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

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August 9, 2024

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. Participants randomly assigned to intensive, holistic, wrap-around services have 10 percentage points higher employment rates after one year compared with a control group offered only help with an immediate need. Most of this effect appears to persist after programming ends. However, we find limited evidence that intensive, holistic services affect areas beyond employment, even when other areas of life are participants' primary goals. We find some evidence that the program works by increasing hopefulness and agency among participants, which may be more useful in supporting labor force participation than in meeting other goals.

J-PAL North America, the Bill & Melinda Gates Foundation, Arnold Ventures, and the Wilson Sheehan Lab for Economic Opportunities (LEO) supported this research. Special thanks to our partners at Action for a Better Community, Catholic Family Center, Community Place, the City of Rochester, and the Rochester-Monroe Anti-Poverty Initiative, especially Lydia Alston-Murphy, Henry Fitts, Shawn Futch, Sue Kervin, Leslie Mosman, Ron Rizzo, Greg Sheldon, and Aaron Wicks. Victor Cardena, Jacqueline Kelley-Cogdell, Shawna Kolka, Aubrey McDonough, Connor Murphy, Grace Ortuzar, Emily Pohl, Kelli Reagan, and Seth Zissette provided excellent research assistance. The University of Wisconsin Survey Center did heroic work on the surveys with Jaime Faus and John Stevenson leading. Christine Steenburgh at NYDOL helped with UI earnings records. Thanks to seminar participants at the US Census Bureau and CEIDS for comments that improved the paper. This paper represents the authors' views and not necessarily those of any of these partners. This study was approved by the University of Notre Dame IRB, protocol #17-03-3707, and has an analysis plan registered on the Open Science Framework (https://osf.io/pkf9r/).

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, 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 single case manager coordinates services from a number of areas and providers to best achieve the client's goals. CCM has been used in a variety of areas including prisoner re-entry, disease and health care management, mental health, substance abuse, developmental disabilities, people enrolled in college or job training, refugee resettlement, and increasingly, programs designed to reduce poverty. Although these situations are very different, most have a singular goal (e.g., reduce recidivism, increase the chance of being housed, reduce emergency department visits), but given the nature of CCM, there can also be

spillovers in other domains.

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., forthcoming), 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

[1] Economic Mobility Exchange, Annual Member Report FY23

a long-term goal of exiting poverty. Whether such CCM programs can affect both a primary target outcome and 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), implemented in Rochester, New York. The program recruited very low-income residents and 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 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. Between February of 2017 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. Only half of participants were employed one year before entering the study. At study entry, participants were actively seeking social services and only one-third were employed with average quarterly earnings of about $2,500. The program was designed by social service agencies as a way to improve economic outcomes for study participants with a focus on employment, which is our primary pre-specified outcome. 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.

This intensive, individualized CCM program increases employment rates. Using data from both surveys and administrative records, we measure employment for 93% of participants for one year and 83% of participants for three years. We find that after one year, being assigned to treatment increased employment by 10 percentage points compared with the control group, which was a 15% increase relative to the control group employment rate. Most of the estimated increase persists over time, becoming a statistically insignificant 8 percentage points three years after random assignment. Despite the increase in employment, the results for earnings were small and statically imprecise. Given the variance in earnings, the study was underpowered to find results for this outcome, requiring a 31% change in earnings to detect a

statistically significant impact.

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% to 52% in one year. Therefore, the treatment effects are only half the size of the change in employment in the control group over time. Since people enter such programs in response to poor economic situations, employment rates return to average values over time. Observational studies following treatment group members over time, a common evaluative device among social service programs like this, will wildly overstate treatment effects. On the other hand, the measured treatment effect is similar in both sign and magnitude to results from an RCT of a similar CCM called the Padua Pilot in Fort Worth, TX (Evans et al., forthcoming). Despite their complexity, anti-poverty CCM programs appear to have similar effects in very different contexts, though the program we study is somewhat less expensive so that the present discounted value of earnings gains to participants would exceed the cost of the program if they persist for about seven years.

CCM was less effective in generating improvements for the 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 mea-

sures 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 outcomes could be Hawthorne 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 align with other evidence that suggests that CCM can impact a more limited set of focused outcomes but has difficulty addressing all what ails program participants.

This pattern of moderate improvements in employment but less progress in individually defined outcomes may be linked to the mechanisms by which BtS actually helps participants. One might expect that an individualized social service program would connect people with services matching their goals; however, while we find some evidence that treatment increased contact with external social services, these services are not more likely to align with the person's stated goals. The treatment group also scores no better than the control group on a lab task that measures ability to resist impulses in pursuit of a goal. On the other hand, we find some evidence that this package of mentoring, goal setting, and incentives affects cognition. Members of the treatment group score higher on a psychometric index of hopefulness. Greater hope runs entirely through items that measure how much action people are taking toward

their goals ('agency') rather than their ability to identify a plan for meeting goals ('pathways'). Together, these results suggest that, rather than building cognitive human capital or connecting people to individualized services, CCM primarily works by supporting participants as they initiate work on their goals. Such initiative may be particularly valuable in starting new employment but less helpful in increasing earnings on the intensive margin or making progress outside employment.

Fitting with this interpretation, the program was particularly effective at increasing employment among people who are unlikely to be employed in the absence of the program. The treatment effects on employment are concentrated among those that were unemployed or had not completed high school at baseline, people who had much greater room to improve labor market outcomes by starting a new job. Though sub-group results are noisy due to small sample sizes and should be taken only as suggestive, they support the idea that intensive supports encourage employment the most among people with the greatest labor market barriers.

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., forthcoming). Similarly, housing programs designed to facilitate moves to high opportunity neighborhoods not only might 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 using 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.

