placement. We find no evidence that portable human capital, barrier resolution, or signaling alone are sufficient to generate sustained improvements.

Finally, the model provides comparative static predictions about when and for whom subsidized employment will be most effective. First, the experimentation value from the subsidy increases with uncertainty about worker productivity. When employers are highly uncertain about applicants' productivity—such as for the long-term unemployed or those with limited work histories—the option value of experimentation is large, and wage subsidies induce many new matches. Second, the benefits are largest when expected productivity is close to the wage. When expected productivity is far below the wage, few subsidized matches will prove successful; when it is far above, those matches would have formed without the subsidy. Thus, small reductions in wage and startup costs for "near-margin" workers can shift a match from unprofitable to profitable, generating both short- and long-run employment gains. These comparative statics inform our additional analysis that predicts program experience using both individual and firm characteristics.

II Experimental Impact Evaluation

We partnered with CDHS to design an RCT evaluation of ReHire's impact on participants' in-program and post-program outcomes.[19] From July 2015 through December 2018, individuals applied to the program on a rolling basis, completed a baseline survey, and were then randomly assigned to either a treatment or control group. Only the treatment group received access to ReHire services, but CDHS tracked outcomes for both groups in administrative data. A follow-up survey administered approximately 18 months after application and administrative credit data provide additional outcomes.

II.A Baseline Survey

All program applicants during the RCT evaluation period ($N = 2,496$) completed a baseline survey, which was collected by staff at the local agency partner prior to randomization. The baseline survey measured an applicant's employment and wage history, existing skills and barriers to employment, education, childcare situation, any health difficulties, criminal background, struggles with homelessness or substance abuse, and other economic hardships.[20,21] The survey also included a measure of mental health using the Center for Epidemiological Studies of Depression (CESD) scale, a scale for grit (Duckworth et al., 2007), Big Five personality traits (Donnellan et al., 2006), cognitive ability (Raven, Court and Raven, 1984), and a timed math test created for the purposes of the baseline survey.[22] At the end of the survey, the caseworker scored the applicant's job readiness along two margins: their "motivation to get back to work" and their "likelihood to overcome employment barriers." In most cases, the intake appointment was the client's first interaction with the caseworker. The subjective scoring was based primarily on this meeting, which could include things observable to the researcher (e.g., survey responses), but also unobservable information (e.g., promptness, dress, behavior during survey, information from small talk, etc.).

[19] While our evaluation was not guided by a formal pre-analysis plan, an April 2015 update on the evaluation design presented to CDHS prior to the launch of the RCT specified the use of state administrative data in an RCT evaluation of ReHire and the analysis in this paper largely follows that original proposal. In the status update, we report power calculations on the following outcomes: annual earnings, annual employment rate, number of quarters worked in a year, quarterly earnings, and quarterly employment. We also specify looking at participation in the Basic Cash Assistance program (TANF) and SNAP, as well as looking at "a full calendar year after [ReHire] participation ends to evaluate labor market effects fully." Finally, the update also notes our plan to use a baseline survey to explore treatment effect heterogeneity. Since that time, the evaluation expanded to include an 18-month follow-up survey and Experian credit data. The April 2015 evaluation progress update, the baseline survey instrument, and the follow-up survey instrument can all be accessed at the AEA RCT Registry (AEARCTR-0011083).

[20] Many of the survey questions regarding previous employment and barriers to future employment were adapted from the Women's Employment Survey (Tolman et al., 2018).

[21] We are missing the baseline survey for one individual, but they can still be linked to administrative data outcomes. They are not included in analysis that relies on the baseline survey (e.g., heterogeneity analysis).

[22] The 3 minute-timed math test included 160 addition, subtraction, or multiplication problems using numbers from 1 to 10.

II.B Randomization

Randomization took place after program intake. Caseworkers submitted an individual's application to CDHS, and CDHS informed both the applicant and the caseworker of the applicant's random assignment status by text and email message, usually within one business day. Applicants were randomly assigned to either a treatment group who received access to ReHire-funded services or to a control group. To ensure that the treatment and control groups were well-balanced within sites and that caseworkers had a steady workflow, randomization was stratified at the service agency level, and the randomization method ensured that treatment and control assignments were balanced over small sets of arriving applicants.[23] The probability of treatment was set to 50 percent at the start of the RCT and was adjusted to be as high as 66 percent for service agencies in rural areas and during time periods when enrollment was low. Appendix Section A.3 provides more details on the randomization procedure.

Once placed into the control group, applicants were ineligible to enter the lottery again, and internal controls prevented repeat applications by the same individual, even if they applied through a second service agency.[24] The control group retained access to the usual services provided in the local area and remained eligible for other job assistance programs operating during the RCT time period, including those offered by ReHire service agencies or elsewhere. These programs may have included access to transitional jobs with alternative funding sources, including the Workforce Innovation and Opportunity Act (WIOA).

II.C Experimental Sample and Baseline Balance

ReHire applicants represent a diverse cross-section of lower-income Colorado residents, reflecting the program's broad eligibility criteria (see Table 1). More than two-thirds of applicants received SNAP and roughly three-quarters were covered by Medicaid during the month when they applied. Applicants had notable barriers to re-employment including inconsistent work histories (the typical applicant worked in only 40 percent of the prior 12 quarters), transportation barriers (20 percent did not have a valid driver's licence), felony convictions (24 percent), work-limiting health problems (10 percent), and history of substance abuse (23 percent). Compared to similar subsidized employment programs that target a single population such as ex-offenders, non-custodial parents, or TANF recipients (Barden et al., 2018; Anderson

[23] A possible concern from the randomization procedure is that it induced serial correlation in treatment status among individuals who applied at the same agency around the same time. In Section III.A3, we discuss how our results are robust to a randomization-based inference procedure that directly accounts for the specific method of randomization.

[24] Contamination of the ReHire program in the control group was minimal. Two members of the control group were accidentally entered into ReHire's administrative database as treated and thus received access to services. They remain members of the control group for analysis.

et al., 2019), the ReHire applicant pool is more diverse, although it includes the target populations from previous evaluations. The ReHire sample is similar to the low-income adult population in Colorado (see Appendix Table A-3), although they tend to be more connected to the social safety net, and veterans and older workers are over-represented in the sample.

Random assignment produced baseline balance as expected. Table 1 provides descriptive statistics and demonstrates treatment/control balance across a wide set of pre-randomization characteristics measured in administrative and survey data, including work-related outcomes, barriers to employment, job readiness, target group membership, and cognitive and non-cognitive skills. The differences in means between the treatment and control groups are minimal for the 37 characteristics—no difference is larger than 0.08 standard deviations and the treatment/control difference is statistically significant at the 10 percent level for only two characteristics: percent male and life satisfaction rating. For precision, we include analysis with and without controls for baseline characteristics, as discussed below in Section II.E.

II.D Outcome Data

Our analysis relies on multiple administrative data sources and an 18-month follow-up survey. Outcomes from state administrative data are created from unemployment insurance (UI) earnings records collected by the Colorado Department of Labor and Employment (CDLE) and SNAP/TANF benefits records from CDHS. The earnings data are available on a quarterly basis from Q1 2010 through Q4 2022, and the benefits data are available on a monthly basis from January 2004 through April 2023. We use these data to construct a balanced panel of outcomes during the three years prior to and four years following an individual's application date, which allows us to examine program impacts both while treatment group members received services and for at least three years after they left the program.

The CDLE data provide quarterly information about earnings from jobs covered by UI in Colorado. Earnings from transitional jobs are reported with the service agency as the employer of record, and we include these earnings when constructing outcome variables. These data do not, however, capture earnings when individuals worked informally or as an independent contractor, which may be the case for jobs held by applicants before or after their transitional job. In quarters when an individual does not have a wage record, we treat them as having zero earnings and code them as not being employed. Outcomes based on this data source, therefore, are best interpreted as measuring formal-sector employment and earnings in the state of Colorado. We deflate all dollar values to July 2015 levels using the CPI-U (US Bureau of Labor

Statistics, 2023), and winsorize earnings at the 99$^{\text{th}}$ percentile within calendar quarters and treatment status. In addition to the dollar amount of earnings, we create a variety of outcomes for having any earnings in a given quarter or for earning any amount over a relevant period of time.

A potential limitation to using state-specific administrative data is that outcomes are observable only when they occur in the state. We unfortunately cannot distinguish between zero earnings in a quarter and earnings that occur outside Colorado as both are indicated as missing in the data. This ambiguity creates a potential interpretation challenge when program applicants move out of state, especially if migration rates are different by treatment status. To quantify the importance of this issue, we linked ReHire applicants to their address histories as compiled by Infutor Data Solutions to measure directly how often individuals in the sample move out of the state. Rates of non-Colorado residencies are low overall and are similar between the treatment and the control group in the two years following application (Appendix Figure A-2a), which suggests that Colorado-specific administrative data are appropriate for measuring key outcomes and that selective interstate migration is unlikely to affect the interpretation of our results.[25]

In order to consider program impacts on a broader set of outcomes, we use data from two additional sources. First, an online follow-up survey was administered roughly 18 months after application, which is approximately one year after the typical participant exited the program.[26] This survey provides a repeated measure of many of the individual skills and barriers measured in the baseline survey, employment and earnings information for all jobs held since application including self-employment or contract work that did not generate a UI record, detailed information on the first unsubsidized job after the respondent applied for ReHire, and information on the respondent's job at the time of the survey. The survey response rate was roughly 40 percent, with a higher response rate in the treatment than the control groups (42 percent vs 34 percent). Details on selective nonresponse and the reweighting procedure used to address attrition are in Section III.B. Baseline characteristics are relatively balanced between treatment and control respondents, even without reweighting. Second, we link ReHire applicants to quarterly data about credit score, credit utilization, and credit-seeking behavior provided by Experian. Match rates are similar between the treatment and control groups—roughly 62 percent. We provide additional details about these supplemental data in Section III.C.

[25]This analysis is consistent with data from the American Community Survey (Ruggles et al., 2020) that show only 3.5 percent of Colorado residents with less than a bachelor's degree left the state between 2015 and 2016.

[26]Given the initial timing of survey implementation (December 2017), first-year applicants would have received the survey up to 2.5 years after application. Respondents typically completed the survey 20 months after ReHire application, and the timing between application and response was similar between the treatment and control groups. See Appendix Figure A-3 for the distribution of months since application for treatment and control group survey respondents. When estimating effects on outcomes from the follow-up survey, we include months since application fixed effects.

II.E Empirical Strategy

We exploit the RCT design and estimate ITT effects of gaining access to ReHire Colorado using the following linear regression specification:

$$y_i = \beta T_i + \gamma_{s(i)} + \epsilon_i, \tag{1}$$

where y_i is an outcome for individual i and T_i is an indicator that takes the value of 1 for individuals assigned to the treatment group and 0 for individuals assigned to the control group. The vector $\gamma_{s(i)}$ is a set of stratification fixed-effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally over the RCT period.[27] In addition to this parsimonious regression, we report additional estimates of β from specifications that use a post-double-selection LASSO procedure (Belloni, Chernozhukov and Hansen, 2014) to select optimal controls from a high-dimensional set of baseline characteristics X_i to address slight baseline imbalances and to improve precision.[28] Results are similar for all outcomes with and without controls.

The parameter β is the average treatment effect of access to ReHire-funded services relative to the standard job search assistance available to the control group, and it includes the effect of any interaction between standard services and ReHire-specific services. The interpretation of β depends on the degree to which the control group had access to services that are similar to ReHire, such as transitional jobs, through other programs offered by the same or other service providers in the area. While the receipt of close-substitute services is not a threat to causal identification, it could reduce the size of ITT effects and lead ReHire to appear less cost-effective (Heckman et al., 2000; Kline and Walters, 2016). We show in Appendix Section A.8 that control group members rarely had UI-covered earnings from a ReHire agency—a proxy for working a transitional job—and less than 10 percent of follow-up survey respondents from the control group report working in a subsidized job following application (see Section III.B). We further show that accounting for access to other transitional jobs programs does not qualitatively change the key

[27] The strata (s) fixed effects allow for treatment-control comparisons within a contiguous block of applicants from the same service agency that faced the same effective randomization probability. Two service agencies had more than one physical location and the randomization was stratified at this sub-agency level to ensure sufficient flow of program participants. The rate of acceptance was also higher for the rural areas. Appendix Section A.3 provides complete details on the randomization procedure and how $\gamma_{s(i)}$ is constructed.

[28] The set of potential controls includes: quarterly employment and earnings in the 12 quarters preceding application; summary measures of employment (e.g., any or no work) in the 1, 2, and 3 years before application; SNAP and TANF participation in each of the 24 months preceding application; total SNAP and TANF benefits received in the last 12 and 24 months; and a set of indicators for gender and educational attainment. The LASSO procedure typically selects pre-program work history measures, which is consistent with the slight imbalance in gender and that prior earnings are predictive of future earnings.

findings (see Section III.A3).

Our analysis reports ITT estimates because program take-up was high. Among the treatment group, 88 percent met with a caseworker to start a ReHire case plan post-randomization, 72 percent received individually-billable direct cost services (supportive services, a transitional job, or both), and 65 percent were placed in a transitional job.[29] Under the assumption that the 28 percent of treatment group members who received no direct-cost services had program experiences similar to the control group, treatment-on-the-treated effects can be calculated by scaling up the ITT effects by 38 percent.[30]

The presence of the COVID-19 pandemic during our evaluation period does not pose a threat to the study's internal validity, because treatment and control group members were exposed to the pandemic at the same time. The pandemic likely depressed the overall level of earnings and employment outcomes for both groups relative to what we would have observed in a more typical labor market.[31] However, the pandemic complicates the interpretation of the post-program effects to the extent it affected the difference in outcomes between the treatment and control groups. If the labor market disruptions caused some treatment group members to lose jobs that they had initially obtained through the program, and those jobs would have persisted absent the pandemic, then the difference in outcomes between the treatment and control groups may understate the program's longer-term impacts in normal conditions.

In light of this potential complication, we estimate Equation (1) using outcomes measured during four distinct time periods: (i) a pre-program period that includes up to three-years prior to application; (ii) an in-program period; (iii) an initial post-program period that was less affected by the pandemic; and (iv) a second post-program period during which a majority of the RCT sample had already experienced pandemic-related disruptions. These time periods are measured relative to each individual's ReHire application (time 0) and consist of different calendar periods from applicant to applicant. The typical transitional job placement started within a month of randomization and lasted 2 to 3 months, but some participants

[29] Just under one in six individuals randomized into the treatment group received no services through ReHire within twelve months of gaining eligibility. Case notes suggest that approximately one third of these participants (4 percent of all participants) found unsubsidized employment independently before beginning the program, and the remaining two-thirds (8 percent of all participants) either left voluntarily or were deemed not to be a good fit for the program by the caseworker.

[30] Scaling the effect this way requires no impact of gaining access to ReHire services among treatment group members who did not receive services, i.e. the no-shows (Bloom, 1984) or never-takers (Jones, 2015). This condition could be violated, for example, if the possibility of a transitional job changed an individual's search behavior. Because we do not have any direct evidence of whether this assumption holds, we report ITT effects as our preferred estimates.

[31] Colorado reported its first COVID-19 cases in March 2020 and quickly issued statewide stay-at-home orders, closing schools, restaurants, and most nonessential businesses. In May 2020, the state shifted to a "Safer-at-Home" phased reopening, with counties along the Front Range gradually lifting restrictions through summer 2020 as case counts allowed. Renewed restrictions were imposed during later waves (fall 2020, winter 2021), and a statewide mask mandate was in place from July 2020 to April 2021. Widespread business reopening occurred in mid-2021 as vaccinations expanded, though conditions remained volatile until late 2021.

were still working in their transitional job within 12 months of application.[32] Because of this variation in service receipt timing, we consider the in-program period to be quarters 0 through 4 (months 0 through 12). For all applicants, the entire in-program period occurred prior to the end of 2019. The post-program pre-COVID period includes quarters 5 through 11 (months 13 through 35) relative to random assignment, and the post-program COVID period includes quarters 12 through 16 (months 36 through 52). Quarter 12 occurs in the first quarter of 2020 or later for more than half of the sample (see Appendix Figure A-4). For each interval, we estimate program impacts on outcomes measured at particular points in time, such as quarterly earnings. We also construct outcomes aggregated over the full interval including, for example, average earnings, an indicator for having any formal sector earnings during the interval, an indicator for working every quarter in the interval, and the share of quarters worked.

To estimate program impacts on outcomes from the follow-up survey and credit data we construct families of similar outcomes from each data source and report the average standardized treatment effect among those outcomes. For each outcome family with K outcomes, we estimate

$$\hat{\tau} = \frac{1}{K} \sum_{k=1}^{K} \frac{\hat{\beta}^k}{\hat{\sigma}_k} \tag{2}$$

where $\hat{\beta}^k$ is the ITT effect of the k-th outcome in the family, which we scale by the standard deviation of that outcome among the control group $\hat{\sigma}_k$. In averaging treatment effects, we re-sign some outcomes so that positive treatment effects represent improvements. We follow Finkelstein et al. (2012) in stacking the data for all K outcomes and jointly estimating the ITT effects in a single regression, clustering standard errors at the individual level.

III Intent-to-treat Impacts of ReHire Colorado

We first provide analysis of outcomes built from UI earnings records and from SNAP and TANF payment records, which are available for all applicants. We then examine outcomes from the follow-up survey and credit data along with an empirical analysis of selection into data coverage for these additional sources.

[32] Appendix Figure A-1 provides additional details on the distribution of time to placement and time to program exit.

III.A Outcomes from State Administrative Data

III.A1 Employment and Earnings

Figures 1a and 1b depict trends in formal sector employment and earnings in Colorado, respectively, by treatment assignment. The horizontal axis shows quarters relative to an individual's application for ReHire. The portion of the graph to the left of the first dashed vertical line indicates the pre-program period. The next two vertical dashed lines separate the in-program and the two post-program periods. Figures 1c and 1d plot coefficient estimates and 95 percent confidence intervals for β from estimating Equation (1) using indicators for being employed or the level of earnings in a given quarter relative to application as the dependent variable.[33]

Prior to the program, roughly 40 percent of applicants worked in any given quarter (Figure 1a), and trends in employment rates were similar in the treatment and control groups.[34] During the in-program period, employment initially rises and then falls for both groups. One quarter after application the employment rate of the control group increased to 57 percent. Control group employment improvements could arise for multiple reasons such as (i) participation in other workforce interventions (e.g., job search assistance, resume writing), (ii) within-person selection whereby individuals apply for assistance when they are particularly motivated to increase their labor market attachment, or (iii) strong mean reversion as transitory labor market shocks resolve and workers return to their long-run average.[35] Despite these improvements among the control group, the treatment group experienced a 20 percentage point larger increase in their employment rate, with more than 75 percent employed one quarter after application. Consistent with the timing of transitional job exits (see Appendix Figure A-1 and Appendix Figure A-5), employment rates among the treatment group decline more rapidly than among the control group, with quarterly differences falling to 12.0 and 5.5 percentage points in quarters 2 and 3, respectively. The differences in quarterly employment rates remain statistically significant at the 1 percent level for each of quarters 0 through 3. By the fourth quarter after application, the gap between the treatment and control group falls to 3 percentage points and is no longer statistically significant. During the two post-program periods, employment rates continue to decline for both groups. During the earlier pre-COVID period,

[33]For reference, Appendix Table A-4 provides the exact numerical values of the coefficients and standard errors for the in-program and post-program effects shown in Figure 1c and Figure 1d, and shows that results are insensitive to the inclusion of controls selected by the post-double selection LASSO procedure.

[34]None of the pre-randomization differences in quarterly employment rates are statistically significant at conventional levels (see Figure 1c). The p-value from a test of the null that the differences for all twelve quarters are jointly zero is 0.456.

[35]For additional discussion on the nature of the selection into program participation in related contexts see Heckman and Smith (1999).

quarterly differences in employment rates range from 2.5 to 4.3 percentage points, but are statistically significant only in quarter 8. During the period that was affected by the COVID-19 pandemic, however, differences are small and close to zero.

Earnings experience a stark downward trend for both groups prior to application (Figure 1b). There was no similar negative trend in quarterly employment, which suggests that these earnings losses occurred through either a loss of work hours, a decline in wage rate, or weeks of non-employment within a quarter with at least some employment, which could have been caused by either transitory or persistent shocks. Earnings rebound in the quarter following application for both groups, and, for the treatment group, average earnings exceed pre-program earnings for all sixteen post-application quarters. Because earnings are a more variable outcome, the quarter-by-quarter effects are not often statistically significant (quarters 0 through 2, which are significant at the 1 percent level, are the exception—see Figure 1d). Differences in the post-program, pre-COVID period are economically meaningful and typically represent a 5–10 percent increase compared to the mean of the control group. As with employment rates, differences become small and close to zero during the final analysis period.

We also construct aggregate employment outcomes measured during and after ReHire to directly assess the program's impact on labor market attachment, which is another important outcome that cannot be observed directly through changes in quarterly employment rates. Similarly, we construct aggregate earnings measures to mitigate quarterly variability. Table 2 reports effects on six outcomes: any employment during the period; the share of quarters employed; employment during every quarter of the period; share of quarters employed at the Q1 employer; average quarterly earnings during the period; and the share of quarters with earnings above 130 percent of the federal poverty level.[36,37] For each outcome, we report the control group mean (column 1), the ITT effect controlling only for stratification fixed effects (column 2), the ITT effect when additionally controlling for LASSO-selected baseline characteristics (column 3), and the estimated program effect from column 3 as a percentage of the control group mean (column 4).

ReHire access improved multiple labor market outcomes during the in-program period (Panel A). Consistent with the quarterly results, the treatment group was 14 percent more likely to work at all (11.6

[36] The "employment with the Q1 employer" variable is coded as follows: for each quarter in a period, we code a participant as one when they have earnings from their first quarter (Q1) employer and zero otherwise. For applicants who were in a transitional job in quarter 1, we count them as working for their Q1 employer if they have earnings from either the relevant local agency (i.e. as part of their initial placement) or from the host site directly (i.e. after successfully transitioning to unsubsidized employment). If applicants have earnings from multiple employers in the first quarter, we treat the employer from whom they earned the most as their Q1 employer. Applicants with no Q1 earnings do not have a Q1 employer and are coded as 0 for this outcome in every quarter.

[37] When determining whether an individual earned more than 130 percent of the federal poverty level, we use the HHS poverty guidelines for a single individual for the calendar year of the wage record.

percentage points) and 31 percent more likely to work every quarter (7.3 percentage points), both of which are statistically significant at the 1 percent level. Earnings were also positively affected. When including baseline controls, the impact on earnings is $301 per quarter and is statistically significant at the 1 percent level. These earnings gains increased the likelihood that an individual had earnings above 130 percent of the federal poverty level by 14 percent.[38]

Some of the impacts persisted into the post-program, pre-COVID period (Panel B). While the treatment group was no more likely to have worked at any point during the period, they worked in 5 percent more quarters (not statistically significant) and were 3.8 percentage points more likely to have worked in every quarter ($p < 0.05$), a 16 percent increase relative to the control group. Moreover, the treatment group was 3.4 percentage points more likely to continue employment with their Q1 employer nearly three years after random assignment.[39] The treatment group also experienced a $154 increase in average quarterly earnings and a 1.6 percentage-point increase in the likelihood of earning at least 130 percent of the FPL, although neither effect is statistically significant. During the post-program COVID period (Panel C), however, all point estimates except employment with Q1 employer are small and statistically indistinguishable from zero. Ninety-five percent confidence intervals rule out differences in employment rates greater than 3.3 percentage points and differences in earnings greater than $254. A small share of the treatment group, however, continued to experience stability in the labor market. Even four years later, the treatment group was 69 percent (2.8 percentage points) more likely to be employed with their Q1 employer.

III.A2 SNAP and TANF Receipt

One stated goal of programs like ReHire is to increase participants' incomes enough to allow them to achieve self-sufficiency and to reduce their reliance on future payments from programs such as SNAP and TANF. Because ReHire was targeted to a broad set of low-income participants, many were not eligible for TANF benefits, and only a relatively small share (10 percent) received a TANF payment in the year prior to application. In contrast, more than two-thirds of applicants received at least one SNAP payment over that same time period, so there was more scope for ReHire to have an impact on future receipt. As shown in Figure 2, the high SNAP participation rate at program application represents the peak of a steep increase

[38] In Appendix Section A.10, we explore whether the program affected the likelihood of earnings above thresholds of the federal poverty line from 0 percent to 300 percent. We find statistically significant gains in the share with earnings above thresholds up to roughly 150 percent of the poverty line.

[39] This effect seems to be driven by increasing the likelihood of Q1 employment. When conditioning on the sample with Q1 employment, the point estimate is 0.2 percentage points (results not reported), which suggests that the wage subsidy is not substantially changing selection into placement. If transitional job placements were negatively selected on match quality, we might expect the difference in the sample with any Q1 employment to have been negative.

in participation that occurred over the prior 12 months. This increase in participation corresponds with the decline in earnings over the four quarters prior to application (Figure 1b), and these two trends suggest that ReHire applicants often experience a shock to their life circumstances prior to application. Following randomization, however, both groups experience similar declines in SNAP and TANF participation over the next 36 months with the exception of the post-program COVID period, where SNAP participation increases in the treatment relative to the control group.

ReHire did not have an appreciable effect on participation in either SNAP or TANF during the three years following random assignment, but may have helped participants connect to resources during the pandemic (Table 3). We find no economically meaningful or statistically significant differences between treatment and control groups in benefit receipt for either program during the in-program and first post-program period.[40] During COVID, however, ReHire increased SNAP participation by 3.1 percentage points and increased average monthly SNAP receipt by \$14 ($p < 0.05$).

III.A3 Robustness

This section shows that the aggregate results on employment and earnings in Table 2 are robust to addressing the possibility that the control group received similar services from other programs, to alternative methods of conducting inference, and to adjustments for multiple hypothesis testing.

First, the type and intensity of services received by the control group potentially affects the interpretation of the estimated ITT impacts. As discussed above, both the UI data and follow-up survey responses suggest that few in the control group worked in a transitional job. Control group individuals at one agency, however, were nearly equally as likely to be employed by the service agency during the in-program period as the treatment group (Appendix Figure A-5b). Appendix Section A.11.1 confirms that program impacts are qualitatively similar, though stronger, when dropping applicants from this provider.

Second, our results are robust to alternative ways of conducting inference that account for the randomization protocol and for concerns about multiple hypothesis testing. Appendix Section A.11.2 discusses how we construct randomization-based p-values that test the sharp null hypothesis of zero treatment effect among all applicants and that take into account the way treatment assignment occurred. Using these p-values that come from 10,000 permutations of the randomization protocol, we show that the results remain significant after adjusting inference to control for the family-wise error rate among the main employment

[40]This lack of a differential is likely due to the fact that the program did not substantially increase the share of participants with earnings above 130 percent of the federal poverty level.

outcomes in Table 2 using the Westfall and Young (1993) step-down procedure (see Appendix Table A-6).

III.A4 Heterogeneity

We find limited support for treatment effect heterogeneity across two alternative approaches. We first present descriptive sample splits that report program effects separately for subgroups of applicants (Figure 5). In defining subgroups, we use characteristics that are known to be important in determining labor market outcomes and could also be related to a firm's expectation of the worker's future productivity—for example, gender, previous labor market attachment, education, grit, cognitive ability (Raven's), acquired skills (math), and the caseworker's assessment.[41] Across both employment and earnings outcomes during the in-program and post-program periods, the distribution of subgroup treatment effects (black circles) is clustered fairly tightly around the full sample average treatment effect (solid black line). Then, because there are many (likely correlated) potential characteristics to stratify on, we complement the subgroup analysis with a data-driven machine-learning approach Chernozhukov et al. (2020), which fails to detect any meaningful treatment effect heterogeneity (see Appendix Section A.12).

We interpret the results of these exercises as reinforcing the conclusion that the treatment effects of ReHire are relatively homogeneous, although confidence intervals are wide and we are likely underpowered to detect meaningful differences.[42] While the program might be able to increase in-program impacts by prioritizing participants who are least likely to work in the absence of the program, we do not find evidence that would allow service providers to improve the program's longer-term effectiveness through targeting.

III.B Outcomes from 18-Month Follow-up Survey

We next take advantage of the broader array of outcomes in the follow-up survey to show that ReHire reduced job turnover and improved job quality and personal well-being. Table 4 reports impacts on employment outcomes (Panel A), as well as standardized treatment effects on job quality (both for an individual's first unsubsidized job after application and their job at the time of follow-up), well-being, employment

[41] For each subgroup listed, the complementary subgroup(s) also appears in the graph. For example, the figure includes both "Did not work last year" and "Worked last year" as subgroups. For baseline characteristics measured continuously, we show splits based on above-median ("High") or below-median ("Low") values of the characteristic.

[42] As an additional test, we estimate the Fréchet-Höffding lower bound on the standard deviation of impact (Heckman, Smith and Clements, 1997; Djebbari and Smith, 2008; Buhl-Wiggers et al., 2024). We estimate a bound of \$164 (p-value = 0.206) for in-program earnings and \$250 (p-value = 0.134) for post-program pre-COVID earnings, where the p-values come from 10,000 simulations of the data under the null hypothesis of homogenous treatment effects (results not reported). These estimates are relatively small, representing 8–8.5 percent of the SD of earnings among the control group, and are imprecisely estimated. While we are unable to reject the null, we note that this result does not imply the absence of heterogeneity, but rather that the marginal distributions of earnings alone do not provide strong evidence of substantial heterogeneity.

barriers, workplace behaviors, and expectations about the future.[43] For all outcomes, we report control group means (column 1), ITT effects estimated using Equation (1) (column 2), estimates from a specification that re-weights the sample using inverse propensity score attrition weights (column 3), and estimates that further condition on a set of rich baseline controls selected using the same LASSO approach as the main analysis (column 4). Appendix Section A.13 provides additional details about the follow-up survey including a description of selection into survey response and details on how we construct the weights used to account for non-response. After re-weighting, the treatment and control respondents have similar baseline characteristics, and, importantly, estimated program impacts on administrative employment outcomes are similar in the full sample and in the subsample of follow-up survey respondents, which provides further confidence that selective attrition does not meaningfully bias the estimated treatment effects on other outcomes. The results in Table 4 are qualitatively similar across specifications, and we focus our discussion on the specification reported in column (4). Despite robustness to weighting for observable non-response, conservative bounds following Lee (2009) and Kling and Liebman (2004) are wide and include zero for all outcomes, as may be expected given the low and differential response rate (42 for treatment and 34 percent for control). As such, we caution the reader against overinterpreting the findings from the survey.

The first two outcomes reported in Panel A of Table 4 confirm that service receipt differed between the treatment and control groups. The treatment group was 45 percentage points more likely to report working a job where the ReHire service agency paid their salary, and only 9.9 percent of the control group reported having such a placement. This difference is consistent with the evidence that uses the administrative data proxy for subsidized employment (Appendix Section A.8). Moreover, the treatment group was 9.9 percentage points more likely to be working in an unsubsidized job that ReHire helped them find, compared to 1.6 percent in the control group.

The remainder of Panel A demonstrates that ReHire increased unsubsidized employment during the time since application. Access to ReHire increased the likelihood of any unsubsidized employment since application (4.7 percentage points) and employment at the time of the follow-up survey (6.0 percentage points), but neither of these effects are statistically significant. These impacts are slightly larger than quarterly effects 5 to 6 quarters after application estimated in the administrative data (Figure 1c). This difference could arise because these survey data capture not only UI-covered employment, but also gig work, contract work, and informal work. As a measure that aligns more closely with the administrative data,

[43] For information on the construction of the outcome families see Appendix Section A.13.3. We report impact estimates for the underlying components for the job quality indices in Appendix Table A-14 and for the well-being, employment barriers, workplace behaviors, and expectations indices in Appendix Table A-15.

we see that the effect on employment in a job that provides a pay stub or other government form is much smaller (less than 1 percentage point). Nevertheless, we find evidence consistent with the administrative data that ReHire reduced job turnover. The treatment group was 6.8 percentage points more likely to be working in the same job as their first post-application unsubsidized job ($p < 0.05$).

We also find evidence that ReHire improved job quality and well-being, but we do not find evidence of lasting improvements in soft skills or reductions in employment barriers. Panel B of Table 4 reports standardized treatment effects on six different outcome families. Job quality is measured for an individual's first unsubsidized job following ReHire application and for their current job at the time of follow-up, and the analysis sample for these two outcomes is restricted to respondents with the respective job.[44] Because these outcomes condition on post-application employment, they provide only descriptive evidence of differences in job quality. Employed treatment group individuals have a 0.17 standard deviation ($p < 0.01$) and 0.09 standard deviation ($p < 0.05$) higher job quality index for the first and current job, respectively, relative to their employed control group peers.[45] This index includes outcomes like self-reported job satisfaction, wage rate, consistency and availability of hours, and indicators for employer-provided benefits like vacation and sick leave or retirement contributions (see Appendix Table A-14). We also estimate a 0.17 standard deviation increase in well-being ($p < 0.01$), which includes improvements in life satisfaction and self-reported health and reductions in expectations of economic hardship and the depression scale (see Appendix Table A-15). Effects on employment barriers, soft skills measured by workplace behaviors, expectations about future employment, and reliance on government benefits are positive but small and not statistically significantly different from zero.

III.C Outcomes from Credit Data

Using a panel of administrative credit data for ReHire applicants, we find no evidence that ReHire improved credit outcomes.[46] Appendix Table A-21 reports control group means and ITT estimates on the

[44] In the case that an individual is still working in their first unsubsidized job following ReHire application, these two measures are based on characteristics for the same job. This is the case for the 27 percent of the control group and nearly 34 percent of the treatment group who have remained employed with the same employer (see Panel A).

[45] In the job quality index, we initially planned to include an indicator for whether the job provided a paystub or other government form as a measure of job formality. However, much of the variation in this measure was driven by movements into self-employment. Because it was not clear whether this indicator was measuring improvements or declines in job quality, we removed it from the index and instead report it as an outcome in Panel A, unconditional of whether the individual is working. If we were to include this measure in the index, the magnitude of the job quality index for current employment for the specification reported in column (4) falls to 0.047 and is not statistically significant.

[46] Appendix Section A.14.1 describes the selection into an Experian match (Appendix Table A-18), provides details on how we construct weights to adjust for attrition, and shows that the resulting matched sample is balanced on baseline characteristics between the treatment and control groups (Appendix Table A-19), and estimated program impacts on outcomes that are

underlying outcomes. During the year after application, the average credit score in the control group was 592, just below the threshold for a prime credit score. The average control group member had roughly \$31,500 in debt, including just under \$1,700 in credit card debt, and one in six had a car loan or lease. Many had accounts negatively impacting their credit—one in seven had a delinquent account, one-third had a derogatory account, and nearly two-thirds had some debt in collections.[47] As summarized by the standardized treatment effects reported in Panel C of Table 4, we find no statistically significant differences in post-randomization outcomes between the treatment and control groups. The 95 percent confidence interval can reject 0.032 and 0.043 standard deviation improvements in in-program and post-program credit, respectively.

IV Mechanisms and Program Impact Persistence

The analysis of state administrative data showed that ReHire had large positive impacts on employment and earnings during service receipt and smaller, but still positive, impacts in the first two years after program exit. We next provide additional analysis to examine the contribution of the three mechanisms discussed in the conceptual framework in Section I.B. Recall that the model (full version in Appendix Section B) demonstrates that ReHire could have affected post-participation labor market outcomes by i) improving a worker's firm-specific or portable productivity, ii) changing potential employers' beliefs about an applicant's quality group, and iii) revealing information about a worker's true productivity through increased experimentation.

We focus on determining whether the information revelation mechanism is operative. Confirming the importance of this mechanism has implications for future program design and for our understanding of how to support lower-wage workers in the hiring process more generally. The balance of the evidence below suggests that subsidizing experimentation so that firms can learn the quality of the match is a key way that the program affects long-term outcomes. Although this mechanism may work in combination with other productivity enhancements, multiple pieces of evidence suggest that this mechanism is necessary to produce meaningful post-program impacts.

The ideal experiment to test the importance of the information revelation mechanism would be to

measured in the administrative data (employment and earnings) are similar for the analysis sample and the credit data subsample (Appendix Table A-20), which reduces concerns about attrition bias in the credit data analysis.

[47] A delinquent account is one where the consumer has missed a required payment. A derogatory account has moved beyond late payment status and the creditor has taken action against the borrower.

randomize participants into two treatment arms with different potential for employer learning to affect post-program outcomes. The first arm would replicate the ReHire model, while the second arm would provide all ReHire services prohibit participants from working for their host-site employer after exiting the program. As we discuss in Appendix Section B, the information revelation mechanism is operative only through the retention decision, so removing this option for a random subset of applicants would isolate this mechanism's influence. This second arm is infeasible, however, because it requires preventing employers from voluntarily hiring workers for whom they are willing to pay the full cost of employment.

Although we cannot run this ideal experiment, we can use observational data from the ReHire RCT to examine post-randomization outcomes for treatment group members with different program experiences and thus different possibly operative mechanisms. We consider three mutually exclusive subgroups: participants who left the program without a TJ placement, participants who were placed in a TJ but who were not subsequently hired at their host site, and participants whose placement was followed by an unsubsidized job at the host site.[48] Comparing the subgroups' treated outcomes to each other and to the control group's informs our understanding of which mechanisms are likely operative. Of course, such comparisons are necessarily descriptive, and we are careful to consider possible sources of selection bias. Importantly, these comparisons allow us to assess the relative importance of the mechanisms behind the treatment effects of access to ReHire-funded services over and above the types of services available to the control group in the local workforce development ecosystem.

IV.A Descriptive Evidence Supporting Information Revelation as a Key Mechanism

Figure 3 provides trends in employment and earnings outcomes for these three subgroups and for the control group. All four groups have remarkably similar experiences in the labor market prior to application—roughly 40 percent work in a given quarter, and all experience a similar "Ashenfelter dip" in earnings. While not definitive, this similarity suggests that there are not substantial differences across these four groups in permanent productivity or in shocks to unobservable characteristics prior to program application.

In the quarters following application, the two treatment subgroups who received a TJ placement see a large increase in employment relative to the control group. Both the subgroup who eventually transitioned to unsubsidized employment at their host site (solid black line with circles) and those who did not (dotted dark gray line with triangles) were more than 30 percentage points more likely to be employed in the first

[48] Appendix Section A.15.1 provides details on how we identified successful subsidized to unsubsidized transitions within an employer across ReHire program records and administrative earnings data.

quarter following random assignment relative to the control group. In contrast, the post-application trend in the employment rate among individuals who did not receive a transitional job (dashed light gray line with squares) closely mirrors the trend among the control group (dashed gold line with diamonds).

The lack of a meaningful gap between outcomes for treatment group members without a placement and the control group's outcomes is consistent with the interpretation that barrier resolution and resume assistance alone had a minimal effect on post-program outcomes. Of course, there are multiple reasons why someone randomized into the treatment group may fail to be placed in a transitional job. They could choose not to continue participating in the program (recall that only 72 percent of treatment group members receive any direct cost services); they could receive some supportive services but fail to match with an available host site; or, they could receive some services and find unsubsidized employment prior to securing a subsidized placement. Nevertheless, this comparison suggests that the mechanisms that operate through TJ placements are quantitatively important in determining treatment effects.

Among those placed in a transitional job, post-program gains in employment relative to the control group persist only among those who were hired into an unsubsidized position at their host site. The employment rate for participants not hired by their host site converged to the rate for the control group by the fourth quarter, and the trends for both groups after that time are remarkably similar through the 11$^{\text{th}}$ quarter. This similarity suggests that improvements in portable human capital and/or future employers' beliefs due to the program are insufficient to lead to lasting employment gains on their own.

The participants who successfully transitioned to an unsubsidized job with the host employer, however, experience lasting employment gains. Although the employment rate for this group fell somewhat from the second through the fifth quarter, it remained roughly 20 percentage points higher than the rates of the other three groups throughout the post-program periods. Moreover, this group experienced substantial and persistent gains in earnings—more than $1,000 per quarter (Figure 3b).

Together, the set of results in Figure 3 is consistent with the interpretation that information revelation is a key mechanism underlying the treatment effects of ReHire. Under this interpretation, the supported and subsidized trial period induces employers to hire workers they otherwise would not have, and the subset of workers who are revealed to be sufficiently productive at the end of the trial period are retained into longer-term employment after the subsidy ends. Note that participants may benefit from this mechanism even if information revelation alone would not have been sufficient for the worker to be retained at the host site. Even workers who needed on-the-job training or other program services to become sufficiently productive to move into an unsubsidized job benefit from being considered for a post-program job based on their true productivity rather than their expected productivity.

Note that this interpretation does not require that employers' beliefs or portable human capital are unimportant in the low-wage lab6or market more generally. Instead, it implies that neither the portable human capital nor the improved work history from the program change future employers' beliefs sufficiently to lead to a more successful job search.

IV.B Alternative Explanations for the Decomposition

As described in more detail in Appendix Section B.6.1, however, it is possible that high employment rates among the subgroup hired and retained by their host site could occur even if information revelation were not an important mechanism. One alternative explanation is that some of these workers would have found post-randomization employment regardless of their treatment status but productivity improvements from the program allowed them to remain with the employer after the subsidy and supports ended. For example, the supports available during the program may have allowed workers to benefit more from on-the-job training or other potential human capital improvements. In this case, the gap in employment rates between this group and the others would still represent positive treatment effects, but information revelation would not have contributed because the worker's true productivity would have been revealed regardless of treatment status. A second alternative is that the participants who successfully transition from subsidized to unsubsidized employment could have higher productivity and thus be more likely to be employed in the post-program period regardless of their treatment status. One possible source of this selection bias is "cream skimming," in which caseworkers assign the most job-ready participants to transitional jobs with better odds of permanent employment so as to improve measured outcomes among participants (see Bell and Orr, 2002; Heckman and Smith, 2011, for further discussion).[49] Finally, the gap could arise if there were systematic differences in the host-site employers where successful transitions happen.

