How Do Holistic Wrap-Around Anti-Poverty Programs Affect Employment and Individualized Outcomes?

Javier Espinosa
Rochester Institute of Technology

William N. Evans
University of Notre Dame

David C. Phillips
University of Notre Dame

Tim Spilde
University of Notre Dame

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Abstract

A new wave of social service programs aims to build a pathway out of poverty by helping clients define their own goals and then supporting them flexibly and intensively over multiple years to meet those goals. We conduct a randomized controlled trial of one such program, Bridges to Success. Two cohorts of participants were randomly assigned to intensive, holistic, wrap-around services that typically last two years versus a control offered help with an immediate need. Since the intervention has a clear goal of exiting poverty but is also holistic, we pre-specified both employment and non-employment outcomes. The measured treatment effect on employment three years after random assignment is 9±9 percentage points (pp). The proportion of people reporting high housing quality after one year has a treatment effect of -2±12 pp.

In exploratory analysis, additional evidence suggests a stronger case for effects on employment than non-employment outcomes. Employment after one year shows a treatment effect of 10±8 pp. Pooling our data with the most similar existing study increases precision relative to either study alone and indicates that such programs likely generate moderately positive employment effects. On the other hand, we find little evidence that intensive, holistic services affect any of a wide variety of other non-employment outcomes beyond housing, even when other areas of life are participants' primary goals.

1   Introduction

Many individuals seeking assistance from social service organizations have a definitive problem they are trying to mitigate or solve, such as substance abuse, mental illness, homelessness, a chronic health condition, a disability, or poverty. Efforts to ameliorate the consequences of their underlying condition can take many forms, but there has been a growing recognition that successful programs require a coordinated, multi-faceted, holistic, individualized approach. This is primarily for two reasons. First, individuals may have multiple underlying conditions that require attention in order to achieve success in one domain. For example, a homeless person may struggle to find permanent housing because they simultaneously face substance abuse or mental health issues, a lack of transportation that limits options, and a lack of workforce skills necessary to find a job with a living wage. Solving a host of problems may be necessary to achieve a particular goal. Second, everyone's situation is different and hence, charting a path forward will require assistance tailored to an individual's needs. To provide holistic and individualized solutions, many social service organizations offer comprehensive case management (CCM), where a case manager coordinates a variety of services to best achieve the client's goals.

Holistic, individualized approaches show some promise, but their success has varied with context. Such programs have been used successfully in some domains to reach a clear goal: low-income adult students completing a degree (Evans et al., 2020; Weiss et al., 2019; Brough et al., 2024), resettling refugees (Shaw et al., 2022), and moving housing voucher recipients to higher income neighborhoods (DeLuca and Rosenblatt, 2017; Bergman et al., 2024). On the other hand, case management programs have been less effective elsewhere, especially in improving prisoner re-entry (Wohl et al., 2011; Guydish et al., 2011; Scott and Dennis, 2012; Doleac, 2023). There are incredibly varied results in disease management or the management of high health care spending patients (Stokes et al., 2015; Bell et al., 2015; Sandberg et al., 2015; Simon et al., 2017; Vanderplasschen et al., 2019; Finkelstein et al., 2020) and similarly varied outcomes in substance abuse treatment programs with CCM (Sorensen et al., 2003;

Vanderplasschen et al., 2007; Joo and Huber, 2015; Prendergast et al., 2011; Scott et al., 2023).

More recently, CCM programs have been proposed as a general purpose response to poverty. For example, the Economic Mobility Pathways (EMPath) Mobility Mentoring® model originated in Boston in 2009 and has quickly spread to a network of 700 organizations serving more than 360,000 participants.[1] Rather than focusing on a single goal for all participants, like finishing a college degree, and allowing flexibility in the path to that goal, these general anti-poverty programs emphasize individualized goals that vary from client to client in the short and medium term, only sharing a long-term goal of exiting poverty. Whether such CCM programs can go beyond a primary target outcome and affect a broader set of individualized outcomes is unknown. For example, homelessness programs that provide long-term housing subsidies, behavioral health services, and case management while giving participants flexibility in deciding their own goals consistently reduce chronic homelessness (Sadowski et al., 2009; Tsemberis and Eisenberg, 2000; Rosenheck et al., 2003), but less evidence is available supporting reduced negative outcomes outside of homelessness (Ponka et al., 2020; Rosenheck et al., 2003; Tsai et al., 2019).

In this paper, we report the results of a randomized controlled trial (RCT) evaluation of a CCM anti-poverty program, "Bridges to Success" (BtS), inspired by Mobility Mentoring and implemented in Rochester, New York. The program recruited low-income residents. We collected detailed baseline information in seven domains: housing, family relationships, health, networks, finances, education, and employment. Participants were asked to identify domains that they wanted to work on to improve. The average person in the study identified 3.5 areas to improve. Treatment participants were assigned a navigator/mentor who worked intensively with clients over two years to identify their short- and long-term goals, create step-by-step plans for progress, and provide cash incentives for completing planned steps. The navigators worked with a variety of community organizations to provide the client with

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[1]Economic Mobility Exchange, Annual Member Report FY23, https://s3.amazonaws.com/empath-website/pdf/FY23_Shortened_Exchange_Report.pdf

the support they needed to proceed and made sure the clients were enrolled in the appropriate government programs for which they were eligible. The control group was instead connected to temporary services to meet their immediate need. Two cohorts of individuals were enrolled in the program with the first cohort enrolled between February 2017 and April 2018, while the second cohort was enrolled between February and July of 2020. A total of 430 people were enrolled in the experiment with 237 assigned to the treatment group.

Study participants were very poor, facing both long-term economic barriers and particular crises around the time of enrollment. At study entry, participants were actively seeking social services, and only one-third were employed with even the 95th percentile of baseline quarterly earnings at $6,000. Because of this starting point and despite its flexible nature, the program was designed by social service agencies to focus on improving economic outcomes for study participants; advertising materials call it a way, "'...to help individuals and families who are struggling to make ends meet achieve their financial, employment, and educational goals." As a result, a primary pre-specified outcome is employment. For participants, though, nearly three-quarters of individuals did not list employment as their primary goal. Many wanted to work first on outcomes in housing, finances, or education. Some of these outcomes will indirectly impact employment, but it is interesting that despite the stark economic situation of most, employment was not the immediate goal for the majority of participants.

In our primary analysis, we measure treatment effects on both employment and non-employment outcomes. The primary pre-specified employment outcome is having any employment three years after random assignment. The 95% confidence interval for its treatment effect is 9±9 pp (randomization inference p-value=0.09), relative to a control group rate of 52%. For outcomes beyond employment, the primary pre-specified outcome is an indicator for reporting high housing quality after one year, which has a measured treatment effect at -2±12 pp (p-value = .70), relative to a control group rate of 34%.

In exploratory analysis, we argue that despite statistical imprecision in the three-year results, the estimates are informative about program employment effects. First, measured

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treatment effects are relatively stable. The measured treatment effect for employment in a larger sample at one year is more statistically precise, 10±8 pp (control mean 64%, p-value = 0.04). Second, the measured treatment effect is similar in both sign and magnitude to results from an RCT of a similar CCM program (Evans et al., 2025). Despite their complexity, anti-poverty CCM programs appear to have similar effects in very different contexts. Both RCTs were challenged with smaller sample sizes, but, as we note at the end of the paper, combining the data in a systematic way considerably narrows the range of likely employment effects. The combined data make a pessimistic view of zero employment effects very unlikely. On the other hand, an optimistic expectation of treatment effects equal to the pre-post change in the treatment group is also very unlikely.

This latter fact indicates that observational studies that attribute changes over time to the effect of CCM programs are overly optimistic. In our data, employment exhibits a high degree of regression to the mean in both the treatment and control groups with the control group employment rate increasing from 34% at baseline to 52% after three years. Since people enter such programs in response to poor economic situations and are actively seeking assistance to resolve those situations, employment rates return to average values over time. As a result, our treatment effect point estimate is about half the size of this pre-post difference, and data pooled across both studies can reject treatment effects more than 14 pp. Overall, our results should move observers toward expecting positive employment effects from CCM, but smaller effects than those implied by observational studies following treatment group members over time.

Exploratory analysis of sub-group effects provides targets for future research on who benefits most from CCM. Measured employment effects at 3 years are particularly large and persistent for single mothers (24±16 pp) and people without high school credentials (18±16 pp). The second cohort of the program has larger effects as well (20±17 pp). Cohort differences may result from program improvement over time, external changes brought by the onset of the COVID-19 pandemic, and/or differences in pre-randomization characteristics,

such as baseline employment levels.

Exploratory analysis for non-employment outcomes other than housing quality finds little evidence that CCM generated improvements for outcomes defined by participants' individualized goals. We measure participants' primary goals with two sets of outcomes. The first are survey and administrative outcomes that most economists would consider as objective measures of success. We also have a set of subjective assessments by study participants about whether outcomes have improved in their primary goal area since random assignment. In domains other than employment, more objective assessments show no statistically significant improvements and, as a result, a composite measure of improvement using these more objective measures does not change noticeably. In contrast, participants in the treatment group are much more likely to report improvement in their goal area relative to the control group when asked for an overall assessment of progress. The results for the more subjective outcome could be survey demand effects. It is also the case that when we measure outcomes like financial stability or housing quality, economists' measurement of what is an improvement could be very different from what participants consider as an improvement. Also, outcomes like credit scores are more downstream compared with employment and require larger samples to detect statistically significant findings, so the results for these other outcomes in this and other CCM studies may be Type II errors. But in general, these results suggest that CCM can impact a more limited set of focused outcomes but has difficulty addressing all what ails program participants.

2 Context

2.1 Comprehensive Case Management

The assumption behind anti-poverty programs with a CCM approach is that exiting poverty is complex. A person attempting to exit poverty likely juggles some combination of income volatility, unstable housing, raising children with limited family support, a need to build

skills valued by employers, and mental stress. The services and public benefits available to meet such needs are often fragmented, each requiring a separate ordeal to access financial assistance, housing subsidies, childcare, workforce training, and healthcare. Solving these simultaneous challenges is particularly difficult as cognitive studies indicate that people tend to misallocate attention in the face of scarcity (Mullainathan and Shafir, 2013).

To address these complex and overlapping barriers, CCM programs provide wrap-around services. Many status quo forms of social assistance focus on providing one particular service, e.g., medical care. Wrap-around services instead simplify this situation by addressing a broad range of barriers through one program. For instance, wrap-around degree programs for adult students not only might help with academic challenges but also with childcare and transportation (Weiss et al., 2019; Evans et al., 2020; Brough et al., 2024). Similarly, housing programs designed to facilitate moves to high opportunity neighborhoods not only help with talking to landlords and providing security deposits but also with providing extensive emotional and psychological support for a major life change (DeLuca and Rosenblatt, 2017; Bergman et al., 2024). Anti-poverty CCM programs generalize this idea also to be holistic, not only wrapping around a range of barriers but also pursuing many and varying goals. The best path out of poverty may vary widely across people.

2.2 Rochester, New York

We study one such program in the context of Rochester, a mid-sized city in western New York and the seat of Monroe County. The city's early economic development was connected to its location at the nexus of the Genesee River, Erie Canal, and Lake Ontario. Rochester evolved into a manufacturing town in the 19th century with firms like Eastman Kodak and Bausch & Lomb leading the way. Like other Great Lakes cities, Rochester experienced a major decline in manufacturing in the 20th century that led to fewer economic opportunities, a smaller population, and a rise in poverty. Additionally, the city experienced an outflow of higher-income families who moved to suburban areas.

A report published in 2013 by the Rochester Area Community Foundation detailed the state of poverty in Rochester, Monroe County, and neighbor counties (Doherty, 2013). The report highlighted how Rochester was among the poorest cities in the United States and among the poorest school districts in the state, and the metropolitan area had the third highest concentration of neighborhoods in extreme poverty in the United States. The report led to a renewed focus on reducing poverty and mitigating its harm in the area and led to the creation of the Rochester-Monroe Anti-Poverty Initiative (RMAPI)–a community collaborative seeking to improve quality of life in the area by reducing poverty and increasing self-sufficiency. In 2016, RMAPI partnered with the New York Governor's State Anti-Poverty Task Force to pilot adult mentoring programs in Rochester, NY, that included the BtS program.

2.3   The Bridges to Success Program

Bridges to Success (BtS) is an intensive adult mentoring program in Rochester, New York. It focuses on a participant who is coached by a mentor, a method inspired by the Mobility Mentoring method created by EMPath. Specialized employment and dependent liaisons provide additional support. Program participants and mentors work together to move participants toward economic self-sufficiency and financial stability. BtS was originally implemented and managed by three local non-profit groups: Catholic Family Center, Action for a Better Community and Community Place of Greater Rochester, with support from the City of Rochester and the Rochester-Monroe Anti-Poverty Initiative. Catholic Family Center was the lead agency overseeing the budget and contracting with local partners, and Action for a Better Community led implementation by providing location sites for staff, managing mentors and other BtS support staff, and providing the information system (CAP 60 database) mentors used to document participant progress.

To give a concrete example of the design of BtS services, consider a hypothetical client with limited income who is attracted to BtS because she has a long-term goal of moving

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into stable employment but most immediately has just been evicted and become homeless. In many social service settings (like the control group), this person would be referred out to a homeless services agency, like an emergency shelter, and the case would be closed. By contrast, BtS would assign this person to a mentor who would help her identify her current goal. The first step might be the same, to help her enter an emergency shelter, but would continue beyond that. For example, they might identify all of the steps necessary to obtain a subsidized apartment. The mentor would provide financial incentives for small steps along the way, like applying for a new Social Security card that was lost in the eviction but is required by the subsidized housing program to verify identity. In all of this, BtS does not lose sight of the person's long-term goal of stable employment. After obtaining a long-term housing solution, the mentor and client would move on to define a new goal, perhaps completing a workforce training program. Overall, the program is designed to have the client and mentor work together over two years to meet a variety of short-term goals all on the path to economic self-sufficiency and the client's long-term goals. This example demonstrates three main features of BtS:

First, BtS provides much more intensive services than a typical social service program through a long-term relationship between a staff mentor and a client. At an initial meeting, the program matches each participant to a mentor, and the mentor and participant create a personalized action plan with short- and long-term goals and specific next steps. Mentors meet with participants for up to two years aiming for at least monthly interactions with participants, but in practice, meetings occurred with greater frequency. Prior to the COVID-19 pandemic, mentoring meetings typically took place in person but largely shifted to virtual meetings thereafter.

Second, BtS provides structure for individuals to set and track progress toward their own goals. Borrowing from EMPath and adapting to the local context, participant progress is measured by an assessment tool called the Bridge to Self-Sufficiency Matrix (see Appendix Figure A.1). The bridge tool tracks progress in nine outcomes that are organized into five

pillars: family stability (housing and family), well-being (health and networks),[2] finances (debt and saving), education/training (educational attainment), and employment (wage and type of job). A participant's status in each area is recorded as crisis, at risk, safe, stable, or thriving; these levels correspond to numerical values of 1 to 5. For example, the housing outcome ranges from 'not permanently housed or living conditions threaten health and/or safety' (crisis; level 1) to having housing with 'no subsidy, housing costs 1/3 or less of household gross pay' (thriving; level 5). In their first meeting, mentors work with treatment group participants to place themselves on a level for each pillar and formulate long-term goals for progress on one or more pillars. The mentor helps the participant track progress by administering the matrix every 3 months.

Third, a final key feature of BtS is its use of financial incentives to encourage progress toward goals. BtS and its fore-runner EMPath are built around ideas from cognitive science that situations of scarcity lead people to take sub-optimal approaches to long-term goals (Mullainathan and Shafir, 2013) and that people have more trouble making progress toward a big long-term goal than many, smaller, near-term goals. To meet these concerns, BtS provides direct financial incentives to participants that are tied to progress toward goals.