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 quarterly 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.

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), 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 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.

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 the idea from cognitive science that situations of scarcity lead people to take sub-optimal approaches to long-term goals (Mullainathan and Shafir, 2013). To meet this concern, BtS provides direct financial incentives to participants that are tied to progress toward goals. For example, a mentor might compensate a participant for completing a job application or for attending a goal-setting session with them.

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.

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 AmeriCorps 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. 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 (post-COVID)

cohort. The difference between in-person and 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 and were unknown to study 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 while entering the BtS program (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 individual participants. A copy of the baseline survey can be found at this link.

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. 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 changes in outcomes. A copy of the follow-up survey is available at this link.

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 keyboard if a green circle appears and 'z' if a red symbol appears. However, the symbols could appear on either 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 out-

comes 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 (SSN) 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. As data are limited to the State of New York, we cannot distinguish between a participant not working and working in another state. 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 implies that if someone appears employed in one dataset but unemployed in the other, we code them as employed.

3.2.4 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.

3.2.5 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 1-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 1-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 ($\leq$\$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.

3.2.6 Infutor Data

To measure housing moves, we use consumer reference 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.

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 assign-

ment. 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 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. 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 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 small,

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. The 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, we use the following specification

$$Y_i = \beta_0 + D_i\beta_1 + \mathbf{X_i\Gamma} + \epsilon_\mathbf{i}. \tag{1}$$

In this model, Y_i is an outcome for person i, such as being employed one year 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 pa-

rameter of interest is β_1, which is the average effect of being offered a spot in the program. We include covariates and fixed effects to estimate more precise standard errors of the treatment effects and to condition on the strata of random assignment. The vector of covariates, $\mathbf{X_i}$, includes the baseline value of the outcome (when available), age, gender, marital status, and sets of mutually exclusive and exhaustive race and education indicators. We also include sets of mutually exclusive and exhaustive dummies for the participant's stratification group, month of random assignment, and months between baseline and follow-up surveys.

The identifying assumption that gives β_1 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 the stratification group effects. 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. Similarly, we construct balance tables for data sources we use for non-employment outcomes. Appendix Tables A.6, A.7, and A.8 demonstrate baseline balance for the Experian, OTDA, and Infutor sub-samples, respectively. 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 Main Results

4.1 Services Received

Consistent with study assignment, people assigned to treatment are much more likely to have frequent interaction with BtS one year after study entry than those assigned to control. Table 2 displays services receipt reported in the one-year follow-up survey. Of people assigned to treatment, 86% recall being involved with BtS in the past year. Since they do complete study enrollment and receive short-term services, 49% of the control group also recalls BtS. However, the gap in intensity of interaction is wide: the treatment group is 65 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: 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 12 percentage points more likely to receive help from community organizations outside the BtS network, though this difference is not statistically significant. 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 an 11 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

BtS increases the employment rate of participants. Our primary pre-specified outcome is an employment indicator, which combines evidence of employment from 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.

A 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 (3) and 18 percentage points using only survey data (Appendix Table A.10). 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 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. Similarly, average unconditional earnings increase by $158 per quarter. Both of these estimates are somewhat imprecise and not statistically different from zero. The 95% confidence interval for earnings effects runs from $-471 to $787 per quarter, or -18% to 31% of the control mean. In both cases we also cannot reject the null that the program increases employment rates but otherwise does not affect earnings; i.e., that earnings increase by $255 or that the proportion of people with rising earnings increases by 10 percentage points.

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 statistally different from the 10 percentage point effect measured in the combined data, but it is slightly lower. Results in Appendix Table A.10 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 over time for both groups, as measured by administrative UI records. The gap in employment

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 8 percentage points more likely to have a positive UI earnings record. This difference is not statistically different from zero, so it should be interpreted cautiously. However, it is close to the main 10 percentage point increase observed at 1 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.

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., forthcoming), 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 for the sub-sample of survey respondents. As in the full sample, members of the treatment group are more likely to experience an increase in earnings since baseline. 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 if a pre-specified measure improves for 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 19 percentage points greater in the treatment group. On the other hand, improvement on more specific measures only happens 2 percentage points more often in the treatment group.[2] The bottom panel 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.[3] 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

[2]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. The difference in sample is not the cause of the difference in treatment effects. See Appendix Table A.11.

[3]See also Appendix Tables A.10, A.12, A.13, A.14, A.15, A.16, A.17 and A.18 which show detailed survey measures for each area and Appendix Table A.19 and Appendix Figure A.3 which show financial measures from credit reports.

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. Hawthorne 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 Discussion

5.1 Mechanisms

A pattern of null results for more individualized outcomes suggests that the program does not achieve observed employment effects primarily through its holistic, individualized approach to goals. As noted above, the bottom panel of Table 2 indicates that services received are not particularly connected to initial goals. And likely as a result, 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. As a result, we observe the results in Table 4, in which outcomes beyond employment rarely show positive treatment effects, even when looking specifically at an area designated as the primary goal at baseline. The only exception to this is an overall subjective rating of progress in the initial goal area. Though, one possibility is that this increased subjective rating is more indicative of changing cognition than changing circumstances.

We also see some direct evidence that the program generates changes in cognition,

which could lead to the observed increases in employment. Programs like EMPath and BtS use growing research in brain science to help design these comprehensive anti-poverty programs (Babcock, 2014). The 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). Table 5 provides some evidence that BtS changes cognition. The top half of the table shows evidence that people offered treatment are more hopeful at follow-up, as measured by the 'state hope' scale (Snyder et al., 1996).[4] The control group scores 0.50, or right in the middle of the scale, while the treatment group scores 0.12 higher, 0.3 standard deviations higher.