We begin by providing multiple pieces of evidence suggesting that selection based on baseline job readiness does not contribute meaningfully to the gap in outcomes shown in Figure 3. This evidence is important because the first alternative interpretation requires participants to have high enough productivity to be hired soon after randomization, even when assigned to the control group. Greater baseline job readiness among those hired and retained by their host site could also lead to better untreated outcomes

[49]These two possible alternatives correspond to subgroups 1b and 1c as described in Appendix Table B-1.

among this subgroup and contribute to selection bias as in the second alternative explanation.

First, the levels and trends of employment and earnings are very similar among all three treatment subgroups prior to application, which is inconsistent with meaningful selection on job readiness. Second, among those with a TJ placement, there are no meaningful differences in the rich set of baseline characteristics measured at the time of application and available to the caseworker and potentially observable by an employer through an interview process. Although there are slight differences between those with a placement and those without a placement, those hired by their host site and those who were not have very similar observable baseline characteristics that are typical measures of future success in a job such as work history, barriers to employment, cognitive skills, or non-cognitive traits (see Appendix Table A-23). Figure 4 demonstrates that the small differences in baseline characteristics between transitional job recipients who were and were not hired by their host site are quantitatively unimportant in explaining the gaps in post-randomization outcomes. The figure plots the within-service-agency gaps in quarterly employment between the two treatment subgroups (black circles). Even after flexibly controlling for both caseworker assessments of job readiness and all of the other characteristics reported in Appendix Table A-23, post-program employment gaps remain little changed (gold triangles). Overall, there is no evidence that the subgroup hired by their host site were more job-ready at the time of program application.

We next consider the possibility that those who are hired by their host site may have experienced more positive productivity shocks for reasons unrelated to treatment, which could lead to selection bias.[50] Although we are unable to rule out this possibility completely, we note that randomization implies that, on average, post-application shocks should be similar in both the treatment group (as a whole) and the control group. If the subsample of the treatment group who are hired by their host site had higher-than-average shocks, then the group not hired should have had lower-than-average shocks and thus should have outcomes that are worse than the control group's on average. Instead, Figure 3 shows that TJ holders not hired by their host site have outcomes that are very similar to the the control group's once their placements end. There is a small divergence in outcomes in quarters 12 through 16, but this gap appears after more than 18 months of similar post-program outcomes. The fact that the divergence appears only after most of the sample had experienced the pandemic-related shutdowns offers an alternative interpretation that the group

[50]It is also possible for this type of productivity shock to contribute to a positive treatment effect in combination with the information revelation mechanism. Suppose a worker is hired only because of the program and experiences a positive productivity shock that leads to retention at the host-site employer. If that increase in productivity would not have been large enough to improve their post-program employment outcomes at a subsequent employer, this worker experiences a positive program effect through the employer learning mechanism. If the increase in productivity is large enough that they would have been employed in the post-program period regardless of treatment, then this type of shock generates selection bias.

placed but not hired by their host site was particularly vulnerable to that labor market shock. Nevertheless, we note that this divergence may indicate a small amount of selection into the three treatment subgroups.

Additional analysis of the employment dynamics among the control group reinforces the conclusion that workers who are hired and retained in jobs they find after program application are not positively selected on prior labor market success or post-hire productivity shocks, which further supports the likely importance of employer learning in the lower-wage labor market. Appendix Figure A-10 examines these dynamics among individuals employed in the quarter following application, splitting the sample by those who are and are not retained by that employer. The figure reveals two relevant descriptive facts. First, pre-application employment and earnings levels and trends are consistent between the two groups, which suggests it is difficult for an employer to predict a worker's eventual productivity in advance. Second, those who are not retained by the employer return to their long-run employment rate and average earnings, which suggests limited selection on post-hire shocks. The fact that most of these employment spells end quickly—60 percent of those employed in Q1 are not employed with the same employer two quarters later—is consistent with the interpretation that match quality is difficult to determine in advance of hiring.

Finally, we use detailed administrative data on the host-site employers to examine the third potential alternative explanation—differences in the placements themselves. We matched host-site employers to administrative data on firm size, employment growth, turnover rates, industry, the distribution of earnings paid by the employer, and an employer fixed effect estimated using the universe of UI earnings records in CO (see Appendix Section A.15.3 for details). Table 5 shows that TJ host sites are different than the typical employer for this population: they are larger, tend to pay higher wages, and have higher earnings value-added. Interestingly, 20 percent of non-host-site employers are temporary employment agencies, which have been shown to not foster longer-term employment (Autor and Houseman, 2010). In contrast, only 0.5 percent of host sites are temp agencies. There are also some differences between host sites for successful and unsuccessful TJ participants. While they are similar in terms of size, growth and turnover rates, successful host sites differ in terms of their industry (more likely to be in wholesale trade and public administration and less likely to be in the health care/social assistance and professional services) and the wages they pay. Successful host sites tend to have relatively more lower-wage jobs, as evidenced by their 10^{th} percentile of wages, but tend to pay higher wages conditional on who they employ (i.e., their firm fixed effect). As discussed in the next section, this difference is consistent with a prediction from the model.

Despite these differences, the third line in Figure 4 (gray squares) shows the employment gap after adding controls for these rich firm-level characteristics and reveals that differences in these characteristics cannot account for the observed gap. Moreover, the solid red line in the figure bounds the employment gap to address the possibility of selection on unobservable following Oster (2019), which provides additional evidence that individual- and firm-level differences are unable to explain the descriptive gap in employment between TJ workers who are and are not hired by their host sites.

Overall, we find little evidence to support alternative interpretations of the differences in outcomes by treatment subgroups, and we conclude that the most likely explanation for the decomposition in Figure 3 is that overcoming information frictions is a key component of the program's effectiveness.

IV.C Predicting Successful Transitional Jobs

Having demonstrated that successful transitions to unsubsidized employment with a host-site employer are a key program experience underlying longer-term program impacts, we now consider two additional hypotheses derived from the model (see Appendix Section B.6.2 for details). This framework highlights that there are two competing forces at work when an employer considers hiring a new worker: (i) the initial-term expected profits, which can be negative depending on the employer's beliefs about the worker's likely productivity, and (ii) an option value from knowing that, in the next period, the firm can retain workers with profitable match quality and let workers go who are revealed to be insufficiently productive. In general, temporary wage subsidies for new hires will be effective at encouraging employers to hire workers who have meaningful positive option value but negative expected initial-term profits. By offsetting the costs of the initial-term employment, the subsidy frees the employer to enjoy the option value of potentially finding a high-quality employee.

The model implies two ways in which the firm's option value can be larger. First, the option value of hiring a worker with unknown productivity should be largest when the employer is more uncertain about the worker's productivity. The combination of a low-cost trial period and costless separation in the event of a match that is revealed to be unprofitable allows the firm to enjoy the upside risk without worrying about the downside risk. When the firm is less certain about the worker's productivity, the ability to benefit from that upside is larger. Second, this option value should be largest when the employee's expected productivity is close to the wage the firm needs to pay if they hire the worker. If expected productivity is far below the wage, the match is almost surely unprofitable and the ability to find the true productivity through a low-cost trial period is less valuable. Similarly, if the expected productivity is very high, then the value of experimentation is minimal because the firm expects the match to be profitable with high probability. We provide additional analysis to examine each of these hypotheses.

First, we used machine learning tools to test whether the individual characteristics measured in the administrative data and baseline survey are predictive of program outcomes: transitional job placement and hiring by the host site conditional on receiving a placement. Appendix Section A.15.4 provides full details of the methods and results. These characteristics span multiple domains that could create additional uncertainty in the employer's beliefs about the worker's productivity, including earnings histories and barriers to employment. Although the tools generate large in-sample differences in predicted program experience, these predictions do not perform well when applied to a holdout sample of treatment group individuals not used to form the prediction (see Appendix Table A-24). For example, those predicted most likely to be hired by the employer host site, do so at a rate of 17 percent, compared to the 15 percent of individuals who are predicted to be least likely to have a successful transition. This inability to predict successful matches, even when using flexible models that allow for rich interactions and nonlinearities in the prediction, is consistent with the limited differences in average characteristics across groups defined by their program experience, as well as with our lack of evidence in support of predictable treatment effect heterogeneity. The inability to detect heterogeneity does not necessarily contradict the model's prediction that the option value is largest for applicants with noisier signals. Instead, ReHire's 100 percent wage subsidy may simply be so large as to make nearly all program participants worth hiring.

Next, we use the rich administrative data on host-site employers, especially data on their earnings distributions, as additional predictors of who was hired by TJ host site among those who were placed in a TJ. Because this population tends to earn near the minimum wage, we expect to see higher rates of success among firms that tend to employ lower-wage workers. Appendix Table A-24 shows that the combination of individual- and firm-level characteristics do generate meaningful predictions among a hold-out sample in the treatment group. In particular, the random forest model generates a large gap in TJ success among those predicted to be most likely (43 percent) and least likely (13 percent) to have a successful transition. Additional results reported in Appendix Section A.15.4 confirm the descriptive differences seen in Table 5. First, having a relatively high 10th percentile of wages (i.e., relatively fewer lower-wage jobs) has a sizable negative impact on the underlying predictions, and, in general, firms with lower earnings distributions tend to be more likely to have successful matches. Interestingly, the individual variables that contribute most to the predictive power of the random forest model after including firm characteristics tend to be non-cognitive traits that might be less easily observable by the firm at the time of hire—grit, Big 5 personality traits, the depression scale, assessment of motivation to get back to work—rather than characteristics from the worker's employment history.

Taken together, the decomposition evidence and the consistency of the data with the model's predictions presented in this section suggest that a key way transitional jobs programs improve labor market attachment is by allowing firms and workers to form matches that otherwise would not have formed and to learn whether they are profitable.

V Discussion

This paper uses a randomized controlled trial to provide comprehensive evidence of the impact and mechanisms of a broadly targeted enhanced transitional jobs program. We develop a model to illustrate the mechanisms through which a wage subsidy with wraparound employment services may affect long-term employment and estimate treatment effects on a wide set of outcomes. We find that the treatment group experienced a large increase in employment and earnings in the first year while receiving services. Although these gains attenuated after services stopped, treatment group members remained somewhat more likely to be employed and had moderately higher earnings compared to the control group during the second and third year following randomization. Post-program effects were concentrated among participants who were hired by their host site, which suggests that the information revelation mechanism—potentially in combination with human capital improvements—is key to the program's effectiveness.

In order to understand the cost-effectiveness of ReHire Colorado and to benchmark it against other programs, we calculate the Marginal Value of Public Funds (MVPF) for expenditures on the program (Hendren and Sprung-Keyser, 2020). Given the uncertainty around the durability of the earnings impacts due to the pandemic, we consider MVPF calculations using only experimental impacts as well as using estimates from a surrogate approach using the method of Athey et al. (2019) over a range of post-application time periods. Full details of the surrogate approach are provided in Appendix Section A.16.[51] Our preferred estimates, which use the surrogate impacts through the 30$^{\text{th}}$ quarter post-randomization and an annual

[51] In addition to methodological details for our MVPF analysis, Appendix Section A.17 discusses important limitations in fully accounting for costs and benefits. For example, we do not account for any utility implications from the labor-leisure tradeoff, any potential impacts on interactions with the criminal justice or homelessness services systems, or impacts experienced by other members of the household, such as children. Additionally, we do not account for salient general equilibrium effects that may occur with the scale up of wage subsidy or job placement programs (Lise, Seitz and Smith, 2004; Crépon et al., 2013).

discount rate of 3 percent, yield an estimated willingness to pay of $4,672 per participant. Under the assumption that none of ReHire's services are available to the control group through other funding sources, the net costs of the program—after adjusting for increased taxes paid out of improved earnings—are $4,890. These estimated benefits and costs combine for an MVPF estimate of 0.96 with a 95 percent confidence interval of $(0.25, 1.93)$.[52] This estimate is well above the MVPF estimate for Job Corps and JobStart and is within the confidence interval of the adult JTPA program. It is also broadly in line with other policies targeting similar adults—unemployment insurance, disability insurance, and the EITC.[53] Overall, we interpret the results of this analysis as suggesting that transitional jobs programs like ReHire Colorado are a valuable policy tool in addressing the needs of unemployed lower-wage workers.

This paper also uses a theoretical framework grounded in the employer learning literature (Jovanovic, 1979, 1984; Altonji and Pierret, 2001; Pries and Rogerson, 2005, 2022) to illustrate the potential mechanisms through which supported work can improve future labor market outcomes. We provide descriptive evidence that facilitating employers' learning the match quality is a key mechanism, which has a clear policy implication: administrators of similar programs should structure their programs to provide participants with placements at firms who are willing to hire workers who prove to be productive by the end of their time of subsidized employment. To illustrate the quantitative importance of this potential improvement, Appendix Table A-26 includes an alternative MVPF calculation under the assumption that 50 percent more participants (22.5 percent vs. 15 percent) were hired by their host site. The MVPF rises to 1.46 in this scenario, meaning that a program meeting this objective would be more efficient than a non-distortionary transfer. Subsequent RCTs should test program enhancements aimed at increasing host-site hiring to allow for an experimental assessment of the information revelation mechanism.

Future research could also examine whether subsidized employment is more effective in job types that require a longer time to reveal workers' true productivity. The framework suggests that subsidized jobs may be especially effective when the match must survive for a longer time in order for match quality to be fully revealed.[54] For example, if employers provide meaningful on-the-job training to workers and there is substantial uncertainty over how well a given worker will be able to learn the required skills, subsidizing

[52] Using only the experimental impacts through quarter 16 yields an MVPF of 0.37. Assuming that the surrogate-estimated impacts last through the remainder of an applicant's working career leads to an MVPF of 1.87. As expected, the MVPF rises substantially under alternative assumptions about the cost of ReHire services relative to the cost of similar services for the control group. Under the most generous assumption that the control group receives services equivalent to those paid for out of all of ReHire's indirect costs, the estimated MVPF is over 4.

[53] Estimates from Hendren and Sprung-Keyser (2020), Table II. More details are available in Appendix Section A.17.

[54] In the two-period discrete-time model, extending the length of time until true productivity is revealed could be modeled as increasing the weight the employer places on initial-term profits relative to the option value.

the initial training period may be especially valuable.

The importance of employer learning in determining outcomes for this population has implications for our understanding of the low-wage labor market beyond the context of this study. First, it suggests that alternative policy changes that encourage employer experimentation could help address persistent unemployment in the lower-wage labor market more generally. For example, experience rating in the Unemployment Insurance system—charging employers who lay off more workers a higher premium—makes hiring a worker who may not work out more costly in expectation.[55] As viewed through the employer learning framework in Appendix Section B, reducing experience rating would increase the option value of hiring a worker of unknown quality and could lead to more willingness to hire workers who do not already obviously meet all of the job's qualifications. More generally, we note that the low firing costs in the US increase the option value from experimentation and are an important piece of context in understanding this study's findings.

Further, these findings support the concern raised by Pries and Rogerson (2022) that, when match quality is revealed only after a worker is hired, improvements in screening tools—such as algorithmic resume evaluation—will increasingly lead to workers with lower expected productivity being passed over by hiring managers. Although the program we study was designed to improve future employers' beliefs about former participants' quality by providing verifiable recent work experience, the decomposition results suggest that those who are not retained by their host site do not see improved hiring outcomes at future employers. Recent experimental studies among low-wage job seekers in Africa (Abebe et al., 2021; Abel, Burger and Piraino, 2020; Bassi and Nansamba, 2021; Carranza et al., 2022) and among youth participating in summer employment programs in New York City (Heller and Kessler, Forthcoming) suggest more promising ways—letters of recommendation or skill certification—to allow workers to credibly signal their quality to future employers. Nevertheless, this paper suggests that signaling interventions alone may be insufficient when employers face substantial uncertainty about worker productivity, underscoring the value of subsidized and supported trial periods as a tool to encourage firms to take chances on risky job seekers.

[55] There are two practical caveats to consider. First, employers who hire workers for very short durations may not necessarily see increases in their UI premiums because of how eligible earnings are calculated in many states—many use the first four of the previous five quarters. Second, employers already do not face premium increases if they fire workers for misconduct, but we expect that, in many cases, workers who are hired and let go will simply not have met performance standards and many will continue to maintain eligibility for UI benefits.

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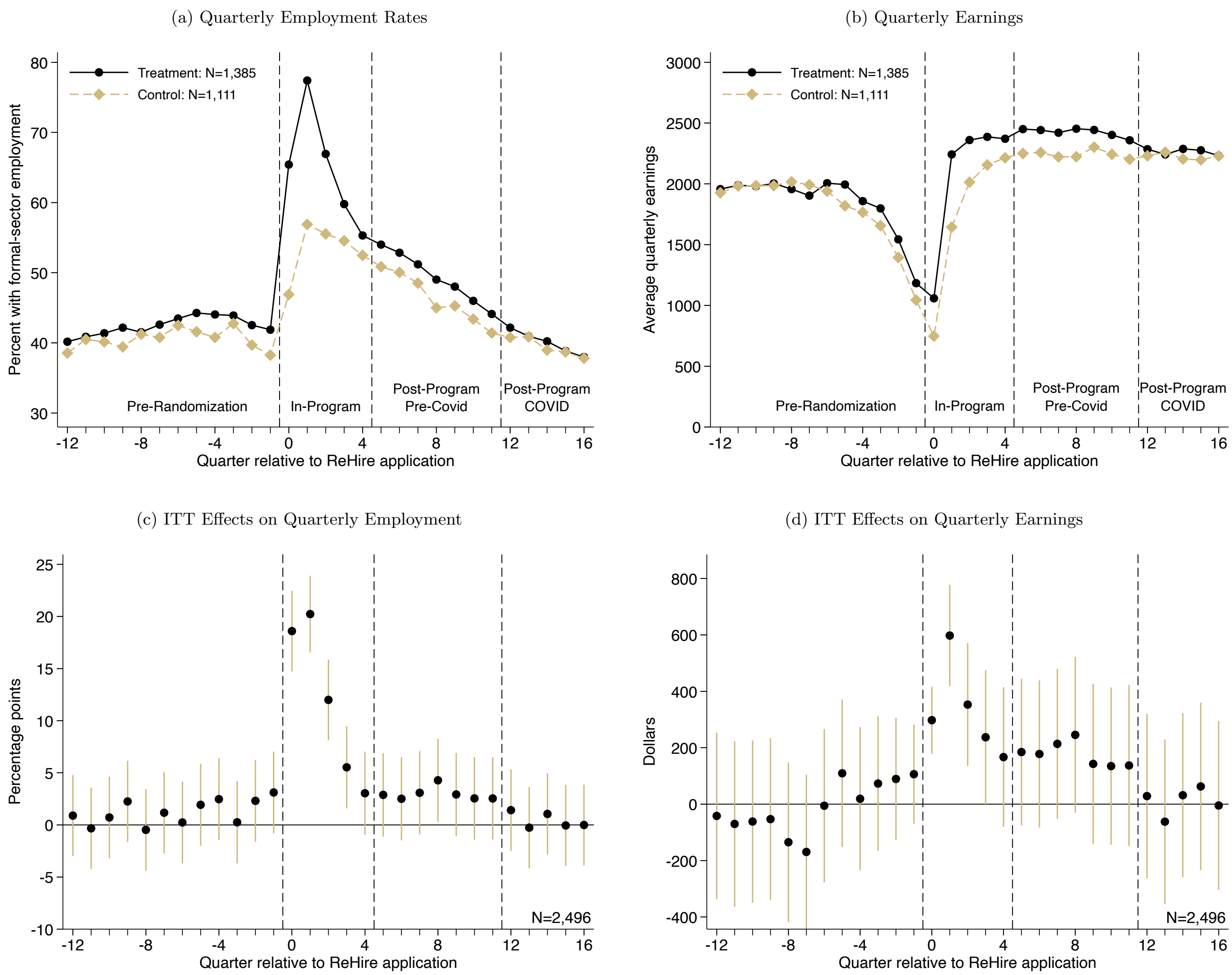
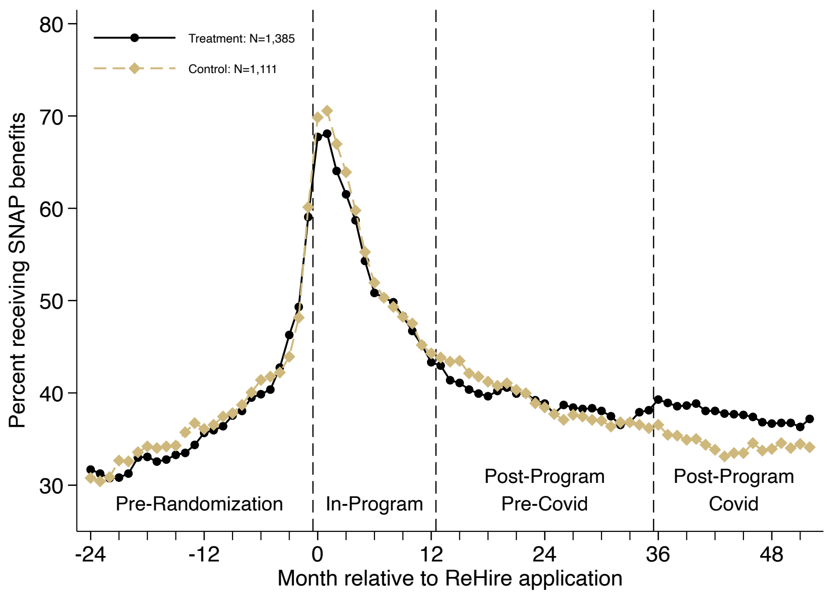
Figure 1: Formal-Sector Employment and Earnings in Colorado by Treatment Status



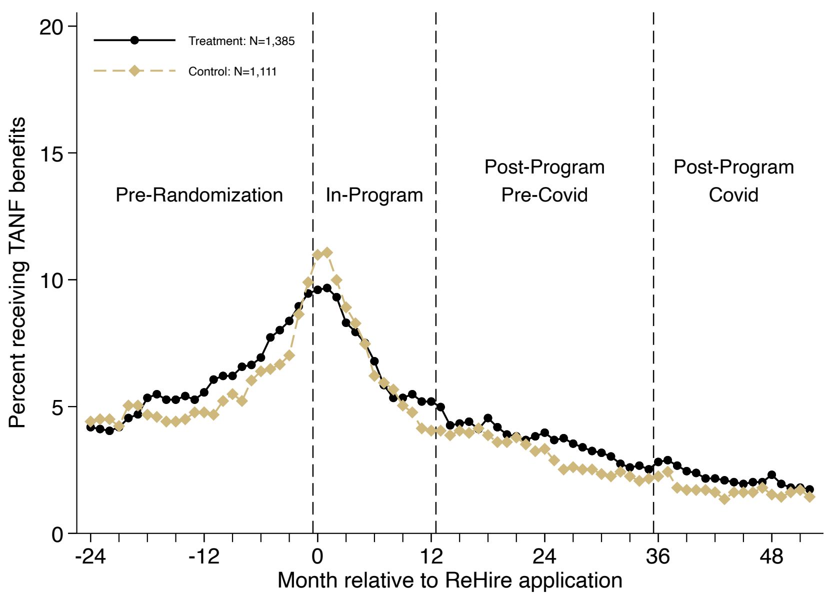
Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Beginning in the 12$^{\text{th}}$ quarter following random assignment, more than half of the sample was potentially experiencing labor market disruptions due to the COVID-19 pandemic. Formal-sector employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal-sector employment. Treatment and control groups are based on an individual's randomly assigned treatment status. Panels (a) and (b) plot the percent of treatment and control applicants with formal-sector employment and average quarterly earnings, respectively. Panels (c) and (d) plot the treatment-control differences in average quarterly employment and earnings, respectively, controlling for stratification fixed effects. Gold vertical bars represent the 95 percent confidence intervals constructed using heteroskedasticity-robust standard errors. The p-value from a test that all pre-treatment differences in employment (earnings) are jointly 0 is 0.456 (0.568). Point estimates and standard errors for post-application differences are reported in Appendix Table A-4.

Figure 2: SNAP and TANF Participation in Colorado by Treatment Status

(a) SNAP



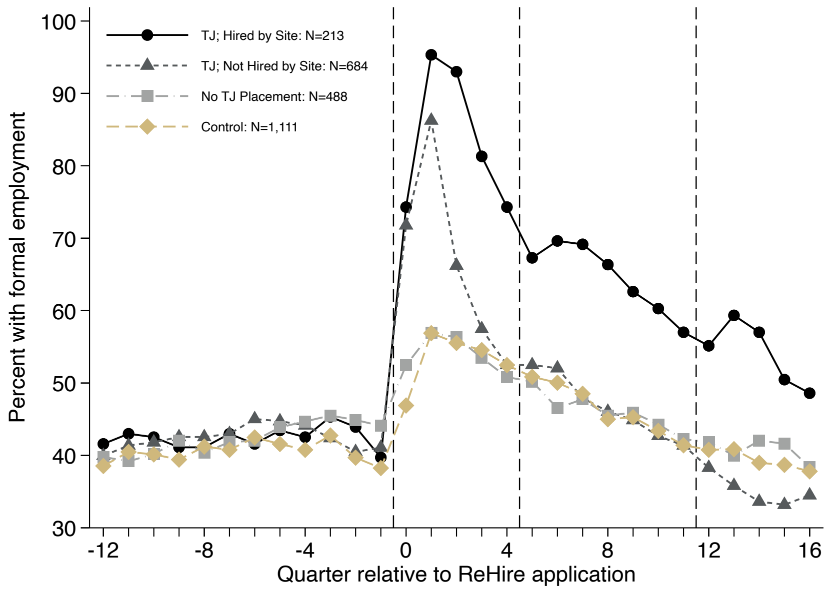
(b) TANF



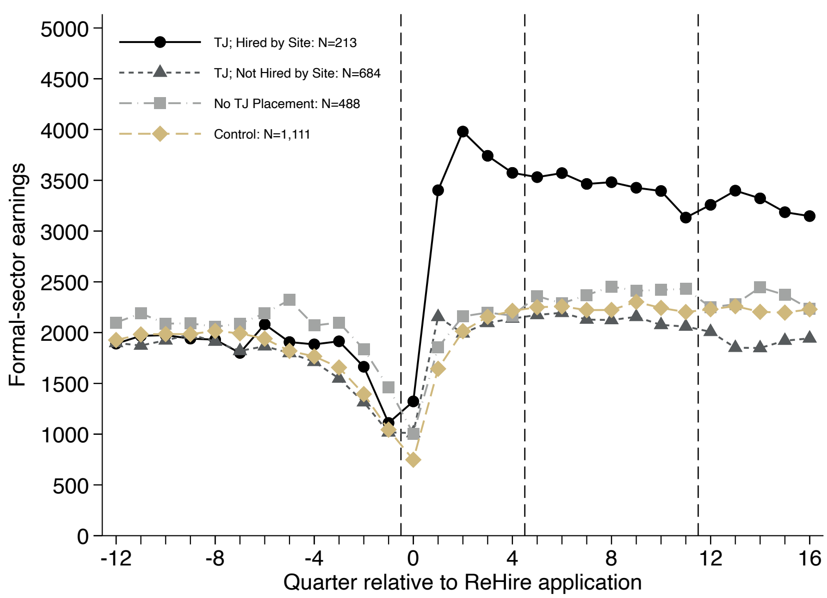
Notes: Data source is administrative SNAP and TANF data from CDHS. Each monthly sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Month 0 represents the month in which an individual completed their application, and is thus a different calendar month from person to person. Beginning in the 36$^{\text{th}}$ month following random assignment, more than half of the sample was potentially experiencing labor market disruptions due to the COVID-19 pandemic. Individuals are coded as receiving SNAP/TANF if they were paid a monthly benefit from CDHS; benefits received in other states are not observed and are treated as zero. Treatment and Control groups are based on an individual's results in the randomization process. The top panel plots the percent of treatment and control applicants participating in SNAP in a given month. The bottom panel plots the percent of treatment and control applicants participating in TANF in a given month.

Figure 3: Formal-Sector Employment and Earnings in Colorado
by Treatment Assignment and Transitional Job Completion

(a) Employment

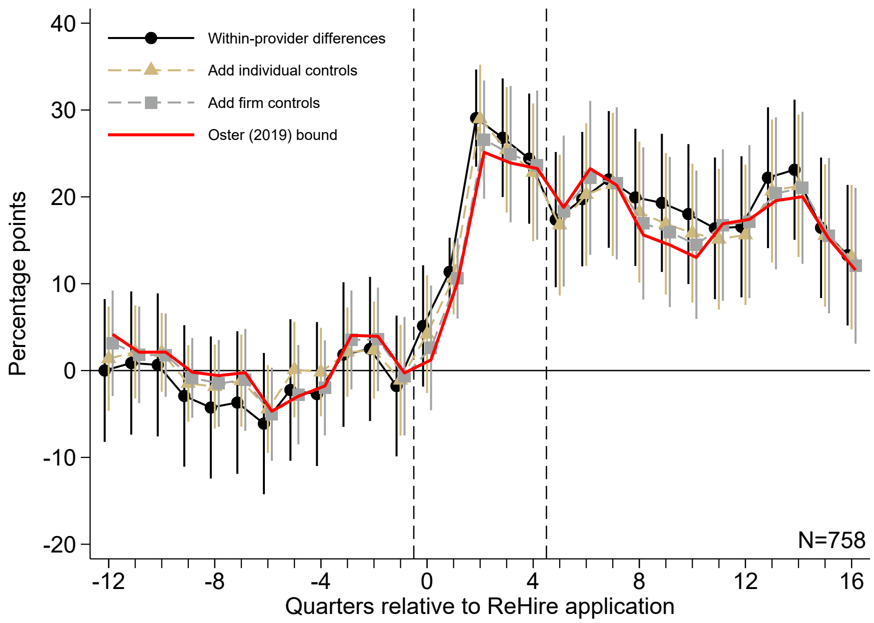


(b) Earnings



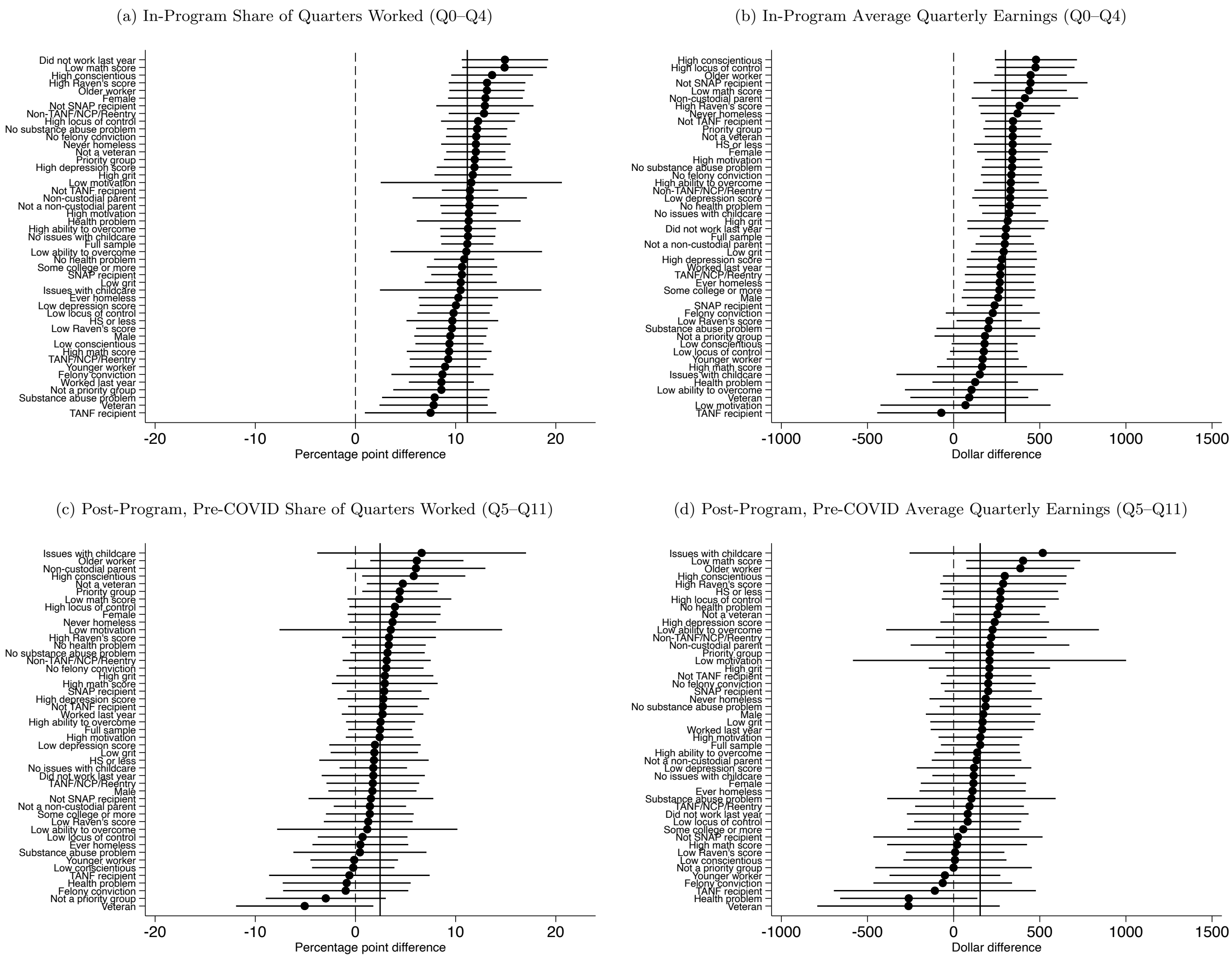
Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Beginning in the 12^{th} quarter following random assignment, more than half of the sample was potentially experiencing labor market disruptions due to the COVID-19 pandemic. Formal earnings is defined as UI-covered earnings in Colorado in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector earnings. Treatment and control groups are based on an individual's randomly assigned treatment status. The treatment group is further divided based on transitional job (TJ) receipt and whether individuals were hired by their transitional job host site. The figure plots the (a) quarterly employment rates, (b) average quarterly earnings, (c) average quarterly earnings among individuals with positive earnings, and (d) percent employed by the same employer as their Q1 employer.

Figure 4: Differences in Employment Rates among Transitional Job Recipients, Hired by Employer Site versus Not Hired by Employer Site



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 758 ReHire applicants who applied between 7/2015 and 12/2018, were assigned to the treatment group, were placed into a transitional job, and could be matched to host-site firm characteristics by LINC. The figure plots differences in quarterly employment rates between TJ recipients who were and were not hired by their employer host site controlling for strata fixed effects. Black circles report the coefficient on an indicator for hire in a regression without any additional controls. Gold triangles report the coefficient on an indicator for hire in a regression that flexibly controls for the two caseworker assessments, as well as linearly controls for the characteristics listed in Table A-23. Grey squares report the coefficient on an indicator for hire in a regression that controls for firm characteristics listed in Table 5. Vertical black, gold, and grey bars represent the 95 percent confidence intervals constructed using heteroskedasticity-robust standard errors. The solid red line provides an adjusted estimate of the employment gap that accounts for potential selection on unobservables (Oster, 2019), using the recommended values of $\delta = 1$ and $R_{\max} = 1.3\tilde{R}$.

Figure 5: Heterogenous Impacts on Employment and Earnings



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Each figure plots ITT effect estimates for subgroups defined by baseline characteristics. Black circles report the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014), where the sample is restricted to individuals who match the criteria listed along the vertical axis. Horizontal black bars represent the 95% confidence intervals constructed using heteroskedasticity-robust standard errors. The solid black vertical line represents the magnitude of the treatment effect in the full sample. The outcomes in Panels (a) and (c) are average quarterly employment rates in the in-program and post-program periods, respectively. Panels (b) and (d) are average quarterly earnings in the in-program and post-program periods, respectively.

Table 1: Applicant Characteristics and Baseline Balance

	Control		Treatment		Difference	t-stat	Diff./	N
	Mean	SD	Mean	SD	(3) – (1)		SD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Administrative Data								
Worked last year	0.599	(0.490)	0.627	(0.484)	0.019	0.95	0.04	2,496
Employment rate last three years	0.405	(0.356)	0.424	(0.357)	0.012	0.84	0.03	2,496
Average quarterly earnings in last year	$1,530	(2,687)	$1,694	(3,010)	$100	0.87	0.04	2,496
Received TANF last year	0.126	(0.332)	0.118	(0.323)	-0.005	-0.38	-0.02	2,496
Received SNAP last year	0.696	(0.460)	0.679	(0.467)	-0.011	-0.58	-0.02	2,496
Panel B: Baseline Survey								
Demographics								
Average Age (years)	46.7	(12.1)	46.1	(12.2)	-0.7	-1.44	-0.06	2,451
Average years of education	13.5	(1.9)	13.5	(1.8)	-0.0	-0.26	-0.01	2,179
Male	0.496	(0.500)	0.532	(0.499)	0.036	1.82	0.07	2,496
Minority	0.405	(0.491)	0.385	(0.487)	-0.025	-1.29	-0.05	2,495
Covered by Medicaid	0.758	(0.429)	0.744	(0.437)	-0.016	-0.93	-0.04	2,495
Barriers to Employment								
Not allowed to drive	0.208	(0.406)	0.229	(0.420)	0.024	1.48	0.06	2,480
Parent	0.304	(0.460)	0.283	(0.450)	-0.018	-1.01	-0.04	2,486
Single parent	0.178	(0.383)	0.164	(0.370)	-0.012	-0.80	-0.03	2,486
Difficulty finding childcare	0.095	(0.293)	0.086	(0.281)	-0.006	-0.54	-0.02	2,485
Expect economic hardship	0.322	(0.467)	0.311	(0.463)	-0.022	-1.19	-0.05	2,456
Health limits work	0.103	(0.305)	0.103	(0.305)	0.003	0.22	0.01	2,429
Homeless (last 5 years)	0.434	(0.496)	0.428	(0.495)	-0.002	-0.12	-0.01	2,480
Ever convicted of felony	0.243	(0.429)	0.242	(0.429)	0.001	0.08	0.00	2,475
Drugs or alcohol have affected life	0.228	(0.420)	0.231	(0.421)	0.003	0.15	0.01	2,424
Caseworker Job Readiness Assessment								
Perceived motivation (out of 10)	8.47	(1.77)	8.48	(1.84)	-0.05	-0.74	-0.03	2,440
Likelihood to overcome barriers (out of 10)	8.13	(1.93)	8.16	(2.00)	-0.07	-0.88	-0.04	2,440
ReHire Target Populations								
Veteran	0.225	(0.418)	0.225	(0.418)	0.003	0.19	0.01	2,495
Non-custodial parent	0.203	(0.402)	0.191	(0.393)	-0.012	-0.77	-0.03	2,495
Older worker	0.484	(0.500)	0.483	(0.500)	-0.007	-0.35	-0.01	2,495
Not in a priority category	0.279	(0.449)	0.282	(0.450)	0.010	0.59	0.02	2,495
Cognitive skills								
Timed math test, percent correct	59.6	(17.4)	59.0	(17.0)	-0.4	-0.59	-0.03	1,877
Number of math questions attempted (out of 160)	98.2	(27.7)	97.2	(27.2)	-0.8	-0.62	-0.03	1,877
Raven's score (out of 36)	30.9	(4.8)	31.1	(4.6)	0.3	1.53	0.06	2,457
Non-cognitive characteristics								
Locus of control (1–5)	4.08	(0.54)	4.06	(0.56)	-0.02	-0.93	-0.04	2,476
Grit (1–5)	3.90	(0.45)	3.90	(0.47)	-0.00	-0.20	-0.01	2,470
Extraversion (1–5)	3.12	(0.78)	3.12	(0.78)	-0.00	-0.02	-0.00	2,471
Agreeableness (1–5)	3.93	(0.55)	3.94	(0.59)	0.02	0.76	0.03	2,471
Conscientious (1–5)	4.01	(0.59)	4.00	(0.60)	-0.01	-0.54	-0.02	2,471
Neuroticism (1–5)	2.44	(0.64)	2.46	(0.66)	0.03	1.14	0.05	2,471
Imagination (1–5)	3.07	(0.44)	3.06	(0.44)	-0.00	-0.12	-0.00	2,471
Life satisfaction ladder (0–10)	5.71	(1.97)	5.62	(2.09)	-0.16	-1.92	-0.08	2,485
Depression scale (0–10)	1.52	(1.37)	1.55	(1.37)	0.05	0.82	0.03	2,421

Notes: Data come from administrative UI earnings data from CDLE, administrative SNAP and TANF data from CDHS, and baseline survey data collected at application. The sample includes ReHire applicants who applied between 7/2015 and 12/2018. One applicant can be linked to administrative data, but is missing a baseline survey. Treatment-control differences reported in column (5) are adjusted for stratification fixed effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally throughout enrollment.