3 Empirical Strategy

3.1 Study Enrollment and Random Assignment

The BtS program was available to a wide variety of people with low-to-moderate income in the Rochester area. To be eligible, residents needed to be working age, a US citizen, able to work, and have household income below 200% of the federal poverty line. The program initially required evidence of high school equivalency and residence in a handful of neighborhoods but relaxed these requirements relatively quickly to have no educational requirement and include the entire City of Rochester.

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[2]Health is composed of physical, mental, and behavioral health including, e.g. substance use.

The BtS program actively advertised its services and recruited participants from this broad population of eligible people. Study enrollment happened in two distinct cohorts. Appendix Figure A.2 displays a timeline for both cohorts. The first study cohort enrolled participants between January 2017 and April 2018, and the second cohort enrolled between February and July of 2020. Early in the study during summer of 2017, a team of Ameri-Corps volunteers and City of Rochester employees jump-started recruitment by visiting all residences in the initial program geography to inform people about the program and the study. This outreach focused on the program as a way to exit poverty; for example, a training script said that, "Bridges to Success is a program designed to help individuals and families who are struggling to make ends meet achieve their financial, employment, and educational goals." Also, the program was operated by social service organizations with many existing programs and connections to the community. So beyond the initial push, many participants arrived informally via word-of-mouth and intra-agency referrals.

After expressing interest in the program, a participant went through a standardized intake process. The initial contact person confirmed verbally with the potential participant that they met program eligibility requirements and were interested in participating. Program staff reviewed documents to confirm eligibility, e.g., verifying location of residence. If eligible and interested, the potential participant was then enrolled in the study through an informed consent process and completed a baseline survey on a tablet computer. Participants received a $25 gift card for completing the baseline survey. These activities often happened in one or more in-person meetings during the first cohort but via virtual meetings during the second cohort after the onset of COVID. The COVID pandemic, either through its broader societal effects or via the switch virtual recruitment, led to some differences across cohorts. For example, 27% of participants in cohort 1 were employed at baseline compared to 48% for cohort 2 (see Appendix Table A.1).

After the baseline survey, program staff entered study participants' information into the program's case management software and conducted random assignment in that software.

People assigned to treatment were immediately enrolled in BtS, as described above. People assigned to control were directly handed off to other programs that would meet their immediate needs. For example, a client facing eviction might be referred to one-time emergency financial assistance.

The method of random assignment and probability of treatment varied across the two cohorts. During the first cohort, program providers were concerned that independent random assignment would create long runs of clients to either treatment or control, causing practical problems for a case management program that requires predictable case loads. So, we stratified random assignment by the time of intake, i.e., within a group of consecutive intakes exactly half would be assigned to treatment and exactly half to control. To avoid predictability in assignment of the final person in a group, group sizes were unknown to program staff and we alternated between groups of size 12 and 16. During the second cohort, the ability to respond to potential lack of excess demand during pandemic conditions was judged to be more important than caseload balancing, so random assignment switched to an iid random number draw on the tablet computer at the end of the baseline survey with the probability of treatment increased to two-thirds.

3.2 Data

3.2.1 Baseline Data

We conducted extensive baseline surveys that every participant completed just prior to random assignment when entering the study (N = 430). These surveys were conducted between January 2017 and April 2018 for cohort 1 and between February 2020 and July 2020 for cohort 2. These surveys include baseline levels of variables grouped into employment, education, financial management, health, housing, family stability, and networks of support. Participants identified the areas that they considered goal areas and selected one area as a primary goal. Finally, they reported contact information for use in follow-up surveys and administrative data matching. In total, the baseline surveys have 652 variables about the

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individual participants.[3]

We link the baseline surveys to program records from Action for a Better Community's CAP 60 database. These data provide some demographic information that we do not ask in the survey, including sex, age, marital status, educational attainment, presence of children, race, and ethnicity. They also record whether the participant was actually enrolled in BtS. Original random assignment comes from these records for cohort 1 and the survey for cohort 2. Because random assignment occurred immediately prior to program enrollment, random assignment and actual treatment are nearly identical.

3.2.2 Follow-up Surveys

Participants were invited to complete an in-person survey approximately one year after being randomized into the program. These surveys were completed between 9 and 23 months after random assignment, with a median follow-up length of 12 months and 89% completed between months 11 and 15. We attempted follow-up surveys with all members of cohort 1 but, due to available funding, only attempted to contact members of cohort 2 who did not report Social Security Numbers at baseline. As a result, while the follow-up rate for cohort 1 is 87%, the overall follow-up rate for cohort 2 is only 6.9%. But this difference results from only attempting surveys with 14 members of cohort 2 and getting a similar response rate (9 of 14). After controlling for pure cohort differences, respondents are similar to non-respondents in demographics, baseline employment, earnings, and employment goals (see Appendix Table A.2). The follow-up surveys ask similar topics and questions to the baseline survey to give us a sense of not only levels of outcomes but also changes in outcomes.[4]

The follow-up survey adds a cognitive task to complete, sometimes referred to as either the Simon task or the dots-mixed task. For a trial in this task, participants see a solid green or red-striped circle on the screen. The participants are instructed to type 'm' on the

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[3]A copy of the baseline survey can be found at https://github.com/dphill12/Papers-by-David-C-Phillips/blob/main/Rochester Survey - Printed from SurveyCTO 11-21-2017.pdf

[4]A copy of the follow-up survey is available at https://github.com/dphill12/Papers-by-David-C-Phillips/blob/main/1 Yr Followup Survey - English - Final.pdf

keyboard if a green circle appears and 'z' if a red symbol appears. However, the symbols could appear on either side of the screen. For example, in a 'congruent trial' the green circle appears on the left side of the screen because the z key is on the left side of the keyboard; in an incongruent trial, the green circle appears to the right. Participants were given 20 practice trials and then 60 trials. The outcomes from this task were response time in milliseconds and percent correct. We winsorize response time at the 5th and 95th percentiles. People tend to answer less accurately and more slowly on incongruent trials (Simon, 1990), and performance on incongruent trials has been used to measure the effect that scarcity has in depleting executive function, attention, and resisting impulses (Shah et al., 2012).

3.2.3   Unemployment Insurance Earnings

Using Social Security Numbers (SSNs) reported by participants, we collect unemployment insurance (UI) system data from the New York Department of Labor. These data include all formal labor market earnings by quarter, by employer from 2012Q1 to 2023Q2. We cannot distinguish between a participant not working and working either in another state or in an informal job outside the payroll tax system. When we attempt to match a person's record but find no match, we assume that this person is not working. They are coded as having zero income and not being employed in that quarter.

We attempt to match to UI earnings records for any person for whom we observe an SSN. Through the surveys, we gathered 9-digit SSNs from 363 of 430 people. We sent the SSNs to the state of New York's UI system, and they returned the data for the corresponding SSNs and the name attached to each SSN. Some people may have given us the incorrect SSN number, e.g., because of a typo or because they are using an SSN not associated with them. We ignore SSNs that match to records with different first and last names in our data and the UI data. This matching technique left 356 remaining valid SSN numbers. In this sample, 323 people have positive reported wages during some quarter. From 16 quarters before random assignment through 8 quarters after, the median quarters employed is 14 of

a possible 25.

In the UI data, we measure outcomes using three quarter windows. For example, one year after random assignment we use UI data from quarters 3 through 5 after random assignment. If someone appears employed in at least one of the three quarters, they are coded as employed. Similarly, we use the average earnings in quarters 3 through 5 to measure the earnings outcome. We follow outcomes through three years after random assignment. Since the UI data ends in 2023Q2, 32 people have UI earnings data but do not have follow-up through quarter 13. For individuals enrolled in the second quarter of 2020, we use quarters 11 and 12 post-enrollment for UI outcomes. For individuals enrolled in the third quarter of 2020, we use only quarter 11 post-enrollment for UI outcomes. For baseline measures, we use the quarter immediately before random assignment.

Following our pre-analysis plan, we define the primary employment outcome using a combination of survey and UI earnings records. We calculate total earnings for people appearing in both datasets as the average of survey and UI values. For participants with only one source of data, such as someone with a follow-up survey but not UI data, we use the non-missing dataset. This means that if someone appears employed in one dataset but unemployed in the other, we code them as employed.

3.2.4 Other Data

While our main results focus on outcomes measured in UI earnings data and surveys, we also match the study sample to outcomes from a variety of other administrative records. For access to public benefits, we use data from the New York Office of Temporary and Disability Assistance. For financial outcomes, we match to credit records from Experian. For housing moves, we link to consumer reference address histories from Verisk/Infutor. For more details on these data sources, see the Appendix.

3.3 Baseline Characteristics of the Study Sample

People who wish to participate in BtS face challenges to labor market success. Table 1 compares our study sample with people in broader populations. Data in columns (1) and (2) come from the 2019 ACS 1-Year estimates and show statistics for all working-age adults living in urban areas and all working-age adults in Rochester, respectively. We show baseline characteristics from our sample in column (3) as reported in the baseline survey. On average, people in our sample are much less likely to be employed and have lower earnings than those in urban communities in general or residents of Rochester in particular. People in the study sample are 26 percentage points less likely to have a high school degree (or equivalent) than others in urban communities. People in our sample are also more likely to be female (77%) and Black (64%).

In particular, study participants have low levels of employment because of experiencing recent shocks. According to the baseline survey taken immediately prior to study enrollment, only 34% of participants are employed at baseline. However, 64% of the sample have positive UI earnings in the quarter before random assignment. This large drop in employment rates just before study entry suggests that people select into applying for the BtS program in response to negative labor market shocks.

3.4 Bridges to Success in Operation

Figure 1 shows how program retention, attrition, and graduation evolved over the course of the program. Mentoring relationships typically lasted 1.5 to 2 years with a mean of 1.65, and only 1 out of 5 participants exited the program before graduating. Graduations tend to happen at the maximum program length of two years, though they may happen sooner if a client has met the goals they set out to achieve initially. These interactions are much longer and more intensive than, for example, programming provided to public benefit recipients that has relatively modest effects even when focused on participant-centered goal setting (Moore et al., 2023). Additionally, the focus on professional mentors distinguishes BtS from other

rapidly growing programs like the Family Independence Initiative (now UpTogether) that also facilitate goal-setting but through intensive peer interactions (Aguinaga et al., 2019).

Given this intensive interaction, caseloads were generally small. The program data underlying Figure 1 indicate that an average mentor in an average month held a caseload of 11 clients during the program's start-up period. Even when operating at full capacity, it would cap caseloads at 25 clients. This intensity is expensive: BtS cost an average of $5,500 per client-year in 2020 if operating at full capacity. If we assume more realistic operation at 80% of capacity (due to attrition and recruitment), costs average about $6,875 per client-year in 2020.

A key characteristic of the program is that individuals identify for mentors the different goal areas in which they would like to progress. The baseline survey asks participants to identify their goal areas, allowing for multiple areas but also specifying a primary area of focus.[5] Figure 2 shows participant responses to this question with the dark navy area representing the primary area and the lighter gold area any response. Each area garners at least 30% positive responses but none more than 70%. When prompted for a primary goal, despite the low employment rates and income levels of study participants at baseline, only about one-quarter of participants want to primarily address employment goals, about the same number that want to work on housing goals. The remaining half of participants have goals ranging across financial management, educational attainment, health, and improving family and social networks.

Direct financial incentives support progress on goals and may be one key reason why participants persist in the program. Cash incentives are substantial, adding up to an average of $620 per treated person over the course of their participation. Figure 3 shows average incentive amounts by category. Most incentives, about $400 per person, are directly related to particular goal areas, with half tied to employment or housing goals. The remaining

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[5]The program uses nine different areas: housing, family, health, networks, debt, saving, education, wage, and job type. For data collection and our analysis plan, we simplified these to seven areas by combining debt with savings and wage with job type.

payments are largely tied to program involvement, like graduating the program or attending a session to complete the bridge matrix, though some smaller amounts also support general household expenses.

Most participants make progress throughout their time in the program. Figure 4 shows program data on average matrix scores at program entry and exit, focusing on the first cohort of study participants in the treatment group. For example, the larger, solid, navy circle shows average matrix scores for the housing category among people who stated at baseline they wanted to improve their housing situation. This point is to the right of the 45-degree line, indicating that the average housing score increased from 2.5 to 3.3 during the course of program participation. This point is quite close on the plot to the corresponding small, hollow, gold circle, which shows the same values for people who did not state a desire to work on housing. The similarity in scores across these two groups indicates that people's baseline interest in working on housing is not closely related to their subsequent progress. Other goal areas similarly show overall progress but not much variation based on whether the person stated it was their goal. These data underscore how BtS focuses on an individually tailored idea of progress across many domains but also foreshadow one of our main results, that participants' actual progress is more uniform.

3.5 Statistical Specification

To estimate treatment effects with continuous outcomes, we use the following linear specification:

$$Y_i = \alpha_0 + D_i\beta_0 + \mathbf{X_i}\mathbf{\Gamma_0} + \epsilon_\mathbf{i}. \tag{1}$$

For dichotomous outcomes, we use a logistic model:

$$ln(\frac{Pr[Y_i = 1]}{Pr[Y_i = 0]}) = \alpha_1 + D_i\beta_1 + \mathbf{X_i}\mathbf{\Gamma_1}. \tag{2}$$

In both models, Y_i is an outcome for person i, such as being employed three years after

random assignment. D_i is the randomly assigned binary treatment assignment. We include a pre-specified vector of baseline individual characteristics, $\mathbf{X_i}$. We measure intent-to-treat effects, comparing outcomes between those randomly offered the chance to participate in BtS and those not receiving an offer. Therefore, our parameter of interest is β_0, which is the average effect of being offered a spot in the program. For logistic regressions, we transform β_1 into marginal effects, calculating $\left\{ \frac{1}{1+exp[-(\alpha_1+\beta_1+\mathbf{X_i\Gamma_1})]} - \frac{1}{1+exp[-(\alpha_1+\mathbf{X_i\Gamma_1})]} \right\}$ and averaging over i. We include covariates to account for the varying probability of treatment across cohorts and to estimate more precise standard errors of the treatment effects. The vector of covariates, $\mathbf{X_i}$, includes the baseline value of the outcome (when available), baseline earnings, gender, marital status, and sets of mutually exclusive and exhaustive indicators for race, education, month of random assignment, and months between baseline and follow-up surveys. To account for stratification of random assignment by study entry time, we report randomization inference p-values calculated from 1,000 draws within that stratification structure.

The identifying assumption that gives β_0 a causal interpretation is that the treatment status, D_i, was randomly assigned. Table 1 provides evidence that randomization was correctly administered. Column (3) provides sample means for the entire BtS sample. Columns (4) and (5) report raw mean baseline characteristics for the treatment and control groups. For example, the treatment and control groups' average earnings prior to random assignment are \$1,229 and \$1,111, respectively. Column (6) displays the difference in raw means of each characteristic after controlling for stratification by study entry timing. In particular, strata controls condition out the different probability of treatment in cohort 1 versus cohort 2. The raw difference in earnings between treatment and control is \$118, but after controlling for stratification group the difference is -\$54. All values in Column (6) are statistically insignificant, implying that the treatment and control groups look similar on average, other than their exposure to the BtS program.

Because we cannot track all participants in the various data sources we use to measure

outcomes, we also test for whether baseline balance remains after limiting the sample to people with observed outcomes. For each set of employment and survey outcomes, we produce balance tables to demonstrate the control group serves as a valid counterfactual for the treatment group. Appendix Tables A.3, A.4, and A.5 demonstrate that baseline balance remains when limiting to the sub-samples covered by the various sources of employment data: surveys, UI earnings records, and the combination of the two.[6] One specific concern with state UI earnings and public benefit records is that participants may move out of state, but Appendix Table A.9 shows that only 1% of both treatment and control groups move out of New York.