Increased motivation and agency, rather than improved problem-solving skills, appear to drive the change in cognition. As shown in the next two rows of Table 5, the observed increase in hope is driven entirely by the agency sub-scale, which measures motivation to take action toward goals, rather than the pathways sub-scale, which measures how well a person can plan particular paths toward goals. Similarly, we see no signs of improved executive function in a real effort task. The second half of Table 5 shows the results of participant responses to the 'dots mixed' or 'Simon'

[4]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.

task that requires pursuing goals and resisting automatic impulses; treatment groups respond neither more accurately nor more quickly (see also Appendix Figure A.4). Improvements in cognition appear to come more from being activated toward goals than building problem-solving skills.

Increased agency and motivation may be particularly useful for participants with the greatest barriers to employment. As noted above, the average participant in BtS faces major barriers to labor market success: limited formal education, low long-term labor force participation, and recent job shocks. Benefits of the program seem concentrated among participants facing the most such barriers. Table 6 shows how 1-year employment effects vary across subgroups. Column (1) replicates the full sample results. The remaining columns each display the result of a regression including a treatment dummy, a sub-group dummy, the interaction of the two, and relevant controls. In particular, we control for the interaction between treatment and a cohort indicator to avoid confounding individual-level heterogeneity with the COVID pandemic and subsequent policy responses. As shown in Appendix Figures A.5 and A.6, employment dynamics differ across cohort 1, which enrolled two to three years before the pandemic, and cohort 2, which enrolled at its onset. Column (2) shows a slightly, but not statistically significant, larger effect for people unemployed at baseline. Column (3) shows the sharpest heterogeneity that appears. Treatment effects on employment are entirely concentrated among people with no high school diploma at baseline. Column (4) generalizes this idea by splitting the sample on an index of employability in which most weight is placed on prior employment and

education.[5] Among people who score in the lower half of the employability index, the treatment group has a 15 percentage point higher probability of employment. The interaction effect of 15 percentage points indicates zero treatment effect among people in the top half of employability. These results are not statistically significant and should be interpreted with caution, but they suggest that BtS has larger effects among people with greater barriers to employment. These results stand in contrast to the final column, which shows that treatment effects on employment do not vary noticeably with whether the person reported at baseline that they wanted to improve their employment situation.

5.2 Cost-Benefit

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. (forthcoming).

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

[5]We generate an index of employability by regressing the outcome on our standard vector of 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.

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 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 extensive margin effects. To get employment rates, we follow Table 3 and assume the employment rate increases by 10 percentage points in year 1 and 8 percentage points in year 3. We interpolate year 2 as 9 percentage points. Whether employment rates increase after year 3 is outside our data, so we consider three scenarios: no effects beyond the data, persistence of the year 3 effect out to 10 years, and persistence to age 65 (28 years). 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 3, we follow Hendren and Sprung-Keyser (2020) and Evans et al. (forthcoming) and impute earnings by adjusting for life-cycle effects, scaling year 3 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. The present discounted value of earnings increases by $4,465 during the three observed years, $13,927 if we extrapolate out to ten years, and $29,801 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 $2,789.

Altogether, we find that one dollar of net spending on BtS generates 0.41 to 3.56 dollars of net benefits via earnings. If benefits only last for three years, the MVPF is 0.41. This value results from $4,465 in earnings gains which are split between $4,036 in after-tax benefits and $429 in taxes, which reduce the $10,386 program cost to a net cost of $9,958. From similar calculations, we estimate that persistent employment effects lasting through 10 years or age 65 yield MVPFs of 1.39 and 3.56, respectively. At 7 years, the MVPF is 0.96, close to the break-even value of 1. These values imply that a participant who cares only about income gains would prefer this program to a cash transfer if employment effects persist for about 7 years. Also, these values are somewhat larger than those in Evans et al. (forthcoming), because BtS is less intensive and expensive than the program they study but generates similar

earnings effects.

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 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.

6 Conclusion

This paper reports on the results of an RCT comparing comprehensive case management, a two-year long holistic individualized wrap-around anti-poverty program, with temporary intervention for basic needs. One year after random assignment, participants assigned to treatment are 10 percentage points more likely to be employed. Most of that difference appears to persist three years after random assignment. On the other hand, we find no evidence of improvements in housing, family supports, health, social networks, education, or financial management, which many participants identify as their primary area for progress.

As is typically the case of randomized trials, our results may be particular to the context of one program and one location. On face value, CCM programs may appear

particularly vulnerable to concerns of external validity because they are complex and personalized. However, our study provides an opportunity to consider external validity compared with a trial of a similar program in Fort Worth, TX (Evans et al., forthcoming). Fort Worth and Rochester are very different places. For example, 37% of Fort Worth residents are Hispanic, compared with only 17% in Rochester. Despite operating highly contingent, individualized programming in these very different places, employment effects observed in the two studies are strikingly similar. In part, 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., forthcoming) are Hispanic. Both programs attract people disconnected from the labor market: 34% and 40% of participants, respectively, are employed at baseline. Despite their complexity, these two programs generate similar results in very different contexts.

Future research needs to measure the long-term effects of such programs on employment and earnings. As we show above, employment effects would need to persist for about 7 years for the present discounted value of after-tax earnings gains to exceed the net cost of providing the program. We show that employment rates increase one year after program entry, and most of this effect likely persists after the program ends. But the cost-benefit evaluation of these relatively expensive programs hinge on how many years these effects last. Long-run follow-up for this study and others is needed.