Table 2: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado

	Control Mean (1)	ITT Effect No Controls (2)	ITT Effect Controls (3)	Percent Change (4)
Panel A: In-Program Employment (Quarters 0–4)				
Any employment	0.805	0.121**	0.116**	14%
		(0.014)	(0.014)	
Share of quarters worked	0.533	0.119**	0.112**	21%
		(0.014)	(0.013)	
Worked every quarter	0.234	0.079**	0.073**	31%
		(0.018)	(0.017)	
Share of quarters worked at Q1 employer	0.312	0.116**	0.111**	36%
		(0.013)	(0.013)	
Average quarterly earnings	$1,755	$330**	$301**	17%
		(83)	(76)	
Share of quarters above 130% FPL	0.183	0.028*	0.025*	14%
		(0.011)	(0.011)	
Panel B: Post-Program, Pre-COVID Employment (Quarters 5–11)				
Any employment	0.660	0.022	0.016	2%
		(0.019)	(0.019)	
Share of quarters worked	0.464	0.030+	0.025	5%
		(0.017)	(0.016)	
Worked every quarter	0.242	0.043*	0.038*	16%
		(0.018)	(0.018)	
Share of quarters worked at Q1 employer	0.096	0.034**	0.034**	35%
		(0.011)	(0.011)	
Average quarterly earnings	$2,243	$177	$154	7%
		(122)	(116)	
Share of quarters above 130% FPL	0.251	0.018	0.016	6%
		(0.015)	(0.014)	
Panel C: Post-Program, Post-COVID Employment (Quarters 12–16)				
Any employment	0.523	0.004	-0.001	-0%
		(0.020)	(0.020)	
Share of quarters worked	0.394	0.004	-0.000	-0%
		(0.018)	(0.017)	
Worked every quarter	0.268	-0.011	-0.013	-5%
		(0.018)	(0.018)	
Share of quarters worked at Q1 employer	0.041	0.028**	0.028**	69%
		(0.009)	(0.009)	
Average quarterly earnings	$2,224	$11	-$7	-0%
		(138)	(133)	
Share of quarters above 130% FPL	0.242	-0.005	-0.007	-3%
		(0.015)	(0.015)	
Agency-Rate Block FEs		X	X	
Individual Baseline Controls			X	
Observations	1,111	2,496	2,496	

Notes: Data source is administrative UI earnings data from CDLE. Panels A, B, and C report estimates on in-program (A) and post-program (B and C) employment outcomes for the sample of ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Panel B (C) reports post-program outcomes during the period before (after) half of the sample was exposed to the COVID-19 pandemic. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. Column (1) reports the mean for control group applicants. Column (2) reports the coefficient on a treatment indicator, controlling for vendor-randomization rate block (stratification) fixed effects. Column (3) reports the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (4) reports the percent change of the ITT effect in column (3) relative to the control group mean. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

Table 3: ITT Effect of ReHire on SNAP and TANF Receipt in Colorado

	Control Mean (1)	ITT Effect No Controls (2)	ITT Effect Controls (3)	Percent Change (4)
Panel A: In-Program Benefits (Months 0–12)				
Any SNAP Receipt	0.785	-0.011	-0.004	-1%
		(0.016)	(0.013)	
Share of months with SNAP	0.556	-0.010	-0.002	-0%
		(0.016)	(0.012)	
Average monthly SNAP receipt	$148.87	-$5.97	-$4.76	-3%
		(6.94)	(4.35)	
Any TANF Receipt	0.128	-0.008	-0.008	-6%
		(0.013)	(0.007)	
Share of months with TANF	0.071	0.002	0.001	2%
		(0.009)	(0.005)	
Average monthly TANF receipt	$30.09	$0.82	-$0.15	-0%
		(3.86)	(2.35)	
Panel B: Post-Program, Pre-COVID Benefits (Months 13–35)				
Any SNAP Receipt	0.602	-0.000	0.004	1%
		(0.020)	(0.018)	
Share of months with SNAP	0.392	-0.004	-0.001	-0%
		(0.016)	(0.014)	
Average monthly SNAP receipt	$99.92	$4.94	$5.61	6%
		(6.43)	(4.63)	
Any TANF Receipt	0.067	0.013	0.013	19%
		(0.010)	(0.009)	
Share of months with TANF	0.031	0.007	0.006	18%
		(0.006)	(0.005)	
Average monthly TANF receipt	$13.25	$1.54	$0.93	7%
		(2.57)	(2.29)	
Panel C: Post-Program, Post-COVID Benefits (Months 36–52)				
Any SNAP Receipt	0.481	0.015	0.019	4%
		(0.020)	(0.018)	
Share of months with SNAP	0.344	0.027	0.030^{+}	9%
		(0.017)	(0.016)	
Average monthly SNAP receipt	$106.69	$13.22^{+}	$14.08*	13%
		(7.32)	(5.88)	
Any TANF Receipt	0.041	0.005	0.005	12%
		(0.008)	(0.008)	
Share of months with TANF	0.017	0.005	0.005	27%
		(0.005)	(0.004)	
Average monthly TANF receipt	$6.83	$1.44	$1.61	23%
		(1.85)	(1.75)	
Agency-Rate Block FEs		X	X	
Individual Baseline Controls			X	
Observations	1,111	2,496	2,496	

Notes: Data source is administrative SNAP and TANF data from CDHS. Panels A, B, and C report estimates on in-program (A) and post-program (B and C) benefit outcomes for the sample of ReHire applicants who applied between 7/2015 and 12/2018. Month 0 represents the month in which a participant completed an application, and is thus a different calendar month from person to person. Benefit receipt is defined as having received any benefit in Colorado greater than $0 in a given month. Panel B (C) reports post-program outcomes during the period before (after) half of the sample was exposed to the COVID-19 pandemic. Column (1) reports the mean for control group applicants. Column (2) reports the coefficient on a treatment indicator, controlling for vendor-randomization rate block (stratification) fixed effects. Column (3) reports the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (4) reports the percent change of the ITT effect in column (3) relative to the control group mean. Heteroskedasticity-robust standard errors in parentheses.

**0.01, *0.05, $^{+}$0.10 significance levels

Table 4: ITT Effect of ReHire on Follow-Up Survey and Credit Outcomes

	Control Group Mean (1)	Unweighted ITT Effect No Controls (2)	Weighted ITT Effect No Controls (3)	Weighted ITT Effect Controls (4)	N (5)
Panel A: Employment Outcomes from Follow-Up Survey					
Worked a subsidized job since application	0.099	0.457**	0.442**	0.442**	954
		(0.027)	(0.028)	(0.028)	
ReHire helped them find current job	0.016	0.122**	0.106**	0.106**	954
		(0.017)	(0.018)	(0.017)	
Any unsubsidized employment since application	0.775	0.058*	0.063*	0.062*	954
		(0.028)	(0.031)	(0.030)	
Currently employed	0.543	0.083*	0.064+	0.064+	954
		(0.034)	(0.037)	(0.036)	
Currently employed in job with paystub	0.513	0.031	0.012	0.009	954
		(0.035)	(0.038)	(0.037)	
Current job same as first job	0.265	0.081*	0.064+	0.064+	954
		(0.031)	(0.034)	(0.033)	
Panel B: Standardized Treatment Effects from Follow-Up Survey (in SD)					
Job quality (first unsubsidized job)		0.147**	0.130**	0.130**	773
		(0.039)	(0.043)	(0.042)	
Job quality (current job)		0.072	0.053	0.053	570
		(0.046)	(0.048)	(0.047)	
Well-being		0.163**	0.169**	0.168**	954
		(0.044)	(0.047)	(0.043)	
Employment barriers		0.024	-0.006	-0.025	954
		(0.041)	(0.046)	(0.043)	
Workplace behaviors		0.046	0.035	0.035	954
		(0.039)	(0.042)	(0.041)	
Expectations about future		0.060	0.034	0.034	954
		(0.055)	(0.060)	(0.059)	
Panel C: Standardized Treatment Effects from Credit Data (in SD)					
In-program credit (Q0–Q4)		0.034	0.018	-0.005	1,521
		(0.023)	(0.023)	(0.020)	
Post-program Pre-COVID credit (Q5–Q11)		0.034	0.021	0.002	1,521
		(0.022)	(0.023)	(0.021)	
Post-program COVID credit (Q5–Q16)		0.029	0.023	0.009	1,521
		(0.022)	(0.023)	(0.022)	

Notes: Data source is an 18-month follow-up survey (Panels A and B) and administrative credit data from Experian (Panel C). The sample includes ReHire applicants who applied between 7/2015 and 12/2018. Panels A and B include respondents to the follow-up survey. Panel C includes individuals who matched to Experian records in the 5 quarters before and 14 quarters following random assignment. The dependent variables in Panel A are indicators measured in the follow-up survey. Column (1) reports unweighted control group means of these outcomes. Panels B and C report average standardized treatment effects for outcomes from the follow-up survey and credit data, respectively. Estimates are measured in standard deviations (SD). Column (2) reports estimates that come from estimating Equation (1) with only vendor-randomization rate block (stratification) fixed effects. Column (3) reports estimates from the same specification as column (2), but reweights the sample using inverse propensity attrition weights. Column (4) reports estimates that come from a regression that selects controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014), and reweights the sample using inverse propensity attrition weights. When estimating effects for outcomes that are measured in the baseline survey or administrative data prior to application (well-being, employment barriers, and credit), we include these covariates in the control choice set. Column (5) reports the number of individuals in the sample for a given outcome. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

Table 5: Firm Characteristics by Type of Employment Spell

	Regular Employer Mean	Host-Site Employers			Conditional Difference in Means	
		Any TJ Mean	Not Hired by TJ Mean	Hired by TJ Mean	(2) - (1)	(4) - (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Firm size (2014–2019)						
Share of quarters with any employees	0.474	0.912	0.911	0.915	0.433**	0.020
Average employees (2014–19)	55.4	609.2	630.1	514.7	579.3**	-4.3
Change in employees (2014–2019)	9.823	166.182	186.475	74.282	157.163**	-35.781
Change in employees (2014–2019) relative to average	1.094	0.443	0.450	0.412	-0.766**	0.030
Turnover rate	0.184	0.160	0.164	0.140	-0.027**	-0.006
Firm size (2014)						
No employees	0.552	0.089	0.087	0.096	-0.460**	-0.022
Small firm (1–50)	0.425	0.526	0.525	0.535	0.087**	-0.001
Medium firm (51–500)	0.012	0.100	0.087	0.159	0.092**	0.052+
Large firm (500+)	0.010	0.285	0.301	0.210	0.280**	-0.029
Industry						
Construction	0.037	0.009	0.007	0.019	-0.026**	0.001
Manufacturing	0.038	0.028	0.026	0.038	-0.010	-0.009
Wholesale Trade	0.017	0.021	0.010	0.070	0.000	0.057**
Retail Trade	0.117	0.274	0.293	0.191	0.157**	0.013
Transportation and Warehousing	0.028	0.000	0.000	0.000	-0.028**	0.000
Information	0.011	0.005	0.003	0.013	-0.006*	0.005
Finance and Insurance	0.011	0.005	0.003	0.013	-0.006*	0.005
Real Estate	0.016	0.004	0.003	0.006	-0.011**	0.003
Professional, Scientific, and Technical Services	0.045	0.022	0.026	0.006	-0.020**	-0.028*
Temporary Agency	0.209	0.005	0.004	0.006	-0.206**	0.001
Administrative Services (excl. Temp. Agency)	0.117	0.020	0.019	0.025	-0.102**	0.008
Educational Services	0.024	0.019	0.019	0.019	-0.004	-0.007
Health Care and Social Assistance	0.117	0.466	0.483	0.389	0.348**	-0.150**
Arts and Entertainment	0.013	0.008	0.006	0.019	-0.003	0.007
Accomodation and Food Services	0.144	0.014	0.010	0.032	-0.136**	0.019
Other Services	0.031	0.046	0.043	0.057	0.020*	0.020
Public Administration	0.011	0.053	0.042	0.102	0.047**	0.065**
Other sectors (Ag, Mining, Utilities, Management)	0.012	0.001	0.001	0.000	-0.012**	-0.004
Firm wage distribution						
Quarterly wages (10th percentile)	$2,092	$10,295	$11,088	$6,794	$8,196**	-$2,735**
Quarterly wages (50th percentile)	$2,973	$52,043	$56,058	$34,320	$49,078**	-$8,272+
Quarterly wages (90th percentile)	$4,393	$111,666	$120,971	$70,593	$107,004**	-$18,644+
Firm fixed effect (all wage records)	-0.569	-0.137	-0.148	-0.085	0.448**	0.041
Firm fixed effect (only quarterly records above $4,000)	-0.074	-0.055	-0.057	-0.048	0.022**	0.004
p-value: Joint F-test, Firm size					< 0.001	0.568
p-value: Joint F-test, Industry					< 0.001	< 0.001
p-value: Joint F-test, Firm wage distribution					< 0.001	< 0.001
p-value: Joint F-test, All characteristics					< 0.001	< 0.001
Observations	5,442	868	711	157	6,310	868

Notes: Data come from participant records from CDHS and firm characteristics provided by the Linked Information Network of Colorado (LINC). The sample includes 5,442 person-employer observations among ReHire applicants that occurred between January 2014 through December 2019. Host-site employers refer to person-employer observations where the ReHire applicant was placed at the employer as part of a transitional job (TJ). Column (1) excludes host-site employers and observations where the employer is a ReHire service agency, which we identify as the modal employer of TJ participants by service agency. Conditional differences reported in columns (5) and (6) are adjusted for vendor-rate block (stratification) fixed effects.

A Appendix – For Online Publication

A.1 Minimum Wage in Colorado Over Time

ReHire participants were paid the hourly minimum wage when working their transitional job, and the direct cost of wages was covered by the state. While in theory employer host sites had the potential to pay wages above this amount, this did not occur in practice. The following table provides the history of the Colorado minimum wage during the evaluation period.

Table A-1: Colorado State Minimum Wage Over Time

Effective Date	Minimum Wage
January 1, 2014	$8.00
January 1, 2015	$8.23
January 1, 2016	$8.31
January 1, 2017	$9.30
January 1, 2018	$10.20
January 1, 2019	$11.10
January 1, 2020	$12.00

Notes: Information on the history of the Colorade minimum wage comes from the Colorado Department of Labor and employment and can be accessed at: https://cdle.colorado.gov/wage-and-hour-law/minimum-wage

A.2 Value and Timing of Program Service Receipt

This section provides additional details on the dollar value and timing of ReHire program service receipt.

Appendix Table A-2 provides a breakdown of the costs associated with the program and the typical experience of a program participant. The typical participant received more than $2,000 in directly billable services, including more than $1,700 in transitional job (TJ) wages (column 1). Among the 65 percent with a transitional job, the average participant worked 280 hours across 10 weeks and earned more than $2,600 in wages through the program (column 3).

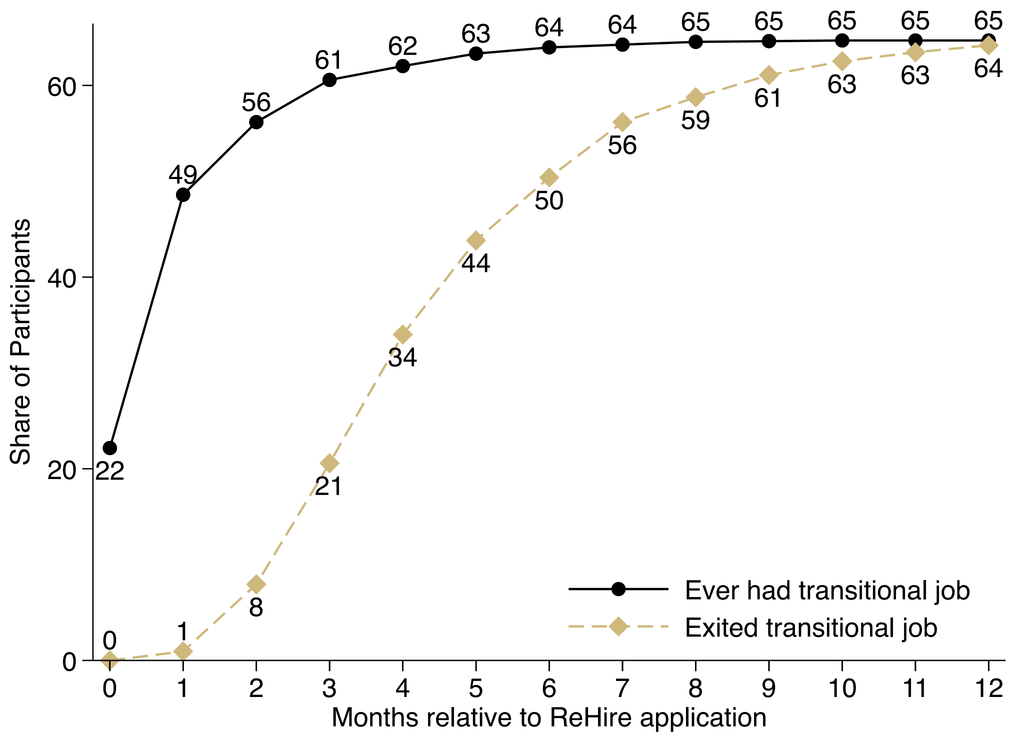
Table A-2: ReHire Program Characteristics

	Treatment Group (N=1,385)		Any TJ (N=897)		Hired by TJ (N=213)	
	Mean	SD	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)
Cost of supportive services	$357	(809)	$395	(618)	$501	(707)
Gross ReHire wages	$1,716	(2,100)	$2,615	(2,097)	$3,443	(1,943)
Total direct costs	$2,101	(2,411)	$3,054	(2,327)	$3,996	(2,073)
Hours worked	184	(223)	280	(221)	372	(210)
Weeks worked	6	(7)	10	(7)	11	(6)

Notes: Data come from program records maintained at services agencies by ReHire caseworkers. The sample consists of 1,385 individuals who applied to ReHire between 7/2015 and 12/2018 and were randomly assigned to the treatment group.

Appendix Figure A-1 shows the timing of transitional job participation over the year following randomization. Month 0 corresponds to the month when a participant completed the ReHire application; months 1–12 are the first through twelfth months following a participant's application. The solid line with circles shows the share of ReHire participants who were placed in a transitional job by the end of the relevant month. Almost 50 percent of the treatment group were placed by the end of the month after they applied. An additional 15 percent were placed over the next eight months after randomization. The dashed line with diamonds shows the share of all participants whose transitional job placement had ended by the relevant month. For example, 34 percent of all treatment group members (and roughly half of participants who ever receive a transitional job) completed their placement by the fourth month following their application. By month 12, the two lines converge, indicating that nearly all transitional job placements are complete one year after randomization. We therefore interpret outcomes observed after 12 months (or four quarters) as post-program outcomes.

Figure A-1: Timing of Transitional Job Entry and Exit



Notes: Sample includes 1,385 participants who applied between 7/2015 and 12/2018 and were assigned to the treatment group. Month 0 represents the month in which a study participant completed their ReHire application, and is thus a different calendar month from person to person. Once an individual has started a transitional job, they are treated as having ever held a transitional job (black circle) in every subsequent month. An individual exits a transitional job in the first month when they do not hold a transitional job in any following month, after having held one (gold diamond). Once an individual has exited, they are treated as having exited a transitional job in every subsequent month. Entry and exit percentages are calculated using all participants in every month.

A.3 Randomization

This appendix section documents the steps taken to conduct random assignment.

Randomization occurred separately for each local service agency. The initial treatment probability at each site was 50 percent. At each site, when the first applicant arrived, a short sequence with an equal number of 0s and 1s was randomly selected in the following manner.

1. Determine the length of the sequence: Draw $x_1 \sim U[0,1]$.
   - If $x_1 \in [0, 1/3)$, then select a sequence with length 6 including three 0s and three 1s.
   - If $x_1 \in [1/3, 2/3)$, then select a sequence with length 8 including four 0s and four 1s.
   - If $x_1 \in [2/3, 1]$, then select a sequence with length 10 including five 0s and five 1s.
2. Determine the actual sequence:
   - First, populate a list with all $\binom{n}{n/2}$ potential sequences of length $n \in \{6, 8, 10\}$.[56]
   - Draw $x_2 \sim U[0,1]$.
   - Randomly select row $r = Int\left[x_2 * \binom{n}{n/2}\right] + 1$ from the list of potential sequences.

Once the treatment-control sequence was fixed, the first applicant at that site was assigned their treatment status based on the first number of the selected sequence: 0 indicated the Control Group; 1 indicated the ReHire Treatment Group. As additional applicants arrived at that agency, they were assigned the next unused number in the sequence until every number in the sequence had been assigned. If an applicant arrived and no unused numbers were remaining in the sequence a new sequence was selected following steps 1 and 2 above. At no point in time did the central office program staff have access to the treatment assignment sequence or know how many unassigned treatment statuses remained at any site.

In practice, a list of daily applicants was constructed by program staff in the CDHS office. Each applicant was assigned a sequential program ID starting with "A-0001" the moment their record was created. CDHS staff sent the list of newly created IDs to the research team. Within the next business day after program application, treatment assignments were assigned to each ID based on the random sequence. Applicants were separated by site and slotted into the next available 0 or 1 in the sequence in the order of their program ID (i.e., their order of appearance in the database). The list of IDs and treatment assignments were then sent to CDHS. Based on their assignment, the central office program staff then toggled the treatment status in the program database for each applicant, which alerted the local program staff of the treatment determination and sent an email to the applicant regarding their treatment assignment and available next steps.

Treatment Probability

At times, program enrollment slowed causing concerns that all available program dollars would not be spent during a contract period. At various times throughout the implementation of the RCT, the treatment probability for all service agencies, or a subset of service agencies, was adjusted to a 2-1 assignment ratio. To implement this change, the potential lengths of sequences were changed to six, nine, and twelve, with exactly 2/3 of the sequence comprising 1s and 1/3 comprising 0s. Accordingly, the choice of the specific sequence in Step 2 was adjusted to account for the number of potential sequences. Each time the decision to change the treatment probability was made (both from 1/2 to 2/3 and from 2/3 to 1/2), the change was implemented *after* the currently selected sequence of 0s and 1s was fully exhausted.

The following list provides the timeline of when the treatment probability was changed throughout the RCT:

[56] This list was sorted by the first through the last number of the sequence. For example, on the list with sequences of length 6, $\{0, 0, 0, 1, 1, 1\}$ was listed first, then $\{0, 0, 1, 0, 1, 1\}$, and so on, ending with $\{1, 1, 1, 0, 0, 0\}$.

- January 14, 2016: treatment probability was changed from 1/2 to 2/3 for all service agencies
- April 11, 2016: treatment probability was changed from 2/3 to 1/2 for all service agencies
- October 11, 2016: treatment probability was changed from 1/2 to 2/3 for Catholic Charities Pueblo and Hilltop Community Resources
- May 18, 2017: treatment probability was changed from 1/2 to 2/3 for all remaining service agencies
- July 13, 2017: treatment probability was changed from 2/3 to 1/2 for all service agencies except Catholic Charities (note: Hilltop Community Resources was no longer providing ReHire at this time)
- July 11, 2018: treatment probability was changed from 1/2 to 2/3 for all remaining service agencies

Service Agencies with Rural Operations

Two of the social service agencies had applicants coming from both the nearby town and from more rural locations. Hilltop Community Resources operated out of Grand Junction. Some of the applicants to Hilltop were applying from nearby Montrose, CO (about an hour away) and these intake sessions were largely occurring in Montrose rather than Grand Junction. Beginning in December 2015, applicants from Montrose were randomized separately from other Hilltop applicants. Similarly, Discover Goodwill in Colorado Springs, CO sometimes received applicants from the more rural but nearby Teller County. Beginning in September 2016, the few applicants who were living in Teller County were randomized separately from other Discover Goodwill applicants.

Implications for Analysis

Because randomization was stratified by social service agency (and sometimes locations within an agency) and treatment probability changed over time, we conduct all of our analysis using a set of stratification fixed effects that account for the service agency at which an individual applied and the treatment probability they faced. Take, for example, applicants to Catholic Charities. At this service agency, we block applicants into 4 strata based on their application date:

1. Applicants randomized with 1/2 treatment probability beginning 7/1/2015
2. Applicants randomized with 2/3 treatment probability beginning 1/21/2016 (the first date a new sequence was drawn after change)
3. Applicants randomized with 1/2 treatment probability beginning 4/21/16 (the first date a new sequence was drawn after change)
4. Applicants randomized with 2/3 treatment probability beginning 1/26/17 (the first date a new sequence was drawn after change)

In total, there are 26 strata across the 6 service agencies that implemented ReHire.

A.4 Comparability to the Low-Income Adult Population

ReHire applicants are relatively representative of the the broader population of low-income adults living in Colorado. Table A-3 compares select characteristics of the ReHire sample with a sample of low-income adults observed in the 2015–2018 American Community Survey (Ruggles et al., 2020). The sample is restricted to ages 18 through 74 and includes individuals who fall below 150 percent of the federal poverty line. Column (2) restricts the sample to individuals living in public-use microdata areas that overlap with cities/counties where ReHire was operated. Columns (3) and (4) report characteristics for adults in Colorado and in the entire US, respectively. Compared to low-income Coloradans, ReHire applicants are more likely to receive SNAP—which is imputed for the ACS sample—and TANF—which is measured as receiving any cash welfare income in the ACS. Additionally, our study population has likely experienced recent homelessness at a higher rate (see Table 1), as study participants are selected on having recently experienced substantial earnings losses (see Figure 1b). Finally, because the program targets older workers and veterans, the sample is skewed older and veterans are over-represented compared to the broader population.

Table A-3: Characteristics of ReHire Applicants and Low-Income Adults

	ReHire	2015–18 American Community Survey		
	Mean	ReHire Area Mean	Colorado Mean	USA Mean
	(1)	(2)	(3)	(4)
Worked last year	0.615	0.580	0.573	0.505
Average quarterly earnings in last year	\$1,621	\$1,503	\$1,552	\$1,372
Received TANF last year	0.122	0.037	0.035	0.039
Received SNAP last year	0.687	0.241	0.240	0.342
Average Age (years)	46.4	37.7	38.7	40.3
Average years of education	13.5	13.0	12.8	12.2
Male	0.516	0.490	0.483	0.461
Minority	0.394	0.402	0.426	0.511
Covered by Medicaid	0.750	0.602	0.592	0.613
Parent	0.292	0.266	0.300	0.341
Single parent	0.170	0.135	0.144	0.188
Veteran	0.225	0.051	0.049	0.040
Older worker	0.483	0.274	0.288	0.320
Observations	2,496	15,957	28,819	2,009,210

Notes: Data come ReHire baseline survey and administrative data, as well as the 2015–2018 American Community Survey (Ruggles et al., 2020). The ACS sample is restricted to adults aged 18–74 with income below 150 percent of the federal poverty line. ReHire area covers public-use microdata areas that include Boulder County, City of Denver, Colorado Springs, El Paso County, Larimer County, and Mesa County. ACS observations are weighted by the IPUMS person weight.

A.5 Migration out of Colorado

We link our analysis sample to consumer reference data from Infutor Data Solutions to measure Colorado residency during the evaluation period. Infutor creates a residential history for most adults in the US using consumer information like magazine subscriptions or utility bills. The resulting data includes exact addresses and includes start and end dates for each address, and these data have been used to measure moves in low-income populations following natural disasters, after the demolition of public housing, and for households at high risk of homelessness (Phillips, 2020).

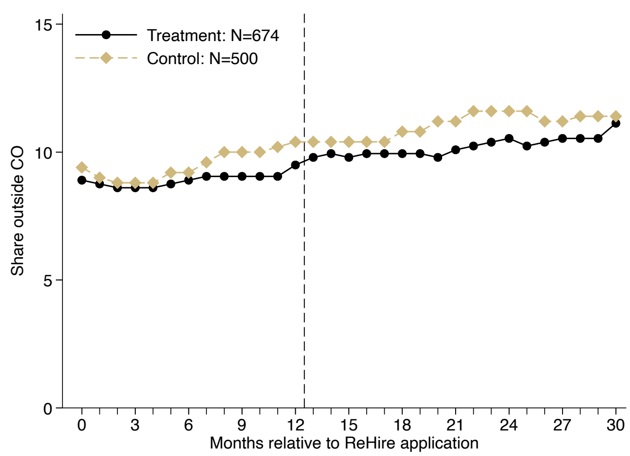
We fuzzy match ReHire study participants to the Infutor data using a number of identifiers including name, address at application, and date of birth. Nearly half of the analysis sample ($N = 1,174$) match to an Infutor address with a start date that precedes their ReHire application date, and match rates are balanced between treatment and control. For each month, we construct an indicator of whether an individual has a non-Colorado address using the state of their most recent address (based on address start date).

Figure A-2a depicts the share of Infutor-matched study participants who have a non-Colorado address. At the time of application, about 10 percent have an address outside Colorado. During the 30 months following application, this share grows to 11.1 and 11.4 percent for the treatment and control groups, respectively.

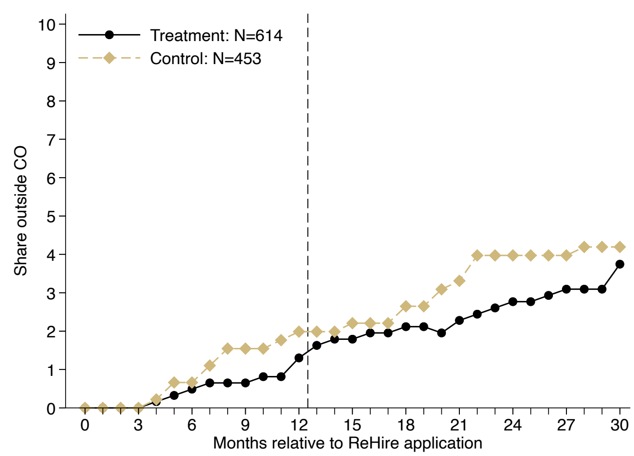
Individuals may have a non-Colorado address at the time of application if they recently moved, or moved into a situation where they did not create a paper trail following them to Colorado (e.g., utility bills in another resident's name). We further investigate differential attrition from Colorado in Figure A-2b by restricting the sample to individuals observed to be in Colorado at the time of application. In the 30 months following application, 3.7 percent of the treatment group and 4.2 percent of the control group move to an address outside Colorado.

Figure A-2: Non-Colorado Address from Infutor Data

(a) Sample: In Infutor by Application Month



(b) Sample: In CO at Application Month



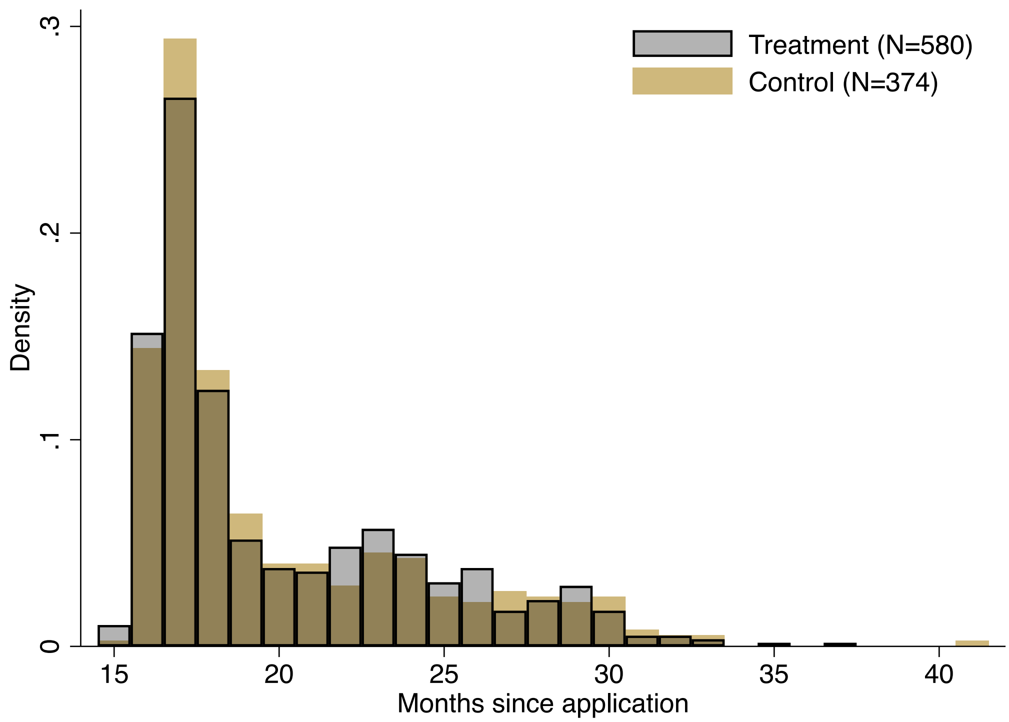
Notes: Data source is address history data from Infutor Data Solutions. The sample includes the 47 percent of ReHire applicants who match to an Infutor address record before ReHire application. Month 0 represents the month in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Panel (a) includes all matched study participants from the main analysis sample. Panel (b) restricts the sample to individuals whose most address start date during the month of application was in CO. The vertical axis plots the share of the same with an address outside Colorado.

A.6 Timing of Follow-up Survey

Beginning in December 2017, an online follow-up survey was fielded to estimate the impact of ReHire Colorado on a broader array of post-program outcomes. Treatment and control respondents were contacted via text and email roughly 18 months after applying for ReHire and were invited to respond to an online survey. Nearly all respondents completed the survey via computer or mobile device, but respondents had the option to respond over the phone.

Most survey respondents completed the follow-up survey 16 to 18 months following application (Figure A-3). Because of the timing of survey implementation, early applicants who applied prior to July 2016 were contacted more than 18 months after application and thus completed their follow-up surveys 18 to 30 months after application. We do not find evidence of differential time from application to follow-up response between treatment and control group participants.[57] Because ReHire participants are in the program for an average of 6 months, the follow-up survey provides results approximately one year after the typical ReHire participant exited the program.

Figure A-3: Months between ReHire Application and Follow-up Survey



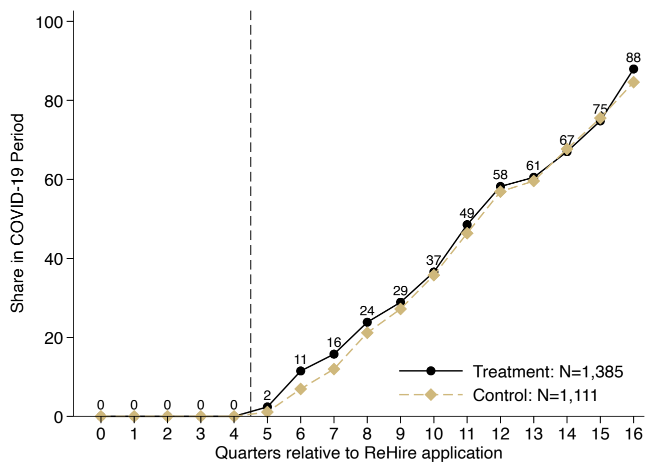
Notes: Data source is application and follow-up surveys. Sample includes the 954 ReHire applicants with a complete follow-up survey. The average number of months between application and follow-up survey completion was 19.94 months in the control group and 20.06 months in the treatment group. A Kolmogorov-Smirnov test of the equality of these two distributions fails to reject the null hypothesis that the samples are drawn from the same distribution ($p = 0.825$).

[57] Using a Kolmogorov-Smirnov test, we reject the null hypothesis that the distribution of months since application is the same between the treatment and control group ($p = 0.825$).

A.7 The Timing of COVID-19 in the ReHire Evaluation

The COVID-19 pandemic and its resulting labor market disruptions occured during much of the follow-up period of this study. Figure A-4 demonstrates that the pandemic began affecting some study participants as early as the 5th quarter following random assignment. Because of randomization, the pandemic affected a fairly balanced set of treatment and control applicants throughout quarters 5 through 16. Beginning in the 12th quarter following random assignment, more than half of the sample was living in the COVID period, which we define as the first quarter of 2020.

Figure A-4: Share of Applicants Experiencing COVID-related Disruptions by Quarter Relative to ReHire Application



Notes: Data source is ReHire administrative data on the timing of application and treatment assignment. The sample includes all 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Both lines plot the share of ReHire applicants whose quarter relative to ReHire application was on or after Q1 2020.

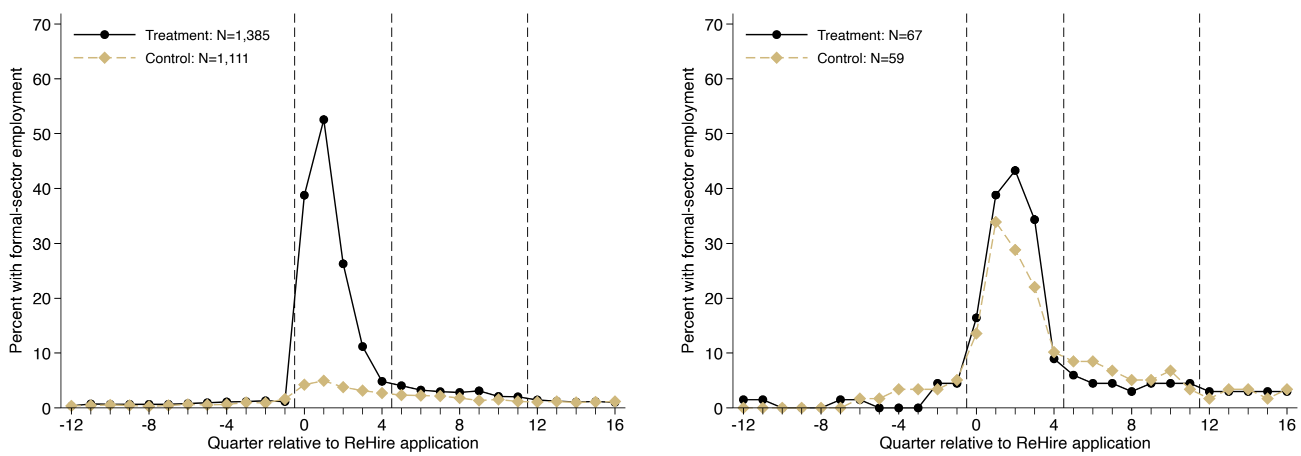
A.8 Control Group Service Access

This section provides supplemental analysis to speak to the question of whether control group members accessed services similar to those provided by ReHire. Control group members were eligible to receive standard employment services offered by the social service provider where they applied for ReHire or by any other service provider. We do not have access to data on other re-employment services the control group accessed, but we can examine how often control group members had positive earnings at a ReHire social service agency, which may indicate a transitional job funded through another program, e.g. WIOA. False positives are also possible because we are unable to distinguish between unsubsidized employment and subsidized employment using the UI data. False positives may be more common at the local agencies that are county workforce offices because they share an employer code with the entire county government.

Figure A-5: Rates of Employment with a ReHire Service Provider by Treatment Status

(a) Analysis Sample

(b) Applicants from Local Service Agency with Highest Rate of Hiring Control Group Members



Notes: Data source is administrative UI earnings data from CDLE. The sample in Panel (a) includes 1,931 ReHire applicants who applied between 7/2015 and 12/2017. Panel (b) further restricts the sample to the 126 applicants at the service agency with the highest rate of hiring control group individuals. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Employment at a ReHire service agency is defined as having UI-covered earnings greater than $0 in a given quarter where the employer was a ReHire service agency. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal-sector employment. Treatment and control groups are based on an individual's randomly assigned treatment status. The figure plots the percent of treatment and control applicants with formal-sector employment at a ReHire service agency.

Figure A-5a shows the share of each group that was employed at a ReHire social service provider for each quarter relative their application dates. Only a small percentage of the control has such employment in any given quarter. We interpret this figure as supporting evidence that the control group did not receive similar services, simplifying the interpretation of the intent-to-treat analysis presented in the main text.

Figure A-5b shows, however, that control group applicants at one local service agency were nearly as likely to be employed by a ReHire service agency as the treatment group was in the quarters following application. Further, both groups experienced similar increasing and decreasing trends in service agency employment, which is consistent with the timing of temporary subsidized employment. In Section A.11.1, we show that ITT effects are similar when excluding applicants from this service agency from the analysis.

A.9 Effects on Employment/Earnings by quarter

Table A-4 provides coefficient estimates and standard errors for the quarter-by-quarter ITT estimates shown in Figure 1c and Figure 1d. Columns (2) and (5) report specifications that include only stratification group fixed effects, while columns (3) and (6) include controls selected by the post-double selection LASSO procedure. The results are insensitive to the inclusion of these controls.