4 Results

4.1 Services Received

Consistent with study assignment, people assigned to treatment are much more likely to have frequent interaction with BtS than those assigned to control. Table 2 displays what participants report about services received in the past year, as of the one-year follow-up survey. Of people assigned to treatment, 86% recall being involved with BtS in the past year; 49% of the control group also recalls working with BtS in the past year, presumably a reference to study enrollment and the short-term referrals offered to the control group. However, the gap in intensity of interaction is wide: the treatment group is 48 percentage points more likely to say they have contact with BtS at least once per month. These survey measures support the program data reported above, indicating that this program is more intensive than traditional case management programs.

People assigned to BtS are also more, rather than less, likely to access other community programs. A small number of other programs in Rochester have relatively intensive services:

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[6]Similarly, we construct balance tables for administrative sources of supplementary non-employment outcomes. Appendix Tables A.6, A.7, and A.8 demonstrate baseline balance for the Experian, OTDA, and Infutor sub-samples, respectively.

Family Independence Initiative, Strengthening Working Families Initiative, Health Professions Opportunity Grants, and Pathway of Hope - Salvation Army. Treatment assignment does not simply shift people among these other potential substitute programs; BtS participants are actually slightly more likely to participate in them. Increased use of community services may also extend beyond those few organizations. In particular, the treatment group is 9 percentage points more likely to receive help from community organizations outside the BtS network. Increased use of other services likely results directly from case management referrals to these other organizations. Altogether, BtS does not crowd out other community services and instead connects participants to them.

Interestingly, though, connections that BtS makes between people in the treatment group and other organizations do not seem tailored to participants' stated goals. By combining with participants' stated goals, we can measure if participants increase their rate of contact with community organizations in their primary goal area. This rate is a statistically insignificant 3 percentage points higher; only a quarter of the overall increase. The most common connection is a 12 percentage point increase for organizations helping with finances, e.g., credit counseling, which is not a common primary baseline goal. Thus, we do not observe much increase in service referrals tied to participants' goals.

4.2 Employment

Being assigned to treatment increases employment during the program. At one year after random assignment, we measure employment combining the person's self-report at the time of the survey and records of positive earnings in administrative UI data during quarters 3, 4, or 5 after random assignment. The first row of Table 3 shows this measure; 71% of the treatment group is employed one year after random assignment, compared to 64% for the control group. Adjusting for pre-specified covariates increases the raw 7-percentage point gap to 10 percentage points, a difference that is statistically significant at the 5% level.[7]

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[7]We follow the specification delineated in our pre-analysis plan and described in section 3.5. This specification (i) incorporates stratified random assignment, (ii) accounts for differences across cohorts in the

Measured treatment effects on employment vary slightly across different data sources but are within the bounds of sampling variation. While our main results in the first panel of Table 3 combine survey and administrative UI records, the second panel shows results only using UI data. The treatment group is 6 percentage points more likely to be employed at one-year follow-up in the UI data. This value is not statistically different from the 10 percentage point effect measured in the combined data, but it is slightly lower. Results in Appendix Table A.13 that use only survey data are very similar to our main results. Differences across data sources in the sample with successful follow-up and the types of employment covered, combined with the relatively small sample of our study, make these small differences unsurprising.

Administrative records follow participants after the program ends and suggest that most labor market benefits persist. BtS participants graduate two years after program entry, so we focus on outcomes three years after random assignment. Since we only have survey data one year after random assignment, longer-run effects can only be tracked in administrative records. Figure 5 plots employment rates and total earnings over time for both groups, as measured by administrative UI records. The plotted pre-trends match the prior evidence on balance from Table 1; the treatment group randomly has slightly higher pre-period earnings but also a slightly larger negative shock at the time of program entry, which tend to cancel out. As shown in the first row of Table 3, controlling for any such differences increases measured treatment effects at one year compared to raw differences. In the post period, Figure 5 shows that the gap in employment and earnings between the two groups appears to persist over time and, if anything, increases after quarter 8 when remaining participants exit the program. The final panel of Table 3 quantifies these results more precisely. Three years after random assignment, the group assigned to BtS is 9 percentage points more likely to

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probability of treatment, and (iii) improves precision by including controls. Accounting for these factors in other ways yields similar results, i.e. if we address stratification with a linear model and strata fixed effects rather than randomization inference with a logit model (Appendix Table A.10), ignore stratification (Appendix Table A.11), or condition on controls in a 'doubly robust' specification that also incorporates inverse propensity weights (Appendix Table A.12).

have a positive UI earnings record. This difference is only statistically different at the 10%
level, so it should be interpreted cautiously. However, it is close to the main 10 percentage
point increase observed at one year, and it is actually larger than the estimate of 6 percentage
points resulting from a comparable sample and UI data. Overall, these results suggest that
most labor market benefits of the program persist after the program ends.

A 9-10 percentage point increase in employment is important and meaningful but also
smaller than what would be expected from observing program participant outcomes. Recall
that only 34% of the treatment group is employed in the baseline survey. That rate increases
dramatically within one year, by 37 percentage points using the combined UI/survey measure
(Table 3) and 18 percentage points using only survey data (Appendix Table A.13). Because of
the presence of a randomly assigned control group, we can determine that this simple before-
after comparison within the treatment group overstates program benefits. The control group
employment rate also rebounds due to individuals' effort and other community resources that
would be present in the absence of the program being studied, from 34% to 64% in combined
UI/survey data and to 52% in the survey alone.

We find no evidence of labor market benefits on the intensive margin. The second and
third rows of each panel of Table 3 show these results. The proportion of participants for
whom earnings increase between baseline and follow-up is 5 percentage points greater for
treatment than control at one year and 6 percentage points greater at 3 years. Similarly,
average unconditional earnings increase by $131 per quarter at one year and $59 per quarter
at 3 years. All of these estimates are somewhat imprecise and not statistically different from
zero. The 95% confidence interval for earnings effects at one year runs from $-516 to $778
per quarter, or -20% to 31% of the control mean. In general, we cannot reject the null that
the program increases employment rates but otherwise does not affect earnings; e.g. that
at one year earnings increase by $255 or that the proportion of people with rising earnings
increases by 10 percentage points. This pattern of labor market effects, combined with very
low baseline earnings, mean that treatment moves few participants above the federal poverty

line, as shown in the final row of each panel of Table 3.

4.3   Multiple Dimensions of Goals

Because BtS is a multi-dimensional program, we test for whether the program affects areas of life beyond employment. As noted above the program works with participants on goals in employment, housing, family/children, health, social networks, education, and finances. While we do not have administrative records for most of these outcomes, we can measure them in the the one-year follow-up survey. Other similar programs find employment and housing to be particularly salient (Evans et al., 2025), and as noted above, most people in our study initially want to work on these two areas, which motivates our prior focus on employment. In the first two rows of Table 4, we report treatment effects on employment and housing quality, our two primary pre-specified outcomes, for the sub-sample of survey respondents. As in the full sample, members of the treatment group see improvement in employment. On the other hand, they do not report improved housing quality.

More generally, the program appears to generate progress on the participant's self-defined goals, but only when measured flexibly. At baseline, we asked all participants to identify one of the 7 program areas as the their primary goal. At follow-up we can measure progress in that area in two ways. First, we prompt respondents with their baseline primary goal and ask if it has improved. Second, we use the detailed survey data to calculate an indicator for if a pre-specified measure improves in the person's goal area. For example, for someone wanting to work on their finances, the first option would result from asking 'how much has your financial situation improved?' For the latter, we use a series of questions to calculate and compare net assets at baseline and follow-up. The third and fourth rows of Table 4 show these results. When directly asked, the proportion of people reporting improvement is a statistically significant 21 percentage points greater in the treatment group. On the other hand, improvement on more specific measures happens no more often in the treatment

group.[8] The bottom half of Table 4 shows the direct measures that, along with earnings and housing quality, compose this measure: none outside of employment show major signs of improvement.[9] These disparate results for participants' overall subjective reports and more specific measures present a puzzle that bears similarity to results from the Oregon Health Insurance Experiment (Baicker et al., 2013). In that paper, the authors found little on impact of insurance coverage on measures of physical health thought to be likely impacted by insurance coverage (e.g., cholesterol levels, blood pressure, glycated hemoglobin, etc.) but a large decline in depression and a large increase in the chance people reported being healthier than one year ago. Survey demand effects could explain such contrasting effects if more general survey responses about progress are more vulnerable to survey demand effects. On the other hand, the program might also benefit participants in ways that are, other than employment, hard to specify in a survey.

5 Exploratory Analysis

This section provides brief, exploratory analysis intended to aid in interpreting the pattern of main results reported above. For more such discussion, see Espinosa et al. (2024).

5.1 Potential Mechanisms

A pattern of null results for more individualized outcomes suggests that the program does not achieve any observed employment effects primarily through its holistic, individualized

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[8]The sample for these two outcomes differ. The first measure excludes 89 participants who were not asked the question due to a survey skip code error (which skipped people who only listed one goal at baseline) or because baseline goal information was missing. Individuals with one baseline goal state similar primary goals as the full sample, and the difference in sample is not the cause of the difference in treatment effects. See Appendix Table A.14.

[9]All outcomes are dummies indicating improvement in a given area. Appendix Table A.15 shows that results are similar for the underlying continuous measures. See also Appendix Tables A.13, A.16, A.17, A.18, A.19, A.20, A.21 and A.22 which show detailed survey measures for each area and Appendix Table A.23 and Appendix Figure A.3 which show financial measures from credit reports. Because there are many of these outcomes and some of them are rare, our main specification is not always feasible. So, in these appendix tables we report a linear specification with strata fixed effects and analytical standard errors identical to that in Table A.10.

approach to goals. As noted above, the bottom panel of Table 2 indicates that services received are not particularly connected to initial goals, and Figure 4 shows that how much progress treatment group participants make in a particular area has almost no relation to whether that area was the person's original goal area. Thus, the operation of the program is consistent with the results in Table 4, in which outcomes beyond employment rarely show positive treatment effects.

We find some suggestive, but mixed, evidence that the program causes cognitive changes that could be a contributing mechanism. The program's combination of goal setting, incentives, and intensive personal support is intended to overcome the cognitive challenge that all humans face in optimally pursuing complex long-term goals in the face of scarcity (Mullainathan and Shafir, 2013). Appendix Table A.24 and Figure A.4 display the results. The treatment group reports 0.12 points (0.3 standard deviations) greater hopefulness on a standard psychological scale, with effects driven by an 'agency' sub-scale on the person's belief that they can actively make choices and pursue their goals, rather than a sub-scale on ability to problem solve and plan particular paths toward goals. The treatment group also does slightly worse on a task (the 'dots mixed' or 'Simon' task) that requires effort to resist automatic impulses in pursuit of goals. Together, these results suggest that the program increased motivation and agency, which could help participants re-enter employment.

5.2   Heterogeneous Effects

While necessarily imprecise due to small sample sizes, sub-group treatment effects can identify groups on which to focus in future research with CCM. Figure 6 shows a variety of sub-group treatment effects on employment; each point is a separate estimate with attached 95% confidence interval.[10] At one year after random assignment, treatment effects are largest when participants are Black (16 ± 10 pp), female (14 ± 10 pp), and have no high school diploma (24 ± 15 pp). These gaps in treatment effects mostly persist out to three years for women

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[10]All of these sub-groups were pre-specified, except for education level.

(10±11 pp) and non-graduates (18±16 pp), but gaps by race fade. Long-run effects for women are particularly concentrated among single mothers. Women with children and no other adults in the household show typical treatment effects at one year (12±15 pp) but very large treatment effects at three years (24±16 pp). Treatment benefits those with the most barriers to employment initially, i.e. those with low vs. high predicted employability (15±12 pp vs. 2±14 pp), but this pattern flips in the longer run (5±12 pp vs. 13±14 pp).[11] We observe little heterogeneity by age.[12]

Perhaps the most interesting heterogeneity is by cohort. The study enrolled two cohorts; one in 2017-2018 and one in 2020. Treatment effects on employment are larger and persistent for the second cohort, 20±17 pp at both one and three years. Despite the fact that the second cohort enrolls less disadvantaged participants (see Appendix Table A.1), baseline demographics do not appear to explain cohort differences in treatment effects. For example, Figure 6 shows that cohort 2 treatment effects were larger among participants both with and without a diploma. Instead, cohort differences may be explained by differential exposure to the COVID-19 pandemic and related changes in the labor market; as shown in Appendix Figures A.5 and A.6, employment dynamics differ across cohorts. Or, the Bridges to Success program, which was newly operational during cohort 1, may have simply improved in quality over time.

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[11]We generate an index of employability by regressing employment on our standard vector of control variables within the control group and predicting fitted values for both treatment and control. The predictors are: baseline employment, gender, marital status, race, educational attainment, month of random assignment, months between surveys, and stratum of random assignment. To avoid stratifying on an endogenous index, we follow Abadie et al. (2018) and compute everything using repeated split samples. We bootstrap standard errors for this specification.

[12]Control group mean earnings are similar across sub-groups so that heterogeneity in effects on employment rates likely correspond closely to heterogeneity in earnings effects. For example, Black participants are the sub-group with the lowest control mean earnings at 3 years ($2,368), but this is only 16% smaller than the full sample mean ($2,803). Meanwhile, the point estimate for their treatment effect on employment is nearly double the full sample estimate.

6 Discussion

We estimate effects on employment that are similar to those observed for another CCM program in Texas. In terms of point estimates, we observe a 9 percentage point increase in employment 3 years after random assignment, while a study of a similar program in Fort Worth, Texas reports a 6.1 percentage point increase in any employment 2 years after random assignment (Evans et al., 2025). Such similar results are surprising: CCM programs may appear particularly vulnerable to concerns of external validity because they are complex and personalized, and the cities of Fort Worth and Rochester are very different places. For example, 37% of Fort Worth residents are Hispanic, compared with only 17% in Rochester. These programs may lead to similar results because they attract similar clients despite their different contexts. For example, 26% of our participants and 30% of those in (Evans et al., 2025) are Hispanic. Both programs also attract people disconnected from the labor market: 34% and 40% of participants, respectively, are employed at baseline.

Because the present study and Evans et al. (2025) both have relatively small samples, the new evidence that we provide significantly constrains reasonable beliefs about the effectiveness of CCM programs. The 95% confidence interval of 6.1±9.8 pp in Evans et al. (2025) still admits considerable uncertainty. Simply averaging treatment effects across the two studies assuming independence yields a treatment effect of 7.4 pp with a 95% confidence interval from 0.1 pp to 14.2 pp.[13] Given the combined evidence, it is very unlikely that CCM programs have negative or null employment effects, but it is also very unlikely that true treatment effects approach the very large values sometimes claimed from pre-post comparisons.

Despite the fact that the program was holistic and most participant's primary interest were improving outcomes in areas outside of employment, we observe minimal impacts be-

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[13]This simple frequentist pooling yields very similar results to an analysis of how a Bayesian decision-maker with uniformed priors would update their beliefs. Appendix Figure A.7 displays how the beliefs of a simple Bayesian decision-maker evolve after seeing evidence from Evans et al. (2025) and then the present study.

yond employment. This pattern of results may be linked to the mechanisms by which BtS actually helps participants. For example, we find that it connects participants with community services and increases agency, but little evidence that it provides services tailored to participants' goals.

Still, beliefs based solely on the available evidence on employment effects imply that such programs likely provide an efficient means of redistribution. We quantify the net return to spending a dollar on comprehensive case management using the Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser, 2020). We assume benefits result entirely from extensive margin employment effects and weigh this against the $10,386 per client direct cost of the program. Other details are similar to those in Evans et al. (2025) and are reported in Appendix A.1. If a program has an MVPF greater than 1, a policymaker who wishes to redistribute income but expects cash transfers to cause disemployment effects (Vivalt et al., 2024) would clearly prefer a program like Bridges to Success to cash. Bridges to Success reaches this threshold if a 7.4 pp increase in employment rates persists for at least 8 years after random assignment.