Overall, we find evidence of moderately positive effects of a comprehensive case management program. We observe significant increases in employment rates in re-

sponse to CCM, similar to another RCT of a similar program (Evans et al., forthcoming). The effects we measure are large enough that earnings gains exceed program costs if they persist for several years. Though, they are also considerably smaller than the doubling of employment that would be expected for the type of simple pre-post comparison often used to motivate such programs, and benefits appear limited to the labor market, not expanding to other parts of life. Overall, we find that an intensive and personalized anti-poverty approach, while not a panacea, creates significant employment gains for participants.

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7 Tables and Figures

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	0.00	0.00	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
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.
Bridges to Success					
Any Involvement	268	0.68	0.86	0.49	0.41***
					(0.06)
Involved Once per Month	268	0.42	0.72	0.09	0.65***
					(0.05)
Similar Organization					
Family Independence	266	0.04	0.07	0.02	0.05**
Initiative					(0.02)
Strengthening Working	270	0.03	0.06	0.01	0.05**
Families Initiative					(0.02)
Health Profession	269	0.05	0.06	0.05	0.01
Opportunity Grants					(0.03)
Pathway of Hope	270	0.05	0.07	0.02	0.04
					(0.03)
Any Outside Organization Related to					
Any Organization	271	0.25	0.31	0.19	0.12**
					(0.06)
Any Org. in Primary Goal	242	0.13	0.14	0.12	0.03
					(0.05)
-Housing	271	0.08	0.10	0.07	0.02
					(0.03)
-Family	271	0.04	0.05	0.03	0.01
					(0.03)
-Physical Health	271	0.05	0.03	0.08	-0.06*
					(0.03)
-Mental Health	271	0.12	0.13	0.11	0.03
					(0.04)
-Education	271	0.06	0.06	0.07	0.00
					(0.03)
-Employment	270	0.14	0.15	0.12	0.03
					(0.04)
-Financial	271	0.13	0.18	0.07	0.11**
					(0.04)

Notes: Outcomes are measured by the one year follow-up survey. 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 coefficient on treatment from a regression of the listed outcome on a random assignment indicator and pre-specified controls, which are the baseline value of the outcome (when available), age, gender, and marital status, as well as indicators for race, educational attainment, month of random assignment, months between surveys, and stratum of random assignment. Heteroskedasticity-robust standard errors are reported in parenthesis. 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 Dummy	396	0.68	0.71	0.64	0.10**	0.03
					(0.05)	
Quarterly Earnings ($)	396	2,708	2,836	2,548	181	0.58
					(327)	
Earnings Increase Dummy	396	0.53	0.53	0.52	0.06	0.24
					(0.05)	
1 Year Results (UI)						
Employed Dummy	356	0.66	0.69	0.62	0.06	0.24
					(0.05)	
Quarterly Earnings ($)	356	2,808	2,955	2,623	83	0.82
					(371)	
Earnings Increase Dummy	356	0.47	0.46	0.47	0.03	0.64
					(0.06)	
3 Year Results (UI)						
Employed Dummy	356	0.60	0.67	0.52	0.08	0.13
					(0.05)	
Quarterly Earnings ($)	356	3,192	3,502	2,803	38	0.94
					(486)	
Earnings Increase Dummy	356	0.44	0.48	0.39	0.06	0.27
					(0.06)	

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 the coefficient on treatment from a 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. 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	0.12*	0.07
					(0.06)	
High Home Quality	271	0.31	0.29	0.34	-0.00	0.97
					(0.06)	
Improvement in Primary Goal	182	0.45	0.50	0.39	0.20**	0.01
					(0.08)	
Improvement in Primary Goal (Bridge Tool)	271	0.43	0.43	0.44	0.01	0.90
					(0.06)	
All Children Enrolled in School	271	0.86	0.88	0.83	0.07	0.13
					(0.05)	
Increased Health	271	0.24	0.23	0.26	0.00	0.97
					(0.06)	
Increased Social Networks	271	0.49	0.50	0.49	0.01	0.82
					(0.06)	
Increased Education or Enrolled	271	0.40	0.35	0.45	-0.08	0.17
					(0.06)	
Increased Net Assets	271	0.34	0.36	0.32	0.03	0.69
					(0.07)	

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 coefficient on treatment from a 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 5: 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	0.51	0.61	0.13*
					(0.07)
Increase in Agency Hope	240	0.55	0.49	0.60	0.17**
					(0.07)
Increase in Pathway Hope	240	0.48	0.50	0.47	-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)

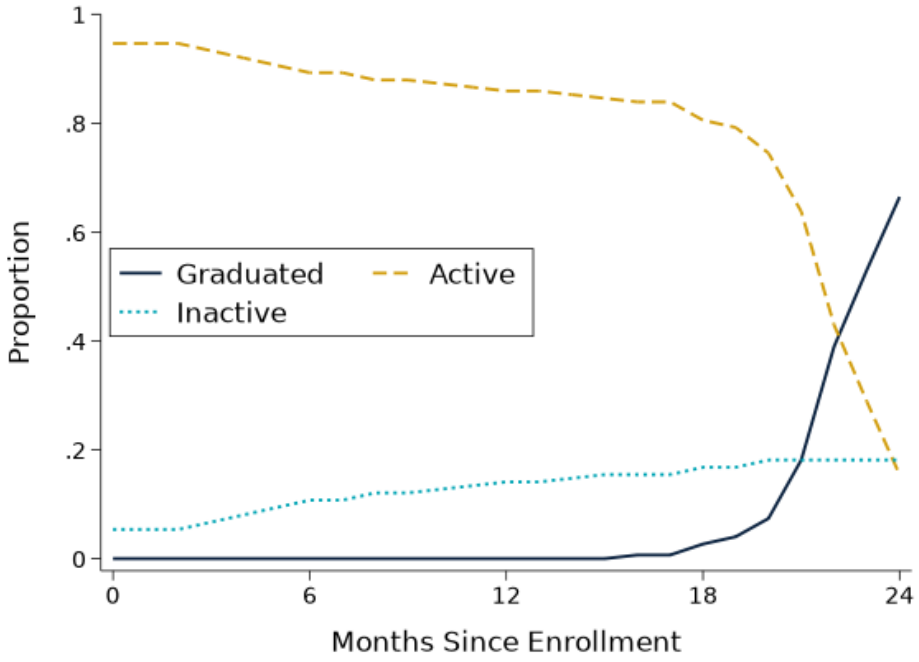
Notes: Outcomes are measured in the one year follow-up survey. Hope is coded as a zero to one index based on the state hope scale (Snyder et al., 1996). The sample for the top panel includes all respondents to the survey who responded to the hope module; total hope requires non-missing agency and pathway 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table 6: Heterogeneous Effects for Employment Outcomes, One Year