Table A-4: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado, By Quarter

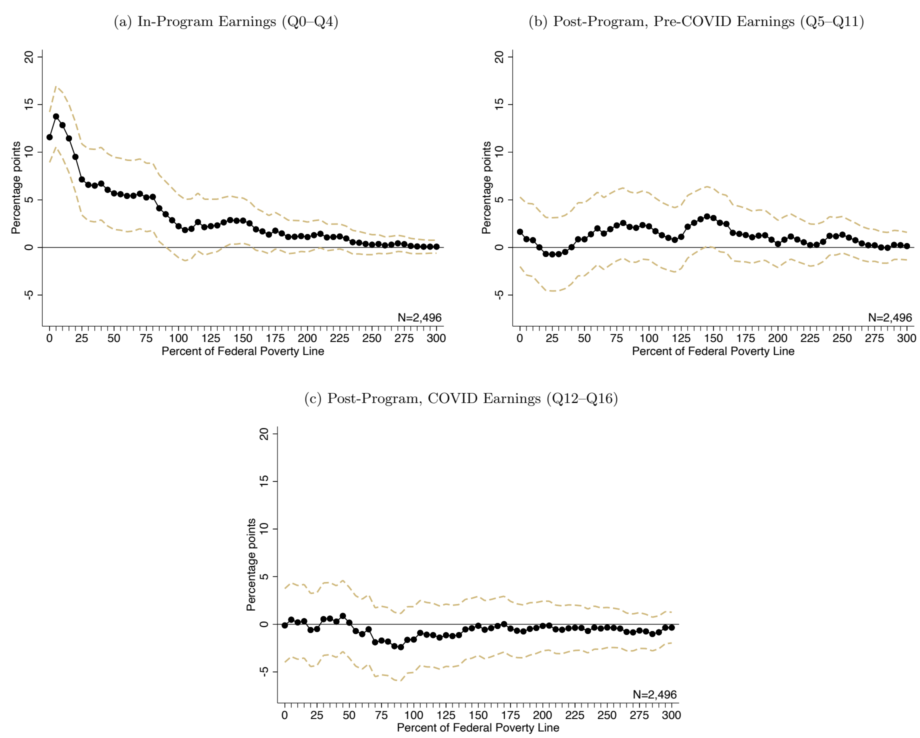
	Any Employment			Earnings		
	Control Mean	ITT Effect No Controls	ITT Effect Controls	Control Mean	ITT Effect No Controls	ITT Effect Controls
	(1)	(2)	(3)	(4)	(5)	(6)
Quarter 0	0.469	0.186**	0.175**	$748	$298**	$255**
		(0.020)	(0.018)		(61)	(50)
Quarter 1	0.569	0.202**	0.197**	$1,644	$598**	$578**
		(0.019)	(0.018)		(92)	(89)
Quarter 2	0.555	0.120**	0.114**	$2,014	$353**	$327**
		(0.020)	(0.019)		(111)	(105)
Quarter 3	0.545	0.055**	0.047*	$2,156	$237+	$204+
		(0.020)	(0.019)		(121)	(114)
Quarter 4	0.525	0.030	0.024	$2,214	$167	$139
		(0.020)	(0.020)		(126)	(119)
Quarter 5	0.509	0.029	0.023	$2,250	$185	$154
		(0.020)	(0.020)		(133)	(126)
Quarter 6	0.500	0.025	0.020	$2,258	$178	$154
		(0.020)	(0.020)		(133)	(128)
Quarter 7	0.485	0.031	0.025	$2,222	$214	$191
		(0.020)	(0.020)		(136)	(130)
Quarter 8	0.450	0.043*	0.038+	$2,223	$246+	$223+
		(0.020)	(0.020)		(141)	(135)
Quarter 9	0.453	0.029	0.024	$2,302	$143	$121
		(0.020)	(0.020)		(145)	(139)
Quarter 10	0.434	0.025	0.020	$2,243	$135	$119
		(0.020)	(0.020)		(142)	(137)
Quarter 11	0.414	0.025	0.020	$2,202	$137	$117
		(0.020)	(0.020)		(146)	(141)
Quarter 12	0.408	0.014	0.009	$2,230	$29	$12
		(0.020)	(0.019)		(149)	(144)
Quarter 13	0.409	-0.003	-0.005	$2,260	-$62	-$79
		(0.020)	(0.019)		(149)	(143)
Quarter 14	0.390	0.011	0.007	$2,204	$32	$12
		(0.020)	(0.019)		(148)	(143)
Quarter 15	0.387	-0.001	-0.004	$2,197	$63	$44
		(0.020)	(0.019)		(152)	(147)
Quarter 16	0.378	-0.000	-0.004	$2,229	-$5	-$23
		(0.020)	(0.019)		(152)	(148)

Notes: Data source is administrative UI earnings data from CDLE. Each row represents outcomes measured in a different quarter relative to ReHire application. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. The dependent variable in columns (1) through (3) is an indicator for formal-sector employment. The dependent variable in columns (4) through (6) is an individual's UI-covered earnings. Columns (1) and (4) report the control group mean. Columns (2) and (5) report the coefficients on a treatment indicator, controlling for service agency-randomization rate block (stratification) fixed effects. Columns (3) and (6) report the coefficients on treatment indicators, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, $^{+}$0.10 significance levels

A.10 Effects on the Likelihood of Earning Above Various Thresholds of the Federal Poverty Line

Figure A-6 provides an alternative analysis of earnings impacts by providing estimated effects across the earnings distribution. Each panel shows ITT estimates of the impact of ReHire on the probability of having quarterly earnings above a variety of federal poverty line thresholds separately for each analysis period. Each point on the graph represents the regression coefficient on treatment group status from estimating Equation (1) using an indicator for earning above the relevant threshold listed on the horizontal axis, and the dashed gold lines provide 95 percent confidence intervals. Panel (a) demonstrates that, during the in-program period, there were statistically significant ($p < 0.05$) and substantial gains in the share with positive earnings (denoted by 0 on the horizontal axis) and in the share earning above thresholds up to roughly 100 percent of the poverty line. Point estimates in Panel (b) are uniformly positive, although the post-program pre-COVID (Q5–Q11) treatment-control differences are generally not statistically significant. Qualitatively, this figure suggests that ReHire may have increased the likelihood of participants having earnings above thresholds up to around 175 percent of the poverty line in the in-program period, but there is no evidence of an increased likelihood of having earnings above higher thresholds.

Figure A-6: ITT Effect of ReHire on the Likelihood That Earnings Exceed Federal Poverty Line Thresholds



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. The figure plots the coefficients from regressions where the outcome is an indicator that an individual's earnings exceeded a given percent of the federal poverty line, assuming a single-person household. Earnings in Panel (a) are measured from the quarter of random assignment through the 4^{th} quarter following random assignment. Earnings in Panel (b) are measured from the 5^{th} quarter following random assignment through the 11^{th} quarter following random assignment. Panel (c) are measured from the 12^{th} quarter following random assignment through the 16^{th} quarter following random assignment—the period where more than half of the sample was experiencing potential labor market disruptions from the COVID-19 pandemic. The horizontal axis depicts the threshold. The vertical axis depicts the magnitude of the point estimate in percentage points. Connected black circles represent each of the estimated ITT effects and the dashed gold lines above and below represent the 95% confidence intervals constructed using heteroskedasticity-robust standard errors.

A.11 Robustness of ITT effects using State of Colorado Administrative Data

A.11.1 Robustness to Excluding Local Service Agency with Highest Rate of Hiring Control Group Members

Section A.8 provides evidence that most individuals in the control group did not receive placement in a transitional job. For one local service agency, however, treatment and control group applicants were nearly as likely to have been employed by a ReHire agency during the in-program period (quarters 0 through 4 following application). While it is possible that control group applicants found unsubsidized work at this employer on their own—some agencies share the same employer ID in the UI data as the broader county government—it is more likely that these individuals were placed in similar transitional jobs given the similar timing of the start and end of these jobs in both the treatment and control groups.

As we note in Section II.E, this similarity in program experience is not a threat to causal identification, but it changes the interpretation of the ITT effects as well as the potential policy conclusions drawn about the program's cost effectiveness. To address this interpretation challenge, we re-estimate the main results from Table 2 using a sample that excludes the 126 applicants from the service agency that employed a large share of the control group. Table A-5 shows that results from both the in-program and post-program periods are similar when excluding applicants at this agency from the analysis.

Table A-5: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado, Excluding Service Agency with Highest Rate of Hiring of Control Group

	Control Mean (1)	ITT Effect No Controls (2)	ITT Effect Controls (3)	Percent Change (4)
Panel A: In-Program Employment (Quarters 0–4)				
Any employment	0.802	0.123**	0.119**	15%
		(0.014)	(0.014)	
Share of quarters worked	0.526	0.121**	0.116**	22%
		(0.015)	(0.014)	
Worked every quarter	0.225	0.085**	0.080**	35%
		(0.018)	(0.017)	
Share of quarters worked at Q1 employer	0.306	0.120**	0.116**	38%
		(0.013)	(0.013)	
Average quarterly earnings	$1,704	$325**	$322**	19%
		(84)	(77)	
Share of quarters above 130% FPL	0.177	0.029*	0.029**	16%
		(0.012)	(0.011)	
Panel B: Post-Program, Pre-COVID Employment (Quarters 5–11)				
Any employment	0.652	0.021	0.017	3%
		(0.020)	(0.019)	
Share of quarters worked	0.458	0.026	0.023	5%
		(0.017)	(0.017)	
Worked every quarter	0.240	0.039*	0.038*	16%
		(0.018)	(0.018)	
Share of quarters worked at Q1 employer	0.094	0.033**	0.033**	35%
		(0.011)	(0.011)	
Average quarterly earnings	$2,208	$136	$141	6%
		(124)	(119)	
Share of quarters above 130% FPL	0.247	0.017	0.018	7%
		(0.015)	(0.015)	
Panel C: Post-Program, Post-COVID Employment (Quarters 12–16)				
Any employment	0.515	-0.000	-0.004	-1%
		(0.021)	(0.020)	
Share of quarters worked	0.387	0.002	-0.000	-0%
		(0.018)	(0.017)	
Worked every quarter	0.260	-0.005	-0.004	-2%
		(0.018)	(0.018)	
Share of quarters worked at Q1 employer	0.041	0.026**	0.026**	62%
		(0.009)	(0.009)	
Average quarterly earnings	$2,169	-$25	-$15	-1%
		(139)	(134)	
Share of quarters above 130% FPL	0.237	-0.007	-0.007	-3%
		(0.016)	(0.015)	
Agency-Rate Block FEs		X	X	
Individual Baseline Controls			X	
Observations	1,052	2,370	2,370	

Notes: Data source is administrative UI earnings data from CDLE. Panels A and B report estimates on in-program and post-program employment outcomes, respectively, for the sample of ReHire applicants who applied between 7/2015 and 12/2017, excluding 126 applicants from the service agency that employed a large share of the control group. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. Column (1) reports the mean for control group applicants. Column (2) reports the coefficient on a treatment indicator, controlling for vendor-randomization rate block (stratification) fixed effects. Column (3) reports the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (4) reports the percent change of the ITT effect in column (3) relative to the control group mean. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, $^{+}$0.10 significance levels

A.11.2 Robustness to Alternative Methods for Statistical Inference

In our primary analysis, we conduct inference using test statistics constructed using heteroskedasticity-robust standard errors. This choice is appropriate given that random assignment occurred at the individual level.

Two potential concerns remain, however. First, the randomization procedure ensured that the number of treatment and control applicants would be balanced over small periods of time so that caseworkers would receive a steady inflow of new participants. In practice, this process meant that the number of treated applicants was fixed for each small set of newly arriving applicants (e.g., 4 of the next 8 applicants at an agency would be treated). This design choice meant that an individual applicant's treatment assignment was potentially correlated with others who applied at the same agency around the same time. We randomized the size of the randomization blocks so that service providers would be unable to predict a given applicant's treatment status. See Appendix Section A.3 for more details.

In order to account for any influence this correlation has on the reported estimates, we conducted randomization-based inference that directly incorporates the way treatment and control assignments were made. We re-ran 10,000 iterations of the treatment assignment algorithm; in each iteration, we re-randomized the treatment/control assignments for each small block of applicants within which the number of treatment individuals was fixed. We then re-estimate Equation (1) for all outcomes reported in Table 2 and collect p-values. This set of p-values represents the distribution of p-values under the sharp null hypothesis of zero treatment effect among all applicants.

A second concern is that the probability of rejecting the null for any one outcome is greater than a chosen significance level because we test hypotheses about program impacts on multiple outcomes both within and across the in-program and post-program periods. To address this concern, we use the joint distribution of p-values estimated above to construct adjusted p-values that control for the family-wise error rate (FWER) following the step-down procedure of Westfall and Young (1993).[58]

Table A-6 provides a set of p-values that address these two potential concerns. Column (1) reproduces the main ITT estimates found in column (3) of Table 2. Then for each outcome, we report naive p-values that are based on heteroskedasticity-robust standard errors (column 2), as well as three randomization-based p-values:

- Per comparison p-values that report the share of permutations where the simulated p-value was smaller than the p-value from the actual treatment assignment (column 3);
- Adjusted p-values that control for the FWER among the five outcomes measured during the same follow-up window (in-program vs. post-program) (column 4);
- Adjusted p-values that control for the FWER among all 15 outcomes included in Table 2 (column 5).

Because we have strong priors that the impact of ReHire differed during the three follow-up periods, our preferred correction for multiple hypothesis testing is in column (4); we present the results in column (5) for completeness.

We draw two conclusions from the results presented in Table A-6. First, the standard p-values in column 2 and the randomization-based p-values in column 3 are strikingly similar, suggesting that the potential concern of serial correlation in treatment assignment imposed by the randomization procedure does not affect our inference. Second, our main results are robust to concerns stemming from testing multiple hypotheses. Most outcomes where effects are significant at the 5 percent level in column (2) remain so even after adjusting for the five hypotheses tested in each panel. The effect on "Worked every

[58] We benefit from the Stata code provided by Jones, Molitor and Reif (2019) and adapt it to rely on the distribution of permutation-based p-values following Young (2019) instead of a bootstrap distribution. See Appendix C in the on-line appendix of Jones, Molitor and Reif (2019) for a detailed description of the step-down procedure.

Table A-6: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado, Inference Robustness

	ITT Effect Controls	Naive p-value	Randomization-Based p-values		
			Per Comparison	Family-Wise: By Panel	Family-Wise: Full Table
	(1)	(2)	(3)	(4)	(5)
Panel A: In-Program Employment (Quarters 0–4)					
Any employment	0.116	< 0.001	< 0.001	< 0.001	0.001
Share of quarters worked	0.112	< 0.001	< 0.001	< 0.001	0.001
Worked every quarter	0.073	< 0.001	< 0.001	< 0.001	0.001
Share of quarters worked at Q1 employer	0.111	< 0.001	< 0.001	< 0.001	0.001
Average quarterly earnings	$300	< 0.001	< 0.001	0.001	0.005
Share of quarters above 130% FPL	0.025	0.020	0.021	0.021	0.121
Panel B: Post-Program, Pre-COVID Employment (Quarters 5–11)					
Any employment	0.016	0.378	0.385	0.385	0.810
Share of quarters worked	0.025	0.131	0.141	0.299	0.462
Worked every quarter	0.038	0.028	0.034	0.101	0.160
Share of quarters worked at Q1 employer	0.034	0.002	0.002	0.010	0.019
Average quarterly earnings	$154	0.185	0.196	0.367	0.575
Share of quarters above 130% FPL	0.016	0.256	0.270	0.434	0.695
Panel C: Post-Program, Post-COVID Employment (Quarters 12–16)					
Any employment	-0.001	0.952	0.949	1.000	1.000
Share of quarters worked	-0.000	0.998	0.998	0.998	0.998
Worked every quarter	-0.013	0.461	0.457	0.808	0.808
Share of quarters worked at Q1 employer	0.028	0.001	0.001	0.004	0.008
Average quarterly earnings	$-7	0.958	0.957	0.998	0.998
Share of quarters above 130% FPL	-0.007	0.640	0.645	0.925	0.925

Notes: Data source is administrative UI earnings data from CDLE. Panels A and B report estimates on in-program and post-program employment outcomes, respectively, for the sample of ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. Column (1) reports the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (2) reports naive p-values. Columns (3) through (5) report randomization-based p-values that come from permuting treatment assignment 10,000 times and re-estimating effects. Column (3) reports per comparison p-values. Columns (4) and (5) report adjusted p-values that control for the family-wise error rate (Westfall and Young, 1993; Jones, Molitor and Reif, 2019) among outcomes reported within the panel and within the table, respectively.

quarter" during the post-program, pre-COVID period becomes marginally significant ($p = 0.105$) after adjusting for the hypotheses tested during that time period. The effect on "Share of quarters above 130% FPL" loses significance when adjusting inference for all fifteen hypotheses tested in the table ($p = 0.139$).

A.12 Using Machine Learning to Test for Heterogeneity

The literature exploring the effects of active labor market programs has found mixed results across program models and types of clients served (Card, Kluve and Weber, 2018). Even within the transitional jobs literature, results have varied across locations and target populations (Barden et al., 2018; Foley, Farrell and Webster, 2018; Cummings and Bloom, 2020). Relying on ReHire's broad eligibility criteria and the breadth of information collected on applicants at baseline, we explore whether individual heterogeneity might reconcile the mixed results found across the literature. One concern for this analysis, however, is that there are many potential ways to construct sub groups to explore heterogeneity and the number of additional hypotheses tested means that we might detect heterogeneity by chance. To address this concern, we rely on a data driven approach to guide this analysis.

We use machine learning tools to test whether a high-dimensional set of baseline characteristics are predictive of treatment effect heterogeneity among the primary outcomes reported in Table 2. In an ideal setting, we would be able to estimate directly a Conditional Average Treatment Effect (CATE) function that would map baseline characteristics Z to an estimated treatment effect $\tau(Z)$. Given the large number of potential characteristics that could be included in Z and the possibility that various characteristics could interact to affect the CATE in linear and nonlinear ways, estimating such a complex function is difficult.

Given this complexity and high-dimensionality, we follow Chernozhukov et al. (2020) and construct a proxy estimate of each individual's CATE and use that proxy to ask whether it is predictive of underlying treatment effect heterogeneity. First, we randomly select an auxiliary sample with half of the treatment and control group applicants. Using control group applicants in the auxiliary sample, we train a machine learning method using baseline characteristics Z to predict the outcome in the untreated state Y^C. Similarly, we use treatment group applicants in the auxiliary sample to predict the outcome in the treated state Y^T. Then, with the remaining half of the sample (main sample), we use the two estimates to predict $\hat{Y^C}(Z_i)$ and $\hat{Y^T}(Z_i)$ in both the treatment and control group. Finally, for each applicant in the main sample we construct a proxy of their own CATE: $\hat{S}(Z_i) = \hat{Y^T}(Z_i) - \hat{Y_i^C}(Z_i)$.

The methods in Chernozhukov et al. (2020) provide an empirical test for whether the proxy CATE, $\hat{S}(Z_i)$, predicts meaningful heterogeneity. To implement this test, we estimate the following regression using weighted least squares:

$$Y_i = \alpha' X_1 + \beta_1(D_i - p(Z_i)) + \beta_2(D_i - p(Z_i))(\hat{S}(Z_i) - E(\hat{S}(Z_i))) + \epsilon \tag{A-1}$$

where D_i is an indicator for whether an individual was randomly assigned to receive access to ReHire services, X_i includes vendor-rate fixed effects, and $p(Z_i)$ is an individual's treatment propensity, which is known from the randomization protocol. The regression is weighted by $w(Z_i) = 1/[p(Z_i)(1-p(Z_i))]$. Under this specification, Chernozhukov et al. (2020) show that $\hat{\beta}_1$ provides an estimate of the Average Treatment Effect (ATE) and that $\hat{\beta}_2$ provides an estimate of the slope of the Best Linear Predictor of the CATE. To deal with uncertainty that stems from sample splitting, we repeat this procedure across 1,000 random splits of the data and report the median estimates of $\hat{\beta}_1$ and $\hat{\beta}_2$, as well as median p-values, and upper and lower bounds of the 95 percent confidence interval. To account for uncertainty induced by randomly splitting of the sample, the confidence intervals reported in tables below are discounted to be 90 percent confidence intervals, and p-values are doubled (or set to the maximum value of 1, if neccessary).

We also estimated the group average treatment effects (GATES) following Chernozhukov et al. (2020). Using the proxy CATE, $\hat{S}(Z_i)$, we divide the main sample into quartiles and define an indicator G_k for each quartile k. We then estimate the following regression:

$$Y_i = \alpha' X_1 + \sum_{k=1}^{4} \gamma_k \cdot (D_i - p(Z_i)) \cdot 1(G_k) + \nu \tag{A-2}$$

The vector of estimates γ represent the average treatment effect within each of the groups. Testing the null hypothesis that the difference between $\gamma_4 - \gamma_1$ is zero provides another test for heterogeneity in program impacts.

We construct three sets of characteristics (Z) to assess the added value of characteristics not typically measured in the literature: (i) a baseline set to mirror the types of characteristics that have been used to target the program; (ii) a skills set that further incorporates age and measures of cognitive and non-cognitive skills; and (iii) an extended set that provides higher-frequency information on employment, earnings, and benefit usage, as well as including information on employment barriers. The sets include the following measures:[59]

1. **Baseline:** Earnings in the year before randomization; SNAP benefit receipt in the month before randomization; TANF benefit receipt in the month before randomization; an indicator for being male; six educational attainment indicators (less than high school, high school diploma or GED, some college, associate's degree, bachelor's degree, missing); three indicators for the ReHire priority groups (veteran, non-custodial parent, older worker); three indicators for having prior felony (yes, no, missing)

2. **Add Skills and Experience:** All variables in the "Baseline" set; age in years and an indicator for missing; motivation scored by caseworker (1–10) and indicator for missing ; likelihood of overcoming barriers assessed by caseworker (1–10) and indicator for missing; score on Raven's progressive matrices (0–36) and indicator for missing; score on timed math test (0–100), number of attempted answers on math test (0–160), and indicator for missing; grit (1–5) and indicator for missing; locus of control (1–5) and indicator for missing; and component scores of Big Five—extraversion, agreeableness, conscientiousness, neuroticism, imagination (1–5) and an indicator for missing.

3. **Extended Predictors:** All variables in the "Add Skills and Experience" set; earnings in each of the eight quarters before randomization; total earnings in the two years before randomization; number of employers in each of the eight quarters before randomization; SNAP receipt in each of the 24 months before randomization; total SNAP receipt in the year before randomization; total SNAP receipt in the two years before randomization; TANF receipt in each of the 24 months before randomization; total TANF receipt in the year before randomization; total TANF receipt in the two years before randomization; four indicators for self-reported race (white, not-white, black, hispanic); seven indicators for marital status (married, divorced, partnered, married living apart, single, separated, and widowed); six indicators for housing type (owned, jointly owned, owned by another resident, renting, transitional, homeless); ability to drive (yes, no, missing); issues with childcare (yes, no, missing); work-limiting health problems (yes, no, missing); ever experienced homelessness (yes, no, missing); expect economic hardship in future (yes, no, missing); alcohol has ever affected work (yes, no, missing); self-identify as alcoholic (yes, no, missing); marijuana has ever affected work (yes, no, missing); self-identified marijuana addiction (yes, no, missing); other drugs have ever affected work (yes, no missing); self-identified drug addiction (yes, no, missing; any reported substance abuse (yes, no, missing); life satisfaction ladder (0–10) and indicator for missing; and CESD depression scale (0–7) and indicator for missing.

We follow Chernozhukov et al. (2020) in considering four different machine learning methods—elastic net, boosted trees, neural network with feature extraction, and random forest—using the caret package (Kuhn, 2009). Specifically, we use glmnet, gmb, pcaNNet, and rf to implement the elastic net, boosted trees, neural network, and random forest, respectively. Tuning parameters for the first three methods are chosen to minimize the mean squared error estimates using 2-fold cross validation. For random forests, we grow 25,000 trees and randomly select a third of the available predictors when identifying nodes.

[59] For continuous measures with missing values, we impute missing values at the sample median and include a dummy that the variable was missing.

Table A-7 reports estimates of the criteria used to pick the best performing machine learning method (see Chernozhukov et al. (2020) for details). Columns (1) through (4) provide estimates when targeting the BLP. Columns (5) through (8) provide estimates when targeting the GATES. For in-program outcomes, elastic net seems to perform best when targeting the BLP, and all are comparably similar when targeting the GATES. For post-program outcomes, random forest seems to perform best (or close to best) for both. Given these results, we report estimates of the BLP using the elastic net and random forest.

Table A-8 and Table A-9 report results from estimating the BLP of treatment heterogeneity using the elastic net and random forest, respectively. In each table, columns (1), (3), and (5) report estimates of the ATE when using limited predictors, adding age and skills as predictors, and adding detailed information on labor market and benefit histories and employment barriers, respectively. Estimates of the heterogeneity parameter, β_2, are reported in columns (2), (4), and (6). 90 percent confidence intervals are reported in parentheses and p-values that test the null hypothesis that the parameter is zero are reported in brackets.

ATE estimates are consistent with the results reported in Table 2. For example, the first estimate of 12.1 percentage points in column (1) of Table A-8 is the same as the 12.1 percentage point effect in Table 2 when only including stratification fixed effects. This similarity is the case across the set of baseline characteristics used as predictors and across machine learning methods.

The heterogeneity parameter, β_2, shows how estimated treatment effects change with a one unit change in the predicted CATE. A value of 1 for this parameter would show that a 1 unit increase (e.g., percentage point or dollar) in the predicted treatment effect is associate with a 1 unit increase in the actual treatment effect. In this scenario, baseline characteristics would be perfectly predictive of treatment effect heterogeneity. A value of 0 indicates that the predicted CATE is not related to any underlying heterogeneity.

We find no strong evidence that baseline characteristics are predictive of underlying heterogeneity. We are able to reject the null hypothesis for only one outcome across both machine learning methods. When using the full set of predictors with an elastic net (Table A-8, column 6), we find that the CATE predicts meaningful heterogeneity in the effect on whether an individual was employed at all during the in-program period. The point estimate on the interaction term is 0.8 and the p-value is 0.044. In nearly all other other cases, p-values are large or close to one.

Table A-10 provides information on the type of characteristics that are correlated with larger predicted effects on any in-program employment. The table reports differences in average baseline characteristics among individuals who are predicted to be least affected (column 1) and most affected (column 2) when using the elastic net. Estimates of the difference across groups is reported in column (3) and 90 percent confidence intervals are reported in brackets in column (4). The p-values for the hypothesis that the parameter is equal to zero are reported in column (5). All estimates come from the median value across 1,000 random splits of the data.

Individuals who are predicted to experience the largest increases in any in-program employment are more disadvantaged on a number of margins. Pre-application earnings and employment rates are substantially lower in the most affected group. The least affected group had a 95 percent employment rate in the four quarters before application and earned on average \$3,154, relative to 25.2 percent and \$327 in the most affected group. The most affected group was substantially older (13.6 years older and 48.2 percentage points more likely to fall in the older worker target population), and were more likely to experience a work-limiting health program. Conversely, they were less likely to experience some common employment barriers such as child care issues or substance abuse problems.

Table A-7: Predicting Conditional Average Treatment Effects, Comparison of ML Methods

	Best BLP (Λ)				Best GATES ($\bar{\Lambda}$)			
	Elastic Net	Boosting	Neural Network	Random Forest	Elastic Net	Boosting	Neural Network	Random Forest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Limited Predictors								
In-Program Employment (Quarter 0–4)								
Any employment	**0.026**	0.017	0.024	0.012	**0.017**	0.016	0.016	0.015
Share of quarters worked	0.014	0.017	**0.017**	0.011	0.015	**0.015**	0.015	0.015
Worked every quarter	**0.019**	0.015	0.018	0.014	0.008	0.008	0.008	**0.008**
Share of quarters worked at Q1 employer	0.018	**0.029**	0.026	0.011	0.016	**0.016**	0.015	0.015
Average quarterly earnings	84	**92**	90	76	134,477	136,122	**149,738**	134,834
Share of quarters above 130% FPL	**0.012**	0.010	0.009	0.012	0.001	0.001	0.001	**0.002**
Post-Program, Pre-COVID Employment (Quarter 5–11)								
Any employment	**0.022**	0.018	0.015	0.014	0.002	**0.003**	0.002	0.002
Share of quarters worked	0.016	0.028	0.023	**0.035**	0.002	0.003	0.003	**0.003**
Worked every quarter	0.023	0.022	0.028	**0.030**	0.004	0.004	**0.004**	0.004
Share of quarters worked at Q1 employer	0.010	**0.014**	0.012	0.011	**0.002**	0.002	0.002	0.002
Average quarterly earnings	97	114	**129**	123	105,080	103,112	**114,356**	107,718
Share of quarters above 130% FPL	0.014	0.012	0.011	**0.021**	0.001	0.001	0.001	**0.002**
Panel B: Add Age and Skills								
In-Program Employment (Quarter 0–4)								
Any employment	**0.037**	0.016	0.013	0.013	**0.017**	0.016	0.016	0.016
Share of quarters worked	**0.018**	0.018	0.015	0.018	0.015	**0.015**	0.015	0.015
Worked every quarter	0.018	0.017	0.017	**0.020**	0.008	**0.008**	0.008	0.008
Share of quarters worked at Q1 employer	0.015	**0.024**	0.016	0.021	**0.015**	0.015	0.015	0.015
Average quarterly earnings	61	78	77	**83**	127,746	128,923	**148,095**	119,627
Share of quarters above 130% FPL	**0.011**	0.009	0.009	0.009	0.001	**0.001**	0.001	0.001
Post-Program, Pre-COVID Employment (Quarter 5–11)								
Any employment	**0.019**	0.016	0.016	0.016	0.002	0.002	**0.003**	0.002
Share of quarters worked	0.016	0.016	**0.022**	0.016	0.002	0.002	**0.003**	0.002
Worked every quarter	0.026	0.022	**0.033**	0.032	0.004	0.004	**0.005**	0.004
Share of quarters worked at Q1 employer	0.009	0.012	0.009	**0.013**	0.002	0.002	0.002	**0.002**
Average quarterly earnings	109	105	122	**127**	92,166	101,083	**120,039**	105,087
Share of quarters above 130% FPL	**0.015**	0.012	0.012	0.013	0.001	0.001	0.001	**0.002**
Panel C: Extended Predictors								
In-Program Employment (Quarter 0–4)								
Any employment	**0.044**	0.021	0.012	0.024	**0.017**	0.016	0.016	0.016
Share of quarters worked	0.011	**0.015**	0.011	0.010	0.014	**0.014**	0.014	0.014
Worked every quarter	0.022	0.018	**0.028**	0.015	0.008	0.007	**0.008**	0.007
Share of quarters worked at Q1 employer	0.020	0.023	0.013	**0.028**	**0.015**	0.015	0.014	0.015
Average quarterly earnings	**95**	76	76	68	120,617	126,872	**143,283**	126,394
Share of quarters above 130% FPL	**0.014**	0.012	0.008	0.011	0.001	0.001	0.001	**0.001**
Post-Program, Pre-COVID Employment (Quarter 5–11)								
Any employment	0.017	0.017	**0.017**	0.016	0.002	0.002	0.002	**0.002**
Share of quarters worked	0.013	0.014	0.013	**0.017**	0.002	0.002	0.002	**0.002**
Worked every quarter	**0.020**	0.015	0.016	0.017	**0.004**	0.004	0.003	0.004
Share of quarters worked at Q1 employer	0.009	**0.012**	0.009	0.010	**0.002**	0.002	0.002	0.002
Average quarterly earnings	**119**	105	117	107	86,133	97,724	**111,386**	100,349
Share of quarters above 130% FPL	**0.020**	0.012	0.014	0.011	0.001	0.001	**0.001**	0.001

Notes: Data source is the baseline survey and administrative data from CDLE and CBMS. The sample include ReHire applicants who applied between 7/2015 and 12/2018. The table compares the ability of four machine learning methods to produce proxy predictors of CATE. Estimates comes from the median of 1,000 sample splits. Columns (1)–(4) and (5)–(8) present estimates of Λ when choosing the optimal machine learning method for BLP and GATES, respectively. See Chernozhukov et al. (2020) for details. For each outcome and target (e.g., BLP or GATES), the maximum estimate is in bold to indicate the optimal method.

Table A-8: Best Linear Predictor of Formal-Sector Employment and Earnings, Elastic Net

	Limited Predictors		Add Age and Skills		Extended Predictors	
	ATE (β_1)	HET (β_2)	ATE (β_1)	HET (β_2)	ATE (β_1)	HET (β_2)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: In-Program Employment (Quarter 0–4)						
Any employment	0.121	0.476	0.120	0.794	0.120	0.800
	(0.084, 0.158)	(-0.273, 1.278)	(0.083, 0.157)	(-0.032, 1.659)	(0.083, 0.157)	(0.122, 1.474)
	[0.000]	[0.423]	[0.000]	[0.118]	[0.000]	[0.044]
Share of quarters worked	0.118	0.090	0.116	0.292	0.114	0.127
	(0.079, 0.156)	(-0.918, 1.095)	(0.078, 0.155)	(-0.573, 1.203)	(0.077, 0.151)	(-0.619, 0.846)
	[0.000]	[1.000]	[0.000]	[1.000]	[0.000]	[1.000]
Worked every quarter	0.079	-0.314	0.079	-0.234	0.074	-0.431
	(0.029, 0.129)	(-1.587, 0.901)	(0.029, 0.128)	(-1.571, 1.072)	(0.026, 0.122)	(-1.425, 0.585)
	[0.004]	[1.000]	[0.004]	[1.000]	[0.005]	[0.806]
Share of quarters worked at Q1 employer	0.118	0.465	0.118	0.330	0.116	0.442
	(0.081, 0.154)	(-0.666, 1.701)	(0.081, 0.154)	(-0.887, 1.704)	(0.079, 0.152)	(-0.377, 1.312)
	[0.000]	[0.837]	[0.000]	[1.000]	[0.000]	[0.560]
Average quarterly earnings	\$306	0.134	\$306	-0.006	\$294	-0.018
	(84, 526)	(-0.351, 0.622)	(85, 528)	(-0.351, 0.339)	(74, 516)	(-0.182, 0.148)
	[0.014]	[1.000]	[0.013]	[1.000]	[0.018]	[1.000]
Share of quarters above 130% FPL	0.026	-0.210	0.026	-0.236	0.025	-0.225
	(-0.005, 0.057)	(-1.224, 0.893)	(-0.005, 0.057)	(-1.157, 0.666)	(-0.005, 0.055)	(-0.981, 0.493)
	[0.191]	[1.000]	[0.200]	[1.000]	[0.211]	[1.000]
Panel B: Post-Program, Pre-COVID Employment (Quarter 5–11)						
Any employment	0.019	0.375	0.017	0.265	0.015	0.206
	(-0.034, 0.072)	(-0.781, 1.530)	(-0.036, 0.069)	(-0.756, 1.306)	(-0.037, 0.067)	(-0.676, 1.040)
	[0.954]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Share of quarters worked	0.027	0.165	0.026	0.161	0.025	0.043
	(-0.019, 0.074)	(-1.015, 1.369)	(-0.021, 0.072)	(-0.766, 1.110)	(-0.021, 0.071)	(-0.743, 0.842)
	[0.502]	[1.000]	[0.558]	[1.000]	[0.564]	[1.000]
Worked every quarter	0.041	0.388	0.042	0.496	0.041	0.197
	(-0.009, 0.091)	(-0.636, 1.462)	(-0.008, 0.091)	(-0.542, 1.592)	(-0.009, 0.090)	(-0.604, 1.070)
	[0.209]	[0.921]	[0.204]	[0.695]	[0.213]	[1.000]
Share of quarters worked at Q1 employer	0.035	0.104	0.035	-0.026	0.034	-0.055
	(0.003, 0.066)	(-1.258, 1.633)	(0.003, 0.066)	(-1.550, 1.424)	(0.002, 0.065)	(-1.779, 1.389)
	[0.065]	[1.000]	[0.065]	[1.000]	[0.072]	[1.000]
Average quarterly earnings	\$138	0.004	\$130	-0.108	\$128	-0.033
	(-194, 469)	(-0.503, 0.537)	(-202, 462)	(-0.478, 0.258)	(-201, 461)	(-0.196, 0.131)
	[0.835]	[1.000]	[0.891]	[1.000]	[0.897]	[1.000]
Share of quarters above 130% FPL	0.016	-0.225	0.015	-0.280	0.015	-0.435
	(-0.024, 0.056)	(-1.346, 0.911)	(-0.025, 0.054)	(-1.227, 0.681)	(-0.024, 0.055)	(-1.259, 0.431)
	[0.863]	[1.000]	[0.942]	[1.000]	[0.891]	[0.662]
Predictors						
Typical Target Populations	X		X		X	
Age and Skills			X		X	
Quarterly Earnings, Monthly Benefits					X	
Employment Barriers					X	
Agency-Rate Block FE	X		X		X	
Observation	2,495		2,495		2,495	

Notes: See Table 2 for sample construction and details on outcome variables. The table reports estimates from Equation A-1 using three specifications that vary the set of predictor variables: columns (1)–(2), columns (3)–(4), and columns (5)–(6). Columns (1), (3), and (5) report estimates of the average treatment effect (ATE) and columns (2), (4), and (6) report estimates of the slope on on conditional average treatment effect (HET). 90 percent confidence intervals are reported in parentheses. The p-values for the hypothesis that the parameter is equal to zero are reported in brackets. All estimates come from the median value across 1,000 random splits of the data. See Appendix Section A.12 for details on the machine learning procedure as well as the baseline characteristics included across the three specifications.

Table A-9: Best Linear Predictor of Formal-Sector Employment and Earnings, Random Forest

	Limited Predictors		Add Age and Skills		Extended Predictors	
	ATE (β_1)	HET (β_2)	ATE (β_1)	HET (β_2)	ATE (β_1)	HET (β_2)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: In-Program Employment (Quarter 0–4)						
Any employment	0.119	-0.029	0.122	0.074	0.121	0.215
	(0.082, 0.157)	(-0.235, 0.176)	(0.085, 0.159)	(-0.207, 0.352)	(0.085, 0.158)	(-0.121, 0.564)
	[0.000]	[1.000]	[0.000]	[1.000]	[0.000]	[0.409]
Share of quarters worked	0.118	0.017	0.118	0.142	0.114	0.061
	(0.080, 0.156)	(-0.199, 0.228)	(0.081, 0.156)	(-0.170, 0.459)	(0.077, 0.150)	(-0.322, 0.452)
	[0.000]	[1.000]	[0.000]	[0.750]	[0.000]	[1.000]
Worked every quarter	0.081	-0.040	0.077	-0.132	0.074	-0.078
	(0.032, 0.130)	(-0.266, 0.188)	(0.027, 0.126)	(-0.486, 0.222)	(0.025, 0.122)	(-0.514, 0.354)
	[0.003]	[1.000]	[0.005]	[0.933]	[0.006]	[1.000]
Share of quarters worked at Q1 employer	0.118	0.037	0.116	0.186	0.115	0.288
	(0.082, 0.155)	(-0.176, 0.248)	(0.079, 0.152)	(-0.140, 0.512)	(0.078, 0.151)	(-0.083, 0.657)
	[0.000]	[1.000]	[0.000]	[0.516]	[0.000]	[0.258]
Average quarterly earnings	$312	-0.032	$299	0.073	$316	0.076
	(88, 537)	(-0.239, 0.179)	(78, 519)	(-0.223, 0.363)	(99, 534)	(-0.293, 0.452)
	[0.013]	[1.000]	[0.016]	[1.000]	[0.009]	[1.000]
Share of quarters above 130% FPL	0.027	0.068	0.025	0.031	0.027	-0.133
	(-0.004, 0.059)	(-0.150, 0.286)	(-0.006, 0.056)	(-0.306, 0.368)	(-0.004, 0.057)	(-0.557, 0.301)
	[0.166]	[1.000]	[0.223]	[1.000]	[0.170]	[1.000]
Panel B: Post-Program, Pre-COVID Employment (Quarter 5–11)						
Any employment	0.022	0.036	0.021	0.046	0.018	0.050
	(-0.031, 0.075)	(-0.182, 0.254)	(-0.032, 0.074)	(-0.308, 0.401)	(-0.035, 0.070)	(-0.403, 0.485)
	[0.846]	[1.000]	[0.863]	[1.000]	[1.000]	[1.000]
Share of quarters worked	0.029	0.156	0.029	0.093	0.025	0.154
	(-0.017, 0.076)	(-0.055, 0.368)	(-0.018, 0.075)	(-0.256, 0.446)	(-0.021, 0.072)	(-0.277, 0.583)
	[0.430]	[0.289]	[0.446]	[1.000]	[0.558]	[0.971]
Worked every quarter	0.042	0.129	0.043	0.223	0.040	0.123
	(-0.007, 0.092)	(-0.086, 0.345)	(-0.007, 0.093)	(-0.126, 0.563)	(-0.009, 0.089)	(-0.283, 0.539)
	[0.191]	[0.477]	[0.179]	[0.423]	[0.221]	[1.000]
Share of quarters worked at Q1 employer	0.035	0.058	0.034	0.116	0.034	0.085
	(0.003, 0.066)	(-0.158, 0.273)	(0.003, 0.066)	(-0.189, 0.420)	(0.002, 0.065)	(-0.305, 0.464)
	[0.062]	[1.000]	[0.068]	[0.913]	[0.075]	[1.000]
Average quarterly earnings	$154	0.061	$131	0.086	$133	0.077
	(-183, 491)	(-0.148, 0.271)	(-202, 464)	(-0.229, 0.402)	(-192, 460)	(-0.310, 0.484)
	[0.740]	[1.000]	[0.883]	[1.000]	[0.849]	[1.000]
Share of quarters above 130% FPL	0.017	0.113	0.016	0.081	0.015	0.044
	(-0.024, 0.057)	(-0.104, 0.329)	(-0.025, 0.056)	(-0.275, 0.431)	(-0.024, 0.054)	(-0.390, 0.474)
	[0.841]	[0.610]	[0.899]	[1.000]	[0.902]	[1.000]
Predictors						
Typical Target Populations	X		X		X	
Age and Skills			X		X	
Quarterly Earnings, Monthly Benefits					X	
Employment Barriers					X	
Agency-Rate Block FE	X		X		X	
Observation	2,495		2,495		2,495	

Notes: See Table 2 for sample construction and details on outcome variables. The table reports estimates from Equation A-1 using three specifications that vary the set of predictor variables: columns (1)–(2), columns (3)–(4), and columns (5)–(6). Columns (1), (3), and (5) report estimates of the average treatment effect (ATE) and columns (2), (4), and (6) report estimates of the slope on on conditional average treatment effect (HET). 90 percent confidence intervals are reported in parentheses. The p-values for the hypothesis that the parameter is equal to zero are reported in brackets. All estimates come from the median value across 1,000 random splits of the data. See Appendix Section A.12 for details on the machine learning procedure as well as the baseline characteristics included across the three specifications.