The available evidence also constrains the possible set of justifications for such programs. An optimist who expects a 14 pp increase in employment still needs employment effects to last at least 5 years to reach an MVPF of 1. The MVPF then increases to 2.7 if effects persist for 10 years and 8.4 if effects persist until participants are age 65. These higher values would place BtS as a cost-effective program but still not among the most effective, such as high quality pre-school (Hendren and Sprung-Keyser, 2020). On the other hand, if employment effects do not persist, BtS shows cost-effectiveness similar to workforce programs. Employment effects of 5-10 pp (25th to 75th percentile of Bayesian beliefs) that stop immediately after the sample period imply an MVPF between 0.19 and 0.41, comparable to an average of 0.44 for workforce programs covered by Hendren and Sprung-Keyser (2020).

A key line of inquiry after any RCT evaluation is to consider what the results indicate about how science should proceed. Despite the larger p-values for the three-year results on

employment, the core set of results should encourage social service organizations to consider more experimentation with models of comprehensive case management. At the three-year mark, employment for single mothers was 24 pp higher than the comparable control group and 18 pp higher for people with no high school credential. Many sub-groups are available so these results should be handled with caution. However, single mothers and people without high school diplomas are a large group, making up 32% of people in poverty,[14] and particularly disadvantaged. Potentially large effects of CCM for these two groups warrants further exploration. Given the cost of such programs, targeting groups with the largest benefit is the key to making the program more cost effective. Also, the substantially larger and precise results for employment at both the one- and three-year follow-ups for cohort 2 could be driven by the unique events of COVID. But, it could also be produced by the case managers having a better understanding of how to generate better outcomes after having a year experience under their belt. Deciphering whether program impact is constant or state-dependent is also possibly a valuable line of research. In this case, like many social experiments, the evaluation of BtS raises many new questions, but the results also outline a clear path for future research on these types of programs.

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[14]Authors' calculations using the 5-Year 2023 American Community Survey microdata.

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Table 1: Mean Baseline Characteristics, Representative Groups and Study Sample

	(1) Urban	(2) Rochester	(3) Full Sample	(4) Treatment	(5) Control	(6) Adj. Diff.	(7) P-Value
Employed at Baseline	0.72	0.68	0.34	0.34	0.34	-0.03	0.54
Employed Quarter Prior			0.64	0.67	0.60	0.04	0.43
Quarterly Earnings ($)	11,391	7,312	1,176	1,229	1,111	-54	0.86
Quarterly Earnings ($, Employed)	15,251	10,233	3,983	4,102	3,831	151	0.88
No High School/GED	0.12	0.14	0.32	0.32	0.32	0.01	0.85
Married	0.38	0.24	0.08	0.08	0.09	-0.02	0.38
Age	39.9	39.6	37.3	36.8	38.0	-0.6	0.58
Has Children	0.33	0.27	0.60	0.59	0.62	-0.04	0.37
Female	0.51	0.51	0.77	0.78	0.75	0.02	0.62
Hispanic	0.23	0.17	0.26	0.27	0.25	0.01	0.74
White	0.58	0.58	0.09	0.09	0.09	-0.00	0.99
Black	0.18	0.29	0.64	0.63	0.65	-0.02	0.62
Other Race	0.24	0.13	0.27	0.28	0.26	0.02	0.58
Primary Goal:							
-Housing			0.27	0.25	0.30	-0.05	0.29
-Family			0.06	0.07	0.05	0.02	0.50
-Health			0.07	0.06	0.08	-0.02	0.33
-Networks			0.02	0.02	0.02	0.00	0.80
-Education			0.16	0.19	0.12	0.06	0.10
-Employment			0.24	0.23	0.24	-0.01	0.83
-Finances			0.11	0.12	0.10	0.02	0.61
-Missing			0.07	0.06	0.09	-0.01	0.36
N	252,067	1,324	430	237	193		

Notes: Columns 1 and 2 use data from the 2019 ACS 1-Year survey with ACS person weights. Column 1 is all working-age adults in urban areas and column 2 is people in Rochester, New York. Columns 3-5 report study baseline data. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Column 3 is the full study sample, column 4 is the treatment group, and column 5 is the control group. Column 6 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 7 are computed using heteroskedaticity-robust standard errors.

Table 2: Receipt of Services over Past Year, One Year Follow-up Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	P-Value
Bridges to Success						
Any Involvement	268	0.68	0.86	0.49	0.37*** (0.04)	0.00
Involved Once per Month	268	0.42	0.72	0.09	0.48*** (0.02)	0.00
Similar Organizations						
Any Other Similar Org.	268	0.12	0.16	0.09	0.10* (0.05)	0.08
Any Outside Organization Related to...						
...Anything	271	0.25	0.31	0.19	0.09** (0.05)	0.04
...Primary Goal Area	242	0.13	0.14	0.12	0.03 (0.04)	0.42
...Housing	271	0.08	0.10	0.07	0.02 (0.04)	0.52
...Family	271	0.04	0.05	0.03	0.02 (0.04)	0.45
...Physical Health	271	0.05	0.03	0.08	-0.12** (0.04)	0.04
...Mental Health	271	0.12	0.13	0.11	0.02 (0.05)	0.63
...Education	271	0.06	0.06	0.07	-0.01 (0.03)	0.60
...Employment	270	0.14	0.15	0.12	0.04 (0.04)	0.52
...Financial	271	0.13	0.18	0.07	0.12*** (0.04)	0.01

Notes: Outcomes are measured by the one year follow-up survey. Similar outside organizations include the Family Independence Initiative, Strengthening Working Families Initiative, Health Profession Opportunity Grants, and Pathway of Hope. The sample includes participants who responded to the relevant questions in the follow-up survey. Differences in sample sizes are typically due to responses of 'I don't know.' The smaller sample for primary goal is due to a survey skip code error. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports the average marginal effects on treatment from logistic regressions of the listed outcome on a random assignment indicator and pre-specified controls, which are the baseline value of the outcome (when available), baseline earnings, gender, and marital status, as well as indicators for race, educational attainment, month of random assignment, and months between surveys. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 3: Employment Outcomes

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-Value
1 Year Results (Survey + UI)						
Employed	396	0.68	0.71	0.64	0.10** (0.04)	0.04
Quarterly Earnings ($)	396	2,708	2,836	2,548	131 (330)	0.69
Earnings Increased	396	0.53	0.53	0.52	0.05 (0.05)	0.32
Earnings Above Poverty Line	394	0.21	0.20	0.22	-0.04 (0.04)	0.33
1 Year Results (UI)						
Employed	356	0.66	0.69	0.62	0.06 (0.05)	0.25
Quarterly Earnings ($)	356	2,808	2,955	2,623	38 (367)	0.90
Earnings Increased	356	0.47	0.46	0.47	0.01 (0.05)	0.86
Earnings Above Poverty Line	354	0.23	0.23	0.24	-0.06 (0.04)	0.24
3 Year Results (UI)						
Employed	356	0.60	0.67	0.52	0.09* (0.05)	0.09
Quarterly Earnings ($)	356	3,192	3,502	2,803	59 (469)	0.89
Earnings Increased	356	0.44	0.48	0.39	0.06 (0.05)	0.29
Earnings Above Poverty Line	354	0.24	0.28	0.20	0.04 (0.05)	0.36

Notes: The top panel measures total earnings for people with either UI earnings records or one year survey responses, taking the mean when both are available. Non-employment is coded as zero earnings. 'Employed' and 'earnings increase' indicate non-zero earnings and an increase in earnings relative to baseline, respectively, according to the composite earnings measure. The middle and bottom panels limit the outcome measure to UI earnings records. When measuring earnings with UI records, we average earnings across a three-quarter window centered on the listed point in time. Participants who enrolled in 2020Q2 or 2020Q3 have less data for the three year outcomes and use either two quarters (2020Q2) or one quarter (2020Q3) data. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for continuous variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Dichotomous outcomes report average marginal effects from a logistic regression. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 4: Outcomes in Many Domains, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-value
Quarterly Earnings Increase	271	0.44	0.47	0.41	0.11* (0.06)	0.09
High Home Quality	271	0.31	0.29	0.34	-0.02 (0.06)	0.70
Improvement in Primary Goal	182	0.45	0.50	0.39	0.21*** (0.07)	0.01
Improvement in Primary Goal (Bridge Tool)	271	0.43	0.43	0.44	-0.00 (0.06)	0.99
All Children Enrolled in School	271	0.86	0.88	0.83	0.07 (0.05)	0.19
Increased Health	271	0.24	0.23	0.26	-0.01 (0.05)	0.84
Increased Social Networks	271	0.49	0.50	0.49	0.01 (0.06)	0.82
Increased Education or Enrolled	271	0.40	0.35	0.45	-0.08 (0.06)	0.16
Increased Net Assets	271	0.34	0.36	0.32	0.01 (0.06)	0.79
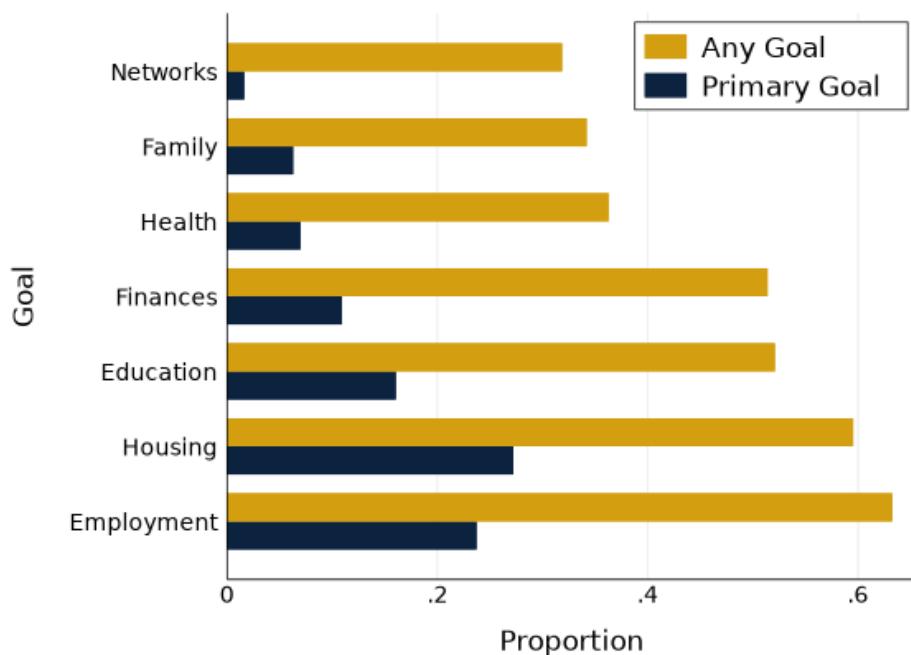
Notes: Outcomes are measured in the one year follow-up survey. The sample includes all respondents to the survey. The sample size for 'improvement in primary goal' is smaller because of a survey skip code error. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports the average marginal effects on treatment from logistic regressions of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Figure 1: Persistence in Programming for Treatment Group
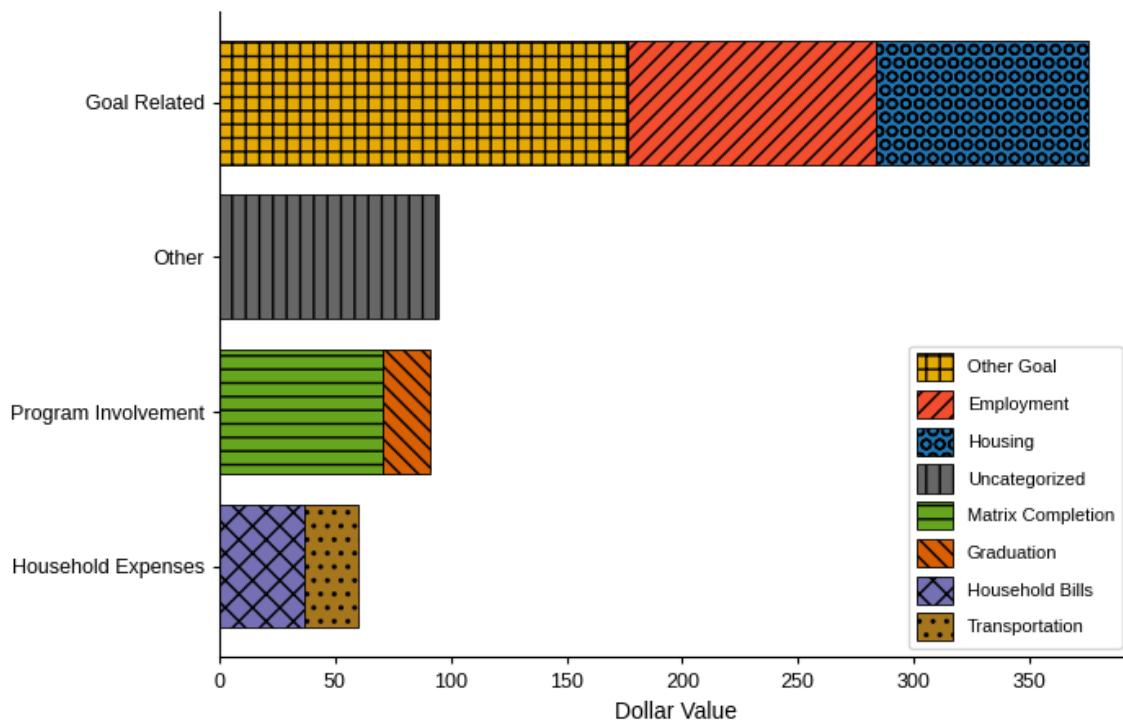
Notes: Data comes from internal BtS records for cohort 1 participants. The lines plot the proportion of participants by enrollment status. Active participants are those who still are meeting with mentors and engaging with BtS. Inactive participants did not graduate but are now longer active with BtS.

Figure 2: Goal Areas Identified by Participants, Baseline
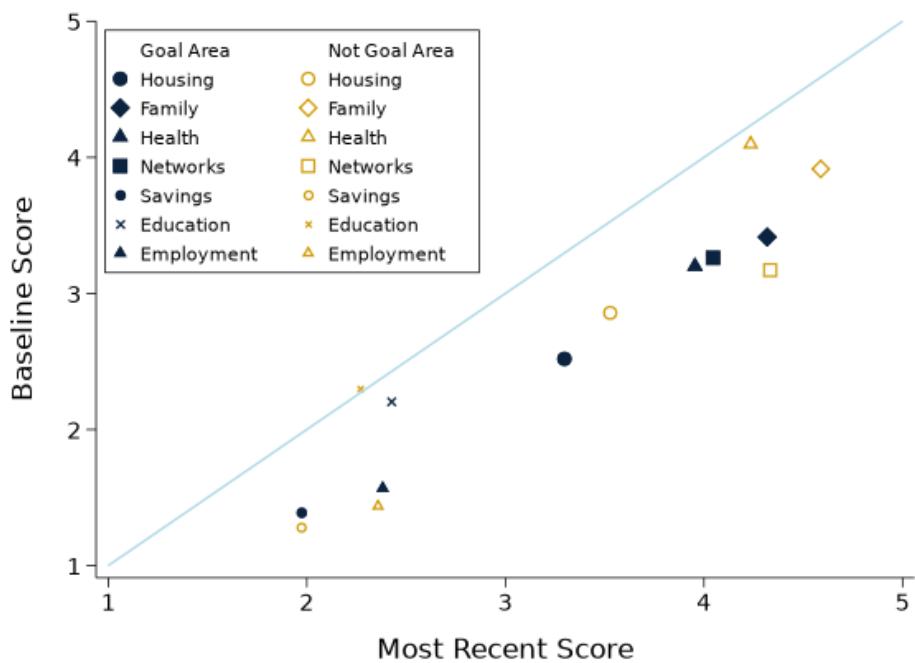
Notes: Data comes from baseline survey responses. The sample is the full study sample. Outcomes measured by participants' responses to a question on their goals. Respondents may select multiple goal areas, but only 1 primary goal area.