	(1)	(2)	(3)	(4)	(5)
Main Effect					
Treatment	0.10**	0.05	-0.01	0.15	0.10*
	(0.05)	(0.08)	(0.06)	(0.08)	(0.05)
Baseline Characteristic Interacted w/ Treatment					
Cohort 2		0.13	0.14	0.10	
		(0.10)	(0.10)	(0.09)	
Unemployed		0.03			
		(0.09)			
No High School			0.21**		
			(0.10)		
High Employability				-0.15	
				(0.15)	
Employment Goal					0.00
					(0.10)
Observations	396	396	396	396	396

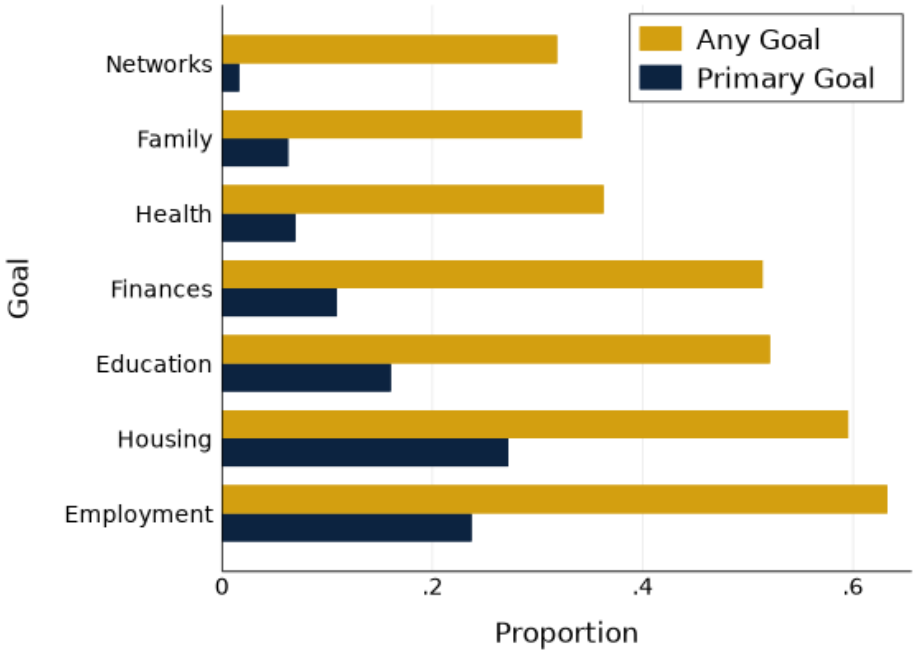
Notes: Data comes from participants with either UI earnings records or one year survey responses, taking the mean when both are available. The outcome in all regressions is a binary variable that takes a value of 1 if the participant is employed in either the survey or UI data. The treatment point estimate is the treatment effect. The other coefficients are interactions between indicator variables and treatment. Column 1 is the full baseline regression in Table 3. Column 2 interacts treatment with a dummy if the participant is unemployed at baseline. Column 3 interacts treatment with a dummy if the participant does not have a high school diploma or GED at baseline. Column 4 interacts treatment with a dummy if the participant is predicted to be employed at follow-up using baseline variables and is interacted with a dummy if the participant is in cohort 2. Column 3 interacts treatment with a dummy if the participant does not have a high school diploma or GED at baseline. Column 5 interacts with treatment with a dummy if the participant listed employment as a goal at baseline. The main effects are included in the regression but not reported. All columns include standard controls, baseline lags when applicable, and standard fixed effects. Heteroskedasticity-robust standard errors are reported in parenthesis. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Figure 1: Persistence in Programming for Treatment Group