Table A-10: Average Characteristics of Most and Least Affected Groups, Any Employment During Quarters 0 through 4, Elastic Net

	Stratify by Predicted Effect on Any Employment (Q0–Q4)				
	Least Affected	Most Affected	Difference	Confidence Interval	p-value
	(1)	(2)	(3)	(4)	(5)
Employment and Benefit Receipt					
Average quarterly earnings in last year	$3,154	$327	$-2,794**	[-3,244, -2,369]	0.000
Share of quarters worked last year	0.95	0.25	-0.69**	[-0.74, -0.64]	0.000
TANF recipient	0.15	0.04	-0.12**	[-0.16, -0.07]	0.000
SNAP recipient	0.61	0.58	-0.03	[-0.11, 0.05]	0.877
Demographics					
Average Age (years)	40.67	54.43	13.61**	[12.02, 15.18]	0.000
Male	0.51	0.52	0.01	[-0.07, 0.09]	1.000
Racial minority	0.39	0.44	0.07	[-0.01, 0.14]	0.197
Less than high school credential	0.15	0.16	0.01	[-0.05, 0.07]	1.000
High school graduate	0.16	0.16	0.00	[-0.05, 0.06]	1.000
Some college	0.30	0.27	-0.03	[-0.10, 0.04]	0.733
Associate's degree	0.12	0.10	-0.02	[-0.07, 0.03]	0.996
Bachelor's degree	0.14	0.16	0.02	[-0.03, 0.08]	0.809
ReHire Target Populations					
Veteran	0.21	0.23	0.01	[-0.05, 0.08]	1.000
Non-custodial parent	0.23	0.11	-0.12**	[-0.18, -0.06]	0.000
Older worker	0.29	0.77	0.48**	[0.41, 0.55]	0.000
Barriers to Employment					
Stable housing	0.63	0.58	-0.06	[-0.13, 0.02]	0.316
Not allowed to drive	0.21	0.19	-0.03	[-0.09, 0.04]	0.857
Issue with childcare	0.14	0.05	-0.09**	[-0.13, -0.04]	0.000
Limiting health problem	0.20	0.31	0.12**	[0.05, 0.18]	0.002
Experience with homelessness	0.43	0.40	-0.03	[-0.11, 0.05]	0.898
Felony	0.20	0.25	0.05	[-0.01, 0.12]	0.196
Alcoholic	0.11	0.13	0.02	[-0.03, 0.07]	0.678
Drinking has affected life	0.19	0.12	-0.07*	[-0.12, -0.01]	0.043
Addicted to marijuana	0.03	0.02	-0.01	[-0.04, 0.01]	0.561
Smoking marijuana has affected life	0.05	0.02	-0.02	[-0.05, 0.00]	0.183
Addicted to drugs	0.10	0.10	0.00	[-0.04, 0.05]	1.000
Drug use has affected life	0.07	0.05	-0.02	[-0.06, 0.01]	0.423
Any substance abuse problem	0.24	0.16	-0.08*	[-0.14, -0.01]	0.033
Cognitive skills					
Timed math test, percent correct	58.45	56.43	-2.08	[-4.55, 0.45]	0.215
Number of math questions attempted (out of 160)	96.48	93.46	-3.11	[-6.96, 0.95]	0.265
Raven's score (out of 36)	31.39	30.01	-1.35**	[-2.12, -0.59]	0.001
Non-cognitive characteristics					
Locus of control (1–5)	4.10	4.04	-0.05	[-0.14, 0.04]	0.538
Grit (1–5)	3.87	3.94	0.08+	[0.00, 0.15]	0.075
Extraversion (1–5)	3.17	3.09	-0.08	[-0.20, 0.04]	0.399
Agreeableness (1–5)	3.95	3.94	-0.00	[-0.09, 0.09]	1.000
Conscientious (1–5)	3.98	4.04	0.05	[-0.05, 0.14]	0.647
Neuroticism (1–5)	2.48	2.42	-0.05	[-0.15, 0.05]	0.695
Imagination (1–5)	3.05	3.08	0.04	[-0.03, 0.11]	0.569
Life satisfaction ladder (0–10)	5.61	5.95	0.32+	[0.00, 0.63]	0.093
Depression scale (0–10)	1.65	1.32	-0.32**	[-0.53, -0.12]	0.004
Caseworker Assessment					
Perceived motivation (out of 10)	8.59	8.42	-0.18	[-0.46, 0.10]	0.407
Likelihood to overcome barriers (out of 10)	8.28	8.10	-0.18	[-0.49, 0.12]	0.467

Notes: Data source is a baseline survey and administrative data from CDLE and CBMS. The sample includes ReHire applicants who applied between 7/2015 and 12/2018 The sample is stratified by an individual's predicted conditional average treatment effect on having any employment in the in-program period. The table reports differences in average baseline characteristics among individuals who are predicted to be least affected (column 1) and most affected (column 2). Estimates of the difference across groups is reported in column (3) and 90 percent confidence intervals are reported in brackets in column (4). The p-values for the hypothesis that the parameter is equal to zero are reported in column (5). All estimates come from the median value across 1,000 random splits of the data. See Section A.15.4 for details on the estimation procedure.

A.13 Additional Follow-up Survey Details

A.13.1 Follow-up Survey Response Rates and Reweighting for Selective Response

Among the 2,496 applicants in our analysis sample, 954 individuals completed the follow-up survey. Response rates were higher in the treatment group (41.9 percent) than the control group (33.7 percent). Table A-11 reports average baseline characteristics of those who did not respond to the follow-up survey (column 1) and those who responded to the follow-up survey (column 2). In general, survey respondents were more likely to have received TANF in the prior year, more likely to be female, and had higher levels of education. Respondents were less likely to be a non-custodial parent, to be an older worker, to be allowed to drive, to have ever been homeless, or to report substance abuse. However, the magnitudes of the differences in means relative to the control group standard deviation are less than 0.20 for all but one characteristic (Imbens and Wooldridge, 2009).

To account for selective survey response, we construct a set of inverse propensity weights to use in our analysis of outcomes from this data source. Separately by treatment assignment, we use a logit specification to predict survey response based on administrative data outcomes measured prior to application and in the months/quarters prior to survey invitation. Specifically, we include 5 indicators for educational attainment (high school; some college; Associate's degree; Bachelor's degree; or flag for missing education), 1 indicator for gender (male), 12 indicators for quarterly employment (the 12 quarters before random assignment), 24 indicators each for monthly SNAP and TANF participation (the 24 months before random assignment), 3 indicators for any employment in the one/two/three year(s) before random assignment, and 3 indicators for having no employment in the one/two/three year(s) before random assignment. We also include 12 controls for quarterly earnings in the 12 quarters before random assignment, 3 controls for average earnings in the one/two/three year(s) before random assignment, and 4 controls for total SNAP and TANF amount received in the one/two year(s) before random assignment. The resulting attrition weight is the inverse of the predicted probability an individual completed the follow-up survey, and we top code the weights at the 99$^{\text{th}}$ percentile and normalize the weights to sum to 1.

A.13.2 Treatment-Control Baseline Balance Among Follow-Up Sample

Among follow-up survey respondents, baseline characteristics are largely balanced between the control group and treatment group, regardless of whether we apply the weights described above. Table A-12 reports average baseline characteristics of respondents in the control group (column 1) and the treatment group (column 2). Columns (3) and (5) report the unweighted and weighted differences in means, respectively. Corresponding test statistics are reported in columns (4) and (6). In the unweighted sample, the treatment group is less educated, more likely to be male, and less likely to expect economic hardship or to have ever been homeless. In the weighted sample, the only statistically significant difference is that the treatment group is less likely to expect economic hardship in the coming months.

Table A-11: Follow-up Survey Response Selection

	Non-Respondent Mean (1)	Respondent Mean (2)	Within-Strata Difference (3)	t-stat (4)	Diff./ SD (5)	N (6)
Panel A: Administrative Data						
Worked last year	0.613	0.617	-0.003	-0.12	-0.00	2,496
Employment rate last three years	0.406	0.432	0.016	1.09	0.03	2,496
Average quarterly earnings in last year	$1,552	$1,732	$85	0.71	0.02	2,496
Received TANF last year	0.101	0.155	0.030	2.16	0.05	2,496
Received SNAP last year	0.687	0.687	0.011	0.55	0.01	2,496
Panel B: Baseline Survey						
Demographics						
Average Age (years)	47.0	45.3	-0.9	-1.68	-0.04	2,451
Average years of education	13.2	14.0	0.6	7.89	0.21	2,179
Male	0.567	0.434	-0.103	-5.03	-0.13	2,496
Minority	0.397	0.389	0.027	1.37	0.03	2,495
Covered by Medicaid	0.745	0.758	0.013	0.70	0.02	2,495
Barriers to Employment						
Not allowed to drive	0.253	0.166	-0.069	-4.15	-0.11	2,480
Parent	0.243	0.371	0.098	5.10	0.13	2,486
Single parent	0.126	0.242	0.094	5.80	0.15	2,486
Difficulty finding childcare	0.071	0.121	0.037	3.01	0.08	2,485
Expect economic hardship	0.339	0.278	-0.017	-0.89	-0.02	2,456
Health limits work	0.092	0.121	0.029	2.13	0.05	2,429
Homeless (last 5 years)	0.474	0.361	-0.070	-3.56	-0.09	2,480
Ever convicted of felony	0.273	0.193	-0.053	-3.08	-0.08	2,475
Drugs or alcohol have affected life	0.257	0.185	-0.068	-3.93	-0.10	2,424
Caseworker Job Readiness Assessment						
Perceived motivation (out of 10)	8.42	8.57	0.13	1.83	0.05	2,440
Likelihood to overcome barriers (out of 10)	8.07	8.28	0.22	2.86	0.07	2,440
ReHire Target Populations						
Veteran	0.237	0.206	0.004	0.25	0.01	2,495
Non-custodial parent	0.222	0.155	-0.055	-3.46	-0.09	2,495
Older worker	0.507	0.445	-0.030	-1.44	-0.04	2,495
Not in a priority category	0.245	0.339	0.043	2.37	0.06	2,495
Cognitive skills						
Timed math test, percent correct	57.1	62.6	4.4	5.60	0.17	1,877
Number of math questions attempted (out of 160)	94.5	102.5	6.2	5.01	0.15	1,877
Raven's score (out of 36)	30.5	31.8	0.9	4.97	0.13	2,457
Non-cognitive characteristics						
Locus of control (1–5)	4.06	4.08	0.04	1.57	0.04	2,476
Grit (1–5)	3.90	3.90	0.02	0.82	0.02	2,470
Extraversion (1–5)	3.10	3.15	0.04	1.12	0.03	2,471
Agreeableness (1–5)	3.90	4.00	0.09	3.57	0.09	2,471
Conscientious (1–5)	4.01	4.00	0.01	0.48	0.01	2,471
Neuroticism (1–5)	2.45	2.46	0.00	0.10	0.00	2,471
Imagination (1–5)	3.05	3.08	0.03	1.40	0.04	2,471
Life satisfaction ladder (0–10)	5.68	5.64	-0.01	-0.12	-0.00	2,485
Depression scale (0–10)	1.55	1.51	-0.04	-0.68	-0.02	2,421

Notes: Data come from administrative UI earnings data from CDLE, administrative benefits data from CBMS, and baseline survey data collected at application. The sample includes all ReHire applicants who applied between 7/2015 and 12/2018. Respondents are individuals with a completed follow-up survey. One applicant can be linked to administrative data, but is missing a baseline survey. Treatment-control differences reported in column (3) are adjusted for stratification fixed effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally throughout enrollment.

Table A-12: Summary Statistics and Baseline Balance, Follow-up Survey Respondents

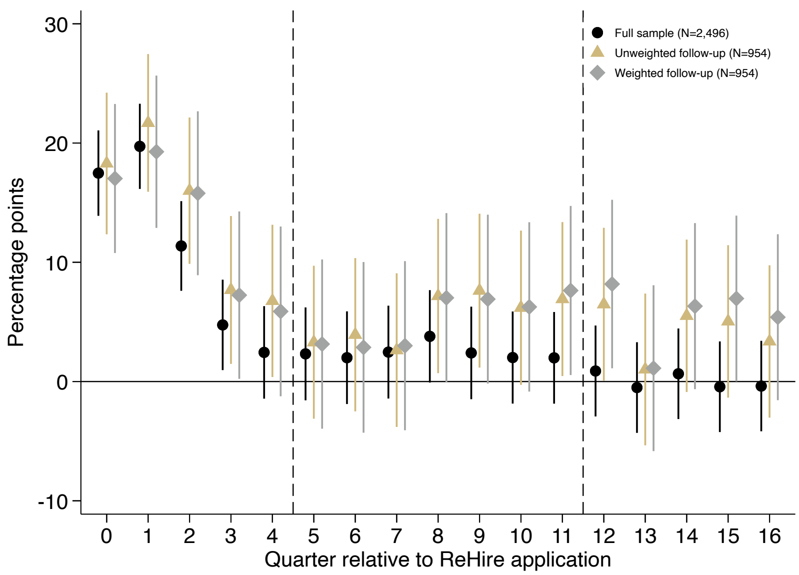
	Control Mean	Treatment Mean	Unweighted Diff.	Unweighted t-stat	Weighted Diff.	Weighted t-stat	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Administrative Data							
Worked last year	0.594	0.633	0.023	0.69	0.002	0.07	954
Employment rate last three years	0.407	0.447	0.029	1.19	0.003	0.10	954
Average quarterly earnings in last year	$1,602	$1,816	$77	0.43	$16	0.10	954
Received TANF last year	0.163	0.150	-0.016	-0.66	-0.003	-0.17	954
Received SNAP last year	0.719	0.666	-0.051	-1.67	-0.032	-0.89	954
Panel B: Baseline Survey							
Demographics							
Average Age (years)	45.4	45.3	-0.2	-0.21	-0.7	-0.76	937
Average years of education	14.1	13.9	-0.3	-2.34	-0.1	-0.63	821
Male	0.404	0.453	0.056	1.75	0.039	1.10	954
Minority	0.422	0.367	-0.045	-1.40	-0.017	-0.48	954
Covered by Medicaid	0.775	0.747	-0.030	-1.05	-0.018	-0.56	954
Barriers to Employment							
Not allowed to drive	0.153	0.174	0.025	0.98	0.011	0.34	953
Parent	0.380	0.365	-0.020	-0.62	0.016	0.50	952
Single parent	0.249	0.237	-0.016	-0.56	0.004	0.16	952
Difficulty finding childcare	0.142	0.107	-0.035	-1.57	-0.014	-0.71	951
Expect economic hardship	0.305	0.261	-0.055	-1.80	-0.074	-2.08	941
Health limits work	0.127	0.117	-0.006	-0.26	-0.001	-0.04	926
Homeless (last 5 years)	0.408	0.330	-0.061	-1.94	-0.057	-1.61	951
Ever convicted of felony	0.202	0.187	-0.017	-0.62	-0.017	-0.51	950
Drugs or alcohol have affected life	0.187	0.183	-0.005	-0.18	-0.039	-1.20	930
Caseworker Job Readiness Assessment							
Perceived motivation (out of 10)	8.60	8.54	-0.13	-1.16	-0.10	-0.86	936
Likelihood to overcome barriers (out of 10)	8.16	8.36	0.07	0.57	0.07	0.48	936
ReHire Target Populations							
Veteran	0.214	0.202	-0.003	-0.11	-0.008	-0.27	954
Non-custodial parent	0.168	0.147	-0.027	-1.09	-0.016	-0.57	954
Older worker	0.428	0.457	0.022	0.68	-0.004	-0.10	954
Not in a priority category	0.340	0.338	0.005	0.17	0.008	0.24	954
Cognitive skills							
Timed math test, percent correct	63.0	62.3	-0.6	-0.52	0.1	0.10	737
Number of math questions attempted (out of 160)	103.3	102.0	-1.1	-0.57	0.0	0.00	737
Raven's score (out of 36)	31.9	31.7	-0.2	-0.84	-0.4	-1.48	943
Non-cognitive characteristics							
Locus of control (1–5)	4.08	4.08	-0.01	-0.16	-0.01	-0.36	949
Grit (1–5)	3.90	3.90	-0.00	-0.16	-0.03	-0.96	946
Extraversion (1–5)	3.14	3.15	0.02	0.44	0.00	0.08	949
Agreeableness (1–5)	4.02	3.99	-0.02	-0.42	-0.01	-0.18	949
Conscientious (1–5)	3.98	4.01	0.03	0.74	0.00	0.10	949
Neuroticism (1–5)	2.44	2.47	0.04	0.84	0.06	1.13	949
Imagination (1–5)	3.08	3.09	0.00	0.06	0.00	0.13	949
Life satisfaction ladder (0–10)	5.62	5.65	-0.05	-0.42	-0.09	-0.65	952
Depression scale (0–10)	1.51	1.50	0.03	0.32	0.07	0.80	934

Notes: Data come from administrative UI earnings data from CDLE, administrative benefits data from CBMS, and baseline survey data collected at application. The sample includes ReHire applicants who applied between 7/2015 and 12/2018 and completed the follow-up survey. One applicant can be linked to administrative data, but is missing a baseline survey. Treatment-control differences reported in columns (3) and (5) are adjusted for stratification fixed effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally throughout enrollment.

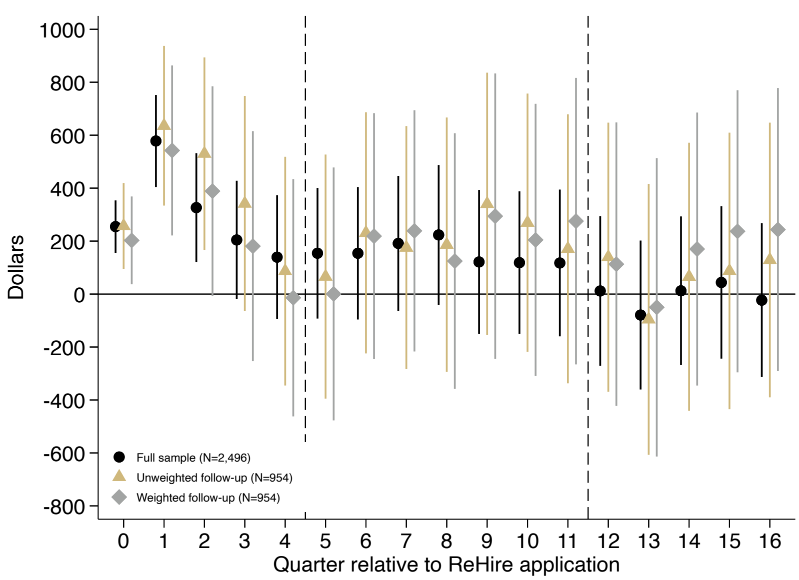
Program impacts on outcomes observed in administrative data are similar between the full sample (N=2,496) and the unweighted and weighted follow-up samples (N=954). Figure A-7 reports quarterly effects on employment (panel a) and earnings (panel b) for the full sample (black circles), unweighted follow-up sample (gold triangles), and weighted follow-up sample (gray diamonds). The figure reports coefficients from a regression of the outcome measured in the quarter relative to ReHire application (x-axis) controlling for stratification fixed effects and selected baseline controls using a post-double selection LASSO procedure (Belloni, Chernozhukov and Hansen, 2014). Vertical bars represent 95 percent confidence intervals. Table A-13 replicates the main effects from Table 2 among the full sample and the weighted follow-up sample. The pattern of results are similar between the full sample and two follow-up samples through the first two years following random assignment, albeit with less precision due to the reduction in sample size. In the later quarters, point estimates are consistently more positive among the follow-up sample.

Figure A-7: ITT Effect of ReHire on Quarterly Employment and Earnings, Comparison of Results among All ReHire Applicants and Follow-up Survey Respondents

(a) Employment



(b) Earnings



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Time 0 represents the quarter in which a participant completed an application, and is thus a different calendar period from person to person. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. Treatment and control groups are based on an individual's randomly assigned treatment status. The figure plots the treatment-control differences in average quarterly employment (a) and earnings (b), controlling for stratification fixed effects and baseline characteristics selected through a post-double selection LASSO procedure (Belloni, Chernozhukov and Hansen, 2014). Black circles represent estimates using the full sample of ReHire applicants. Gold triangles (gray diamonds) depict estimates from an unweighted (weighted) specification using all 954 follow-up survey respondents. Vertical bars represent the 95 percent confidence intervals constructed using heteroskedasticity-robust standard errors.

Table A-13: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado, Comparison of Results among All ReHire Applicants and Follow-up Survey Respondents

	All Applicants		Followup Respondents	
	Control Mean	ITT Effect Controls	Weighted Control Mean	Weighted ITT Effect Controls
	(1)	(2)	(3)	(4)
Panel A: In-Program Employment (Quarters 0–4)				
Any employment	0.805	0.116**	0.839	0.111**
		(0.014)		(0.022)
Share of quarters worked	0.533	0.112**	0.574	0.133**
		(0.013)		(0.024)
Worked every quarter	0.234	0.073**	0.270	0.105**
		(0.017)		(0.032)
Share of quarters worked at Q1 employer	0.312	0.111**	0.339	0.136**
		(0.013)		(0.023)
Average quarterly earnings	$1,755	$301**	$2,060	$253+
		(76)		(142)
Share of quarters above 130% FPL	0.183	0.025*	0.227	0.009
		(0.011)		(0.021)
Panel B: Post-Program, Pre-COVID Employment (Quarters 5–11)				
Any employment	0.660	0.016	0.704	0.045
		(0.019)		(0.032)
Share of quarters worked	0.464	0.025	0.505	0.053+
		(0.016)		(0.030)
Worked every quarter	0.242	0.038*	0.282	0.023
		(0.018)		(0.033)
Share of quarters worked at Q1 employer	0.096	0.034**	0.113	0.040+
		(0.011)		(0.021)
Average quarterly earnings	$2,243	$154	$2,592	$176
		(116)		(219)
Share of quarters above 130% FPL	0.251	0.016	0.291	0.020
		(0.014)		(0.027)
Panel C: Post-Program, Post-COVID Employment (Quarters 12–16)				
Any employment	0.523	-0.001	0.529	0.064+
		(0.020)		(0.036)
Share of quarters worked	0.394	-0.000	0.407	0.054+
		(0.017)		(0.031)
Worked every quarter	0.268	-0.013	0.270	0.037
		(0.018)		(0.032)
Share of quarters worked at Q1 employer	0.041	0.028**	0.050	0.023
		(0.009)		(0.016)
Average quarterly earnings	$2,224	-$7	$2,471	$122
		(133)		(244)
Share of quarters above 130% FPL	0.242	-0.007	0.252	0.032
		(0.015)		(0.027)
Agency-Rate Block FEs		X		X
Individual Baseline Controls		X		X
Observations	1,111	2,496	374	954

Notes: Data source is administrative UI earnings data from CDLE. This table reports effects on main employment outcomes for the full sample (columns 1 and 2) and the sample of follow-up survey respondents (columns 3 and 4). Columns (1) and (3) report the control group means. Columns (2) and (4) report the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

A.13.3 ITT Effects on Components of Follow-up Survey Outcome Indices

In Table 4, we report the average standardized treatment effect of ReHire among groups of outcomes constructed from the follow-up survey: job quality for an individual's first unsubsidized job following ReHire application; job quality for an individual's current unsubsidized job at the time of follow-up; well-being; employment barriers; workplace behaviors; and expectations about the future.

Table A-14 reports effects on the underlying components of the two job quality indices. Columns (1) through (5) includes results based on the job characteristics of an individual's first unsubsidized job following ReHire application. For these columns, the sample is restricted to the 637 follow-up survey respondents who report working an unsubsidized job since they applied to ReHire. Columns (6) through (10) report results based on the job characteristics of an individual's job they were working at the time of survey response. For these columns, the sample is restricted to the 472 indiviuals working at the time of follow-up. Each row in the table represents a different characteristic. The table reports control group means (columns 1 and 6), ITT effects from a regression that controls for stratification fixed effects (columns 2 and 7), estimates from a weighted sample using inverse propensity attrition weights (columns 3 and 8), and estimates that selects baseline controls using a post-double selection LASSO procedure (columns 4 and 9). Columns (5) and (10) report sample sizes for each outcome. For a few respondents, we were unable to construct an estimate of their hourly wage. Results are relatively stable across specifications.

The final row of Table A-14 presents the standardized treatment effect found in Table 4. In constructing the standardized treatment effect, estimates from some of underlying components (worked for hourly wage; would like to work more hours; work hours change a lot or fair amount) are re-signed so that an increase in the outcome represents an improvement in job quality.

Similarly, Table A-15 reports effects on the components of the remaining outcome indices measured in the following up survey: well-being (Panel A); employment barriers (Panel B); workplace behaviors (Panel C); and expectations about the future (Panel D). This table reports the same specifications as Table A-14. In Panels A and B, the set of covariates used to select controls in the post-double selection LASSO procedure includes measures of the outcomes observed at ReHire application. For outcomes in Panels C and D, the respondent was asked the extent to which they agreed or disagreed with the given statement (strongly disagree, disagree, neither agree nor disagree, agree, and strongly agree). We construct indicators for whether an individual responded that they agree or strongly agree with the statement.

The final row of each panel in Table A-15 reports the standardized treatment effect found in Table 4. In constructing this index, some of the outcomes (expect hardship in next 2 months; depression score; and all 5 employment barriers) are re-signed so that increases in the outcomes represent improvements.

Table A-14: ITT Effect of ReHire on Components of the Job Quality Index, Follow-Up Survey Respondents

	First Unsubsidized Post-Application Job					Job at Time of Survey				
	Control Mean	Unweighted ITT Effect No Controls	Weighted ITT Effect No Controls	Weighted ITT Effect Controls	N	Control Mean	Unweighted ITT Effect No Controls	Weighted ITT Effect No Controls	Weighted ITT Effect Controls	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Very satisfied with job	0.186	0.155**	0.155**	0.155**	773	0.419	0.056	0.090+	0.090+	570
		(0.033)	(0.036)	(0.035)			(0.045)	(0.048)	(0.047)	
Worked for hourly wage	0.914	-0.040+	-0.052*	-0.052*	773	0.857	-0.015	-0.025	-0.025	570
		(0.023)	(0.024)	(0.024)			(0.033)	(0.033)	(0.032)	
Hourly wage	$13.16	0.53	1.04	1.04+	757	$15.11	-0.33	0.24	0.24	556
		(0.54)	(0.63)	(0.62)			(0.84)	(0.77)	(0.75)	
Non-temporary employee	0.717	0.015	0.011	0.011	773	0.837	-0.001	-0.029	-0.029	570
		(0.034)	(0.037)	(0.036)			(0.035)	(0.035)	(0.034)	
Hours worked per week	30.0	0.7	0.4	0.4	773	31.9	-0.5	-0.9	-0.9	570
		(1.0)	(1.1)	(1.1)			(1.2)	(1.2)	(1.2)	
Would like to work more hours	0.666	-0.061+	-0.053	-0.053	773	0.665	-0.051	-0.014	-0.014	570
		(0.036)	(0.039)	(0.038)			(0.044)	(0.048)	(0.046)	
Work hours change a lot or fair amount	0.314	-0.047	-0.040	-0.040	773	0.305	-0.059	-0.050	-0.050	570
		(0.035)	(0.040)	(0.039)			(0.042)	(0.045)	(0.043)	
One-way commute time (minutes)	26.8	-1.3	-1.1	-1.1	773	26.1	-3.3	-3.5	-3.5	570
		(1.8)	(2.0)	(1.9)			(2.1)	(2.4)	(2.3)	
Any employer benefits	0.317	0.074*	0.045	0.045	773	0.448	0.048	0.032	0.032	570
		(0.036)	(0.039)	(0.038)			(0.045)	(0.047)	(0.046)	
Employer-provided health insurance	0.197	0.050	0.022	0.022	773	0.305	-0.006	-0.029	-0.029	570
		(0.031)	(0.034)	(0.033)			(0.042)	(0.045)	(0.043)	
Employer contributes to retirement	0.128	0.068*	0.049	0.049+	773	0.222	0.055	0.034	0.034	570
		(0.028)	(0.030)	(0.029)			(0.039)	(0.041)	(0.040)	
Paid vacation days	0.238	0.083*	0.056	0.056	773	0.394	0.035	0.021	0.021	570
		(0.034)	(0.037)	(0.037)			(0.044)	(0.047)	(0.045)	
Paid sick leave	0.169	0.083**	0.059+	0.059+	773	0.256	0.088*	0.064	0.064	570
		(0.031)	(0.034)	(0.033)			(0.041)	(0.044)	(0.042)	
Standardized treatment effect	0.000	0.147**	0.130**	0.130**	773	0.000	0.072	0.053	0.053	570
		(0.039)	(0.043)	(0.042)			(0.046)	(0.048)	(0.047)	

Notes: Data source is an 18-month follow-up survey. The sample includes follow-up survey respondents who applied between 7/2015 and 12/2018. The first four columns report information on the first unsubsidized job worked following ReHire application. The last four columns report information on the job worked at the time of the survey. The dependent variables, given by row labels, are job characteristics, and the sample is limited to respondents who worked in the listed job. The final row reports the standardized treatment effect across all characteristics, which is measured in standard deviations. Columns (1) and (6) report control group means. Columns (2) and (7) report ITT effect estimates controlling for service agency-randomization rate block fixed effects and months since application fixed effects. Columns (3) and (8) reweights the sample using inverse propensity attrition weights. Columns (4) and (9) further select controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Columns (5) and (10) report sample sizes. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

Table A-15: ITT Effect of ReHire on Other Outcomes, Follow-up Survey Respondents

	Control Mean (1)	Unweighted ITT Effect No Controls (2)	Weighted ITT Effect No Controls (3)	Weighted ITT Effect Controls (4)	N (5)
Panel A: Components of Well-Being Index					
Subjective Well-Being					
Life satisfaction ladder (0–10)	5.158	0.600**	0.567**	0.597**	954
		(0.155)	(0.171)	(0.162)	
Expect hardship in next 2 months	0.297	-0.062*	-0.078*	-0.064*	954
		(0.030)	(0.035)	(0.032)	
Self-Reported Physical and Mental Health					
Very good or excellent health	0.294	0.069*	0.084*	0.079*	954
		(0.032)	(0.033)	(0.031)	
Health improved over last year	0.307	0.053+	0.063+	0.070*	954
		(0.032)	(0.034)	(0.033)	
Depression score	2.254	-0.238*	-0.179	-0.187+	954
		(0.112)	(0.121)	(0.107)	
Standardized treatment effect	0.000	0.163**	0.169**	0.168**	954
		(0.044)	(0.047)	(0.043)	
Panel B: Components of Employment Barriers Index					
Lack of childcare affected work	0.182	-0.022	0.000	0.009	954
		(0.026)	(0.025)	(0.021)	
Homeless	0.307	-0.072*	-0.068*	-0.037	954
		(0.030)	(0.034)	(0.030)	
Convicted of crime	0.043	0.014	0.027+	0.027+	954
		(0.015)	(0.016)	(0.015)	
Incarcerated	0.024	0.005	0.008	0.008	954
		(0.011)	(0.012)	(0.012)	
Substance abuse affected work	0.059	-0.002	-0.005	-0.005	954
		(0.016)	(0.019)	(0.019)	
Standardized treatment effect	0.000	0.024	-0.006	-0.025	954
		(0.041)	(0.046)	(0.043)	
Panel C: Components of Workplace Behaviors Index					
Ask about opportunities	0.773	-0.039	-0.038	-0.038	954
		(0.029)	(0.032)	(0.031)	
Speak out in group setting	0.671	0.012	0.039	0.039	954
		(0.032)	(0.037)	(0.036)	
Positive attitude about self	0.778	0.030	0.003	0.003	954
		(0.027)	(0.028)	(0.027)	
Confident in own abilities	0.837	0.043+	0.032	0.032	954
		(0.024)	(0.024)	(0.024)	
Don't worry about what others think about me	0.532	0.054	0.044	0.044	954
		(0.034)	(0.036)	(0.035)	
Standardized treatment effect	0.000	0.046	0.035	0.035	954
		(0.039)	(0.042)	(0.041)	
Panel D: Components of Expectations About Future Index					
Expect to work	0.802	0.028	0.032	0.032	954
		(0.027)	(0.031)	(0.030)	
Expect to not need government assistance	0.604	0.025	-0.005	-0.005	954
		(0.034)	(0.036)	(0.035)	
Standardized treatment effect	0.000	0.060	0.034	0.034	954
		(0.055)	(0.060)	(0.059)	

Notes: Data source is an 18-month follow-up survey. The sample includes follow-up survey respondents who applied between 7/2015 and 12/2018. Columns (2) reports ITT effect estimates controlling for service agency-randomization rate block fixed effects and months since application fixed effects. Column (3) reweights the sample using inverse propensity attrition weights. Column (4) further selects controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (5) reports sample sizes. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

A.13.4 Bounding ITT Effects on Follow-up Survey Outcomes to Deal with Attrition Bias

In Section A.13, we documented improvements in job quality and overall well-being, outcomes that were measured in the 18-month follow-up survey. As noted in Section A.13.1, response rates varied between the treatment group (41.9 percent) and the control group (33.7 percent), which raises concerns of selective attrition biasing these results. To better understand the robustness of these findings, this section reports bounds on the ITT effects estimated on follow-up survey outcomes following Lee (2009) and Kling and Liebman (2004).

The treatment group was more likely to respond to the follow-up survey. In order to implement Lee (2009) bounds, we trim outcomes among the treatment group until the response rate in the treatment group equals that of the control group. Lower bounds are constructed by trimming the treatment group individuals with the largest outcome values. Upper bounds are constructed by trimming the treatment group individuals with the smallest outcome values. We break ties using the estimated propensity to respond to the survey and trimming individuals who were least likely to respond. When constructing lower (upper) bounds for the average standardized treatment effect, we use the upper (lower) bound for outcomes that are negatively signed in the index. We estimate the Lee bounds using Equation (1) controlling only for stratification fixed effects and by weighting the sample using inverse propensity weights.

We also report bounds following Kling and Liebman (2004) that assume attritors have outcome values that are one standard deviation away from the mean outcome. Lower bounds are constructed by assuming the treatment (control) group attritors have outcomes one standard deviation below (above) than the mean. Upper bounds are constructed by assuming the treatment (control) group attritors have outcomes one standard deviation above (below) the mean. We estimate the Kling-Liebman bounds using Equation (1) controlling only for stratification fixed effects.

Table A-16 and Table A-17 report the ITT effects from a weighted regression that controls for stratification fixed effects and months since application, as well as the upper and lower bound estimates using the procedures described above. Bounds on the effects on the job quality index are wide, ranging from roughly -0.3 SD to 0.6 SD (Table A-16). Similarly, bounds on the effect on well-being are wide, ranging from -0.2 SD to 0.6 SD (Table A-17).

Table A-16: Bounds on the ITT Effect of ReHire on Components of the Job Quality Index, Follow-Up Survey Respondents

	First Unsubsidized Post-Application Job					Job at Time of Survey				
	Weighted	Lee		Kling-Liebman		Weighted	Lee		Kling-Liebman	
	ITT Effect	Lower	Upper	Lower	Upper	ITT Effect	Lower	Upper	Lower	Upper
	No Controls	Bound	Bound	Bound	Bound	No Controls	Bound	Bound	Bound	Bound
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Very satisfied with job	0.155**	-0.107**	0.334**	-0.438**	0.763**	0.090+	-0.197**	0.388**	-0.683**	0.861**
	(0.036)	(0.031)	(0.040)	(0.013)	(0.013)	(0.048)	(0.046)	(0.046)	(0.013)	(0.013)
Worked for hourly wage	-0.052*	-0.120**	0.076**	-0.462**	0.382**	-0.025	-0.130**	0.134**	-0.569**	0.534**
	(0.024)	(0.030)	(0.016)	(0.009)	(0.009)	(0.033)	(0.043)	(0.025)	(0.009)	(0.009)
Hourly wage	1.04	-2.00**	2.62**	-8.52**	9.26**	0.24	-2.96**	1.97*	-13.95**	13.32**
	(0.63)	(0.33)	(0.75)	(0.19)	(0.19)	(0.77)	(0.53)	(0.94)	(0.23)	(0.23)
Non-temporary employee	0.011	-0.137**	0.275**	-0.605**	0.637**	-0.029	-0.137**	0.148**	-0.558**	0.584**
	(0.037)	(0.041)	(0.029)	(0.013)	(0.013)	(0.035)	(0.043)	(0.026)	(0.009)	(0.010)
Hours worked per week	0.4	-5.1**	6.9**	-18.1**	19.9**	-0.9	-7.1**	6.3**	-21.8**	21.4**
	(1.1)	(1.1)	(1.0)	(0.4)	(0.4)	(1.2)	(1.2)	(1.1)	(0.4)	(0.4)
Would like to work more hours	-0.053	-0.252**	0.183**	-0.727**	0.613**	-0.014	-0.276**	0.278**	-0.816**	0.679**
	(0.039)	(0.042)	(0.034)	(0.014)	(0.014)	(0.048)	(0.051)	(0.041)	(0.013)	(0.012)
Work hours change a lot or fair amount	-0.040	-0.309**	0.106*	-0.677**	0.591**	-0.050	-0.300**	0.130*	-0.744**	0.641**
	(0.040)	(0.033)	(0.043)	(0.014)	(0.014)	(0.045)	(0.035)	(0.051)	(0.012)	(0.012)
One-way commute time (minutes)	-1.1	-11.1**	5.1*	-36.2**	31.1**	-3.5	-14.4**	3.4	-38.5**	30.2**
	(2.0)	(1.5)	(2.1)	(0.7)	(0.7)	(2.4)	(1.9)	(2.6)	(0.6)	(0.6)
Any employer benefits	0.045	-0.185**	0.249**	-0.582**	0.741**	0.032	-0.230**	0.343**	-0.717**	0.834**
	(0.039)	(0.034)	(0.042)	(0.014)	(0.014)	(0.047)	(0.046)	(0.047)	(0.013)	(0.013)
Employer-provided health insurance	0.022	-0.196**	0.151**	-0.515**	0.625**	-0.029	-0.318**	0.180**	-0.696**	0.735**
	(0.034)	(0.027)	(0.040)	(0.012)	(0.012)	(0.045)	(0.036)	(0.052)	(0.012)	(0.012)
Employer contributes to retirement	0.049	-0.122**	0.145**	-0.432**	0.571**	0.034	-0.226**	0.224**	-0.599**	0.728**
	(0.030)	(0.023)	(0.036)	(0.011)	(0.011)	(0.041)	(0.032)	(0.048)	(0.011)	(0.011)
Paid vacation days	0.056	-0.187**	0.230**	-0.533**	0.707**	0.021	-0.264**	0.296**	-0.719**	0.809**
	(0.037)	(0.031)	(0.043)	(0.013)	(0.013)	(0.047)	(0.043)	(0.049)	(0.013)	(0.013)
Paid sick leave	0.059+	-0.172**	0.193**	-0.475**	0.643**	0.064	-0.220**	0.295**	-0.610**	0.808**
	(0.034)	(0.027)	(0.041)	(0.012)	(0.012)	(0.044)	(0.036)	(0.051)	(0.012)	(0.012)
Standardized treatment effect	0.130**	-0.356	0.538**	-1.316	1.624**	0.053	-0.452	0.543**	-1.491	1.686**
	(0.043)	(0.037)	(0.048)	(0.024)	(0.023)	(0.048)	(0.044)	(0.050)	(0.020)	(0.020)

Notes: Data source is an 18-month follow-up survey. The sample includes follow-up survey respondents who applied between 7/2015 and 12/2018. The first five columns report information on the first unsubsidized job worked following ReHire application. The last five columns report information on the job worked at the time of the survey. The dependent variables, given by row labels, are job characteristics. The final row reports the standardized treatment effect across all characteristics, which is measured in standard deviations. Columns (1) and (6) report ITT effect estimates controlling for service agency-randomization rate block fixed effects and months since application fixed effects and weighting the sample using inverse propensity attrition weights. Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

Table A-17: Bounds on the ITT Effect of ReHire on Other Outcomes, Follow-up Survey Respondents

	Weighted ITT Effect No Controls	Lee Lower Bound	Lee Upper Bound	Kling-Liebman Lower Bound	Kling-Liebman Upper Bound
	(1)	(2)	(3)	(4)	(5)
Panel A: Components of Well-Being Index					
Subjective Well-Being					
Life satisfaction ladder (0–10)	0.567**	-0.188	1.467**	-2.206**	3.459**
	(0.171)	(0.168)	(0.156)	(0.073)	(0.073)
Expect hardship in next 2 months	-0.078*	-0.292**	0.038	-0.614**	0.486**
	(0.035)	(0.029)	(0.037)	(0.014)	(0.014)
Self-Reported Physical and Mental Health					
Very good or excellent health	0.084*	-0.108**	0.228**	-0.508**	0.670**
	(0.033)	(0.030)	(0.036)	(0.015)	(0.015)
Health improved over last year	0.063+	-0.132**	0.208**	-0.536**	0.637**
	(0.034)	(0.030)	(0.036)	(0.015)	(0.015)
Depression score	-0.179	-0.835**	0.240+	-2.348**	1.863**
	(0.121)	(0.108)	(0.124)	(0.053)	(0.053)
Standardized treatment effect	0.169**	-0.168	0.545**	-1.084	1.422**
	(0.047)	(0.045)	(0.044)	(0.026)	(0.026)
Panel B: Components of Employment Barriers Index					
Lack of childcare affected work	0.000	-0.149**	0.059*	-0.491**	0.463**
	(0.025)	(0.019)	(0.029)	(0.012)	(0.012)
Homeless	-0.068*	-0.305**	0.025	-0.631**	0.468**
	(0.034)	(0.028)	(0.037)	(0.014)	(0.014)
Convicted of crime	0.027+	-0.037**	0.055**	-0.262**	0.288**
	(0.016)	(0.010)	(0.020)	(0.007)	(0.007)
Incarcerated	0.008	-0.026**	0.023	-0.204**	0.214**
	(0.012)	(0.009)	(0.016)	(0.005)	(0.005)
Substance abuse affected work	-0.005	-0.063**	0.023	-0.293**	0.283**
	(0.019)	(0.014)	(0.022)	(0.008)	(0.007)
Standardized treatment effect	-0.006	-0.151	0.338**	-1.271	1.324**
	(0.046)	(0.058)	(0.035)	(0.026)	(0.026)
Panel C: Components of Workplace Behaviors Index					
Ask about opportunities	-0.038	-0.159**	0.173**	-0.576**	0.505**
	(0.032)	(0.035)	(0.026)	(0.014)	(0.014)
Speak out in group setting	0.039	-0.096*	0.237**	-0.584**	0.588**
	(0.037)	(0.039)	(0.033)	(0.015)	(0.015)
Positive attitude about self	0.003	-0.073*	0.193**	-0.486**	0.533**
	(0.028)	(0.032)	(0.021)	(0.013)	(0.013)
Confident in own abilities	0.032	-0.017	0.153**	-0.407**	0.475**
	(0.024)	(0.029)	(0.020)	(0.011)	(0.011)
Don't worry about what others think about me	0.044	-0.111**	0.244**	-0.565**	0.675**
	(0.036)	(0.037)	(0.033)	(0.016)	(0.016)
Standardized treatment effect	0.035	-0.208	0.466**	-1.229	1.303**
	(0.042)	(0.050)	(0.036)	(0.026)	(0.026)
Panel D: Components of Expectations About Future Index					
Expect to work	0.032	-0.044	0.213**	-0.461**	0.512**
	(0.031)	(0.035)	(0.025)	(0.013)	(0.013)
Expect to not need government assistance	-0.005	-0.155**	0.208**	-0.577**	0.642**
	(0.036)	(0.038)	(0.033)	(0.016)	(0.016)
Standardized treatment effect	0.034	-0.213	0.473**	-1.153	1.283**
	(0.060)	(0.066)	(0.051)	(0.028)	(0.028)

Notes: Data source is an 18-month follow-up survey. The sample includes follow-up survey respondents who applied between 7/2015 and 12/2018. Columns (1) reports ITT effect estimates controlling for service agency-randomization rate block fixed effects and months since application fixed effects and weighting the sample using inverse propensity attrition weights.
**0.01, *0.05, +0.10 significance levels

A.14 Additional Credit Outcomes Details

A.14.1 Experian Match Rates and Reweighting for Selective Matching

Among the 2,496 applicants in our analysis sample, 1,556 individuals matched to a balanced panel of Experian records during the 5 quarters before and 14 quarters after random assignment. Match rates are similar between the treatment group (61.9 percent) and the control group (62.8 percent). Matching to credit data occurred using name, date of birth, address, and social security number, all of which were collected using identification cards at the time of application. As a result, failure to match is likely due to the study participant lacking a credit history with Experian. Table A-18 reports average baseline characteristics of those who did not match to the Experian data panel (column 1) and those who did match to the Experian data panel (column 2). In general, applicants matched to the Experian data were more likely to have worked in the time leading up to application, earned more money, and had more years of education. Matched individuals were less likely to be male, be covered by Medicaid, and faced more employment barriers such as lack of transportation, prior involvement with the criminal justice system, and experience with homelessness.