Figure 3: Average Incentives Received by Category
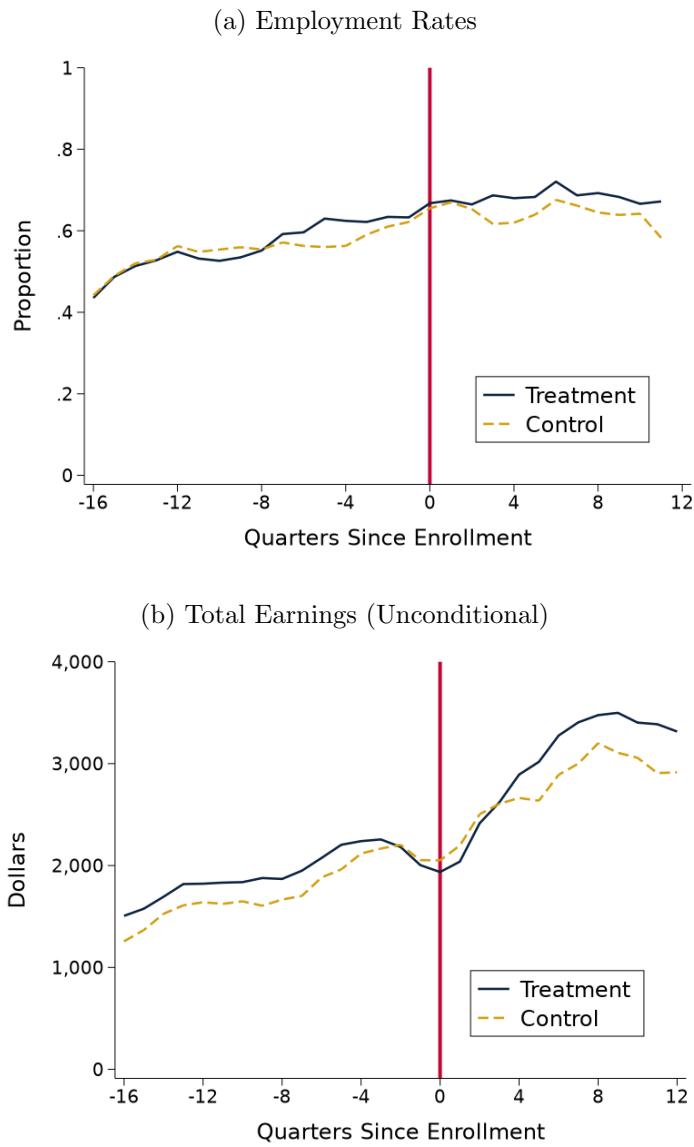
Notes: Data comes from internal BtS reports on the amount of and rationale for financial incentives paid. Sample is limited to cohort 1 participants in the treatment group. We aggregate more detailed categories into those presented using the text description of the incentive payment. The graph plots the average amount of incentives received. No record of incentives is coded as zero.

Figure 4: Progress in Matrix Scores for Treatment Group, by Area and Initial Goal
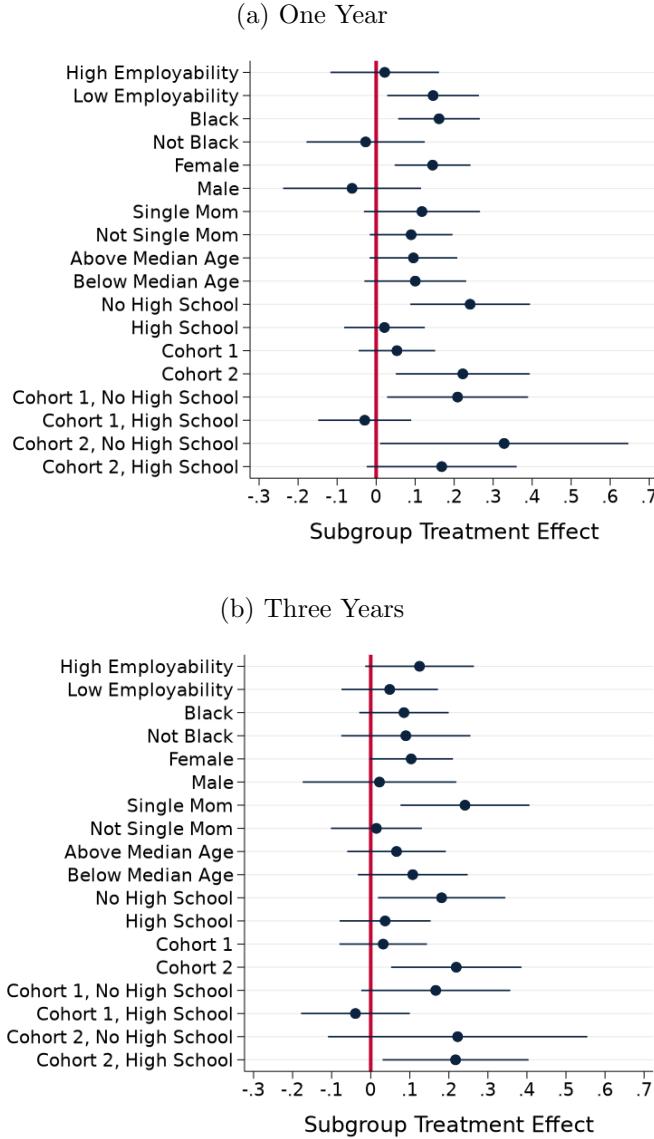
Notes: Data comes from internal BtS records for cohort 1 participants. Matrix scores are measured as in Appendix Figure A.1. The vertical axis shows a participant's initial matrix score, and the horizontal axis shows the matrix score for a participant's last observed matrix. Each symbol shows the score for a different goal area. Each pair of filled blue versus hollow gold symbols divides the sample. Filled blue symbols are participants that listed that respective area as an initial goal, and hollow gold symbols represent participants that did not list that respective area as an initial goal.

Figure 5: Employment Outcomes by Quarter, UI Earnings Records

(a) Employment Rates

(b) Total Earnings (Unconditional)

Notes: The sample includes all study participants who report a valid SSN. This figure displays employment rates and unconditional mean earnings measured using UI earnings records. For each quarter, we report the fraction of people employed (or mean earnings) in the 3-quarter window centered on the listed quarter. We re-weight across cohorts with inverse propensity weights that account for the different probability of treatment across cohorts. Participants who enrolled in 2020Q2 or 2020Q3 have less than 3 quarters of data for the three year outcomes and use either two quarters (2020Q2) or one quarter (2020Q3) data.

Figure 6: Effect of Treatment on Probability of Employment, by Sub-Group

(a) One Year

(b) Three Years

Notes: We estimate pairs of sub-group treatment effects using a logit regression that is identical to those in Table 3 except it replaces the treatment indicator with the listed characteristic, the characteristic interacted with treatment, and the negation of the characteristic interacted with treatment. We report the latter two estimates. Whiskers show 95% confidence intervals using heteroskedasticity-robust standard errors. The outcome in all regressions is a binary variable that takes a value of 1 if the participant is employed. Employment at one year is from UI earnings records and/or one year survey responses. Employment at 3 years comes from UI earnings records. Each regression includes the same controls as Table 3. We generate an index of employability by regressing the outcome on those same control variables within the control group and predicting fitted values for both treatment and control. To avoid stratifying on an endogenous index, we follow Abadie et al. (2018) and compute everything using repeated split samples. We bootstrap standard errors for this specification.

A Appendix

A.1 Other Data Sources

A.1.1 Benefits Data

We collect SNAP and TANF benefits data from the Office of Temporary and Disability Assistance (OTDA) in the state of New York. These data contain monthly records of received benefits in the form of food benefits from the Supplemental Nutrition Assistance Program (SNAP) and cash benefits from Public Assistance (PA), which largely covers the federal Temporary Aid for Needy Families (TANF) program. We have public benefits data for cohort 1 through an agreement with OTDA, but cohort 2 is not covered by that agreement and thus excluded. Among the 299 people in the first cohort, OTDA successfully matched 274 observations using name, date of birth, and SSN. These data are measured monthly, and we observe this sample for 12 months post-randomization with no pre-period data. They measure both whether and how much assistance a household receives, by program.

A.1.2 Experian Data

Experian is a data analytics and consumer credit reporting company. For this project, they provided an extract covering credit report data for 380 out of 430 study participants. The match was based on name, address, and SSN. Match rates are lower for Experian than other sources because (a) 45 study participants did not report an address and Experian required an address to make a match and (b) a small number of study participants opted out of linking to credit reports (an option required by our IRB). Also, there are many missing values in the data for our sample. For example, 25% of the participants with Experian data only have it for 8 or fewer quarters. We focus on a sample of 285 people who have non-missing data in the one-year follow-up period. The extract includes quarterly Experian data from quarter 2 of 2014 to quarter 2 of 2022, giving everyone in cohort 1 a horizon of 14 quarters post-enrollment and everyone in cohort 2 5 quarters of post-enrollment data. We combine the cohorts for a full sample analysis of one-year outcomes. Additionally, we examine just cohort 1 using all 14 quarters of post-enrollment data to measure longer-term effects. The primary outcome from these data is a credit score, and additional outcomes include a dummy if the participant has a prime credit score ($\geq$650) and various measures of debt. Similar to the UI data, we use the data's long timeframe and high frequency in two ways. We construct measures to be similar to the surveys by using 1 quarter pre-enrollment as a baseline measure and using quarters 3 through 5 after enrollment as outcome measures. For continuous variables such as credit score, the outcome is the average credit score over the three quarters. For binary variables such as an indicator, if the participant has a prime credit score, the outcome is 1 if the person had a prime credit score in any of the quarters and zero if not.

A.1.3 Infutor Data

To measure housing moves, we use consumer reference data provided by Infutor Data Solutions (now Verisk Marketing Solutions). Infutor aggregates consumer data (cell phone bills,

magazine subscriptions, etc.) into an address history for most adults in the United States. The data have been used frequently to measure housing moves and housing stability, starting with Diamond et al. (2019). It successfully measures housing stability for vulnerable groups, though it is more likely to miss adults under age 25 and immigrants (Phillips, 2020). We look up our study sample in the September 2022 Infutor extract using a fuzzy matching algorithm taking into account name, month of birth, year of birth, and SSN. We limit the analysis sample to matches containing at least one address starting before random assignment. We successfully match 50% of our sample, which is similar to other studies using Infutor data. In this sample, we define a move as an instance of a household starting a new address.

A.2 Marginal Value of Public Funds

We quantify the net return to spending a dollar on comprehensive case management using the Marginal Value of Public Funds (MVPF) framework. Following Hendren and Sprung-Keyser (2020), we measure the ratio of benefits of the program to program costs, net of taxes paid. For comparability, we apply this framework in essentially the same manner as in Evans et al. (2025).

Because of its intensity, BtS is expensive compared with other social service programs. As discussed above, the program operates on lower caseloads than typical case management, which gives additional mentor time to each participants but also raises staffing costs. Based on program data, the typical participant costs about $6,875 (2020 dollars) to serve for one year, or $6,295 when deflated to 2015. The program also lasts longer than typical social service programs, with the average participant completing 1.65 years, yielding an average of $10,386 per client.

On the benefits side, we compute increased earnings via changes in the employment rate. While it would be simple to measure treatment effects on total earnings, Table 3 shows that these estimates are too noisy to be informative in our sample. Instead, we estimate employment effects, multiply these effects by earnings among employed members of the control group, and assume no intensive margin effects. For each reported MVPF value, we take one particular employment effect from the distribution of beliefs reported in Figure A.7. For example, the median belief about employment effects pooling across both studies is 7.3 pp at three years and 5.8 pp at one year. We interpolate year two as the average of year one and year three. Whether employment rates increase after year three is outside our data, so we vary the assumed time horizon for persistence of the year three effect. To get earnings effects, we multiply these treatment effects on the employment rate by average earnings for employed members of the control group. In years 1-3, we simply infer this value from Table 3. Beyond year three, we follow Hendren and Sprung-Keyser (2020) and Evans et al. (2025) and impute earnings by adjusting for life-cycle effects, scaling year three earnings by the ratio of earnings of non-college 2012-2016 ACS respondents at age 40 versus later ages. Finally, we compute the present, discounted value of earnings effects using a discount rate of 3% and the national consumer price index for urban consumers. In the end, earnings gains from BtS are substantial but depend on whether employment effects last beyond the time horizon of our data. At median beliefs about employment effects, the present discounted value of earnings increases by $3,273 during the three observed years, $11,869 if we extrapolate out

to ten years, and \$26,290 if we allow relative effects to persist through age 65.

Taxes on earnings do not play a significant role in program benefits or costs. Following Hendren and Sprung-Keyser (2020), we use Congressional Budget Office tax rate estimates for groups defined by earnings relative to the poverty line. Even after bouncing back from negative shocks near the time of enrollment, average annual earnings among employed members of the control group are still only \$18,732 (2015 dollars), which is less than the 2015 family of four poverty rate of \$24,250. Even adjusting for life-cycle effects that peak at age 52, average earnings never exceed the poverty line. As a result, we apply an estimated tax rate of 9.6% throughout, with the exception of near-retirement years when earnings and tax rates fall further. Overall, even in a scenario with employment effects persisting until age 65, the present discounted value of tax revenue only increases by \$2,459.

We combine these values in a ratio to calculate MVPF. If benefits only last for three years, the MVPF is 0.29. This value results from \$3,273 in earnings gains which are split between \$2,958 in after-tax benefits and \$314 in taxes, which reduce the \$10,386 program cost to a net cost of \$10,072. Other scenarios are calculated in a similar manner.

Some caution should be taken as these calculations will underestimate the value of BtS to the extent that it generates improvements beyond earnings. Since we measure but do not observe improvement in housing, education, health, social networks, family, or financial outcomes, focusing solely on earnings may be reasonable. On the other hand, we observe evidence that BtS generates cognitive benefits, like increased hopefulness and subjective improvements in goal achievement. We also only observe outcomes for (or reported by) the head of household. To the extent that participants value these other benefits or benefits accrue to other members of the household (e.g., children), this MVPF exercise will underestimate the return to investing in such programs.