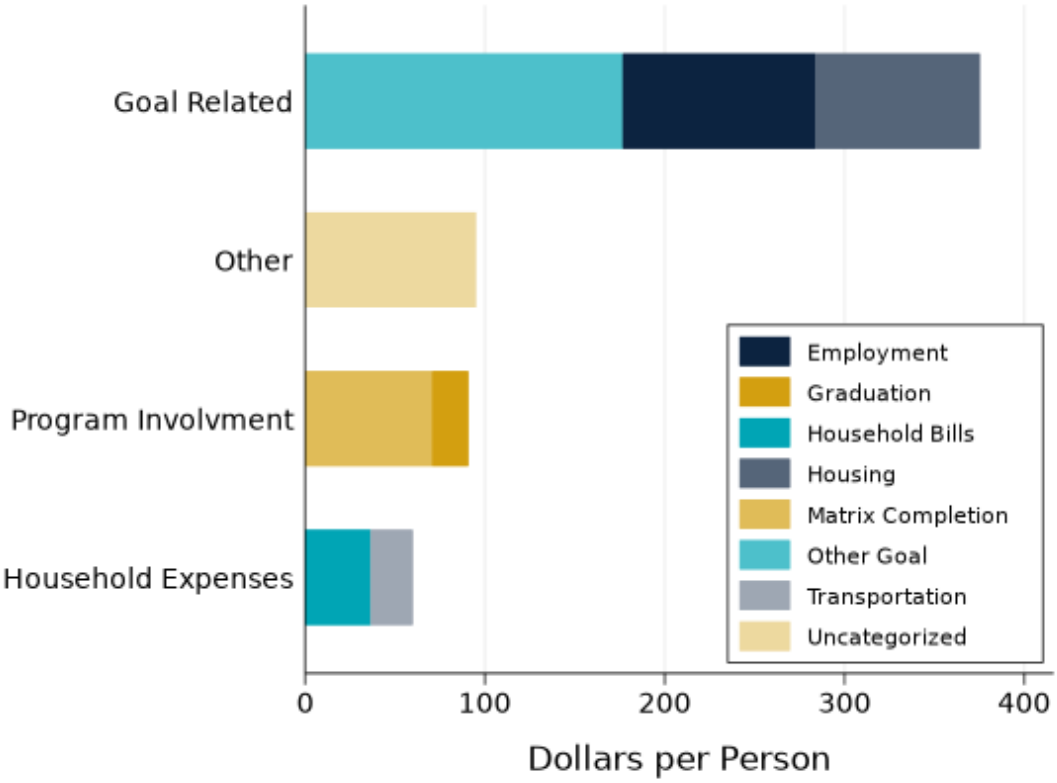
Notes: Data comes from internal BtS records for cohort 1 participants. The lines plot the proportion of participants by enrollment status. Active participants are those who still are meeting with mentors and engaging with BtS. Inactive participants did not graduate but are now longer active with B2t.

Figure 2: Goal Areas Identified by Participants, Baseline



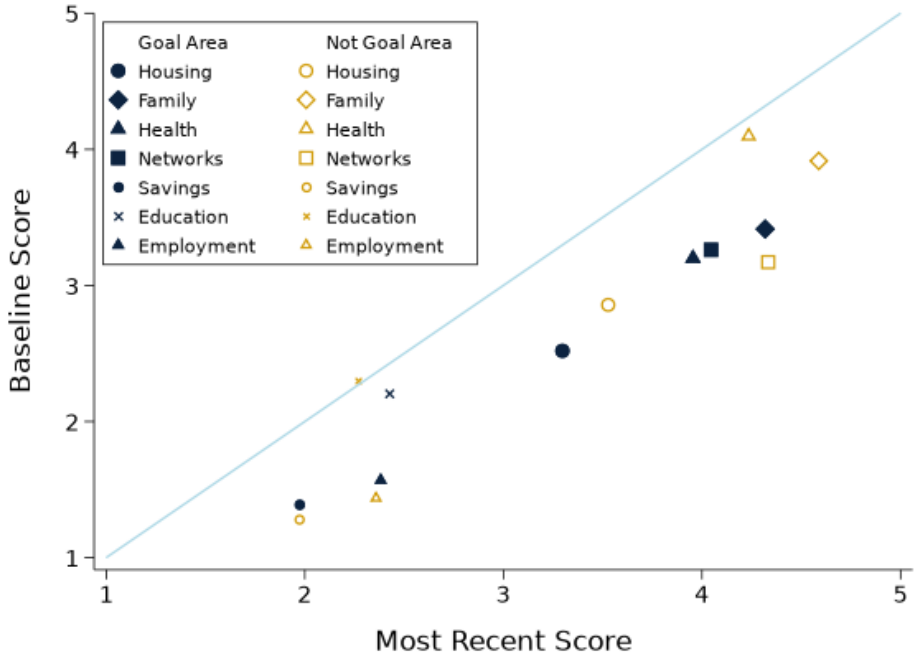
Notes: Data comes from baseline survey responses. The sample is the full study sample. Outcomes measured by participants' responses to a question on their goals. Respondents may select multiple goal areas, but only 1 primary goal area.

Figure 3: Average Incentives Received by Category



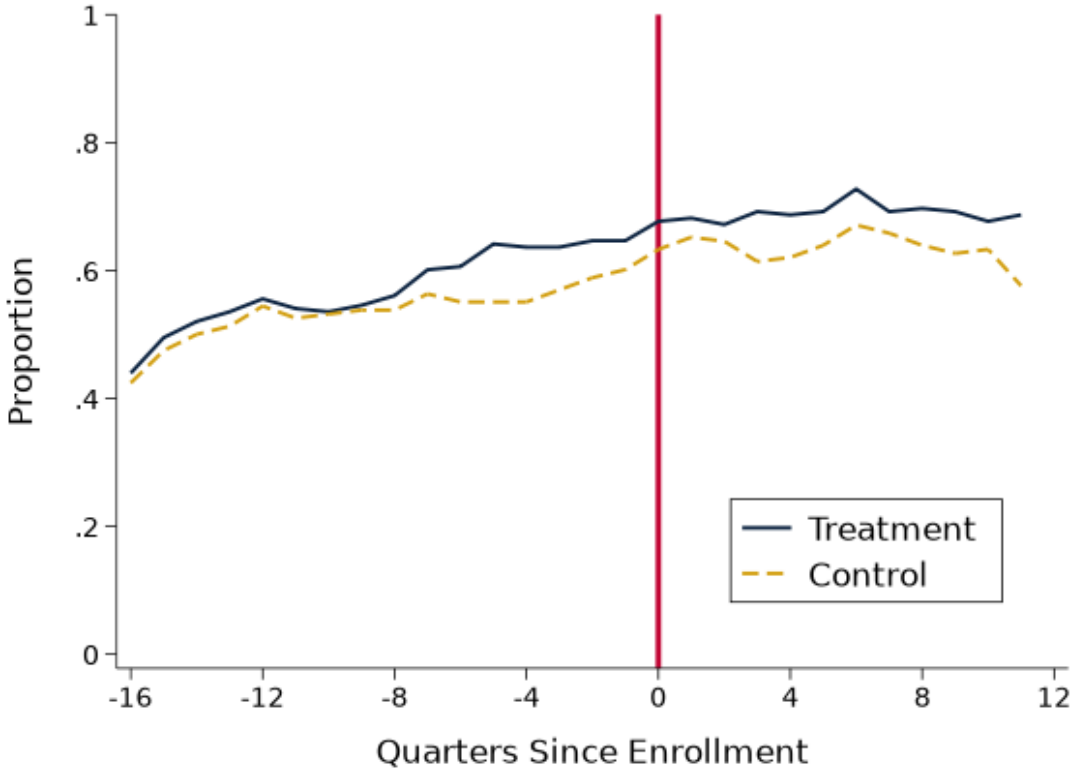
Notes: Data comes from internal BtS reports on the amount of and rationale for financial incentives paid. Sample is limited to cohort 1 participants in the treatment group. We aggregate more detailed categories into those presented using the text description of the incentive payment. The graph plots 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.