To account for selective matching to the Experian data, we construct a set of inverse propensity weights to use in our analysis of outcomes from this data source. These weights are constructed analogously to the weights used for outcomes from the follow-up survey. See Section A.13 for details.

Among Experian-matched applicants, baseline characteristics are largely balanced between the control group and treatment group. Table A-19 reports average baseline characteristics of Experian-matched applicants in the control group (column 1) and the treatment group (column 2). Columns (3) and (5) report the unweighted and weighted differences in means, respectively. Corresponding test statistics are reported in columns (4) and (6). In the unweighted sample, the treatment group is slightly younger and less likely to be covered by Medicaid. This pattern is similar in the weighted sample.

Program impacts on outcomes observed in administrative data are similar between the full sample (N=2,496) and the weighted credit sample (N=1,556). Table A-20 replicates the main effects from Table 2 among the full sample and the weighted credit sample. The pattern of in-program results are similar between the full sample and the weighted matched sample. Effects in the post-program periods are smaller and all estimates are less precise due to the reduction in sample size.

Table A-18: Experian Match Selection

	Non-Match Mean	Match Mean	Within-Strata Difference	t-stat	Diff./SD	N
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Administrative Data						
Worked last year	0.551	0.655	0.102	4.91	0.16	2,496
Employment rate last three years	0.341	0.463	0.112	7.64	0.24	2,496
Average quarterly earnings in last year	$1,049	$1,988	$845	8.20	0.26	2,496
Received TANF last year	0.097	0.137	0.015	1.12	0.04	2,496
Received SNAP last year	0.724	0.663	-0.049	-2.58	-0.08	2,496
Panel B: Baseline Survey						
Demographics						
Average Age (years)	46.2	46.5	1.0	2.05	0.07	2,451
Average years of education	13.0	13.9	0.8	9.63	0.33	2,179
Male	0.611	0.455	-0.111	-5.41	-0.17	2,496
Minority	0.406	0.387	0.032	1.61	0.05	2,495
Covered by Medicaid	0.794	0.722	-0.080	-4.45	-0.14	2,495
Barriers to Employment						
Not allowed to drive	0.312	0.161	-0.134	-7.31	-0.23	2,480
Parent	0.198	0.353	0.122	6.69	0.21	2,486
Single parent	0.106	0.211	0.078	5.28	0.17	2,486
Difficulty finding childcare	0.045	0.119	0.058	5.53	0.18	2,485
Expect economic hardship	0.357	0.290	-0.029	-1.49	-0.05	2,456
Health limits work	0.097	0.108	0.011	0.85	0.03	2,429
Ever homeless	0.596	0.325	-0.215	-10.71	-0.34	2,480
Ever convicted of felony	0.339	0.181	-0.121	-6.47	-0.21	2,475
Drugs or alcohol have affected life	0.280	0.196	-0.075	-4.06	-0.13	2,424
Caseworker Job Readiness Assessment						
Perceived motivation (out of 10)	8.47	8.48	0.03	0.37	0.01	2,440
Likelihood to overcome barriers (out of 10)	8.04	8.22	0.22	2.82	0.09	2,440
ReHire Target Populations						
Veteran	0.249	0.210	0.007	0.39	0.01	2,495
Non-custodial parent	0.249	0.162	-0.075	-4.34	-0.14	2,495
Older worker	0.498	0.474	0.001	0.04	0.00	2,495
Not in a priority category	0.240	0.306	0.015	0.87	0.03	2,495
Cognitive skills						
Timed math test, percent correct	56.1	61.1	2.8	3.48	0.13	1,877
Number of math questions attempted (out of 160)	93.1	100.4	3.9	2.99	0.11	1,877
Raven's score (out of 36)	30.5	31.2	0.3	1.46	0.05	2,457
Non-cognitive characteristics						
Locus of control (1–5)	4.06	4.08	0.04	1.74	0.06	2,476
Grit (1–5)	3.87	3.92	0.07	3.68	0.12	2,470
Extraversion (1–5)	3.06	3.15	0.06	1.95	0.06	2,471
Agreeableness (1–5)	3.88	3.98	0.07	3.10	0.10	2,471
Conscientious (1–5)	4.02	3.99	0.01	0.27	0.01	2,471
Neuroticism (1–5)	2.45	2.45	-0.03	-1.09	-0.04	2,471
Imagination (1–5)	3.06	3.07	-0.00	-0.14	-0.00	2,471
Life satisfaction ladder (0–10)	5.61	5.70	0.10	1.19	0.04	2,485
Depression scale (0–10)	1.54	1.53	0.00	0.02	0.00	2,421

Notes: Data come from administrative UI earnings data from CDLE, administrative benefits data from CBMS, and baseline survey data collected at application. The sample includes all ReHire applicants who applied between 7/2015 and 12/2018. Match denotes an individual who matched to a credit record in each of the 5 quarters before and 14 quarters following random assignment. One applicant can be linked to administrative data, but is missing a baseline survey. Treatment-control differences reported in column (3) are adjusted for stratification fixed effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally throughout enrollment.

Table A-19: Summary Statistics and Baseline Balance, Experian Sample

	Control Mean	Treatment Mean	Unweighted Diff.	Unweighted t-stat	Weighted Diff.	Weighted t-stat	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Administrative Data							
Worked last year	0.647	0.662	0.012	0.46	0.030	1.09	1,521
Employment rate last three years	0.450	0.474	0.017	0.89	0.015	0.78	1,521
Average quarterly earnings in last year	$1,846	$2,100	$156	0.93	$119	0.88	1,521
Received TANF last year	0.145	0.131	-0.007	-0.41	-0.004	-0.23	1,521
Received SNAP last year	0.672	0.655	-0.016	-0.65	-0.010	-0.38	1,521
Panel B: Baseline Survey							
Demographics							
Average Age (years)	47.1	46.0	-1.3	-2.11	-1.6	-2.39	1,492
Average years of education	13.9	13.8	-0.1	-0.73	-0.1	-1.03	1,303
Male	0.439	0.468	0.025	1.00	0.036	1.34	1,521
Minority	0.412	0.366	-0.042	-1.73	-0.037	-1.42	1,521
Covered by Medicaid	0.743	0.705	-0.040	-1.74	-0.037	-1.56	1,521
Barriers to Employment							
Not allowed to drive	0.149	0.170	0.025	1.35	0.041	1.89	1,512
Parent	0.360	0.347	-0.007	-0.30	0.001	0.03	1,514
Single parent	0.211	0.211	0.007	0.31	0.010	0.49	1,514
Difficulty finding childcare	0.130	0.110	-0.016	-0.96	-0.010	-0.62	1,513
Expect economic hardship	0.298	0.283	-0.026	-1.09	-0.032	-1.25	1,501
Health limits work	0.109	0.106	-0.002	-0.15	-0.008	-0.48	1,479
Ever homeless	0.346	0.308	-0.035	-1.48	-0.037	-1.43	1,512
Ever convicted of felony	0.196	0.169	-0.022	-1.10	-0.004	-0.15	1,509
Drugs or alcohol have affected life	0.198	0.195	-0.001	-0.04	0.003	0.12	1,472
Caseworker Job Readiness Assessment							
Perceived motivation (out of 10)	8.51	8.46	-0.05	-0.58	-0.07	-0.70	1,486
Likelihood to overcome barriers (out of 10)	8.20	8.24	-0.02	-0.17	-0.03	-0.25	1,486
ReHire Target Populations							
Veteran	0.217	0.204	-0.008	-0.42	-0.011	-0.47	1,521
Non-custodial parent	0.162	0.163	0.001	0.05	0.017	0.77	1,521
Older worker	0.482	0.468	-0.022	-0.85	-0.040	-1.51	1,521
Not in a priority category	0.300	0.312	0.020	0.90	0.020	0.86	1,521
Cognitive skills							
Timed math test, percent correct	61.7	60.7	-0.8	-0.89	-0.7	-0.75	1,168
Number of math questions attempted (out of 160)	101.3	99.7	-1.4	-0.95	-1.2	-0.78	1,168
Raven's score (out of 36)	31.1	31.4	0.4	1.64	0.3	1.22	1,500
Non-cognitive characteristics							
Locus of control (1–5)	4.09	4.06	-0.02	-0.86	-0.02	-0.64	1,508
Grit (1–5)	3.92	3.91	-0.01	-0.32	-0.00	-0.12	1,504
Extraversion (1–5)	3.14	3.15	0.02	0.50	0.03	0.64	1,505
Agreeableness (1–5)	3.98	3.97	0.01	0.37	0.01	0.20	1,505
Conscientious (1–5)	4.01	3.98	-0.02	-0.61	-0.01	-0.26	1,505
Neuroticism (1–5)	2.42	2.48	0.06	1.71	0.06	1.55	1,505
Imagination (1–5)	3.06	3.07	0.01	0.36	0.01	0.53	1,505
Life satisfaction ladder (0–10)	5.74	5.66	-0.09	-0.85	-0.11	-0.94	1,514
Depression scale (0–10)	1.48	1.57	0.09	1.29	0.10	1.28	1,474

Notes: Data come from administrative UI earnings data from CDLE, administrative benefits data from CBMS, and baseline survey data collected at application. The sample includes ReHire applicants who applied between 7/2015 and 12/2018 and linked to a credit record during each of the 5 quarters before application and 14 quarters following application. One applicant can be linked to administrative data, but is missing a baseline survey. Treatment-control differences reported in columns (3) and (5) are adjusted for stratification fixed effects to account for the fact that randomization occurred separately by local agency and that the treatment probability changed occasionally throughout enrollment.

Table A-20: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado, Comparison of Results among All ReHire Applicants and Credit Outcome Sample

	All Applicants		Experian Sample	
	Control Mean	ITT Effect Controls	Weighted Control Mean	Weighted ITT Effect Controls
	(1)	(2)	(3)	(4)
Panel A: In-Program Employment (Quarters 0–4)				
Any employment	0.805	0.116**	0.814	0.111**
		(0.014)		(0.018)
Share of quarters worked	0.533	0.112**	0.548	0.108**
		(0.013)		(0.018)
Worked every quarter	0.234	0.073**	0.233	0.079**
		(0.017)		(0.023)
Share of quarters worked at Q1 employer	0.312	0.111**	0.326	0.098**
		(0.013)		(0.017)
Average quarterly earnings	$1,755	$301**	$1,896	$248*
		(76)		(101)
Share of quarters above 130% FPL	0.183	0.025*	0.202	0.018
		(0.011)		(0.015)
Panel B: Post-Program, Pre-COVID Employment (Quarters 5–11)				
Any employment	0.660	0.016	0.678	-0.009
		(0.019)		(0.026)
Share of quarters worked	0.464	0.025	0.484	0.011
		(0.016)		(0.022)
Worked every quarter	0.242	0.038*	0.266	0.020
		(0.018)		(0.024)
Share of quarters worked at Q1 employer	0.096	0.034**	0.111	0.018
		(0.011)		(0.015)
Average quarterly earnings	$2,243	$154	$2,464	$28
		(116)		(162)
Share of quarters above 130% FPL	0.251	0.016	0.278	-0.004
		(0.014)		(0.019)
Panel C: Post-Program, Post-COVID Employment (Quarters 12–16)				
Any employment	0.523	-0.001	0.542	-0.001
		(0.020)		(0.027)
Share of quarters worked	0.394	-0.000	0.413	-0.003
		(0.017)		(0.023)
Worked every quarter	0.268	-0.013	0.280	-0.022
		(0.018)		(0.023)
Share of quarters worked at Q1 employer	0.041	0.028**	0.045	0.019+
		(0.009)		(0.011)
Average quarterly earnings	$2,224	-$7	$2,392	-$95
		(133)		(176)
Share of quarters above 130% FPL	0.242	-0.007	0.254	-0.014
		(0.015)		(0.020)
Agency-Rate Block FEs		X		X
Individual Baseline Controls		X		X
Observations	1,111	2,496	674	1,521

Notes: Data source is administrative UI earnings data from CDLE. This table reports ITT effects on primary employment outcomes for the full sample (columns 1 and 2) and the sample of ReHire applicants linked to the credit outcome panel (columns 3 and 4). Columns (1) and (3) report the control group means. Columns (2) and (4) report the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Heteroskedasticity-robust standard errors in parentheses.
**0.01, *0.05, +0.10 significance levels

A.14.2 Additional Credit Outcomes Results

In Panel C of Table 4, we report the average standardized treatment effect of ReHire on credit outcomes measured during the in-program period (Q0–Q4), post-program pre-COVID period (Q5–Q11), and post-program post-COVID period (Q12–Q14).

Table A-21 reports effects on the underlying credit outcomes during those two periods. The sample is restricted to the 1,556 ReHire applicants who match to a credit record for each of the 5 quarters preceeding application through the 14 quarters following application. Each row in the table represents a different outcome averaged over the in-program period (Panel A), post-program pre-COVID period (Panel B), and post-program post-COVID period (Panel C). The table reports control group means (column 1), ITT effects from a regression that controls for stratification fixed effects (column 2), estimates from the weighted sample using inverse propensity attrition weights (column 3), and estimates that selects baseline controls using a post-double selection LASSO procedure (column 4). Column (5) reports sample sizes for each outcome.

The final row of each panel in Table A-21 presents the standardized treatment effect found in Table 4. In constructing the standardized treatment effect, estimates from some of underlying outcomes (total debt, credit card debt, any delinquent accounts, any derogatory accounts, and any accounts in collections) are re-signed such that an increase in the outcome represents an improvement in credit outcomes.

Table A-21: ITT Effect of ReHire on Credit Outcomes, Experian Sample

	Control Mean	Unweighted ITT Effect No Controls	Weighted ITT Effect No Controls	Weighted ITT Effect Controls	N
	(1)	(2)	(3)	(4)	(5)
Panel A: In-Program Credit Outcomes (Q0–Q4)					
Credit score	593.82	0.73	-0.79	-0.96	1,521
		(4.77)	(4.59)	(1.95)	
Total debt	\$32,252	-\$5,842^{+}	-\$5,432^{+}	-\$790	1,521
		(3,277)	(2,883)	(1,354)	
Credit card debt	\$1,712	-\$531*	-\$403^{+}	-\$6	1,521
		(239)	(210)	(101)	
Has auto loan or lease	0.169	0.021	0.020	0.020	1,521
		(0.019)	(0.018)	(0.018)	
Any delinquent accounts	0.150	0.010	0.018	0.018	1,521
		(0.014)	(0.015)	(0.015)	
Any derogatory accounts	0.350	-0.006	0.007	0.007	1,521
		(0.019)	(0.020)	(0.020)	
Any accounts in collections	0.619	-0.001	0.005	0.005	1,521
		(0.023)	(0.024)	(0.023)	
Standardized treatment effect	0.000	0.034	0.018	-0.005	1,521
		(0.023)	(0.023)	(0.020)	
Panel B: Post-Program Pre-COVID Credit Outcomes (Q5–Q11)					
Credit score	603.67	2.30	0.51	0.84	1,521
		(4.69)	(4.52)	(2.69)	
Total debt	\$34,158	-\$4,012	-\$4,727	-\$636	1,521
		(3,329)	(3,035)	(2,029)	
Credit card debt	\$1,556	-\$299	-\$258	\$4	1,521
		(210)	(184)	(133)	
Has auto loan or lease	0.187	0.000	-0.006	-0.006	1,521
		(0.018)	(0.018)	(0.018)	
Any delinquent accounts	0.122	-0.013	-0.008	-0.008	1,521
		(0.011)	(0.013)	(0.013)	
Any derogatory accounts	0.282	-0.010	0.000	0.000	1,521
		(0.016)	(0.017)	(0.017)	
Any accounts in collections	0.598	0.001	0.009	0.009	1,521
		(0.023)	(0.023)	(0.023)	
Standardized treatment effect	0.000	0.034	0.021	0.002	1,521
		(0.022)	(0.023)	(0.021)	
Panel C: Post-Program Post-COVID Credit Outcomes (Q12–Q16)					
Credit score	617.00	2.30	0.95	1.43	1,521
		(4.71)	(4.57)	(3.12)	
Total debt	\$42,659	-\$5,288	-\$6,005	-\$2,565	1,521
		(4,040)	(3,702)	(2,993)	
Credit card debt	\$1,686	-\$284	-\$324	-\$90	1,521
		(222)	(205)	(158)	
Has auto loan or lease	0.218	-0.022	-0.020	-0.020	1,521
		(0.020)	(0.020)	(0.019)	
Any delinquent accounts	0.091	-0.007	-0.004	-0.004	1,521
		(0.011)	(0.012)	(0.012)	
Any derogatory accounts	0.208	-0.014	-0.008	-0.008	1,521
		(0.015)	(0.016)	(0.016)	
Any accounts in collections	0.567	-0.010	-0.001	-0.001	1,521
		(0.023)	(0.024)	(0.024)	
Standardized treatment effect	0.000	0.029	0.023	0.009	1,521
		(0.022)	(0.023)	(0.022)	

Notes: Data source is administrative credit data from Experian. This table reports ITT effects on credit outcomes for the sample of ReHire applicants who applied between 7/2015 and 12/2018 and matched to an Experian record in the 5 quarters before and 14 quarters following random assignment. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Column (1) reports the mean for control group applicants. Column (2) reports the coefficient on a treatment indicator, controlling for vendor-randomization rate block fixed effects. Column (3) reports the coefficient on a treatment indicator, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Column (4) further weights the sample using inverse propensity attrition weights. Column (5) reports the percent change of the ITT effect in column (4) relative to the control group mean. Heteroskedasticity-robust standard errors in parentheses.

**0.01, *0.05, $^{+}$0.10 significance levels

A.14.3 Bounding ITT Effects on Credit Outcomes to Deal with Attrition Bias

In Section III.C, we documented negligible differences in credit outcomes between the treatment and control groups. As noted in Section A.14.1, match rates varied slightly between the treatment group (61.9 percent) and the control group (62.8 percent). For completeness, this section reports bounds on the ITT effects estimated on credit outcomes following Lee (2009) and Kling and Liebman (2004).

The control group was more likely to match to the credit data than the treatment group. In order to implement Lee (2009) bounds, we trim outcomes among the control group until the match rate equals that of the treatment group. Lower bounds are constructed by trimming the control group individuals with the smallest outcome values. Upper bounds are constructed by trimming the control group individuals with the largest outcome values. We break ties using the estimated propensity to match to the credit data and trimming individuals who were least likely to match. When constructing lower (upper) bounds for the average standardized treatment effect, we use the upper (lower) bound for outcomes that are negatively signed in the index. We estimate the Lee bounds using Equation (1) controlling only for stratification fixed effects and by weighting the sample using inverse propensity weights.

We also report bounds following Kling and Liebman (2004) that assume attritors have outcome values that are one standard deviation away from the mean outcome. Lower bounds are constructed by assuming the treatment (control) group attritors have outcomes one standard deviation below (above) than the mean. Upper bounds are constructed by assuming the treatment (control) group attritors have outcomes one standard deviation above (below) the mean. We estimate the Kling-Liebman bounds using Equation (1) controlling only for stratification fixed effects.

Table A-22 report the ITT effects from a weighted regression that controls for stratification fixed effects, as well as the upper and lower bound estimates using the procedures described above. Given match rates are high and similar between the two experimental groups, it is unsurprising that the Lee bounds on the standardized treatment effects are relatively narrow and center around 0.

Table A-22: Bounds on the ITT Effects of ReHire on Credit Outcomes, Experian Sample

	Weighted ITT Effect No Controls	Lee Lower Bound	Lee Upper Bound	Kling-Liebman Lower Bound	Kling-Liebman Upper Bound
	(1)	(2)	(3)	(4)	(5)
Panel A: In-Program Credit Outcomes (Q0–Q4)					
Credit score	-0.79	-2.12	0.40	-49.72**	51.39**
	(4.59)	(4.57)	(4.60)	(3.44)	(3.45)
Total debt	-$5,432+	-$7,533**	-$5,003+	-$36,610**	$24,830**
	(2,883)	(2,754)	(2,903)	(2,275)	(2,282)
Credit card debt	-$403+	-$622**	-$367+	-$2,708**	$1,733**
	(210)	(191)	(209)	(164)	(164)
Has auto loan or lease	0.020	0.008	0.025	-0.170**	0.201**
	(0.018)	(0.018)	(0.018)	(0.013)	(0.013)
Any delinquent accounts	0.018	0.006	0.023	-0.132**	0.159**
	(0.015)	(0.015)	(0.015)	(0.010)	(0.011)
Any derogatory accounts	0.007	-0.005	0.015	-0.199**	0.196**
	(0.020)	(0.020)	(0.020)	(0.014)	(0.014)
Any accounts in collections	0.005	-0.004	0.017	-0.254**	0.236**
	(0.024)	(0.024)	(0.023)	(0.017)	(0.017)
Standardized treatment effect	0.018	-0.001	0.049*	-0.503	0.569**
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Panel B: Post-Program Pre-COVID Credit Outcomes (Q5–Q11)					
Credit score	0.51	-0.98	1.97	-30.54**	33.58**
	(4.52)	(4.49)	(4.50)	(3.37)	(3.40)
Total debt	-$4,727	-$6,969*	-$4,169	-$23,476**	$16,198**
	(3,035)	(2,906)	(3,049)	(2,301)	(2,305)
Credit card debt	-$258	-$458**	-$230	-$1,494**	$1,072**
	(184)	(166)	(186)	(154)	(153)
Has auto loan or lease	-0.006	-0.018	-0.001	-0.108**	0.118**
	(0.018)	(0.018)	(0.018)	(0.013)	(0.013)
Any delinquent accounts	-0.008	-0.014	-0.005	-0.087**	0.068**
	(0.013)	(0.013)	(0.013)	(0.009)	(0.009)
Any derogatory accounts	0.000	-0.015	0.006	-0.110**	0.105**
	(0.017)	(0.016)	(0.017)	(0.012)	(0.012)
Any accounts in collections	0.009	-0.001	0.020	-0.156**	0.150**
	(0.023)	(0.023)	(0.023)	(0.017)	(0.017)
Standardized treatment effect	0.021	0.003	0.052*	-0.307	0.364**
	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)
Panel C: Post-Program Post-COVID Credit Outcomes (Q12–Q16)					
Credit score	0.95	-0.55	2.55	-23.52**	25.17**
	(4.57)	(4.54)	(4.53)	(3.45)	(3.48)
Total debt	-$6,005	-$9,184**	-$5,509	-$20,288**	$12,410**
	(3,702)	(3,480)	(3,736)	(2,818)	(2,832)
Credit card debt	-$324	-$515**	-$287	-$1,042**	$693**
	(205)	(189)	(205)	(156)	(156)
Has auto loan or lease	-0.020	-0.033+	-0.015	-0.091**	0.073**
	(0.020)	(0.019)	(0.020)	(0.015)	(0.015)
Any delinquent accounts	-0.004	-0.015	-0.002	-0.041**	0.055**
	(0.012)	(0.011)	(0.012)	(0.008)	(0.008)
Any derogatory accounts	-0.008	-0.016	-0.003	-0.081**	0.051**
	(0.016)	(0.016)	(0.016)	(0.012)	(0.012)
Any accounts in collections	-0.001	-0.011	0.012	-0.122**	0.086**
	(0.024)	(0.024)	(0.024)	(0.018)	(0.018)
Standardized treatment effect	0.023	0.005	0.055*	-0.209	0.248**
	(0.023)	(0.023)	(0.022)	(0.020)	(0.020)

Notes: Data source is administrative credit data from Experian. This table reports ITT effects on credit outcomes for the sample of ReHire applicants who applied between 7/2015 and 12/2018 and matched to an Experian record in the 5 quarters before and 14 quarters following random assignment. Quarter 0 represents the quarter in which a participant completed an application, and is thus a different calendar quarter from person to person. Column (1) reports the coefficient on a treatment indicator, controlling for vendor-randomization rate block fixed effects and weighting the sample using inverse propensity weights. Heteroskedasticity-robust standard errors in parentheses. **0.01, *0.05, +0.10 significance levels

A.15 Analysis of Mechanisms

This section provides additional details and supporting evidence for the discussion of mechanisms in Section IV in the main paper.

A.15.1 Identifying Subsidized to Unsubsidized Employment Transitions

One difficulty of measuring within-firm transitions from subsidized to unsubsidized employment is that the employer of record for the transitional job in the administrative earnings data is the local service agency. In order to identify which ReHire participants transitioned from subsidized to unsubidized work with the same employer, we combine information from UI wage records with participant information tracked in the ReHire administrative database. Program records tracked employer names for the transitional job host site, as well as the first unsubsidized employment spell following program participation. We use this information, as well as employer names in UI wage records, to identify participants who transitioned to unsubsidized employment with the same employer host site.

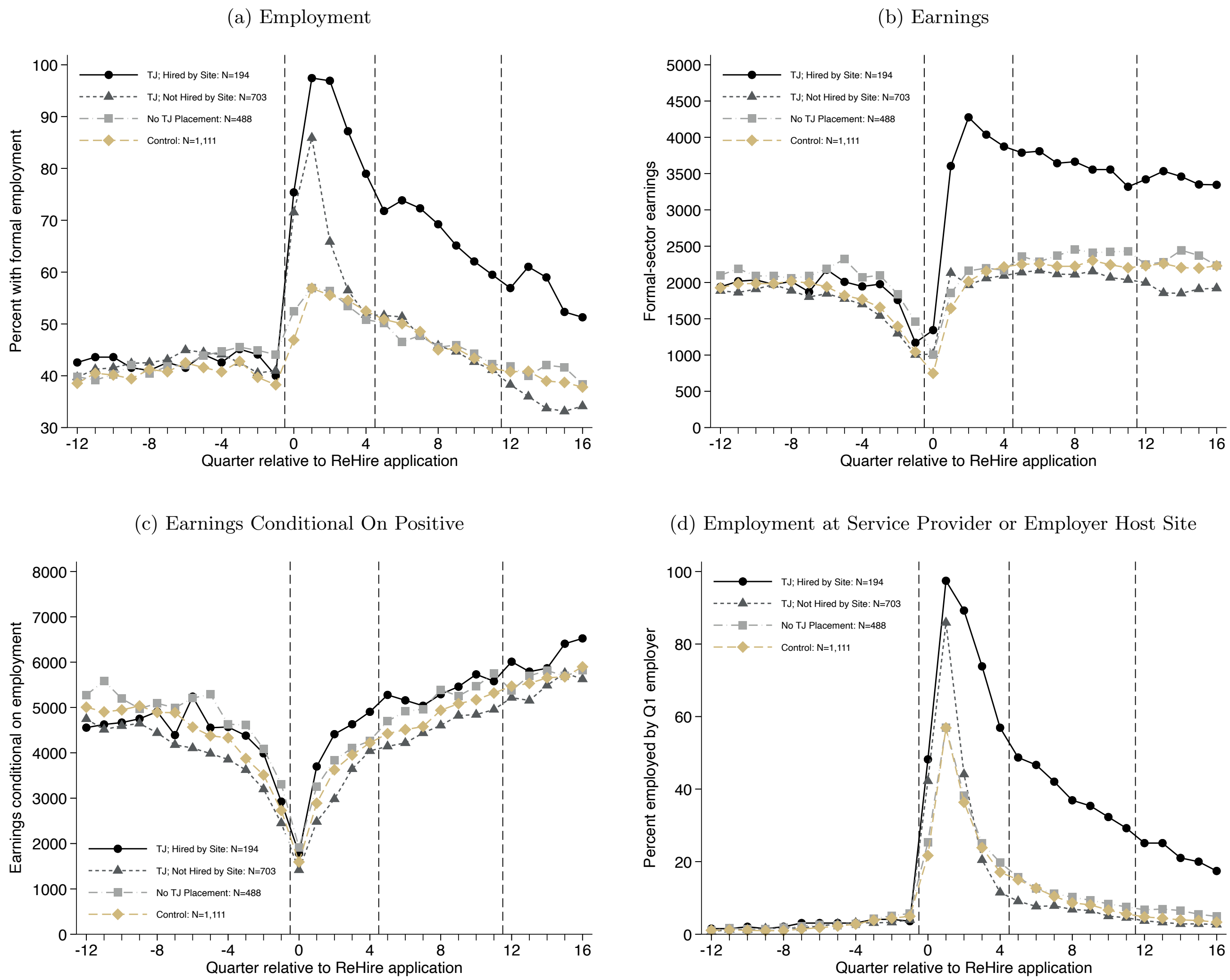
We code successful transitions in the following ways:

1. **Compare employer names within ReHire case notes:** ReHire case records include reports of employment spells while the participant remained on the ReHire caseload. The records include employer names, start and end dates, employer industry, and employer size. The employment records are reported separately for subsidized jobs and unsubsidized jobs. For each participant, we hand-matched names of subsidized and unsubsidized employers and coded a successful transition when the employer names matched.

2. **Compare subsidized employer names from ReHire case notes with employer names in administrative earnings records:** The administrative earnings records from CDLE included employer name and employer industry. For individuals with a recorded transitional job in the ReHire database, we hand-matched names of the employer(s) in the ReHire case notes to all employers linked to the individual in the UI wage records. In some cases, though the name of the employer did not match, we verified through information on-line that the employer name was linked to the given name of the employer in the ReHire case notes as a "d.b.a" name. For example, the ReHire case notes may have had an employer as "ABC Cafe" but the UI records had "XYZ Restaurant Group". When such matches could be verified through an internet search, they were also coded as successful transition.

While most successful transitions recorded in the ReHire case notes could be identified in the administrative earnings records, there were 19 individuals where the case record indicated unsubsidized employment at the employer host site, but this employment spell could not be verified in the administrative earnings records. Appendix Figure A-8 reproduces the analysis depicted in Figure 3 by only relying on successful transitions that could be verified in the administrative earnings records. The pattern of results across all four panels are very similar when using either classification.

Figure A-8: Formal-Sector Employment and Earnings in Colorado
by Treatment Assignment and Transitional Job Completion
Alternative Definition of Successful Transition



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 2,496 ReHire applicants who applied between 7/2015 and 12/2018. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Beginning in the 12$^{\text{th}}$ quarter following random assignment, more than half of the sample was potentially experiencing labor market disruptions due to the COVID-19 pandemic. Formal earnings is defined as UI-covered earnings in Colorado in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector earnings. Treatment and control groups are based on an individual's randomly assigned treatment status. The treatment group is further divided based on transitional job (TJ) receipt and whether individuals were hired by their transitional job host site. The figure plots the (a) quarterly employment rates, (b) average quarterly earnings, (c) average quarterly earnings among individuals with positive earnings, and (d) percent employed by the same employer as their Q1 employer.

A.15.2 Differences in Individual Characteristics by Program Experience

Table A-23 demonstrates that the three treatment groups and the control group had similar observable baseline characteristics that could be related to their ability to find a job (subsidized or unsubsidized). Columns (1) through (4) report the mean characteristics of the control group and of the three treatment subgroups, respectively. Column (5) provides differences in means among the treatment group by transitional job placement, and column (6) reports differences based on subsequent permanent hire among those with a transitional job.[60] A few observable characteristics are statistically different by transitional job placement status (columns 3 and 4 vs. column 2). Participants who are male, have been homelessness, or had a prior felony conviction were less likely to receive a transitional job placement, and a test of the null hypothesis that job placement is unrelated to all of the listed baseline characteristics is rejected ($p < 0.01$). However, few characteristics are different between those who were hired by their transitional job host site and those who were not (column 4 vs. column 3), and we fail to reject the null hypothesis that, among those placed into a transitional job, being hired by one's host site is unrelated to the full set of baseline characteristics ($p = 0.33$). There are small differences in the caseworkers' scoring of an applicant's job readiness such as their "motivation to get back to work" or their "likelihood to overcome employment barriers" (roughly one third of a point on a ten-point scale), as well as small differences in grit and two components of the Big 5 (roughly one tenth of a point on a five-point scale).[61]

Figure A-9 shows how program experience varies across the entire distribution of caseworkers' assessment of the applicant's likelihood to overcome barriers (Figure A-9a) and their motivation to obtain and maintain employment (Figure A-9b). In each panel, the horizontal axis divides the sample based on the assessment of the caseworker, grouping individuals with a score of 4 or lower into one group, and showing the information for the full sample in the final bar. Grey circles connected by a dotted line shows the share of the treatment group with a given score. For both assessments, the modal score was a 10 out of 10. The height of the vertical black bar reports the share of the group who were placed into a transitional job. While some groups were more or less likely to have been placed, across both figures, there is not a consistent increasing or decreasing pattern of placement. Finally, the height of the gold bar reports share of each group who were placed into a transitional job and then was subsequently hired on by that employer. In both figures, successful transition rates are slightly increasing in the caseworker assessment, consistent with the differences reported in Table A-23.

[60] The differences in these two columns control for the same vendor-randomization rate block fixed effects as in the main analysis.

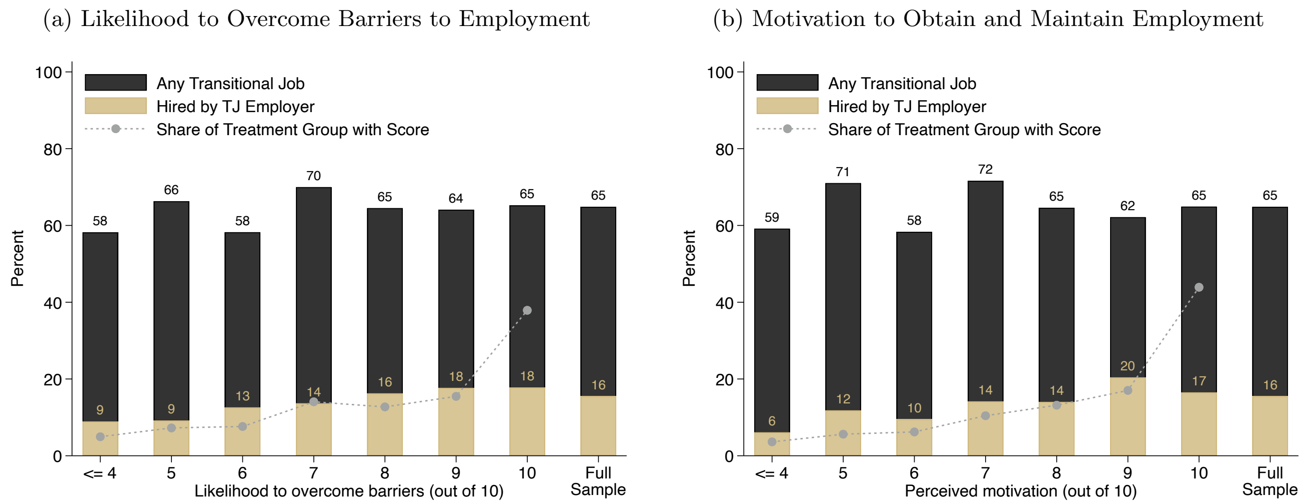
[61] Appendix Section A.15.2 provides more detail on how the distribution of caseworker scores relates to transitional job placement rates and subsequent hiring rates (Appendix Figure A-9).

Table A-23: Applicant Characteristics by Transitional Job Receipt and Subsequent Hire

	Control	Treatment Group			Difference in Means	
	Mean	No TJ	Transitional Job		TJ	Hired
		Mean	Not Hired by TJ Mean	Hired by TJ Mean	Take-up (3 & 4) − (2)	by TJ (4) − (3)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Administrative Data						
Worked last year	0.599	0.645	0.621	0.601	-0.022	-0.021
Employment rate last three years	0.405	0.423	0.424	0.424	0.023	-0.011
Average quarterly earnings in last year	\$1,530	\$2,047	\$1,449	\$1,673	-\$311^{+}	\$100
Received TANF last year	0.126	0.107	0.129	0.113	0.021	-0.017
Received SNAP last year	0.696	0.656	0.702	0.662	0.025	-0.035
Panel B: Baseline Survey						
Demographics						
Average Age (years)	46.7	45.3	46.9	45.5	0.6	-1.1
Average years of education	13.6	13.6	13.6	13.5	0.1	-0.1
Male	0.496	0.588	0.496	0.521	-0.129**	0.043
Minority	0.406	0.336	0.418	0.390	0.002	0.012
Covered by Medicaid	0.758	0.736	0.756	0.723	0.014	-0.028
Barriers to Employment						
Not allowed to drive	0.208	0.230	0.228	0.232	-0.032	0.027
Parent	0.303	0.284	0.272	0.311	0.029	0.038
Single parent	0.178	0.163	0.157	0.189	0.019	0.034
Difficulty finding childcare	0.095	0.085	0.092	0.071	0.022	-0.019
Expect economic hardship	0.322	0.273	0.342	0.291	0.018	-0.016
Health limits work	0.103	0.109	0.107	0.077	-0.001	-0.023
Ever homeless	0.435	0.425	0.441	0.389	-0.060*	0.006
Ever convicted of felony	0.242	0.241	0.236	0.265	-0.051*	0.050
Drugs or alcohol have affected life	0.228	0.224	0.241	0.214	-0.013	-0.013
Caseworker Job Readiness Assessment						
Perceived motivation (out of 10)	8.47	8.49	8.39	8.80	0.00	0.32*
Likelihood to overcome barriers (out of 10)	8.13	8.11	8.10	8.53	0.12	0.37**
ReHire Target Populations						
Veteran	0.225	0.215	0.240	0.202	-0.027	-0.015
Non-custodial parent	0.203	0.193	0.193	0.183	-0.023	-0.020
Older worker	0.484	0.451	0.513	0.460	0.018	-0.031
Not in a priority category	0.279	0.297	0.256	0.329	0.034	0.056^{+}
Cognitive skills						
Timed math test, percent correct	58.3	59.5	55.5	57.3	-1.1	0.5
Number of math questions attempted (out of 160)	96.2	98.1	91.9	94.2	-1.9	0.2
Raven's score (out of 36)	30.8	31.3	30.8	31.4	0.1	0.2
Non-cognitive characteristics						
Locus of control (1–5)	4.08	4.03	4.06	4.11	0.02	0.05
Grit (1–5)	3.89	3.86	3.90	3.98	0.05^{+}	0.09*
Extraversion (1–5)	3.11	3.12	3.11	3.14	0.02	0.01
Agreeableness (1–5)	3.93	3.92	3.96	3.96	0.08*	-0.00
Conscientious (1–5)	4.01	3.96	3.99	4.09	0.02	0.12**
Neuroticism (1–5)	2.44	2.51	2.46	2.36	-0.05	-0.09^{+}
Imagination (1–5)	3.07	3.09	3.04	3.07	-0.03	0.00
Life satisfaction ladder (0–10)	5.71	5.57	5.64	5.67	0.04	0.02
Depression scale (0–10)	1.52	1.58	1.57	1.42	-0.10	-0.05
Observations	1,111	488	684	213	1,385	897
Prob > F					0.002	0.328

Notes: Data come from administrative UI earnings data from CDLE, administrative SNAP and TANF data from CDHS, and baseline survey data collected at application. The sample includes ReHire applicants who applied between 7/2015 and 12/2018. One applicant can be linked to administrative data, but is missing a baseline survey. Estimates of the difference in means control for vendor-randomization rate block (stratification) fixed effects. The final row reports the p-value from the test of the null hypothesis that all characteristics are jointly unrelated to the listed difference in program experience.