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A.3 Figures and Tables

Figure A.1: Bridge to Self-Sufficiency Matrix

Family Stability		Well-Being		Financial Management		Education & Training	Employment & Career Management	
Housing	Family	Physical and Mental Health	Networks	Debts	Savings	Educational Attainment	Hourly wage	Type of job
No subsidy, housing costs 1/3 or less of household gross pay	Fully able to engage in work, school, and family life; children or family needs don't get in the way (OR) No children or dependent family members	Fully able to engage in work, school, and family life; health and mental health needs don't get in the way	Can always rely on networks to provide useful advice, guidance, and support; has the ability to advocate for others	No debt other than mortgage, education, and/or car loans, and current in all debts	Savings of 3 months' expenses or more	Bachelor's degree or higher complete	Job with earnings equal to or greater than Rochester/ Monroe County hourly living wage* Your rate=	Full-time stable employment with excellent opportunities to advance
No subsidy, housing costs exceed 1/3 household gross pay	Mostly able to engage in work, school, and family life; children or family needs rarely get in the way	Mostly able to engage in work, school, and family life; health or mental health needs rarely get in the way	Can often rely on networks to provide useful advice, guidance, and support	Current in all debts and making more than minimum payments on one or more debts	Savings of more than 2 months' expenses but less than 3 months' expenses	Associate's degree or professional certification complete	Job with earnings 66-99% of Rochester/ Monroe County hourly living wage* Your rate=	Full-time employment with some opportunities to advance
Subsidized Housing – desirable location	Somewhat able to engage in work, school, and family life because of children or family needs	Somewhat able to engage in work, school, and family life because of health or mental health needs	Can sometimes rely on networks to provide useful advice, guidance, and support	Making minimum payments on all debts	Savings of at least one month and up to 2 months' expenses	Job training or certificate complete (beyond high school)	Job with earnings 33-65% of Rochester/ Monroe hourly living wage* Your rate=	Full-time employment with no opportunities to advance
Subsidized Housing – undesirable location	Barely able to engage in work, school, and family life because of children or family needs	Barely able to engage in work, school, and family life because of health or mental health needs	Can rarely rely on networks to provide useful advice, guidance, and support	Behind in payments of one or more debts and making payments on at least one	Savings of less than one month's expenses	High School Diploma / GED or HS equivalency	Job with earnings of less than 33% of Rochester/ Monroe hourly living wage* Your rate=	Temporary or part time/seasonal employment with no benefits
Not permanently housed or living conditions threatens health and/or safety	Not able to engage in work, school, and family life because of children or family needs	Not able to engage in work, school, and family life because of health or mental health needs	Can never rely on networks to provide useful advice, guidance, and support	Has debts; currently not making any payments	No savings	Less than High School Diploma / GED or HS equivalency	Not currently employed *work with Coach to discuss hourly rate www.livingwage.mit.edu	Unemployed with no prospects for employment

THINKING ABOUT THE FUTURE

Figure A.2: Study Timeline

Cohort 1 Entry (Jan '17 - Apr '18)

Cohort 1 Exit (Jan '19 - Apr '20)

Prelim. Results w/ DOL Data (Nov '21)

Prelim. Results w/ Follow-Up Survey Data (Feb '22)

Prelim. Results w/ DOL + OTDA Data (Oct '22)

Final Results (Oct '23)

2017 2018 2019 2020 2021 2022 2023

12 Month Survey (May '18 - June '19)

OTDA & DOL (Aug '19)

Cohort 2 Entry (Jan '20 - Jul '20)

DOL (Aug '21)

Cohort 2 Exit (Dec '21)

OTDA (Early '22)

DOL (Aug '22)

DOL (Aug '23)

Survey: 12 Mo C2 & 40 Mo C1 (Sept '21 - Dec '21)

Figure A.3: Treatment Effects on Credit Score, Over Time
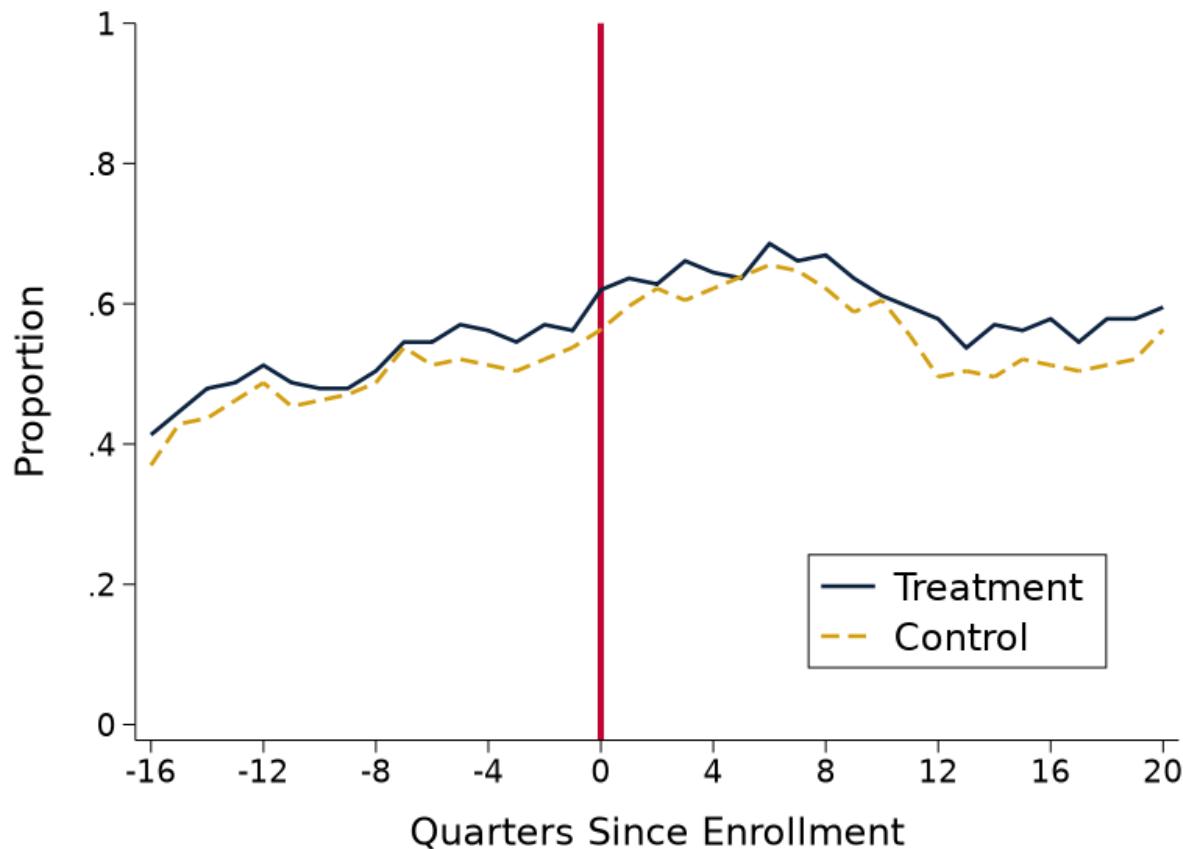
Notes: Outcomes measured used study data matched to credit reports from Experian. The sample is limited to people successfully match to an Experian record and have a non-missing credit score for quarters 3-5. Each plotted point comes from the coefficient on treatment in a regression of credit score in that quarter on a random assignment indicator and the pre-specified controls listed in the notes of Table 2, with the sample limited to people with non-missing data in that quarter. The whiskers shows 95% confidence intervals based on heteroskedasticity-robust standard errors. Treatment effects in quarter -1 are mechanically zero since the lagged credit score is included as a control.

Figure A.4: Responses to Executive Control Test by Number of Trials Since Starting the Test
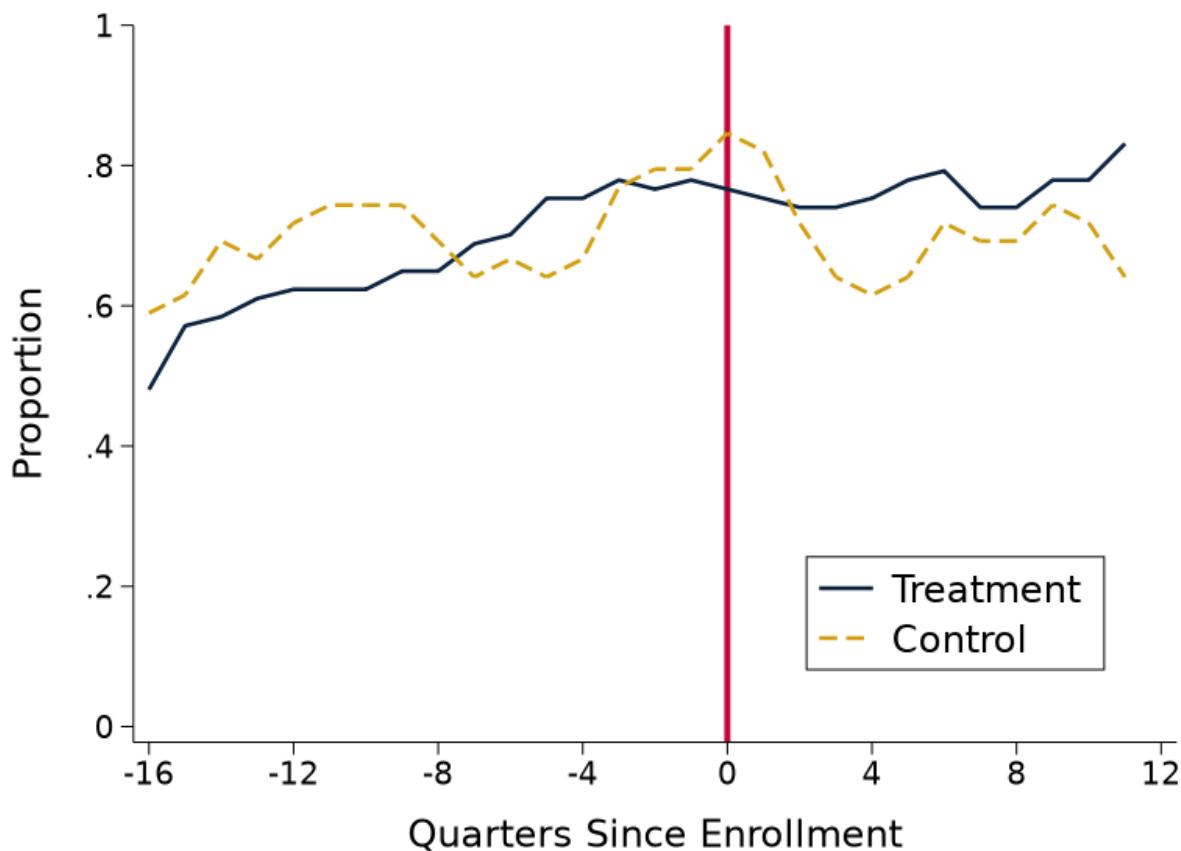
Notes: Data comes from dots-mixed/Simon executive control task on the one year follow-up survey. The sample includes all study participants who respond to the survey. The graph displays average response times in milliseconds, winzorized at the 5th and 95th percentiles. The first 20 trials were practice trials. Every 20 questions, participants had to click through an extract screen which accounts for the spikes at questions 21, 41, and 61.

Figure A.5: UI Employment Rates: Cohort 1
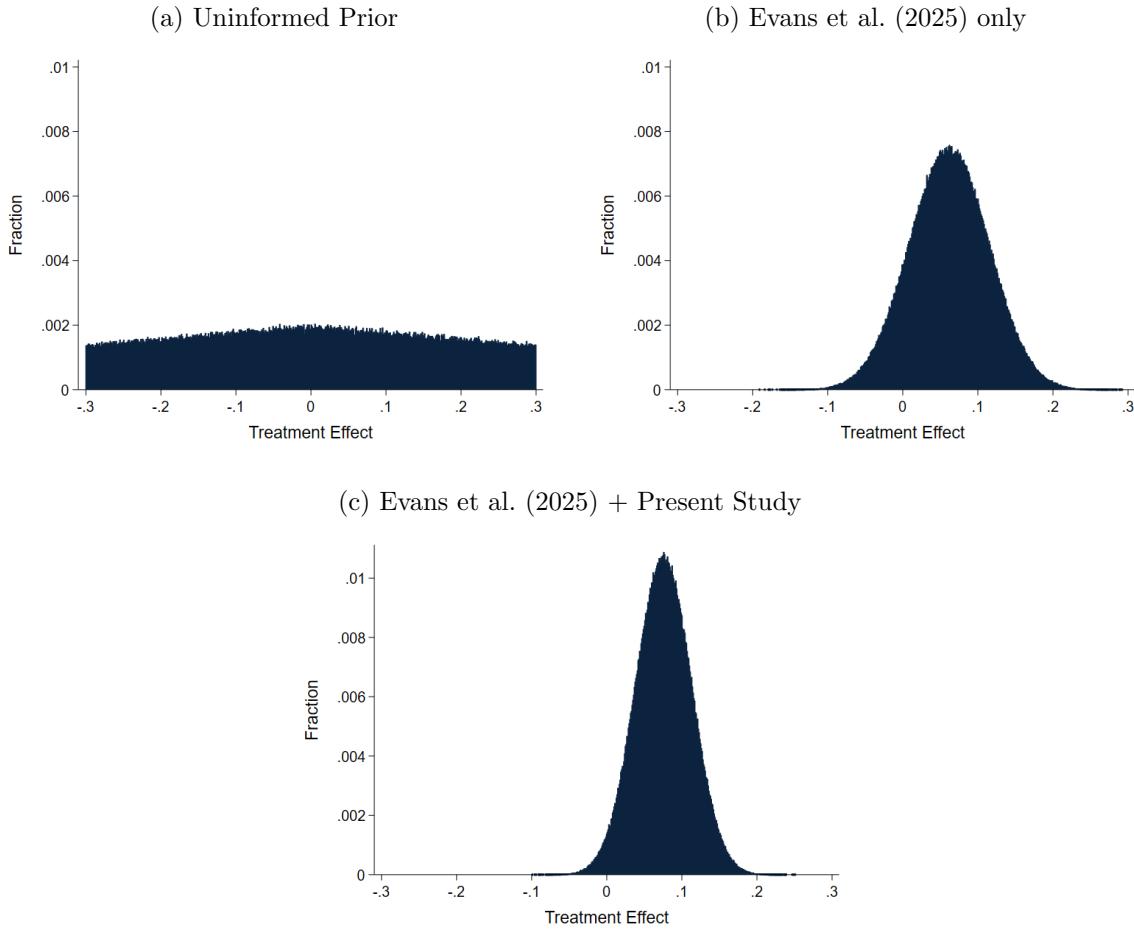
This figure displays employment rates measured using UI earnings records. For each quarter, we report the fraction of people employed in the 3-quarter window centered on the listed quarter. The sample includes all study participants from cohort 1 who report a valid SSN.

Figure A.6: UI Employment Rates: Cohort 2

This figure displays employment rates measured using UI earnings records. For each quarter, we report the fraction of people employed in the 3-quarter window centered on the listed quarter. The sample includes all study participants from cohort 2 who report a valid SSN.

Figure A.7: Bayesian Beliefs about Treatment Effects on Post-Program Employment Rates

(a) Uninformed Prior

(b) Evans et al. (2025) only

(c) Evans et al. (2025) + Present Study

Notes: To compute distributions of beliefs over treatment effects, we first define separate distributions of beliefs over employment rates for the control group and the treatment group. We then draw rates independently from the treatment and control distributions and calculate the difference, repeating one million times. In (a), the decision-maker has standard uniform prior beliefs over both treatment group and control group employment rates. Beliefs update to a beta distribution in panels (b) and (c). Beliefs parameters are calculated by simple counting of employment and non-employment in the control group. In the treatment group, we use the number of employed implied by adding employment treatment effects to the control group rate, rounded to the nearest whole value. In particular, we derive values from our three-year results and two-year results from Evans et al. (2025): (a) standard uniform for both treatment and control, (b) beta[100,92] for control and beta[92,66] for treatment, (c) beta[100+82,92+76] for control and beta[92+120,66+78] for treatment. The uninformed prior is truncated at ±0.3 for visual clarity.