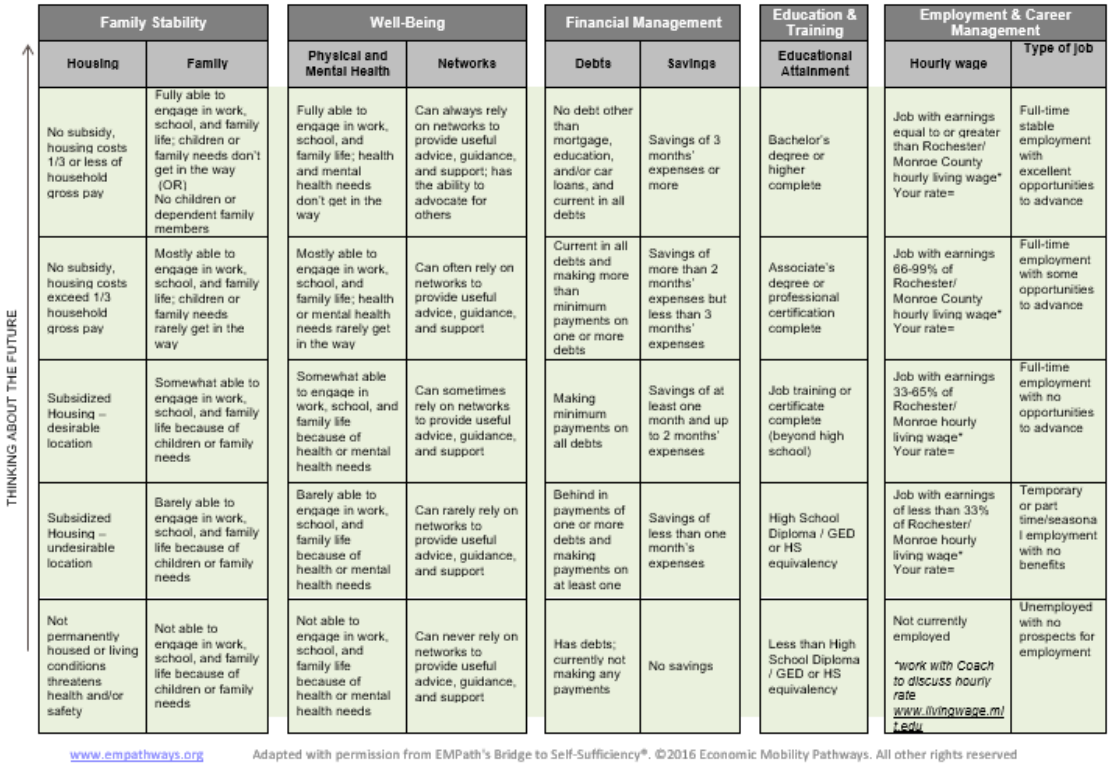
Figure 5: Employment Rates by Quarter, UI Earnings Records



Notes: The sample includes all study participants who report a valid SSN. 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. 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.

A Appendix

Figure A.1: Bridge to Self-Sufficiency Matrix



THINKING ABOUT THE FUTURE

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

Notes: Received from the BtS program, as of 2017.