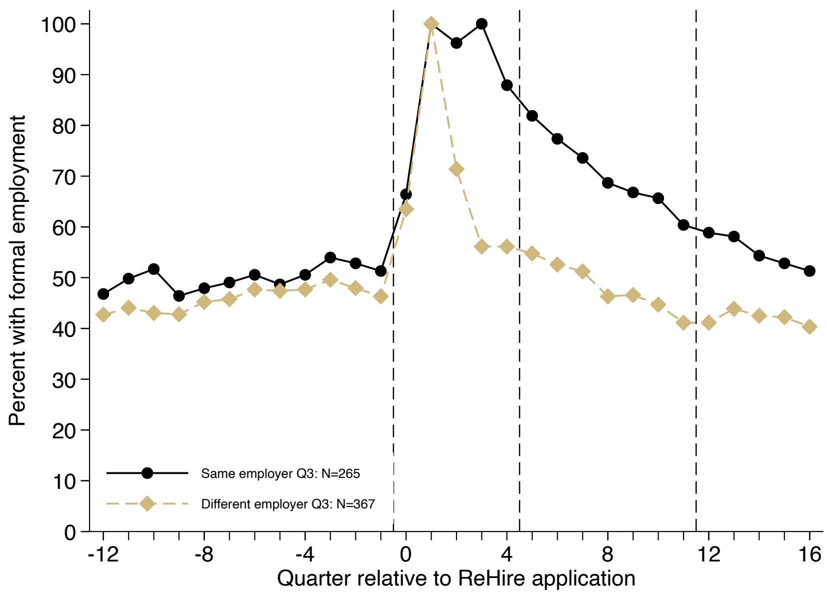
Figure A-9: Transitional Job Take-up and Subsequent Hire by Caseworker Assessment at Intake



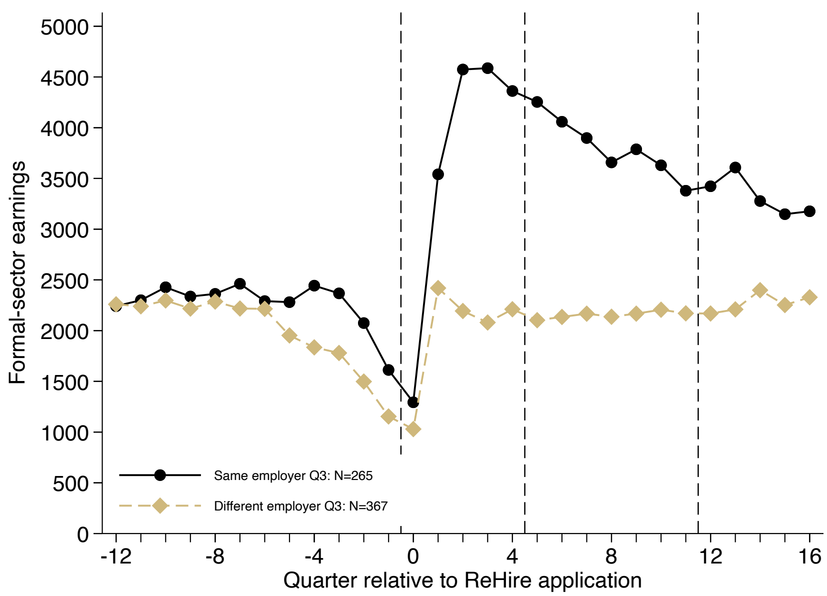
Notes: Data source is the baseline survey, ReHire program records, and administrative UI earnings data from CDLE. The sample includes 1,351 ReHire applicants who applied between 7/2015 and 12/2018, were scored by assessment after their intake, and were assigned to the treatment group. Panel (a) divides the treatment group based on a scale of how motivated the individual was to obtain and maintain full-time employment. Panel (b) divides the treatment group based on a scale of the likelihood that the individual would overcome obstacles to full-time employment. For each score designated on the x-axis, the figure plots the share of the treatment group that was placed into a transitional job (black bar) and was placed into a transitional job and were subsequently hired by the same employer (gold bar). The gray circle and dashed line shows the distribution of scores across the sample.

Figure A-10: Control Group Formal-Sector Employment and Earnings in Colorado
by Quarter 1 and Quarter 3 Employment

(a) Employment



(b) Earnings



Notes: Data source is administrative UI earnings data from CDLE. The sample includes 632 ReHire applicants who applied between 7/2015 and 12/2018, were assigned to the control group, and were employed in the first quarter following application. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Beginning in the 12^{th} quarter following random assignment, more than half of the sample was potentially experiencing labor market disruptions due to the COVID-19 pandemic. Formal earnings is defined as UI-covered earnings in Colorado in a given quarter. Two groups are defined by employment in quarters 1 and 3: worked for the same employer in quarters 1 and 3 (black circles); and worked in quarter 1, but did not work or worked for different employer in quarter 3 (gold diamonds). The figure plots the (a) quarterly employment rates and (b) average quarterly earnings.

A.15.3 Differences in Firm Characteristics by Transitional Job Success

We leverage a panel of employer-level characteristics from 2014–2019 for firms that employed ReHire study participants to explore the extent to which differences in the types of placement explain differences in program experience.

We accessed information about firms through a data sharing agreement with the Linked Information Network of Coloraod (LINC). Using the universe of UI earnings records, LINC constructed quarterly data on the number of employees, number of incumbent employees, number of new employees, average earnings paid per quarter, the percentiles of earnings paid each quarter (10th, 50th, and 90th), and sector. These data also include estimates of firm fixed effects from a standard AKM model (Abowd, Kramarz and Margolis, 1999).[62]

Unfortunately, the employer of record in the UI earnings data for a transitional job is the local area contractor, not the host-site employer. To identify these host site employers, we provided LINC with name and address information of all of the participants' host sites, which came from the ReHire program database. Using CDLE data, we also identified the FEIN of employers where the TJ participant was subsequently hired.[63] We provided LINC with 491 unique host-site employer name-address pairs for matching, and the resulting data linkage resulted in 85% of transitional jobs participants matching to characteristics about their host-site employer. This match rate is somewhat higher among successful transitional jobs participants who are subsequently hired by the host site (96%) compared to those who are not (81%).

Table 5 provides the descriptive statistics on the firms that employed ReHire study participants between 2014 and 2019. Column (1) includes information about all non-host-site employers, and also excludes the LACs that operate the program.[64] The next three columns provides statistics on host-site employers (column 2), host-site employers where the participant was not hired (column 3), and host-site employers where the participant was hired (column 4). Importantly, because many host sites employed multiple ReHire participants, a host-site employer could be represented in more than one column based on the program experience of each participant.

The data reveal a number of interesting stylized facts. First, host-site employers looks different than the typical firm that employs study participants (column 5). Host sites are more consistently in the UI records, are much larger on average, and experienced less turnover. They also operate in different industires. Host sites are more likely to be in Retail Trade, Health Care and Social Assistance and Public Administration, and are much less likely to be a Temporary Agency or otherwise operate in the Administrative Aervices sector or Accomodation and Food Services sector. Host sites also tend to pay higher wages, both in terms of levels, but also in terms of their value-added.

Second, successful host-site employers look different than other placements across some but not all characteristics. In general, there are only small but not significant differences in terms of firm size, growth, and turnover. We do, however, find meaningful differences in terms of the sector and pay. Successful placements are more likely to be in the Wholesale and Public Administration sectors and less likely to be in Health Care and Social Assistance than unsuccessful placements. Finally, while successful firms tend to have a slightly larger (though not statistically significant) firm fixed effect, they tend to pay lower wages across the 10th, 50th and 90th percentile relative to their unsuccessful host site peers.

[62] We provided the LINC team with R code adapted from estimate_akm code from Tino (2023).

[63] We supplemented this information by identifying additional FEINs by searching for firms on https://eintaxid.com.

[64] The data we access are deidentified at the person and firm level. We identify LACs in the data as the employer that employs the most treatment group participants within each ReHire vendor. For example, we identify the modal employer for all treatment group members at Catholic Charities in Pueblo and code those participant-employer observations as LAC observations.

A.15.4 Using Machine Learning to Predict Program Experience and to Estimate Heterogeneity Across Predicted Program Experience

The primary component of ReHire is placement into a transitional job. Roughly 62 percent of treatment group members are placed into a transitional job, and about 15 percent of the treatment group go on to work in an unsubsidized job with the same employer. Table A-23 shows some selection on baseline characteristics into who is placed into a transitional job, although characteristics are very similar when comparing TJ workers who are subsequently hired on by their host site to those who are not.

We combine machine learning methods with a repeated split-sample (RSS) procedure motivated by Abadie, Chingos and West (2018) and Chernozhukov et al. (2020) to more rigorously explore whether baseline characteristics are predictive of program experiences. Let T_i^j be an indicator for whether individual i had one of two program experiences j: take-up of a transitional job (T_i^1) or take-up of a transitional job and then transition to unsubsidized work with the same employer (T_i^2). In this exercise, we aim to predict T_i^j using either OLS, logit, or one of four machine learning methods—elastic net, boosted trees, neural network with feature extraction, and random forest—following Chernozhukov et al. (2020).[65] We then use these predictions to ask how the predicted probability of having these program experiences relates to the size of an individual's program impacts for the outcomes Y reported in Table 2.

We adapt the estimation and inference methods of Chernozhukov et al. (2020) who estimate target parameters among a sample stratified by a proxy for the conditional average treatment effect, rather than predicted program experience. For a given prediction target T^j and prediction method, the adapted RSS estimation procedure proceeds as follows:

1. Randomly partition the treatment group into two, creating an auxiliary sample, A, which includes half of the treatment group, and a main sample, M, which includes the remaining treatment group members and all control group members.

2. In sample A, estimate a model that predicts T^j using a set of baseline characteristics X.

3. In sample M, predict the likelihood of T_i^j for each individual $\hat{p}_i^j$.

4. Stratify sample M into quartiles based on $\hat{p}_i^j$.

5. In each quartile sample, calculate estimates, as well as the upper and lower bounds of the 95 percent confidence interval of the estimates, of the following:

   (a) The share of the treatment group who were actually placed into a transitional job

   (b) The share of the treatment group who were actually placed into a transitional job and then transitioned to unsubsidized work with the same employer

   (c) The impact of ReHire on all outcomes $y \in Y$ from a regression with an indicator for treatment assignment, as well as stratification fixed effects.

   (d) The mean of all outcomes $y \in Y$ among the control group and treatment group

   (e) The average of each baseline characteristic $x \in X$

6. Calculate the difference between the top and bottom quartile for each of the estimates from step 5, as well as the upper and lower bounds of the 95 percent confidence interval around the difference.

7. Repeat steps 1 through 5 1,000 times and calculate the median of each set of estimates, including the median of the upper and lower bounds of the confidence intervals.

[65] Specifically, we use glmnet, gmb, pcaNNet, and rf from the caret package (Kuhn, 2009) to implement the elastic net, boosted trees, neural network, and random forest, respectively. Tuning parameters for the first three methods are chosen to maximize the mean squared error estimates using 2-fold cross validation. For random forests, we grow 25,000 trees and randomly select a third of the available predictors when identifying nodes.

We use a number of potential baseline characteristics measured in the baseline survey and administrative data to predict take-up and subsequent transition.[66] The variables include:

- **Employment and Earnings:** Total earnings in the year before randomization; total earnings in the two years before randomization; earnings in each of the eight quarters before randomization; number of employers in each of the eight quarters before randomization

- **Government Benefit Receipt:** Total SNAP receipt in the year before randomization; total SNAP receipt in the two years before randomization; SNAP receipt in each of the 24 months before randomization; total TANF receipt in the year before randomization; total TANF receipt in the two years before randomization; TANF receipt in each of the 24 months before randomization

- **Demographics:** An indicator for being male; age in years and an indicator for missing age; six educational attainment indicators (less than high school, high school diploma or GED, some college, associate's degree, bachelor's degree, missing); three indicators for the ReHire priority groups (veteran, non-custodial parent, older worker); four indicators for self-reported race (white, not-white, black, hispanic); seven indicators for marital status (married, divorced, partnered, married living apart, single, separated, and widowed); six indicators for housing type (owned, jointly owned, owned by another resident, renting, transitional, homeless)

- **Barriers to Employment:** Indicators for having a prior felony (yes, no, missing); ability to drive (yes, no, missing); issues with childcare (yes, no, missing); work-limiting health problems (yes, no, missing); ever experienced homelessness (yes, no, missing); expect economic hardship in future (yes, no, missing); alcohol has ever affected work (yes, no, missing); self-identify as alcoholic (yes, no, missing); marijuana has ever affected work (yes, no, missing); self-identified marijuana addiction (yes, no, missing); other drugs have ever affected work (yes, no missing); self-identified drug addiction (yes, no, missing; any reported substance abuse (yes, no, missing);

- **Caseworker Assessment:** Motivation to get back to work assessed by caseworker (1–10) and indicator for missing; likelihood of overcoming barriers assessed by caseworker (1–10) and indicator for missing

- **Skills:** Score on Raven's progressive matrices (0–36) and indicator for missing; score on timed math test (0–100), number of attempted answers on math test (0–160), and indicator for missing; grit (1–5) and indicator for missing; locus of control (1–5) and indicator for missing; and component scores of Big Five—extraversion, agreeableness, conscientiousness, neuroticism, imagination (1–5) and an indicator for missing

- **Mental Well-Being:** Life satisfaction ladder (0–10) and indicator for missing; and CESD depression scale (0–7) and indicator for missing.

We first explore whether our rich set of *individual-level* baseline covariates is predictive of program experience. Table A-24 reports actual transitional job placement rates (Panel A) and rates of hire by transitional job sites (Panel B) for treatment group individuals, as well as rates of hire by transitional job sites when making predictions only among treatment group individuals placed in a transitional job (Panel C). For each panel, the target for prediction is the program experience considered in that panel. Column (1) and (2) report the actual program experience rate among those who were predicted to be

[66] For continuous measures with missing values, we impute missing values at the sample median and include a dummy that the variable was missing. The variable with the most observations missing was the results of the math test. Individuals completed the timed math test on a piece of paper that was to be scanned into the ReHire program database. In some instances scans were not attached to individuals in the database. In total, 1,729 complete tests were scanned into the program database and subsequently scored.

least likely (bottom quartile) and most likely (top quarter) to have that program experience, respectively. Column (3) reports the difference in rates across the two groups, and column (4) reports the 90 percent confidence interval around that estimate. All come from the median estimate among the 1,000 repeated split samples.[67]

While each predictive model generates differences in predicted likelihoods, no model is able to generate large differences in actual take-up/transition. When predicting transitional job placement, OLS does best. According to this method, 61.3 percent of those predicted to be least likely to be placed in a transitional job did so versus 69.6 percent among the most likely group. The estimated 8.2 percentage point difference, however, is not statistically significant and the 90 percent confidence interval is wide. When trying to predict successful transitional job placements, no method generates large differences and logit does best in generating differences between the most and least likely groups (2.9 percentage points). Finally, when making predictions about hiring by the transitional job site among those who worked a transitional job, random forest creates the largest difference among the groups predicted to be most and least likely (6.2 percentage points, respectively). In every case, 90 percent confidence intervals for the estimated difference between the two groups are wide.

We then extend this analysis to ask whether differences in *firm-level* characteristics are predictive of program experience. By definition, we only observe host-site employers for individuals placed into transitional jobs, so we use machine learning methods to ask whether we can successfully predict successful transition among transitional jobs placements. In the bottom half of Panel C, we further include in our predictor set the firm-level characteristics reported in Table 5. Unlike before, we are able to generate groups of study participants that vary in their transitional job success, and random forest generates the most variation in success rates across the most likely and least likely quartiles. For example, those who are predicted to be most likely to have a successful transition according to the random forest model do so at a 43.2 percent rate, which is 30.5 percentage points higher than those assigned to the least likely quartile. Our repeated split sampling procedure confirms that this difference is statistically significant.

Having identified the random forest model as the best method at predicting successful transition, we then trained three random forest models using data from all transitional job participants who were matched to host-site characteristics to understand how (i) individual-level characteristics, (ii) firm-level characteristics, and (iii) the combination of both influence the underlying predictions. Figure A-11 summarizes the SHAP values for the twenty most important variables (i.e., the variables that add the greatest predictive power to the model). Variables are sorted from most important (top) to less important (down). For each variable given in a row, the SHAP values are plotted, which show how the additional change in an individual's prediction that corresponds to the variable relative to the horizontal axis. The color of the circle shows the normalized value of that predictor for the individual (red is above average, blue is below average). Figure A-11c shows the results for the random forest model with both set of predictors, firm and individual characteristics. The most imporant variable is the indicator for being in the Heatlh & Social Assistance sector. The red values on the left of the horizontal axis show that being in that sector decreases the predicted likelihood of success. Conversely, the red circles in the Wholesale Trade row show the increase in the predicted likelihood for observations in that sector. Beyond those sector variables, the three variables related to the earnings distribution of the firm are the most important. The figure demonstrates there are a set of individuals at firms with larger than average values of 10^{th} percentile wages (dark red) that led to a decrease in the predicted likelihood of success.

[67] As noted in step 5 above, the estimation procedure collects the upper and lower bound of the 95 percent confidence interval for each estimate. Chernozhukov et al. (2020) note that the "price of splitting uncertainty is reflected in the discounting of the confidence level from $1-\alpha$ to $1-2\alpha$" (see page 19). Similarly, p-values reported come from the median of the estimated p-values and are doubled (and top-coded at 1, if necessary) to account for this uncertainty.

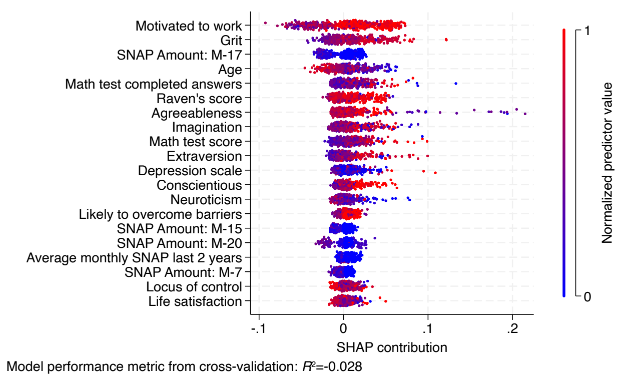
Table A-24: Predicting Program Experience, Comparison of ML Methods

	Bottom Quartile (1)	Top Quartile (2)	Difference (3)	Confidence Interval (4)
Panel A: Transitional Job Take-up				
OLS	0.613	0.696	0.082	[-0.018, 0.182]
Logit	0.649	0.665	0.014	[-0.086, 0.114]
Elastic Net	0.575	0.628	0.045	[-0.066, 0.154]
Boosting	0.585	0.540	-0.024	[-0.148, 0.098]
Neural Network	0.606	0.676	0.075	[-0.027, 0.175]
Random Forest	0.645	0.645	0.001	[-0.101, 0.109]
Panel B: Hired by Transitional Job Rate				
OLS	0.153	0.152	0.000	[-0.075, 0.076]
Logit	0.139	0.170	0.031	[-0.045, 0.108]
Elastic Net	0.156	0.154	-0.003	[-0.080, 0.074]
Boosting	0.148	0.163	0.014	[-0.062, 0.091]
Neural Network	0.146	0.162	0.016	[-0.059, 0.092]
Random Forest	0.146	0.169	0.022	[-0.056, 0.101]
Panel C: Hired by Transitional Job Rate Conditional on Placement				
Only individual-level predictors				
OLS	0.228	0.240	0.009	[-0.101, 0.122]
Logit	0.236	0.237	0.000	[-0.113, 0.111]
Elastic Net	0.230	0.239	0.009	[-0.102, 0.122]
Boosting	0.212	0.265	0.052	[-0.061, 0.162]
Neural Network	0.219	0.255	0.036	[-0.077, 0.149]
Random Forest	0.207	0.270	0.062	[-0.053, 0.173]
Individual- and firm-level predictors				
OLS	0.211	0.337	0.126	[-0.004, 0.250]
Logit	0.253	0.305	0.053	[-0.076, 0.181]
Elastic Net	0.158	0.368	0.211	[0.086, 0.333]
Boosting	0.158	0.400	0.242	[0.119, 0.368]
Neural Network	0.189	0.358	0.168	[0.042, 0.294]
Random Forest	0.126	0.432	0.305	[0.182, 0.423]

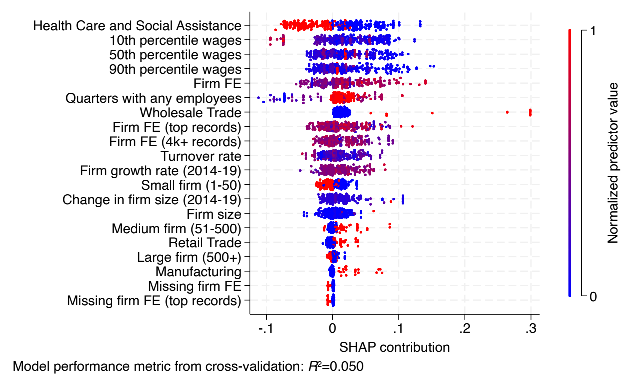
Notes: The data come from the ReHire baseline survey, program records, and administrative data from CDLE and CBMS. The sample includes 2,495 ReHire applicants who applied between 7/2015 and 12/2018. The table compares the ability of six machine learning methods to predict transitional job take-up (Panel A), the likelihood that a ReHire client takes-up a transitional job and is then hired on without the subsidy by that employer (Panel B), and the likelihood that a ReHire client is hired on without the subsidy by the employer host site conditional on transitional job placement (Panel C). All estimates comes from the median of 1,000 sample splits where half of the treatment group is used to train the machine learning model and the remaining half is used to predict the program outcome and stratify the sample. The table reports the share of the treatment group in the hold-out sample that actually had the given program experience. Column (1) includes treatment group individuals who were predicted to be least likely to have the program experience (bottom quartile. Column (2) includes treatment group individuals who were predicted to be most likely to have the program experience. Column (3) reports the estimate of the differences between these two groups. Column (4) reports the 90 percent confidence interval.

Figure A-11: SHAP Summary Plots for Random Forest Models Predicting Successful Transitional Jobs

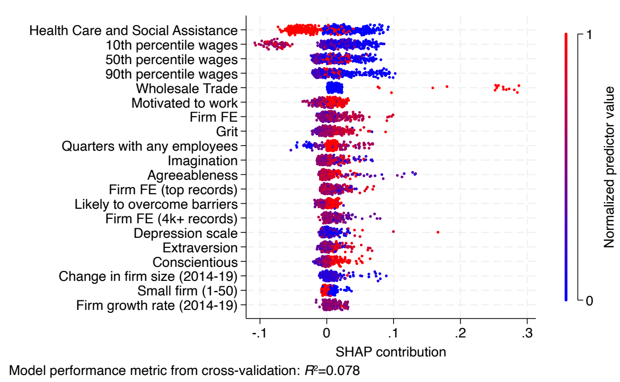
(a) Individual-level Predictors Only



(b) Firm-level Predictors Only



(c) Both Individual- and Firm-level Predictors



Notes: Data source is the baseline survey, ReHire program records, administrative UI earnings data from CDLE, administrative SNAP records from CDHS, and firm characteristics from LINC. The sample includes 758 transitional jobs participants who were matched to host site characteristics.

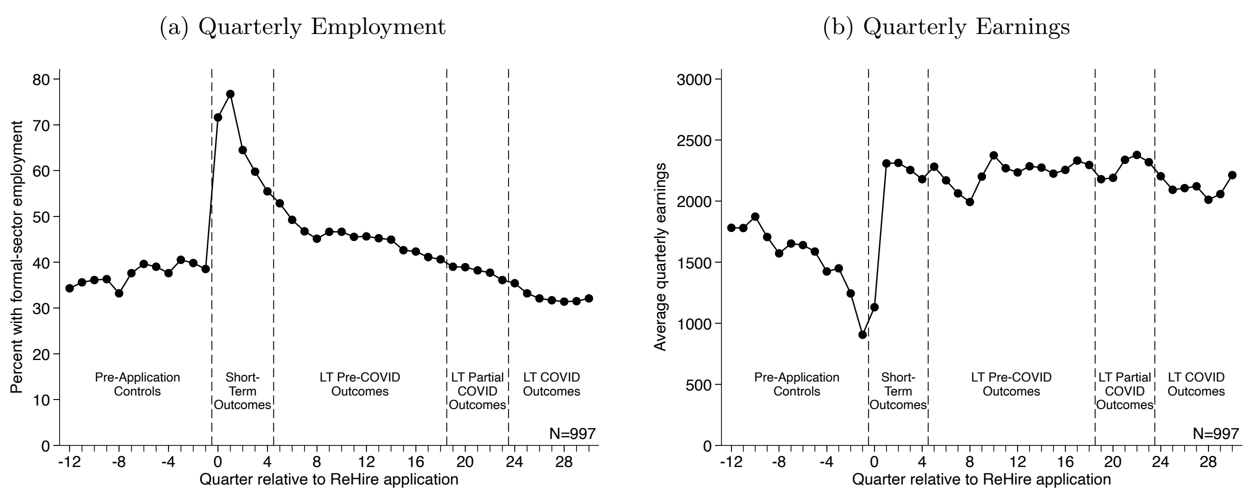
A.16 COVID-19 Pandemic and Long-Term Effects of ReHire

This section provides details about the surrogate index approach we use to construct estimates of the long-term effects of ReHire access on earnings. The surrogate index approach serves two purposes. First, it allows us to generate predictions of what program effects during our evaluation period (Q5–Q16) would have been had the COVID-19 pandemic not occurred. Second, it allows us to generate predictions for long-term effects—five to eight years after random assignment—that are not yet observed in our RCT sample.

We combine observational data from an earlier (pre-RCT) sample of ReHire applicants with data from the RCT to estimate the "long-term" effect of ReHire on employment and earnings. We implement the surrogate index approach of Athey et al. (2019) who estimate the long-term effects of the Riverside GAIN intervention using proxies of long-term employment and earnings outcomes constructed from short-term employment and earnings outcomes. In addition to using their approach to estimate longer-term outcomes, we adapt their approach and replace outcome data potentially affected by the COVID-19 pandemic with predictions of outcomes based on pre-COVID data to shed light on what the effect of ReHire access might have been had the pandemic not occurred.

The analysis requires two separate samples: (i) an experimental sample for whom access to ReHire is randomly assigned that includes measures of short-term outcomes; and (ii) a sample drawn from a similar population that includes measures of both short-term outcomes and long-term outcomes. We supplement data from the RCT ($N = 2,496$) with long-term observational data of ReHire participants who applied and entered the program between January 2014 and June 2015, prior to the implementation of the RCT ($N = 997$). During this time frame, ReHire was largely being operated by the same service agencies in the same geographic areas as the RCT sample.

Figure A-12: Formal-Sector Employment Rates in Colorado, Pre-RCT ReHire Participants



Notes: Data source is administrative UI earnings data from CDLE. The sample includes pre-RCT ReHire participants who applied for ReHire between 1/2014 and 6/2015. Quarter 0 represents the quarter in which a study participant completed their ReHire application, and is thus a different calendar quarter from person to person. Formal-sector employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal-sector employment. Treatment and control groups are based on an individual's randomly assigned treatment status. Panel (a) plots the percent of participants with formal-sector employment, and Panel (b) plots the average quarterly earnings of the sample.

For the pre-RCT sample, we are able to observe employment and earnings outcomes in the 12 quarters prior to application and up to 30 quarters following application. Figure A-12 presents the trends in quarterly employment rates and average earnings for the observational sample. For this sample, the data were unaffected by any labor market disruptions of the COVID-19 pandemic through the 18$^{\text{th}}$ quarter following application. Some of the pre-RCT participants started experiencing the COVID-19 pandemic 19 quarters after application. By quarter 24, all were experiencing the pandemic.

The intuition of the surrogate index approach is as follows. We observe the relationship between short-term outcomes (Q0–Q4) and long-term outcomes (Q5 and later) in the pre-RCT observational data. Using correlations between short- and long-term outcomes estimated in the observational sample, we can predict long-term outcomes in the RCT sample. These predicted long-term outcomes can proxy for quarters not yet observed in the data, or proxy for quarters affected by the pandemic in the RCT sample, but not affected by the pandemic in the observational sample. The RCT provides experimental variation in access to treatment, which allows us to estimate causal effects on the proxied data.

Three assumptions are needed in order for the surrogate index approach to identify the average treatment effect on long-term employment and earnings—(1) unconfoundedness, (2) surrogacy, and (3) comparability—all of which are likely satisfied in this context.

1. Under unconfondoudedness, we assume that access to treatment in the experimental sample is uncorrelated with any unobservable characteristics that affect short-term outcomes. This assumption is satisfied by the random assignment of ReHire among individuals in the experimental sample.

2. The surrogacy assumption requires that the effect of ReHire on long-term outcomes is fully mediated through the effect of ReHire on short-term employment and earnings. This assumption is reasonable for two reasons. First, all ReHire services were received during the period over which short-term outcomes are measured. Second, the anticipated mechanisms through which the program was expected to affect long-term outcomes—barrier removal, job matching, providing recent verifiable work experience, and work-based learning (see Section I.B)—should have affected outcomes in the short-run (Q0–Q4).

3. The comparability assumption requires that the distribution of long-term outcomes conditional on short-term employment and earnings is the same for both the experimental sample and the observational sample. Both samples are individuals who sought out ReHire services in the same geographic locations in Colorado. Both sets of individuals, therefore, would have likely had similar expected outcomes had they faced similar labor markets.

An interesting feature of our data, relative to the setup in Athey et al. (2019), is that our experimental sample faced different labor market conditions than the observational sample. The experimental sample was exposed to labor market disruptions posed by the COVID-19 pandemic much sooner after their ReHire application. For this reason, any actual experimental effects that materialize may differ from those estimated with the surrogate index approach.

This feature of the data allows us to address an important policy-relevant research question: what would the effects of ReHire had been had the pandemic not occurred? The surrogate predictions prior to quarter 19 are constructed using pre-COVID data. The interpretation of the surrogate index is that it predicts an individual's outcome in a labor market not yet affected by the pandemic. ITT effects using the surrogate index, thus, can be interpreted as the effects of ReHire had the pandemic not occurred.

We implement the surrogate index approach using the following process:

1. Use the observational (pre-RCT) sample to estimate the following regression:

$$y_{it} = \sum_{k=-12}^{4} \beta_t^{Emp,k} Emp_{ik} + \sum_{k=-12}^{4} \beta_t^{Earn,k} Earn_{ik} + \epsilon_{it} \tag{A-3}$$

where y_{it} is an outcome—an indicator for formal-sector employment or formal-sector earnings—measured for person i in the observational sample for quarters $t \in [5, 30]$. Emp_{ik} are indicators for quarterly employment and $Earn_{ik}$ are formal-sector earnings, measured in quarters $k \in [-12, 4]$.

2. Use estimates of $\beta_t^{Emp,k}$ and $\beta_t^{Earn,k}$ to predict $\hat{y}_{it}$ in the RCT sample.

3. Construct a surrogate outcome S_{it}. Let $q_i(t)$ be the calendar quarter that an outcome in relative quarter t is or will be measured for person i. We construct two versions of this surrogate outcome:

   (a) Replace only COVID-19 (Q1 2020) and later quarters:

$$S_{it}^{\text{COVID}} = \begin{cases} y_{it} & q_i(t) \leq \text{ Q4 2019} \\ \hat{y}_{it} & q_i(t) > \text{ Q4 2019} \end{cases} \quad \text{for } t \in [5, 30]$$

   (b) Surrogate long-term (LT) outcomes for all individuals:[68]

$$S_{it}^{\text{LT}} = \hat{y}_{it} \text{ if } t \geq 5 \text{ for } t \in [5, 30]$$

4. Estimate the effect of ReHire on S_{it}^{COVID} or S_{it}^{LT} using Equation 1, selecting controls using the post-double selection LASSO procedure (Belloni, Chernozhukov and Hansen, 2014).

Table A-25 provides estimates of the effects of ReHire on employment (columns 1 through 6) and earnings (columns 7 through 12) using the surrogate index approach. In columns (1)–(2) and (7)–(8), we reproduce control group means and the experimental effects using the observed data. In columns (3)–(4) and (9)–(10), we report control group means and effects using S_{it}^{COVID}. In columns (5)–(6) and (11)–(12), we report control group means and effects using S_{it}^{LT}. Each row reports the estimates using the outcome measured in a different quarter relative to application. The final two rows report results averaging across either the observed period (Q5–Q16) or the not yet observed period (Q17–Q30). Standard errors that come from 1,000 bootstrap samples of the data are reported in parentheses.

As noted in Section III.A, observed ITT effects on employment and earnings during quarters 5 through 11 are small but consistently above 2 percentage points or around \$100. Between quarters 12 and 16, observed effects are small and close to 0.

We find evidence that access to ReHire would have increased employment by 8 percent and earnings by 9 percent during the three years following program participation had the pandemic not occurred. When replacing COVID-affected data with the surrogate index, we estimate an average employment effect during Q5 through Q7 of 3.4 percentage points ($p < 0.01$) relative to a control group mean of 33.9 percent. This increase would have occurred alongside a \$198 increase in average quarterly earnings ($p < 0.05$) relative to a control group mean of \$2,216.

This approach also allows us to predict the effect of ReHire beyond the period during which we have a balanced panel. Predicted long-term effects, five to 8 years (Q17–Q30) after random assignment are small, but persistent. The surrogate evidence suggests the long-term effect of ReHire access on employment to be 2.3 percentage points ($p < 0.05$) and on earnings to be just under \$150 per quarter ($p < 0.10$), which is roughly a 7 percent increase. While suggestive that the program could have had continued impact in the absence of the program, the observed impacts in the third year following random assignment suggests that these employment and earnings gains will not materialize, potentially because of the labor market disruptions posed by the COVID-19 pandemic.

The results in columns (6) and (12) suggest that the surrogate index approach does a reasonable job of predicting actual treatment effects. During the early quarters (Q5–Q8) when the majority of the experimental sample had not yet been affected by the pandemic, the approach that replaces all data with the surrogate index finds effects similar in magnitude to the observed effects. For example, the actual effects on employment during that time period range from 2 to 3.8 percentage points, and the surrogate effects similarly range between 2.2 and 4 percentage points.

[68] Note that $S_{it}^{\text{LT}} = S_{it}^{\text{COVID}}$ for $t \geq 19$ because all individuals in the the pre-RCT observational sample were experiencing the COVID-19 pandemic by the 19$^{\text{th}}$ quarter following random assignment.

Table A-25: ITT Effect of ReHire on Formal-Sector Employment and Earnings in Colorado with Surrogate Outcomes, By Quarter

	Any Employment						Earnings					
	Observed Data		Surrogate (COVID)		Surrogate (Q5+)		Observed Data		Surrogate (COVID)		Surrogate (Q5+)	
	Control Mean	ITT Effect Controls	Control Mean	ITT Effect Controls	Control Mean	ITT Effect Controls	Control Mean	ITT Effect Controls	Control Mean	ITT Effect Controls	Control Mean	ITT Effect Controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Quarter 5	0.509	0.023	0.509	0.021	0.500	0.032^{+}	$2,250	$154	$2,244	$154	$2,266	$148
		(0.020)		(0.020)		(0.017)		(126)		(126)		(107)
Quarter 6	0.500	0.020	0.497	0.024	0.461	0.040**	$2,258	$154	$2,244	$158	$2,164	$173^{+}
		(0.020)		(0.019)		(0.015)		(128)		(125)		(98)
Quarter 7	0.485	0.025	0.481	0.031^{+}	0.448	0.027*	$2,222	$191	$2,200	$227^{+}	$2,002	$189*
		(0.019)		(0.018)		(0.013)		(129)		(124)		(84)
Quarter 8	0.450	0.038*	0.441	0.049**	0.432	0.022^{+}	$2,223	$223	$2,162	$287*	$1,917	$151^{+}
		(0.019)		(0.018)		(0.013)		(136)		(126)		(81)
Quarter 9	0.453	0.024	0.448	0.048**	0.431	0.037**	$2,302	$121	$2,214	$299*	$2,074	$211*
		(0.019)		(0.018)		(0.013)		(139)		(125)		(88)
Quarter 10	0.434	0.020	0.437	0.034*	0.429	0.035**	$2,243	$119	$2,267	$221^{+}	$2,235	$204*
		(0.020)		(0.017)		(0.013)		(139)		(124)		(94)
Quarter 11	0.414	0.020	0.417	0.044**	0.422	0.033**	$2,202	$117	$2,154	$267*	$2,139	$200*
		(0.020)		(0.016)		(0.012)		(141)		(114)		(85)
Quarter 12	0.408	0.009	0.424	0.044**	0.421	0.037**	$2,230	$12	$2,224	$196^{+}	$2,122	$213**
		(0.019)		(0.015)		(0.011)		(144)		(115)		(82)
Quarter 13	0.409	-0.005	0.432	0.028^{+}	0.426	0.029**	$2,260	-$79	$2,240	$177	$2,164	$206*
		(0.019)		(0.015)		(0.011)		(148)		(112)		(82)
Quarter 14	0.390	0.007	0.423	0.039**	0.428	0.022*	$2,204	$12	$2,203	$240*	$2,140	$191*
		(0.019)		(0.014)		(0.011)		(145)		(107)		(82)
Quarter 15	0.387	-0.004	0.403	0.028*	0.413	0.016	$2,197	$44	$2,157	$221*	$2,164	$146^{+}
		(0.019)		(0.014)		(0.011)		(146)		(103)		(80)
Quarter 16	0.378	-0.004	0.410	0.017	0.414	0.016	$2,229	-$23	$2,209	$165^{+}	$2,223	$140
		(0.019)		(0.012)		(0.011)		(147)		(94)		(86)
Average Q5–Q16	0.435	0.014	0.443	0.034**	0.435	0.029**	$2,235	$87	$2,210	$218*	$2,134	$181*
		(0.015)		(0.012)		(0.011)		(116)		(93)		(79)
Average Q17–Q30			0.340	0.023**	0.340	0.023**			$2,141	$155*	$2,144	$152*
				(0.009)		(0.009)				(76)		(76)

Notes: Data source is administrative UI earnings data from CDLE. Each row represents outcomes measured in a different quarter relative to ReHire application. Formal employment is defined as having UI-covered earnings in Colorado greater than $0 in a given quarter. Earnings from a ReHire-sponsored transitional job are covered by the UI system and are thus counted as formal sector employment. The dependent variable in columns (1) and (2) is an indicator for formal-sector employment and columns (5) and (6) is an individual's UI-covered earnings. The dependent variable in columns (3) and (4), as well as (7) and (8) are the surrogate outcomes as specified by S_{it}^{COVID} in the preceding section. Columns (1), (3), (5), and (7) report the mean of the outcome for the control group, respectively. Columns (2), (4), (6), and (8) report the coefficients on treatment indicators, selecting controls from a high-dimensional set of baseline characteristics using the post-double selection LASSO procedure from Belloni, Chernozhukov and Hansen (2014). Standard errors reported in parentheses are constructed using 1,000 bootstrap samples and take into account the uncertainty from the first-stage prediction.
**0.01, *0.05, $^{+}$0.10 significance levels

A.17 MVPF

In order to better understand the relative costs and benefits of ReHire, we construct an estimate of the Marginal Value of Public Funds (MVPF) following Hendren and Sprung-Keyser (2020). The MVPF compares the aggregate willingness to pay for a particular policy to the cost to provide that policy net of any fiscal externalities. A MVPF of 0 suggests small benefits relative to the overall program cost. An MVPF greater than 1 suggests that aggregate benefits exceed costs. Finally, if the fiscal externality (i.e., savings from additional taxes or reduced transfers) exceeds program costs such that the net cost is negative, then the MVPF is defined to be ∞, which means the program more than pays for itself.