Table A.1: Mean Baseline Characteristics by Cohort

	(1) Full Sample	(2) Cohort 1	(3) Cohort 2	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.34	0.27	0.48	-0.21***	0.00
Employed Quarter Prior	0.64	0.57	0.79	-0.23***	0.00
Quarterly Earnings ($)	1,176	804	2,027	-1,223***	0.00
Quarterly Earnings ($, Employed)	3,983	3,483	4,577	-1,095	0.19
No High School/GED	0.32	0.33	0.31	0.02	0.65
Married	0.08	0.06	0.13	-0.07**	0.01
Age	37.3	38.7	34.3	4.4***	0.00
Has Children	0.60	0.59	0.64	-0.06	0.28
Female	0.77	0.73	0.86	-0.13***	0.00
Hispanic	0.26	0.24	0.31	-0.06	0.16
White	0.09	0.09	0.09	-0.00	0.88
Black	0.64	0.64	0.64	0.00	0.99
Other Race	0.27	0.27	0.27	0.00	0.94
Primary Goal:					
-Housing	0.27	0.26	0.31	-0.05	0.31
-Family	0.06	0.06	0.08	-0.02	0.44
-Health	0.07	0.07	0.07	0.00	0.95
-Networks	0.02	0.02	0.01	0.01	0.35
-Education	0.16	0.14	0.21	-0.07*	0.09
-Employment	0.24	0.25	0.21	0.03	0.45
-Finances	0.11	0.10	0.12	-0.02	0.57
N	430	299	131		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the full study sample, column 2 is limited to cohort 1, and column 3 is limited to cohort 2. Column 4 reports raw mean differences between the two cohorts estimated by a linear regression of the listed variable on a cohort dummy. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.2: Mean Baseline Characteristics by Attrition

	(1) Full Sample	(2) Follow-Up	(3) No Follow-Up	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.34	0.28	0.42	-0.03	0.54
Employed Quarter Prior	0.64	0.57	0.77	0.04	0.43
Quarterly Earnings ($)	1,176	880	1,681	-54	0.86
Quarterly Earnings ($, Employed)	3,983	3,614	4,381	151	0.88
No High School/GED	0.32	0.31	0.33	0.01	0.85
Married	0.08	0.05	0.14	-0.02	0.38
Age	37.3	38.4	35.5	-0.6	0.58
Has Children	0.60	0.59	0.62	-0.04	0.37
Female	0.77	0.75	0.80	0.02	0.62
Hispanic	0.26	0.22	0.33	0.01	0.74
White	0.09	0.08	0.09	-0.00	0.99
Black	0.64	0.67	0.59	-0.02	0.62
Other Race	0.27	0.24	0.31	0.02	0.58
Primary Goal:					
-Housing	0.27	0.25	0.31	-0.05	0.29
-Family	0.06	0.06	0.08	0.02	0.50
-Health	0.07	0.07	0.06	-0.02	0.33
-Networks	0.02	0.03	0.00	0.00	0.80
-Education	0.16	0.15	0.17	0.06	0.10
-Employment	0.24	0.23	0.25	-0.01	0.83
-Finances	0.11	0.10	0.12	0.02	0.61
N	430	271	159		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the full study sample, column 2 is limited to people who respond to the one year follow-up survey, and column 3 is limited to those who do not. Column 4 reports adjusted differences between responders and attriters, estimated by a linear regression of the listed variable on a response dummy and strata fixed effects. Since cohort 2 is a stratum, this controls for cohort differences. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.3: Mean Baseline Characteristics - Follow-Up Survey Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.28	0.26	0.31	-0.06	0.31
Employed Quarter Prior	0.57	0.60	0.54	0.05	0.41
Quarterly Earnings ($)	880	834	931	-228	0.32
Quarterly Earnings ($, Employed)	3,614	3,562	3,667	-395	0.56
No High School/GED	0.31	0.33	0.29	0.03	0.61
Married	0.05	0.03	0.07	-0.05*	0.08
Age	38.4	37.9	38.9	-1.0	0.53
Has Children	0.59	0.58	0.60	-0.03	0.60
Female	0.75	0.77	0.74	0.04	0.47
Hispanic	0.22	0.25	0.19	0.07	0.19
White	0.08	0.07	0.10	-0.02	0.54
Black	0.67	0.65	0.69	-0.06	0.31
Other Race	0.24	0.28	0.21	0.08	0.13
Primary Goal:					
-Housing	0.25	0.23	0.27	-0.02	0.70
-Family	0.06	0.08	0.03	0.04	0.16
-Health	0.07	0.05	0.10	-0.06**	0.04
-Networks	0.03	0.03	0.02	-0.00	0.93
-Education	0.15	0.18	0.12	0.05	0.24
-Employment	0.23	0.25	0.22	0.04	0.42
-Finances	0.10	0.10	0.11	-0.02	0.62
N	271	141	130		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who respond to the one year follow-up survey, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.4: Mean Baseline Characteristics - UI Data Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.35	0.35	0.35	-0.04	0.45
Employed Quarter Prior	0.64	0.67	0.60	0.04	0.43
Quarterly Earnings ($)	1,192	1,218	1,160	-125	0.73
Quarterly Earnings ($, Employed)	3,894	3,953	3,819	-31	0.98
No High School/GED	0.33	0.34	0.33	0.00	0.96
Married	0.08	0.09	0.08	-0.01	0.83
Age	36.9	36.6	37.4	-0.5	0.70
Has Children	0.62	0.60	0.64	-0.06	0.27
Female	0.79	0.78	0.79	-0.01	0.79
Hispanic	0.25	0.28	0.22	0.04	0.36
White	0.08	0.08	0.09	-0.01	0.83
Black	0.67	0.64	0.70	-0.07	0.20
Other Race	0.25	0.28	0.22	0.07	0.13
Primary Goal:					
-Housing	0.29	0.27	0.31	-0.04	0.46
-Family	0.06	0.07	0.04	0.02	0.38
-Health	0.08	0.07	0.09	-0.02	0.47
-Networks	0.01	0.01	0.02	-0.01	0.57
-Education	0.16	0.19	0.13	0.05	0.24
-Employment	0.23	0.22	0.23	-0.00	0.94
-Finances	0.11	0.12	0.09	0.01	0.68
N	356	198	158		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who provided a valid SSN at baseline, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.5: Mean Baseline Characteristics - Follow-Up Survey or UI Data Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.34	0.34	0.34	-0.04	0.45
Employed Quarter Prior	0.64	0.67	0.60	0.04	0.43
Quarterly Earnings ($)	1,167	1,229	1,089	-51	0.85
Quarterly Earnings ($, Employed)	3,949	4,097	3,759	159	0.85
No High School/GED	0.32	0.32	0.31	0.01	0.91
Married	0.08	0.08	0.07	-0.00	0.91
Age	37.2	36.9	37.6	-0.2	0.84
Has Children	0.61	0.60	0.63	-0.05	0.33
Female	0.78	0.79	0.77	0.01	0.83
Hispanic	0.25	0.27	0.22	0.04	0.34
White	0.09	0.08	0.10	-0.02	0.60
Black	0.66	0.65	0.68	-0.04	0.41
Other Race	0.25	0.27	0.22	0.06	0.21
Primary Goal:					
-Housing	0.27	0.25	0.29	-0.04	0.36
-Family	0.06	0.07	0.05	0.02	0.38
-Health	0.07	0.06	0.09	-0.02	0.37
-Networks	0.02	0.02	0.02	0.00	0.91
-Education	0.17	0.20	0.13	0.05	0.17
-Employment	0.23	0.23	0.24	0.00	0.94
-Finances	0.11	0.11	0.10	0.01	0.75
N	396	220	176		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who either responded to the one year follow-up survey or provided a valid SSN at baseline, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.6: Mean Baseline Characteristics - Experian Credit Report Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.40	0.39	0.40	-0.03	0.57
Employed Quarter Prior	0.70	0.74	0.65	0.06	0.31
Quarterly Earnings ($)	1,381	1,431	1,320	-116	0.80
Quarterly Earnings ($, Employed)	3,936	4,084	3,755	30	0.98
No High School/GED	0.31	0.30	0.31	-0.02	0.66
Married	0.09	0.09	0.09	-0.01	0.76
Age	36.4	36.1	36.7	-0.5	0.75
Has Children	0.67	0.64	0.71	-0.08	0.16
Female	0.80	0.81	0.80	0.00	0.97
Hispanic	0.29	0.26	0.32	-0.06	0.27
White	0.07	0.08	0.07	-0.00	0.93
Black	0.64	0.66	0.62	0.03	0.60
Other Race	0.28	0.26	0.31	-0.03	0.61
Primary Goal:					
-Housing	0.27	0.25	0.30	-0.03	0.56
-Family	0.06	0.06	0.05	0.00	0.97
-Health	0.07	0.06	0.09	-0.04	0.21
-Networks	0.01	0.02	0.01	0.01	0.38
-Education	0.19	0.22	0.16	0.06	0.18
-Employment	0.26	0.25	0.28	-0.01	0.82
-Finances	0.13	0.14	0.12	0.01	0.90
N	285	157	128		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who are in the Experian dataset and have at least 1 nonmissing outcome variable quarters 3 through 5 post-enrollment, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.7: Mean Baseline Characteristics - OTDA Benefit Record Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.28	0.24	0.32	-0.09	0.11
Employed Quarter Prior	0.59	0.61	0.57	0.05	0.46
Quarterly Earnings ($)	833	651	1,006	-362	0.10
Quarterly Earnings ($, Employed)	3,499	3,092	3,807	-520	0.37
No High School/GED	0.34	0.36	0.32	0.04	0.49
Married	0.07	0.04	0.09	-0.06**	0.04
Age	38.2	37.5	38.9	-1.1	0.46
Has Children	0.63	0.62	0.63	-0.03	0.56
Female	0.73	0.76	0.69	0.06	0.26
Hispanic	0.25	0.25	0.25	-0.01	0.80
White	0.07	0.08	0.06	0.02	0.49
Black	0.64	0.62	0.66	-0.04	0.53
Other Race	0.28	0.29	0.27	0.01	0.79
Primary Goal:					
-Housing	0.26	0.26	0.27	-0.02	0.66
-Family	0.06	0.08	0.04	0.04	0.23
-Health	0.07	0.05	0.09	-0.04	0.23
-Networks	0.02	0.02	0.02	-0.00	0.98
-Education	0.15	0.18	0.12	0.05	0.24
-Employment	0.27	0.27	0.26	-0.01	0.81
-Finances	0.11	0.12	0.11	0.01	0.88
N	273	133	140		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who match to an Infutor record with an address starting prior to random assignment, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.8: Mean Baseline Characteristics for Infutor Address History Sample

	(1) Full Sample	(2) Treatment	(3) Control	(4) Adj. Diff.	(5) P-Value
Employed at Baseline	0.34	0.34	0.34	-0.03	0.65
Employed Quarter Prior	0.65	0.67	0.64	0.03	0.65
Quarterly Earnings ($)	1,242	1,328	1,146	16	0.98
Quarterly Earnings ($, Employed)	4,450	4,842	4,032	597	0.81
No High School/GED	0.30	0.30	0.29	-0.02	0.69
Married	0.08	0.08	0.08	-0.00	0.96
Age	40.6	40.8	40.4	0.8	0.62
Has Children	0.59	0.58	0.60	-0.04	0.58
Female	0.80	0.81	0.80	-0.01	0.83
Hispanic	0.19	0.21	0.16	0.05	0.33
White	0.09	0.08	0.11	-0.02	0.63
Black	0.72	0.72	0.73	-0.01	0.92
Other Race	0.19	0.20	0.17	0.03	0.63
Primary Goal:					
-Housing	0.27	0.25	0.29	-0.06	0.34
-Family	0.06	0.07	0.04	0.04	0.21
-Health	0.07	0.05	0.09	-0.03	0.45
-Networks	0.03	0.03	0.03	-0.00	0.87
-Education	0.13	0.16	0.09	0.06	0.24
-Employment	0.24	0.21	0.26	-0.04	0.56
-Finances	0.12	0.15	0.08	0.06	0.16
N	215	113	102		

Notes: Data measured as of baseline. Educational attainment, marital status, age, presence of children, sex, race, and ethnicity are from BtS program records; employment in the prior quarter is from UI earnings records; and all other variables are from the baseline survey. Columns 1-3 report raw means. Column 1 is the set of people who respond to the one year follow-up survey, column 2 further limits to the treatment group, and column 3 to the control group. Column 4 reports the coefficient on treatment in a regression of the listed variable on a random assignment dummy and strata fixed effects. P-values in column 5 are computed using heteroskedaticity-robust standard errors.

Table A.9: Residential Moves within 12 Months, Infutor Data

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Any Move	215	0.04	0.05	0.02	0.04
					(0.03)
Moved to					
City of Rochester	215	0.03	0.04	0.02	0.01
					(0.03)
Monroe County	215	0.03	0.04	0.02	0.03
					(0.03)
New York State	215	0.03	0.04	0.02	0.03
					(0.03)
Out of New York State	215	0.00	0.01	0.00	0.01
					(0.01)

Notes: Data comes from all study participants who match to an Infutor address history that includes at least one address starting before random assignment. We define a move as any address spell that starts between the month of random assignment and 12 months later. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.10: Employment Outcomes - All OLS, Strata Fixed Effects

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-Value
1 Year Results (Survey + UI)						
Employed	396	0.68	0.71	0.64	0.10** (0.05)	0.03
Quarterly Earnings ($)	396	2,708	2,836	2,548	181 (327)	0.58
Earnings Increased	396	0.53	0.53	0.52	0.06 (0.05)	0.24
Earnings Above Poverty Line	394	0.21	0.20	0.22	-0.03 (0.04)	0.37
1 Year Results (UI)						
Employed	356	0.66	0.69	0.62	0.06 (0.05)	0.24
Quarterly Earnings ($)	356	2,808	2,955	2,623	83 (371)	0.82
Earnings Increased	356	0.47	0.46	0.47	0.03 (0.06)	0.64
Earnings Above Poverty Line	354	0.23	0.23	0.24	-0.03 (0.04)	0.43
3 Year Results (UI)						
Employed	356	0.60	0.67	0.52	0.08 (0.05)	0.13
Quarterly Earnings ($)	356	3,192	3,502	2,803	38 (486)	0.94
Earnings Increased	356	0.44	0.48	0.39	0.06 (0.06)	0.27
Earnings Above Poverty Line	354	0.24	0.28	0.20	0.04 (0.05)	0.36

Notes: The top panel measures total earnings for people with either UI earnings records or one year survey responses, taking the mean when both are available. Non-employment is coded as zero earnings. 'Employed' and 'earnings increase' indicate non-zero earnings and an increase in earnings relative to baseline, respectively, according to the composite earnings measure. The middle and bottom panels limit the outcome measure to UI earnings records. When measuring earnings with UI records, we average earnings across a three-quarter window centered on the listed point in time. Participants who enrolled in 2020Q2 or 2020Q3 have less data for the three year outcomes and use either two quarters (2020Q2) or one quarter (2020Q3) data. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for all variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports analytical p-values from the same regression. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.11: Employment Outcomes - No Accounting for Stratification

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-Value
1 Year Results (Survey + UI)						
Employed	396	0.68	0.71	0.64	0.10** (0.04)	0.03
Quarterly Earnings ($)	396	2,708	2,836	2,548	131 (330)	0.69
Earnings Increased	396	0.53	0.53	0.52	0.05 (0.05)	0.30
Earnings Above Poverty Line	394	0.21	0.20	0.22	-0.04 (0.04)	0.28
1 Year Results (UI)						
Employed	356	0.66	0.69	0.62	0.06 (0.05)	0.24
Quarterly Earnings ($)	356	2,808	2,955	2,623	38 (367)	0.92
Earnings Increased	356	0.47	0.46	0.47	0.01 (0.05)	0.85
Earnings Above Poverty Line	354	0.23	0.23	0.24	-0.06 (0.04)	0.18
3 Year Results (UI)						
Employed	356	0.60	0.67	0.52	0.09* (0.05)	0.07
Quarterly Earnings ($)	356	3,192	3,502	2,803	59 (469)	0.90
Earnings Increased	356	0.44	0.48	0.39	0.06 (0.05)	0.27
Earnings Above Poverty Line	354	0.24	0.28	0.20	0.04 (0.05)	0.32

Notes: The top panel measures total earnings for people with either UI earnings records or one year survey responses, taking the mean when both are available. Non-employment is coded as zero earnings. 'Employed' and 'earnings increase' indicate non-zero earnings and an increase in earnings relative to baseline, respectively, according to the composite earnings measure. The middle and bottom panels limit the outcome measure to UI earnings records. When measuring earnings with UI records, we average earnings across a three-quarter window centered on the listed point in time. Participants who enrolled in 2020Q2 or 2020Q3 have less data for the three year outcomes and use either two quarters (2020Q2) or one quarter (2020Q3) data. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for continuous variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Dichotomous outcomes report average marginal effects from a logistic regression. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports analytical p-values from the same regression. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.12: Employment Outcomes - Doubly Robust (Inverse Propensity Weights)

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-Value
1 Year Results (Survey + UI)						
Employed	396	0.68	0.71	0.64	0.10** (0.04)	0.02
Quarterly Earnings ($)	396	2,708	2,836	2,548	114 (335)	0.73
Earnings Increased	396	0.53	0.53	0.52	0.06 (0.05)	0.25
Earnings Above Poverty Line	394	0.21	0.20	0.22	-0.04 (0.04)	0.24
1 Year Results (UI)						
Employed	356	0.66	0.69	0.62	0.06 (0.05)	0.23
Quarterly Earnings ($)	356	2,808	2,955	2,623	-23 (365)	0.95
Earnings Increased	356	0.47	0.46	0.47	0.01 (0.05)	0.83
Earnings Above Poverty Line	354	0.23	0.23	0.24	-0.06 (0.04)	0.11
3 Year Results (UI)						
Employed	356	0.60	0.67	0.52	0.09* (0.05)	0.07
Quarterly Earnings ($)	356	3,192	3,502	2,803	-33 (512)	0.95
Earnings Increased	356	0.44	0.48	0.39	0.06 (0.05)	0.26
Earnings Above Poverty Line	354	0.24	0.28	0.20	0.05 (0.04)	0.27

Notes: The top panel measures total earnings for people with either UI earnings records or one year survey responses, taking the mean when both are available. Non-employment is coded as zero earnings. 'Employed' and 'earnings increase' indicate non-zero earnings and an increase in earnings relative to baseline, respectively, according to the composite earnings measure. The middle and bottom panels limit the outcome measure to UI earnings records. When measuring earnings with UI records, we average earnings across a three-quarter window centered on the listed point in time. Participants who enrolled in 2020Q2 or 2020Q3 have less data for the three year outcomes and use either two quarters (2020Q2) or one quarter (2020Q3) data. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for continuous variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Dichotomous outcomes report average marginal effects from a logistic regression. All treatment effect regressions use inverse propensity weights, which are calculated from a logistic regression of treatment assignment on the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports analytical p-values. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.13: Detailed Employment Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-value
Employed	271	0.55	0.58	0.52	0.11* (0.06)	0.05
Hours Worked per Week	271	19.31	19.39	19.22	3.07 (2.26)	0.15
Hourly Wage (Simple)	148	12.51	12.54	12.48	-0.44 (1.23)	0.60
Hourly Wage (Dynamic)	147	12.30	11.92	12.75	-1.09 (1.75)	0.43
Total Household Income	258	14694.84	13160.70	16352.70	-3,053 (2,020)	0.13

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. Employed and hours worked are reported for the full sample. Hourly wage rates limit the sample to employed people. The 'simple' hourly wage assumes 40 hour work-weeks for all participants while the 'dynamic' version uses reported hours. The missing observation for dynamic hourly wage is due to one person reporting employment but zero hours worked. The smaller sample for income results from 'I do not know' responses. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for continuous variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Dichotomous outcomes report average marginal effects from a logistic regression. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.14: Outcomes in Many Domains, One Year Survey, Narrower Sample

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-value
Quarterly Earnings Increase	182	0.47	0.46	0.48	0.05 (0.08)	0.52
High Home Quality	182	0.31	0.29	0.34	0.00 (0.08)	0.94
Improvement in Primary Goal	182	0.45	0.50	0.39	0.21*** (0.07)	0.01
Improvement in Primary Goal (Bridge Tool)	182	0.46	0.42	0.51	-0.06 (0.07)	0.44
All Children Enrolled in School	182	0.84	0.85	0.82	0.04 (0.07)	0.56
Increased Health	182	0.23	0.23	0.23	0.02 (0.07)	0.69
Increased Social Networks	182	0.52	0.50	0.56	-0.07 (0.07)	0.39
Increased Education or Enrolled	182	0.44	0.43	0.46	-0.02 (0.08)	0.80
Increased Net Assets	182	0.38	0.38	0.39	-0.05 (0.08)	0.54

Notes: Outcomes are measured in the one year follow-up survey. The sample includes all respondents to the survey who have a valid response for "Improvement in Primary Goal". Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports treatment effects, which for continuous variables is the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Dichotomous outcomes report average marginal effects from a logistic regression. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.15: Outcomes in Many Domains, One Year Survey, Continuous Outcomes

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.	(6) P-Value
Quarterly Earnings ($)	271	2363.93	2323.98	2407.27	158.22 (345.95)	0.63
Home Quality (Likert)	271	2.97	2.93	3.02	-0.04 (0.17)	0.82
Primary Goal (Likert)	271	3.19	3.36	2.96	0.66*** (0.20)	0.00
Primary Goal - Bridge Tool (Z-Score)	271	-0.04	-0.04	-0.04	0.09 (0.19)	0.60
% Children in Enrolled School	151	0.75	0.71	0.79	-0.02 (0.06)	0.79
Health Status (Likert)	271	2.74	2.80	2.68	0.15 (0.13)	0.25
Social Networks Index (Sum of Z-Scores)	271	0.01	-0.01	0.02	0.59 (0.47)	0.15
Net Assets ($)	271	-1.3e+04	-1.2e+04	-1.5e+04	988.72 (2432.54)	0.67

Notes: Outcomes are measured in the one year follow-up survey. The sample includes all respondents to the survey who have a valid response for "Improvement in Primary Goal". Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Column 6 reports p-values calculated by randomization inference, accounting for stratification by study entry timing. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.16: Detailed Public Benefit Outcomes

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Survey					
Amount ($) HH Received Last Month From:					
SNAP	254	209	226	192	3
					(24)
TANF	260	53	45	61	-20
					(19)
SSI	252	212	166	260	-44
					(41)
SSA	253	72	62	83	-38
					(34)
WIC	260	10	8	12	-6
					(6)
UI	270	31	32	30	3
					(18)
Child Support	163	31	30	32	-10
					(11)
Gifts	270	38	25	51	-12
					(19)
OTDA					
Received Any:					
SNAP	273	0.68	0.74	0.63	0.06
					(0.05)
PA	273	0.21	0.21	0.21	0.02
					(0.04)
Total Benefits	273	0.68	0.74	0.63	0.07
					(0.05)
Amount ($) Received:					
SNAP	273	219	255	186	44**
					(21)
PA	273	119	124	113	15
					(28)
Total Benefits	273	338	379	299	68*
					(38)

Notes: In the top panel, data comes from survey responses for all study participants who responded to the one year follow-up survey. Sample sizes vary due to non-response and participants responding 'I don't know.' In the bottom panel, data comes from public benefit records for people from cohort 1 successfully matched to OTDA records. Program acronyms refer to Temporary Aid for Needy Families (TANF), Social Security (SS), Social Security Income for disability (SSI), Unemployment Insurance (UI), Women Infants and Children (WIC), and Supplemental Nutrition Assistance Program (SNAP). Public Assistance (PA) is New York's TANF program. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.17: Detailed Housing Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Owns or Pays Rent	271	0.84	0.83	0.85	-0.06
					(0.05)
Broken or Boarded Windows	271	0.10	0.10	0.11	-0.02
					(0.04)
Leak in Home	269	0.27	0.32	0.21	0.09
					(0.06)
Heating Issue	270	0.13	0.13	0.14	-0.03
					(0.05)
Home Quality	271	2.97	2.93	3.02	0.03
					(0.18)
Crime Severity	271	3.31	3.48	3.14	0.45*
					(0.25)
School Quality	271	1.94	2.10	1.76	0.36
					(0.41)
Neighborhood Quality	271	2.72	2.71	2.72	0.07
					(0.17)
Evicted in Past Year	270	0.08	0.09	0.07	0.03
					(0.04)
Evicted Past Year	221	0.12	0.11	0.12	0.02
					(0.05)
Number of					
People in HH	271	3.08	3.16	2.98	0.14
					(0.16)
Kids in HH	271	1.31	1.31	1.30	-0.00
					(0.11)
Seniors in HH	271	0.07	0.08	0.05	0.04
					(0.04)
Nonworking Adults in HH	271	0.28	0.32	0.23	0.09
					(0.07)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.18: Detailed Family Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Lack of Childcare is Reason for					
Not Working	271	0.00	0.00	0.01	-0.01
					(0.01)
Missing Work	271	0.00	0.01	0.00	0.01
					(0.01)
Other Outcomes					
Number of Child's School Absences	135	3.98	4.24	3.71	0.17
					(0.58)
Amount Spent on Dependents ($)	271	5.90	9.36	2.15	4.08
					(3.37)
Family is Goal Area	271	0.04	0.04	0.03	-0.00
					(0.02)
Involved in Child Custody Case	271	0.05	0.04	0.05	-0.04
					(0.08)
Z-Score Index	271	0.01	0.13	-0.12	0.05
					(0.32)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. The Z-score Index is the sum of the z-scores of the other variables. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively. The outcomes for school absences exclude families without children or who respond 'I don't know.'

Table A.19: Detailed Health Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Any Chronic Health Condition	271	0.34	0.33	0.35	0.00
					(0.06)
Any ER Vist (Year)	271	0.56	0.55	0.57	-0.02
					(0.06)
Number Doctor Visits (Year)	271	7.34	8.31	6.28	3.71**
					(1.64)
Has go-to Doctor	271	0.89	0.89	0.88	0.01
					(0.04)
Any Doctor visits (6 Months)	270	0.85	0.86	0.83	0.02
					(0.05)
Any Dentists Visits (6 Months)	271	0.44	0.42	0.47	-0.01
					(0.06)
Any Delayed Medical Treatment	271	0.16	0.15	0.17	-0.04
					(0.05)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.20: Social Networks Detailed Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Control	(4) Treatment	(5) Adj. Diff.
Number of:					
Organizations Involved in	271	0.58	0.71	0.45	0.26*
					(0.14)
People to Borrow From	271	1.53	1.50	1.56	0.12
					(0.20)
Close Friends	271	2.23	2.13	2.33	0.18
					(0.62)
Close Relatives	271	2.56	2.58	2.55	0.81
					(1.16)
Any Religious Group	271	0.36	0.35	0.38	-0.02
					(0.06)
Interactions with Neighbors	271	1.42	1.24	1.61	-0.48
					(0.31)
Religious Group Interactions	271	3.28	3.28	3.29	1.79
					(1.50)
Z-Score Index	271	0.01	-0.01	0.02	0.62
					(0.45)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. The index is defined as the sum of the z-scores of the other variables. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.21: Detailed Education Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
In Highschool	271	0.03	0.04	0.02	0.02
					(0.02)
In College	271	0.08	0.06	0.09	-0.01
					(0.03)
In Technical School	271	0.15	0.13	0.18	0.01
					(0.04)
Has High School Degree	271	0.34	0.36	0.32	0.10*
					(0.05)
Has College Degree	271	0.17	0.17	0.18	-0.08**
					(0.04)
Has Professional Certificate	271	0.31	0.28	0.35	-0.01
					(0.05)
Increased Education	271	0.19	0.17	0.21	-0.06
					(0.05)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.22: Detailed Financial Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Any Bank Account	268	0.64	0.70	0.57	0.16***
					(0.06)
Amount in Bank Account	261	703	760	641	170
					(240)
HH Income ($)	258	14,695	13,161	16,353	-2,688
					(2,001)
Any Food Pantry	271	1.82	1.84	1.81	0.01
					(0.05)
Claimed EITC	264	1.66	1.68	1.62	0.09*
					(0.05)
Used Payday Loan	271	0.01	0.01	0.02	0.00
					(0.01)
Multiple Payday Loans	271	0.01	0.01	0.02	0.00
					(0.01)
Any Rollover from a Payday Loan	271	0.00	0.00	0.01	0.00
					(0.00)
Liabilities ($)	270	13,878	12,453	15,413	-1,538
					(2,604)
Any HH Credit Card Debt	270	0.18	0.17	0.19	-0.03
					(0.05)
HH Credit Card Debt ($)	270	598	566	633	-371
					(304)
Any HH Budget	271	0.47	0.45	0.49	-0.06
					(0.06)

Notes: Data comes from survey responses for all study participants who responded to the one year follow-up survey. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.23: Detailed Credit Report Outcomes, Experian

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Total Amount of Credit Card Debt	285	332	349	311	-30
					(143)
Vantage Credit Score	276	553	553	553	-6
					(8)
Prime Credit Score (≥ 650)	276	0.13	0.13	0.13	-0.03
					(0.05)
Total Balance on All Open Trades	285	10,768	10,890	10,618	-1,956
					(2,797)
Has Debt	285	0.61	0.65	0.57	0.04
					(0.06)
Has Credit Card Debt	285	0.31	0.32	0.29	-0.04
					(0.06)
Total Balance on Open Mortgage-Type Trades	285	2,749	2,508	3,045	-1,042
					(1,842)
Total Debt Without Mortgage	285	8,019	8,383	7,573	-914
					(1,982)

Notes: Data comes from Experian credit reports for the set of people who successfully match to a credit report and have non missing data for quarters 3, 4, or 5 after enrollment. Column 1 shows the number of participants with nonmissing data, column 2 is the full sample mean, column 3 is the treatment mean, column 4 is the control mean, and column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.24: Effects on Cognition, One Year

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Hope					
Increase in Total Hope	238	0.56	0.61	0.51	0.13*
					(0.07)
Increase in Agency Hope	240	0.55	0.60	0.49	0.17**
					(0.07)
Increase in Pathway Hope	240	0.48	0.47	0.50	-0.03
					(0.07)
Executive Control					
All Trials					
Average Push Time	263	92.8	95.5	90.4	2.1
					(3.0)
Percent Correct	263	97.7	96.5	98.8	-2.8*
					(1.5)
Congruent Trials					
Average Push Time	263	93.7	96.9	90.8	3.3
					(3.1)
Percent Correct	263	98.0	96.8	99.1	-2.8*
					(1.5)
Incongruent Trials					
Average Push Time	263	91.9	94.0	90.0	0.9
					(3.1)
Percent Correct	263	97.4	96.3	98.5	-2.8*
					(1.5)

Notes: Outcomes are measured in the one year follow-up survey. Hope is coded as a zero to one index based on the state hope scale (Snyder et al., 1996). We compose the index by adding up the Likert scales for agreement with the following scales and dividing by the maximum total score: '1. If I should find myself in a jam, I could think of many ways out of it', '2. At the present time, I am energetically pursuing my goals', '3. There are lots of ways around any problem that I am facing now', '4. Right now, I see myself as being pretty successful', '5. I can think of many ways to reach my current goals', '6. At this time, I am meeting the goals that I have set for myself.' Items 1, 3, and 5 are in the pathways sub-scale, while 2, 4, and 6 are in the agency sub-scale. The sample for the top panel includes all respondents to the survey who responded to the hope module; total hope requires non-missing agency and pathway values. Some participants skipped one section but not the other so the total hope variable has a smaller sample size. The bottom panel reports results from the dots-mixed/Simon task (Simon, 1990). The sample excludes people who did not complete this task during the survey. Outcomes are averages across 80 trials. Push time is measured in milliseconds. Column 1 counts non-missing observations and columns 2-4 report raw means. Column 5 reports the coefficient on treatment from a linear regression of the listed outcome on a random assignment indicator, strata fixed effects, and the pre-specified controls listed in the notes of Table 2. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.