We measure the willingness to pay (WTP) of ReHire as the change in the present value of future earnings net of taxes and transfers. The WTP depends on the time horizon over which earnings gains are assumed to persist. We report different scenarios: effects observed over the four years following random assignment and reported in Section III.A; predicted effects in the absence of the COVID-19 pandemic measured during the same time horizon and reported in Section A.16; predicted long-term effects through the 8 years following random assignment; and projecting earnings gains that persist throughout the remainder of a worker's life (18 years). Our baseline estimates use an annual discount factor of 3 percent and assume the typical ReHire applicant is 47 years old at baseline. We follow Hendren and Sprung-Keyser (2020) in imputing tax and transfer rates based on CBO estimates tied to various incomes relative to the federal poverty level.[69]

Information on program costs are provided by CDHS. Our baseline estimate of the per person cost of ReHire is \$5,932, which is based on 2015–2018 program expenditures spread across the 1,385 individuals placed into the treatment group. This estimate assumes that treatment group members do not forgo any non-ReHire services that they would have received in the absence of the program. Many service agencies provide additional re-employment services outside of the scope of ReHire (e.g., two agencies in the study are the local American Jobs Centers). As we reported in Table A-2, the average participant received \$2,101 in direct cost services—roughly \$1,700 related to wage subsidies and \$360 from supportive services—which would leave roughly \$3,800 in indirect costs from operating the program. In considering cost-effectiveness, we present results based on alternative assumptions about the additional cost of providing ReHire services by assuming the control group received services that were proportional to the indirect costs of providing ReHire.

Table A-26 reports MVPF estimates across different scenarios that vary the time horizon of earnings impacts, discount rates, assumptions about program costs, and potential improvements in program targeting. Columns (1), (3), and (5) report estimates of the MVPF, WTP, and cost net of fiscal externalities, respectively. 95 percent confidence intervals in columns (2), (4), and (6) are based on 10,000 bootstrap samples.

The estimated MVPF of ReHire varies depending on the time horizon over which earnings gains persist (Panel A). In our most conservative estimate, which relies only on estimated experimental effects and assumes that program effects fall to zero after quarter 16, we estimate the MVPF of ReHire to be 0.320 [-0.104, 0.860]. Under this scenario, the present value of the ReHire earnings impacts net of taxes and transfers totals \$1,788. The key inputs to these calculations are the estimated program effects on quarterly earnings reported in Appendix Table A-4, column 6. As individuals experienced increases in their earnings, they also paid more in taxes. These additional taxes paid over the two years following ReHire decrease the net cost of the program to \$5,593, or by roughly \$340. The remainder of Panel A provides MVPF estimates under alternative assumptions about replacing data affected by the COVID-19 pandemic with predicted surrogates and how long earnings gains persist beyond the two years after application. We first replace post-2019 data affected by the pandemic with predicted outcomes from the surrogate index approach, which are presented in Appendix Table A-25. Replacing pandemic-affected data while holding constant the time horizon of earnings increases the MVPF estimate to 0.564 [0.148, 1.073]. Some of this gain in cost

[69] We use the 2016 threshold for one person under age 65, \$12,486. Roughly 70 percent of our sample have no kids in the household and about half live alone.

efficiency comes through a higher WTP ($2,984) and some through a slightly lower net cost of the program ($5,293). Next, we extend the impacts through the 30th quarter using the surrogate index approach, and find that the MVPF increases to 0.882. Finally, we assume that the predicted relative earnings gains in quarters 27 through 30 persist through retirement (i.e 18 years after application). Projecting this gain into the future, taking into account the evolution of earnings across different ages, gives an MVPF estimate of 1.741 [0.286, 4.687].[70]

The remainder of Table A-26 presents estimates based on changes in other assumptions. Each panel takes as its starting point the scenario that combines experimental impacts (Q0–Q8) with surrogate impacts (Q9–Q18). Panel B shows that varying the discount rate from 0 percent to 10 percent leads to MVPF estimates ranging from 0.698 to 0.989. Panel C presents results under different assumptions about cost. Our baseline cost measure—$5,932—assumes that the cost of services received by the control group is $0. While the control group was not eligible for ReHire-funded services, they were eligible for other services that the local service agency provided, as well as other programs in the area (e.g., WIOA-funded programs or programs aimed at veterans) potentially provided by other service providers. Estimates range from 0.952 to 3.864 depending on whether we make adjustments to account for contamination in the control group (0.952), assume that the control group receives services equivalent to 50 percent of the indirect costs of ReHire (1.437), or assume the control group receives services equivalent to the entire indirect costs of ReHire (3.864).[71,72]

Finally, we ask what the MVPF could be if the program were able to improve the share of participants who were hired into unsubsidized employment at their host site. Section IV documented that post-program impacts are concentrated among the 15 percent of the treatment group who work a transitional job and then transition from subsidized to unsubsidized employment with the same employer. We construct a set of weights that holds constant the relative size of the treatment group but increases the share of treatment individuals hired by their TJ job site by 50 percent to 22 percent. We re-estimate experimental impacts and long-term surrogate effects using this weighted sample and find that this change would increase the MVPF to 1.364.

This analysis has the limitation that it does not explicitly account for some likely costs and benefits that could affect the MVPF. First, our measure of WTP does not include any utility implications from a labor-leisure tradeoff. We do find that earnings gains occur alongside an increased employment rate, which might suggest that changes in earnings overestimate a participant's WTP. However, stable employment may provide a worker with improvements in mental and physical well-being such that earnings gains represent a lower bound in WTP. Evidence from the follow-up survey shows the treatment group experienced improvements in well-being as measured by subjective well-being and self-report physical and mental health (Section III.B). Second, there could be other public finance implications that we have not measured in this study. In Section III.A, we ruled out reductions in participation in government benefit programs like SNAP and TANF. However, one fifth of our sample reported some prior involvement with the criminal justice system, two fifths of the sample reported ever being homeless, and one third of employed follow-up survey respondents reported having employer-provided health insurance. Moreover, many participants are single parents or non-custodial parents where positive benefits might spillover to children within the households. The MVPF would be larger in the event that ReHire reduces involvement with the criminal

[70]When making these projections, we follow Hendren and Sprung-Keyser (2020) in measuring the age-earning profile in the 2014–16 American Community Survey (ACS) downloaded from IPUMS Ruggles et al. (2020). Specifically, we calculate the average earnings at each age for adults with 2 or fewer years of post-secondary education. We assume that the relative magnitude of the earnings gain, roughly 8 percent, stays constant until age 65, and project the evolution of earnings in the control group using the age-earnings profile estimated in the ACS.

[71]Appendix Figure A-5 shows the share of the control group in any given quarter employed at a ReHire service agency, which proxies for transitional job placement. We assume these individuals receive services equal to the typical ReHire participant.

[72]Direct costs are measured as transitional job wages and other services or supports that were directly billable to specific participants (e.g., gas cards, work uniforms, training tuition). Appendix Table A-2 reports average direct costs in the sample. Indirect costs are then assumed to be the per person program cost less average direct cost services.

justice system, reduces usage of shelter or other housing services, increases private insurance coverage, or improves outcomes to the next generation. Finally, our MVPF analysis does not explicitly account for the benefits accrued by the employer over the period during which they have a fully subsidized worker. The worker's marginal value of production is likely somewhere between zero and the worker's subsidized wage.

We also note that our partial equilibrium analysis does not consider general equilibrium effects that would occur if the program were to scale up more broadly. Subsidized jobs for program participants likely crowded out employment from other members of the community. For example, Lise, Seitz and Smith (2004) analyze the general equilibrium implications of the Canadian Self-Sufficiency Program, a wage subsidy paid to welfare recipients, and Crépon et al. (2013) experimentally estimate displacement from a job placement assistance for French youth. Our cost-benefit analysis does not account for these displacement effects.

How do these estimates compare to other similar job training or re-employment programs? Hendren and Sprung-Keyser (2020) construct MVPF estimates using reported impact estimates from a number of experimentally-evaluated programs. The typical job training program has an MVPF of 0.44 (Table II, Hendren and Sprung-Keyser, 2020) with a confidence interval that often does not rule out 0. For job training programs, their primary specification assumes that earnings gains do not persist beyond estimated effects given the presence of fadeout in the literature, which most closely aligns with our estimates of 0.32 and 0.56. The MVPF of ReHire exceeds that of Job Corps (0.15) and JobStart (0.20), and is within the confidence interval of the adult JTPA program [-0.21, 2.13]. More broadly, our estimates are largely in line with other programs targeting similar adult participants: unemployment insurance policies (0.43–1.03); disability insurance expansions (0.74–0.96); and the EITC (1.12–1.20).

Table A-26: Marginal Value of Public Funds

	MVPF		WTP		Net Cost	
	Estimate	CI	Estimate	CI	Estimate	CI
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Time Horizon of Impacts						
Q0–Q16: Experimental Impacts	0.374	[-0.082, 0.921]	$2,066	[-508, 4,526]	$5,524	[4,917, 6,138]
Q0–Q16: Surrogate Impacts, replace COVID	0.612	[0.183, 1.136]	$3,208	[1,052, 5,379]	$5,239	[4,698, 5,765]
Q0–Q30: Surrogate Impacts, replace COVID and not yet observed	0.955	[0.249, 1.930]	$4,672	[1,413, 7,935]	$4,890	[4,090, 5,677]
18 Years: Project COVID Surrogate Impacts	1.869	[0.342, 4.924]	$7,981	[1,953, 13,971]	$4,270	[2,827, 5,700]
Panel B: Discount Rates						
0%	1.072	[0.262, 2.246]	$5,127	[1,477, 8,766]	$4,781	[3,897, 5,663]
3%	0.955	[0.249, 1.930]	$4,672	[1,413, 7,935]	$4,890	[4,090, 5,677]
5%	0.889	[0.235, 1.763]	$4,405	[1,337, 7,437]	$4,954	[4,212, 5,693]
10%	0.754	[0.208, 1.440]	$3,838	[1,194, 6,435]	$5,091	[4,462, 5,725]
Panel C: Cost Assumptions						
Per person cost : $5,932	0.955	[0.249, 1.930]	$4,672	[1,413, 7,935]	$4,890	[4,090, 5,677]
Net Control Contamination : $5,570	1.032	[0.266, 2.117]	$4,672	[1,413, 7,935]	$4,528	[3,728, 5,316]
Direct + 0.5 x Indirect Costs : $4,016	1.571	[0.376, 3.616]	$4,672	[1,413, 7,935]	$2,974	[2,174, 3,761]
Direct Costs Only : $2,100	4.417	[0.768, 28.536]	$4,672	[1,413, 7,935]	$1,058	[257, 1,845]
Panel D: Program Improvement						
50% Increase in Hired by TJ Rate	1.460	[0.583, 2.672]	$6,502	[3,075, 9,768]	$4,455	[3,655, 5,279]

Notes: Data source is administrative UI earnings data from CDLE and program data from CDHS. The sample includes all ReHire applicants who applied between 7/2015 and 12/2018. Columns (1), (3), and (5) report estimates of the marginal value of public funds (MVPF), willingness to pay of the program, and per person program cost net of fiscal externalities, respectively. Columns (2), (4), and (6) report 95% confidence intervals that come from 1,000 bootstrap trials of the individual-level data. Our baseline scenario uses a 3% annual discount rate and a per treatment group member cost of $5,932. Panel A reports estimates that vary the time horizon of earnings impacts: only the experimental impacts reported in Table A-4; keeping time horizon the same but replacing quarters affected by COVID-19 pandemic with quarterly surrogate estimates (Table A-25, column 10); extending the time horizon beyond Q16 by replacing quarters affected by the COVID-19 pandemic and by filling in not yet observed quarters with quarterly surrogate estimates and (Table A-25, column 10, final row); and projecting the previous estimates through age 65 assuming a constant relative earnings impact. The remaining panels vary assumptions using experimental and COVID-19 surrogate impact estimates (Q0–Q30) as the time horizon of impacts. Panel B varies the annual discount rate. Panel C assumes different assumptions about program costs. The estimate in Panel D comes from a re-weighted sample that increases the share of the treatment group who worked at a transitional job and transitioned to unsubsidized work with the same employer by 50%. See Appendix Section A.17 for additional details.

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B Theoretical Appendix – For Online Publication

B.1 Introduction

This appendix section provides a model that shows how ReHire's multiple mechanisms may affect in-program and post-program outcomes. The model follows in the tradition of employer learning where firms initially see only a noisy signal of an employee's productivity but more information is revealed as the employee works for the employer (Jovanovic, 1979, 1984; Altonji and Pierret, 2001; Pries and Rogerson, 2005, 2022). We model hiring and retention decisions for workers who start as unemployed. We maintain the following simplifying assumptions throughout the discussion:

- Working for the minimum wage provides greater utility than unemployment. This assumption distinguishes the theory of change from other contexts that leverage wage subsidies to solve the problem of a high reservation wage (e.g the Canadian Self-Sufficiency Project).
- At the start of each period, workers without a surviving match meet exactly one firm with probability 1.
- There is no on-the-job search.

B.2 Hiring and Retention Decisions

Unemployed workers and employers meet at the start of a period t, and the firm decides whether to hire the worker and pay them an exogenously given wage w. We treat the wage costs of hiring a worker as exogenous because many of the jobs in which the study population work are paid the state minimum wage, and, importantly, transitional job participants were also paid the prevailing minimum wage during their internship. When deciding whether to hire the worker, firms must make their decision based on imperfect information. Although they can form an expectation of a worker's match quality, they cannot know match quality for certain unless they hire the worker and observe their performance on the job.

We use a two-period discrete-time structure for tractability. If the worker is hired at the beginning of period t, they remain with that employer until the end of the period. The firm receives the worker's productivity from that period and must pay wages w, as well as startup costs c. At the end of period t, the firm decides whether to retain the worker. They make this decision with full information about the worker's actual productivity. If they are retained, they remain employed with that employer for period $t+1$, with the employer again receiving the worker's productivity and the worker receiving the wage. If the worker is not retained, the relationship ends and the worker and firm can consider new potential matches.

B.2.1 Hiring Decisions

Firms are forward-looking and discount future payoffs at rate $\beta \in (0, 1]$. When deciding whether to hire a worker, firms consider both the short-run net surplus (expected productivity minus costs) and the option value that comes from learning the true match quality at the end of the initial period if the match is formed.

Firms form an expectation of the worker's productivity in the initial period by combining information from the employee's application with an unbiased but noisy signal of the match quality. Employers first observe the worker's application quality group $g_{it} \in \{H, L\}$ by reviewing materials such as a resume and cover letter. Workers in group H have strong application materials, including good work histories with few gaps; workers in group L have weaker application materials and may have gaps in employment and/or may have been unemployed for a long time. The distribution of true productivity for a worker from group g is known and normally distributed:

$$y_g \sim \mathcal{N}(\bar{y_g}, \sigma_y^2) \tag{B-1}$$

Employers receive an additional signal after meeting and interviewing the worker. The signal (s_{ijt}) is worker i's true productivity if they were to be hired at employer j at the start of period t (y_{ijt}) plus a mean-zero normally distributed error term with known variance that is independent of y:

$$s_{ijt} = y_{ijt} + \eta_{ijt} \tag{B-2}$$

with

$$\eta_{ijt} \sim \mathcal{N}(0, \sigma_\eta^2) \tag{B-3}$$

After receiving the signal, the firm uses Bayesian updating to form an expectation of the worker's productivity as a weighted average of the worker's application quality group mean and the signal of the worker's true productivity if they were to be hired at firm j:

$$\mathbf{E}[y_{ijt}] = (1-\gamma)\bar{y}_{g_{it}} + \gamma s_{ijt}, \tag{B-4}$$

with weights that depend on the variance of the group's true productivity and the variance of the noise in the signal: $\gamma = \frac{\sigma_y^2}{\sigma_y^2+\sigma_\eta^2}$.

Hiring a worker also provides option value, $V_{ijt}^{\text{cont}}(s_{ijt}, g_{it})$, because at the end of the initial period, the next period's match quality is revealed and the firm has the ability to retain productive workers. The next period's match quality may differ from the initial period's, especially if workers become more productive with experience on the job.

$$\underbrace{V_{ijt}^{\text{cont}}(s_{ijt}, g_{it})}_{\text{option value of experimentation}} \equiv \beta\,\mathbb{E}\big[(y_{ij,t+1} - w)^+ \mid s_{ijt}, g_{it}\big]\,. \tag{B-5}$$

For tractability, we represent this option value as the expected excess productivity above the wage in the next period, conditional on the surplus being positive—$(y_{ij,t+1} - w)^+$—such that the worker is retained.[73] The firm forms its expectation of this value based on information available at the time of the hiring decision—the group and the signal. The firm's expectation of period $t+1$ productivity is increasing in the period t signal because it expects at least a portion of match quality to persist into period $t+1$. This exact structure of the continuation value is not needed to reach the qualitative conclusions from the model. This continuation value could also be interpreted as incorporating an indefinitely continuing relationship that may end exogenously, or it could be modified to allow for productivity growth from job tenure. The key feature is that the option value is increasing in expected productivity at the start of the next period ($y_{ij,t+1}$).

The firm chooses to hire workers ($h_{ijt} = 1$) when the sum of the expected period t profits and the option value are greater than zero. Expected period t profits are the expected productivity less the wage w and startup costs c that must be paid during the first period of a new employer-employee match. The hiring rule based on the signal and group is therefore:

$$\begin{aligned} h_{ijt}\,[s_{ijt}, g_{it}] = 1 \iff\ & (1-\gamma)\bar{y}_{g_{it}} + \gamma s_{ijt} - w - c \\ & + \beta\,\mathbb{E}\big[(y_{ij,t+1} - w)^+ \mid s_{ijt}, g_{it}\big] > 0, \end{aligned} \tag{B-6}$$

with h_{ijt} an indicator for whether the firm j chooses to hire worker i in period t. Because the match value is monotonically increasing in the signal s_{ijt}, the hiring rule implicitly defines a group-specific cutoff s_g^* above which the worker is hired and below which they are not.

[73] That is, $(x)^+ \equiv \max\{0, x\}$.

B.2.2 Retention Decision Depends on Known Match Quality

At the end of the initial period, employers decide whether to continue employing workers who were previously hired. For these workers, the productivity of the worker-firm match for period $t+1$ is fully revealed at the end of period t. Because startup costs no longer apply, the employer retains the worker if their productivity is above the cost of continuing to employ them.

$$r_{ij,t+1} = 1 \iff y_{ij,t+1} - w > 0 \tag{B-7}$$

Again, for clarity, this retention rule could be adjusted to allow for further option value from learning about future shocks to productivity and/or from expected gains to productivity through experience-based human capital. Although these adjustments would change the precise level of period $t+1$ productivity needed for retention, the retention rule would continue to have a cutoff structure, with workers retained if and only if their known match quality is above a specific threshold.

B.3 ReHire Participation Affects Employment Outcomes

We next use this basic setup to understand how observed employment outcomes for the ReHire study population can be affected by access to the ReHire program. For concreteness, we consider employment outcomes in two specific periods: period 0, which corresponds to the period of time during which the treatment group receives services, and period 1, which is the period after the treatment group leaves the program. We assume that periods correspond with the timing described above with the worker's true productivity revealed at the end of period 0.

A worker's observed employment outcomes depend on firms' hiring and retention decisions. Specifically, we assume that a job seeker begins the initial period unemployed and matches with one potential employer who decides whether to hire them. Because the worker is unemployed to start, their period 0 employment outcome is equivalent to the initial firm's hiring decision.

A worker who was not hired in period 0 or who was hired but was not retained meets a new employer at the start of period 1. If they are hired by this second firm, they are employed for period 1. If not, they remain unemployed for the period. Figure B-1 provides a summary of the paths to employment or non-employment in the post-program period (Period 1). We next consider how access to the ReHire program affects hiring and retention decisions and thus affects the employment outcomes shown in Figure B-1.

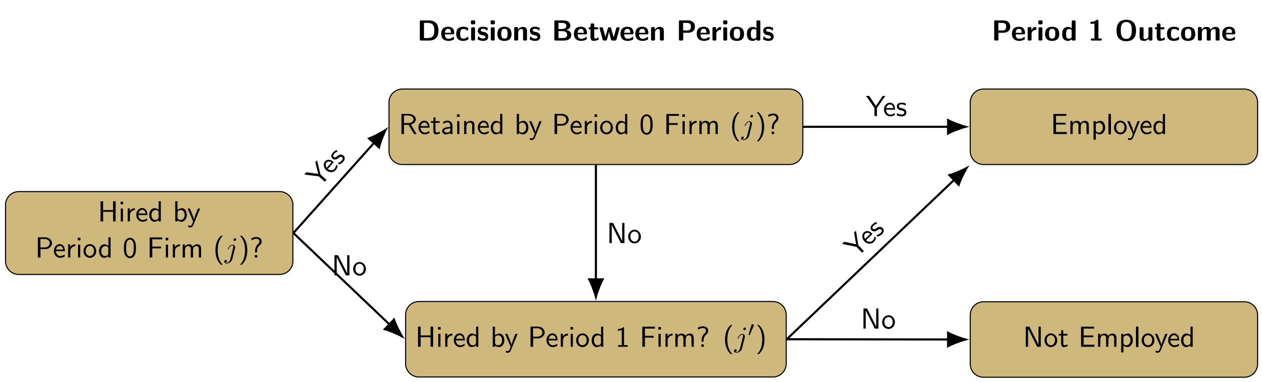

Figure B-1: Decision Timing and Post-Program Outcomes

B.3.1 Treatment Affects Period 0 Hiring Decision

Participating in ReHire ($T_i = 1$) can affect the initial hiring decision by increasing expected period 0 profits and by potentially changing the option value. Case workers provide resume assistance, which can change the worker's perceived application group quality, even with no change to their work history. By addressing barriers and providing coaching, the program can also improve the employee's true productivity. Each of these changes would improve the expected productivity in period 0.[74]

Expected productivity in the initial period as a function of treatment status is therefore:

$$\mathbf{E}[y_{ij0}(T_i)] = (1-\gamma)\bar{y}_{g_{i0}(T_i)} + \gamma s_{ij0}(T_i) \tag{B-8}$$

ReHire participation also reduces the costs of employing a worker in the initial period. Employers hiring a ReHire participant pay no wage costs in the initial period. Further, the local contractor operating as the employer of record eliminates a portion $\lambda \in (0,1)$ of startup costs, such as training, supervision, and human resources costs such as paperwork, payroll, and liability insurance.[75] Period 0 costs as a function of treatment status are therefore $(1-T_i)w + (1-\lambda T_i)c$.

Finally, ReHire participation can affect the option value of hiring a worker. The option value depends on the signal and the group, both of which depend on treatment status. Further, the mapping of the signal and the group to expected future productivity may also depend on the treatment status. For example, employers may believe that workers in the treatment group are more likely than workers in the control group to improve their true productivity over time (due to supports and/or coaching). Of course, the opposite could be true if employers expect ReHire participants to regress once supports end.

The option value as a function of treatment status is therefore:

$$\beta\,\mathbb{E}\big[(y_{ij1}-w)^+ \mid s_{ij0}(T_i), g_{i0}(T_i), T_i\big] \tag{B-9}$$

Combining these pieces yields a hiring rule that depends on treatment status in multiple ways:

$$\begin{aligned} h_{ij0}\left[s_{ij0}(T_i), g_{i0}(T_i), T_i\right] = 1 \iff\ & (1-\gamma)\bar{y}_{g_{i0}(T_i)} + \gamma s_{ij0}(T_i) - (1-T_i)w - (1-\lambda T_i)c \\ & + \beta\,\mathbb{E}\big[(y_{ij1}-w)^+ \mid s_{ij0}(T_i), g_{i0}(T_i), T_i\big] > 0. \end{aligned} \tag{B-10}$$

B.3.2 Treatment Affects Retention Decision

Workers in the treatment group are no longer eligible for subsidized wages and the startup costs no longer apply to either group, so the decision to retain a worker $r_{ij1}(T_i)$ depends only on whether worker i's productivity with employer j is above the wage. Program effects on the retention decision therefore operate through program effects on period 1 productivity:

$$r_{ij1}(T_i) = 1 \iff y_{ij1}(y_{ij0}(T_i), h_{ij0}(T_i), T_i) - w > 0 \tag{B-11}$$

The productivity of a worker-firm match in period 1 is a function of the true productivity in period 0, the worker's period 0 employment (hiring) status, and whether the worker is a member of the treatment group. Workers who are hired may gain productivity with the firm beyond their initial productivity through mentoring and on-the-job learning. Members of the treatment group may additionally improve their productivity through services they receive, such as job coaching or cash or in-kind benefits that address barriers with lasting benefits into period 1.

[74] Note that we assume that treatment does not affect the noise in the signal and thus any treatment effects on the signal come through true productivity, i.e. $s_{ij0}(T_i) = y_{ij0}(T_i) + \eta_{ij0}$.

[75] Employers hiring a ReHire participant pay no payroll costs, and face no risk from liability or worker's compensation. Their UI rating is also unaffected if they choose to dissolve the match at the end of the initial period.

B.3.3 Treatment Affects Period 1 Hiring Decision

A worker who was not retained or who was not hired in period 0 meets a new potential employer j' who decides whether to hire them (see Figure B-1). This decision is based on the worker's period 1 group and a new unbiased signal of the worker's productivity with the new firm in period 1. The firm decides whether to hire this applicant using a decision rule similar to Equation (B-10). Key differences are that the identity of the firm has changed from j to j', the group is potentially updated, and the worker provides an updated signal. In addition, wage and startup costs no longer depend on treatment status.

$$\begin{aligned} h_{ij'1}\left[s_{ij'1}(T_i), g_{i1(T_i)}, T_i\right] = 1 \iff & (1-\gamma)\bar{y}_{g_{i1(T_i)}} + \gamma s_{ij'1}(T_i) - w - c \\ & + \beta\,\mathbb{E}\left[(y_{ij'2} - w)^+ \mid s_{ij'1}(T_i), g_{i1(T_i)}, T_i\right] > 0. \end{aligned} \tag{B-12}$$

The worker's period 1 group may be different than their period 0 group depending on whether the worker was hired in period 0, ($g_{i1} = f(g_{i0}(T_i), h_{ij0}(T_i))$). Workers who were in the high group in period 0 and were hired remain in the high group. Workers who were in the low group and were not hired remain in the low group. Not being hired may move high-group workers to the low group and being hired may move low-group workers to the high group.

$$1 = Pr(f(H,1) = H) \geq Pr(f(H,0) = H) \tag{B-13}$$

$$Pr(f(L,1) = H) \geq Pr(f(L,0) = H) = 0 \tag{B-14}$$

As in period 0, the signal is a noisy measure of true productivity, which is potentially affected by treatment directly, through treatment's effect on period 0 productivity, or through treatment's effect on the period 0 hiring decision:

$$s_{ij'1}(T_i) = y_{ij1}(y_{ij0}(T_i), h_{ij0}(T_i), T_i) + \eta_{ij'1}. \tag{B-15}$$

The period 1 signal ($s_{ij'1}$) can be different than the period 0 signal (s_{ij0}), both because of differences in true productivity and because the worker receives a new draw from the noise distribution. The true productivity can be different either because of worker-level changes over time or because the identity of the firm changed, which potentially affects that worker's fit with the job. Worker-level changes include human capital improvements, changes in work barriers, and new productivity shocks that affect their productivity at all employers.

B.4 Employment Outcomes depend on T

Within this framework, ReHire participation can affect employment outcomes in both periods through multiple channels.

B.4.1 Employment Rates During the Program

During Period 0, the treatment group receives a bundled intervention, which improves their chances of being hired (and thus employed) for four reasons as seen in Equation (B-10): 1) the subsidy improves the net profitability of forming a match by removing monetary costs completely and by reducing startup costs; 2) case management, coaching, and administrative and financial supportive services may reduce barriers and could increase the worker's productivity, which would improve the signal the employer receives prior to making the hiring decision; 3) assistance with application materials may change g_{i0} from L to H, holding constant true productivity; and 4) treatment may improve the firm's belief that the match will be revealed to be profitable in the future. Because the intervention is bundled and there is only one path to employment in period 0, is not possible to disentangle the importance of each mechanism, but there is a clear prediction that program participation will increase employment (i.e., $h_{ij0}(T_i = 1) \geq h_{ij0}(T_i = 0)$).

B.4.2 Employment Rates After the Program

A worker is observed to be employed in period 1 if they are hired in period 0 by firm j and retained or if they enter period 1 unmatched but are hired by the new firm they meet (j').

$$Emp_{i1} = \max\{h_{ij0} \cdot r_{ij1}, h_{ij'1}\} \tag{B-16}$$

To understand the program's treatment effects on participants, it is useful to consider potential outcomes at the multiple stages shown in Figure B-1. In order for individual i to experience a positive treatment effect on employment, they must not end up employed through any of the available paths shown in Figure B-1 when untreated and must become employed through one of these paths when treated. Letting $Emp_{i1}(T_i)$ denote individual i's period 1 employment status under treatment status T_i, that individual's treatment effect can be written as:

$$Emp_{i1}(1) - Emp_{i1}(0) = \max\{h_{ij0}(1) \cdot r_{ij1}(1), h_{ij'1}(1)\} - \max\{h_{ij0}(0) \cdot r_{ij1}(0), h_{ij'1}(0)\} \tag{B-17}$$

B.5 Mechanisms Underlying Treatment Effects

Participation in the program can affect post-program outcomes through three mechanisms: (1) improvements in true productivity through human capital or barrier mitigation, (2) changing employers' beliefs through a change in the period 1 application quality group, and (3) information revelation. Below we discuss each of these channels in the context of the model and describe the paths to period 1 employment for which they are relevant.

B.5.1 Productivity improvements

The first mechanism through which the program can affect period 1 employment is by improving period 1 productivity, either by increasing the worker's human capital or by addressing a barrier to work. For workers who are hired in period 0, their period 1 productivity is fully observable to the employer who takes it into account when making the retention decision (Equation (B-11)). For workers who are not hired in the initial period or who are hired but not retained, improvements in true productivity at the second potential employer affect the hiring decision by improving the signal (Equation (B-12)).

An important determinant of improved human capital is additional work experience. Because being treated increases the likelihood that an individual works in period 0, it allows participants to gain additional skills from their time on the job through direct experience, training, and mentorship. Participants may also gain additional skills through coaching from their caseworker. Some increases in human capital may be firm-specific. In that case, the gains will be relevant only for the retention decision. Any increase in portable human capital will affect both the retention decision at the initial firm for those hired in period 0 and the hiring decision at the period 1 firm for those who meet a new employer.[76]

The program also provided supportive services to address barriers to employment. To the extent that the barrier reduction was durable (e.g. reinstating a driver's license, fixing a broken vehicle, purchasing work-apprpriate clothes), these period 0 services may affect the retention and hiring decision in period 1. Changes in productivity due to barrier reduction may also be differently valuable across employers.

[76]We use the term "portable" human capital to describe human capital that is relevant for both the period 0 and period 1 firms. It need not be "general" human capital that would be valued by all firms. For example, if both jobs are in the same occupation, portable human capital would include both occupation-specific and general human capital.

B.5.2 Potential employers' beliefs

Participating in the program can also lead to a positive treatment effect on period 1 employment by changing the future employers' beliefs about the applicant's quality group. This mechanism is relevant only for workers who are not working for employer j at the start of period 1. The program provides resume writing assistance as part of the bundled treatment designed to improve period 0 hiring rates. If this assistance moves a worker from the low to the high group, they may continue to benefit from this change when being evaluated by employer j'. Furthermore, the program may improve hiring in period 1 if the program helped the participant "fill a hole" in their resume. As discussed above in Equations (B-13) and (B-14), the probability that the worker's applicant-quality group is "High" (H) is weakly increasing in their period 0 employment. Because the program moves some participants from non-employment to employment in period 0, some individuals may have $\bar{y}_{g_{i1}(T_i=1)} = \bar{y}_H$ and $\bar{y}_{g_{i1}(T_i=0)} = \bar{y}_L$. These individuals can benefit from improved employer beliefs and thus a higher expected productivity when the second employer makes its hiring decision. Note that any increase in expected productivity through the application quality group is independent of any increase due to a change in true productivity.

B.5.3 Information revelation

The final mechanism through which the program can increase period 1 employment is information revelation. When the period 0 employer chooses to hire an applicant, the employer and the employee learn the profitability of a period 1 employer-employee match with certainty. For workers who would have been hired in the absence of the program ($h_{ij0}(0) = 1$), this mechanism is not operative because their true period 1 productivity with firm j is revealed regardless of their treatment status. Therefore, only participants who are hired in period 0 because of the program (i.e., $h_{ij0}(0) = 0$ and $h_{ij0}(1) = 1$) stand to benefit through the information revelation mechanism. Specifically, information revelation can improve employment outcomes for the subset of participants who are hired only because they participated in the program and whose period 1 productivity is revealed to make the match profitable (Equation B-11). In order for these individuals to experience a positive treatment effect, it must be the case that they would not have been hired by the subsequent employer j' if they had been untreated.

Importantly, employers decide whether to retain workers they hired in period 0 based on their period 1 productivity, which can also depend on the worker's treatment status (see Equation B-11). Therefore, the information revelation mechanism may work alone or in combination with the other mechanisms that improve productivity as discussed above.

For some workers, resolving the uncertainty about the match quality alone is sufficient to lead to period 1 employment with firm j. Consider the case of a worker whose period 1 productivity with firm j would be above the wage even if they had not been hired by firm j in period 0 and had not received any program services:

$$y_{ij1}(y_{ij0}(0), 0, 0) > w. \tag{B-18}$$

For these workers, the program improves their period 1 employment by changing the relevant decision from a hiring one based on incomplete information to a retention one based on full information. Even in the absence of any productivity improvements from on-the-job training or other supportive services, for example, these workers would be employable at firm j in period 1 if the firm knew their productivity with certainty.

For other workers, information revelation is necessary but not sufficient. These workers also experience a positive treatment effect on period 1 employment because they are hired and retained at firm j when treated and would not be hired by firm j' if they were untreated. Information revelation is not sufficient, however, because their untreated true productivity at firm j would have been too low for the period 1 match to be profitable, even if it had been revealed completely.

$$y_{ij1}(y_{ij0}(1), 1, 1) > w > y_{ij1}(y_{ij0}(0), 0, 0) \tag{B-19}$$

This set of workers needs both productivity improvements and information revelation to experience an employment improvement from the program. For example, they could gain firm-specific human capital from their work experience in period 0, which happens only because the subsidy changes the firm's initial hiring decision.

Whether information revelation is sufficient or merely necessary, the opportunity to be evaluated in period 1 based on true productivity at firm j rather than on an unbiased but incorrect expectation of productivity at firm j' can improve employment outcomes for a subset of workers.

B.6 Empirical Implications of the Model

B.6.1 Descriptive Analysis Motivation

We next consider what can be learned about the contribution of the various mechanisms through an examination of treatment effects among subgroups, which are defined by their potential outcomes with the period 0 employer. There are sixteen possible combinations (i.e., hiring and retention decisions in the treated and untreated states), but we eliminate cases where either a) the worker is retained without being initially hired (not possible) or b) treatment status harms the outcome.

Table B-1 provides a summary of the operative mechanisms for the six remaining possibilities grouped by the observed period 0 outcomes when treated, which are listed in the final column. The first four columns define each possibility by providing the worker's period 0 hiring and period 1 retention outcomes as a function of treatment status. The next column lists the mechanisms through which treatment can improve period 1 employment outcomes for the listed subgroup. The next column provides the average treatment effect for workers in that subgroup.

Workers in subgroups 1a–1c are hired and retained when treated. Because retained workers are employed in period 1, the treatment effects for this subgroup depend on the rate at which they would have been employed had they been in the control group. For workers in case 1c—those who are hired and retained regardless of treatment status—the treatment effect is 0 by construction. Therefore, the treatment effect among workers who are hired and retained when treated comes only from cases 1a and 1b and is larger when the untreated hiring rate at the new period 1 firm is lower.

Those who would not have been hired when untreated (1a) benefit from the information revelation mechanism and possibly from other productivity improvement mechanisms. As discussed above, some period 1 worker-firm matches may be profitable regardless of the worker's period 0 hiring status. For these workers, information revelation is sufficient to produce a positive treatment effect. Other workers within 1a are hired only because of the program and become productive enough to be retained only through other program mechanisms.

In subgroup 1b, workers benefit only from productivity improvements. These workers would have been hired regardless of their treatment status, but they are retained only when treated. For these workers, the additional supports offered by the program, including barrier resolution and job coaching, improve their productivity sufficiently to induce the firm to retain them. Note that this subgroup of workers gains experience-based human capital regardless of treatment status, and therefore improvements in productivity from human capital must come through job coaching or other program-specific sources.

In subgroups 2a and 2b, workers are never retained by their period 0 employer, regardless of treatment status, so they become employed only with the period 1 firm. Therefore, the treatment effects on period 1 employment for these two groups depend on the treatment effect on the period 1 hiring decision. When workers are hired in period 0 only when treated (2a), the additional work experience can improve productivity through portable human capital and by improving the application quality group in period 1. In both 2a and 2b, the program can improve the hiring rate at the second firm through coaching, lasting barrier resolution, and resume assistance.

The final subgroup is workers who are not hired by the firm they meet in period 0, regardless of their treatment status. For these workers, treatment effects also reflect differences in hiring rates at the period 1 firm. The mechanisms potentially operative for this group are the same as for case 2b because the program does not change period 0 employment status.

The differences in operative mechanisms across subgroups motivates our later descriptive analysis. Decomposing the average treatment effects by the treatment group's in-program hiring and retention outcomes can provide insight into the relative importance of the various mechanisms. Although these subgroups are not randomly assigned, we provide evidence that there is minimal selection into them, which allows us to approximate the subgroup-specific treatment effects by comparing the average outcomes in each group to the average outcomes in the control group. The descriptive analysis shows that nearly all of the treatment effects come from workers who are hired and retained by their host site. This result suggests that information revelation is a key mechanism behind the program's effects. Although other mechanisms may interact with information revelation, we find no evidence that portable human capital, barrier resolution, or signaling are sufficient to improve post-program outcomes on their own.

Table B-1: Relevant Mechanisms for Subgroups Based on Potential Period 0 Outcomes

Subgroup	$h_{ij0}(1)$	$h_{ij0}(0)$	$r_{ij1}(1)$	$r_{ij1}(0)$	**Mechanisms Operative**	**Subgroup Treatment Effect**	**Observed Period 0 Outcomes if Treated**
1a	1	0	1	0	Firm HC—Experience Firm HC—Coaching Portable HC—Experience Portable HC—Coaching Barrier Resolution Information Revelation	$1 - \mathbb{E}[h_{ij'1}(0)]$	Hired; Retained
1b	1	1	1	0	Firm HC—Coaching Portable HC—Coaching Barrier Resolution	$1 - \mathbb{E}[h_{ij'1}(0)]$	Hired; Retained
1c	1	1	1	1	None	$1 - 1 = 0$	Hired; Retained
2a	1	0	0	0	Portable HC—Experience Portable HC—Coaching Barrier Resolution Beliefs—Work History Beliefs—Resume Assistance	$\mathbb{E}[h_{ij'1}(1)] - \mathbb{E}[h_{ij'1}(0)]$	Hired; not Retained
2b	1	1	0	0	Portable HC—Coaching Barrier Resolution Beliefs—Resume Assistance	$\mathbb{E}[h_{ij'1}(1)] - \mathbb{E}[h_{ij'1}(0)]$	Hired; not Retained
3	0	0	0	0	Portable HC—Coaching Barrier Resolution Beliefs—Resume Assistance	$\mathbb{E}[h_{ij'1}(1)] - \mathbb{E}[h_{ij'1}(0)]$	Not Hired; not Retained

B.6.2 Additional Comparative Statics

Beyond motivating a decomposition of treatment effects by subgroups, the model also provides useful comparative statics that motivate our analysis of treatment effect heterogeneity. If we make the further assumption that period 1 productivity is normally distributed and the mean of the distribution is determined by the period 0 signal and group,

$$y_{ij1}|s_{ij0}, g_{i0}, T_i \sim \mathcal{N}(\mu_1(s_{ij0}, g_{i0}, T_i), \sigma_1^2), \tag{B-20}$$

then it is possible to show analytically how the continuation value depends on potentially observable characteristics of the participant and period-0 employer.

The continuation value from Equation (B-10) is

$$\begin{aligned} V_{ijt}^{\text{cont}} = \beta\,\mathbb{E}\big[(y_{ij1} - w)^+\big] =& \beta\left[\sigma_1\phi(k) + (\mu_1(s_{ij0}, g_{i0}, T_i) - w)\big(1 - \Phi(k)\big)\right], \\ k =& \frac{w - \mu_1(s_{ij0}, g_{i0}, T_i)}{\sigma_1}, \end{aligned} \tag{B-21}$$

where $\mu_1(\cdot)$ and σ_1 denote the mean and standard deviation of period-1 productivity conditional on the firm's information at the time of hiring, and $\phi(\cdot)$ and $\Phi(\cdot)$ are the standard normal pdf and cdf, respectively.

Uncertainty About Match Quality. Differentiating the continuation value with respect to σ_1 gives $\partial V_{ij1}^{\text{cont}}/\partial\sigma_1 > 0$, indicating that the option value increases with uncertainty about productivity. Because firms can terminate unprofitable matches but retain productive ones, the continuation payoff is convex in y_{ij1}: hiring under uncertainty resembles holding a call option. Greater dispersion in potential outcomes therefore raises the expected value of experimentation.

Proximity to the retention margin. For a given σ_1, the option value is maximized when the expected productivity is near the wage, $\mu_1(\cdot) \approx w$. When $\mu_1(\cdot)$ is far below w, the match is almost surely unprofitable and $V^{\text{cont}} \approx 0$. Similarly, when $\mu_1(\cdot)$ is far above w, the match is almost surely profitable and the option to end unprofitable matches adds little value. The informational payoff is largest when the firm expects to be close to indifferent about whether to retain the worker in the next period.

We consider each of these predictions in the data. First, we ask whether individual characteristics that could be proxies for having a noisier signal predict greater treatment effects. Second, we ask whether features of the firm's wage distribution predict successful hiring and retention. Specifically, if the typical worker has expected productivity near the minimum wage, the prevalence of lower-wage entry-level jobs at the firm would be important for maximizing the option value.

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