Figure A.2: Study Timeline

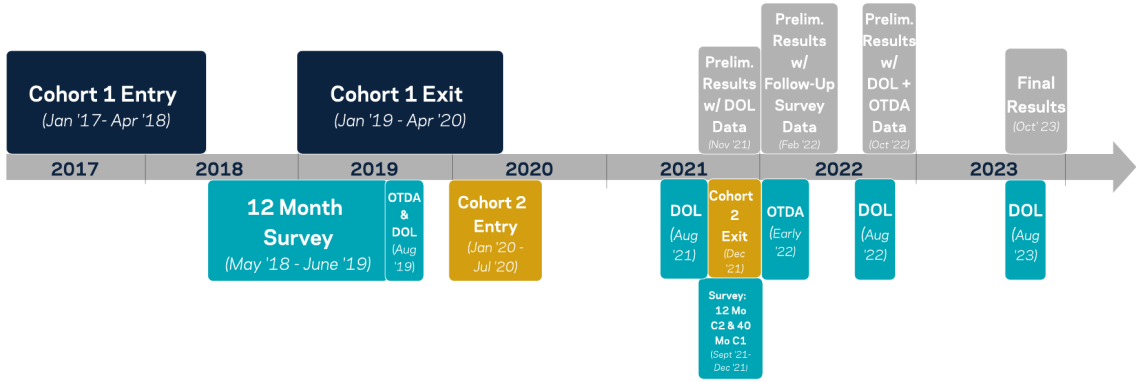
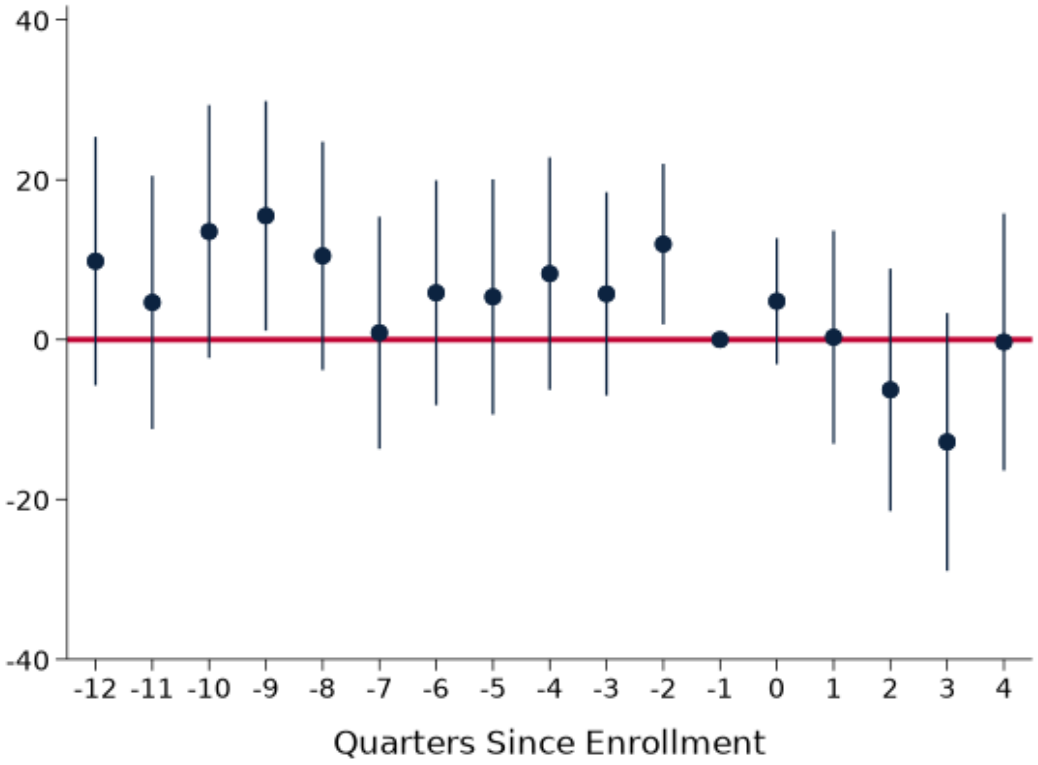
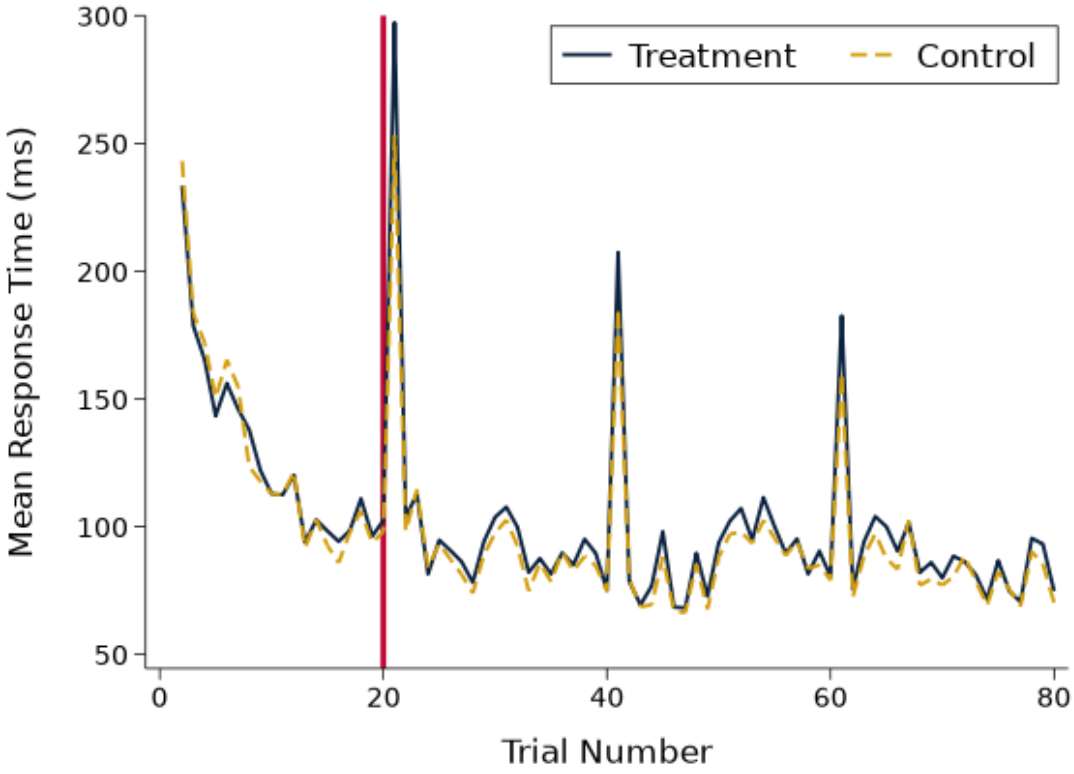


Figure A.3: Treatment Effects on Credit Score, Over Time



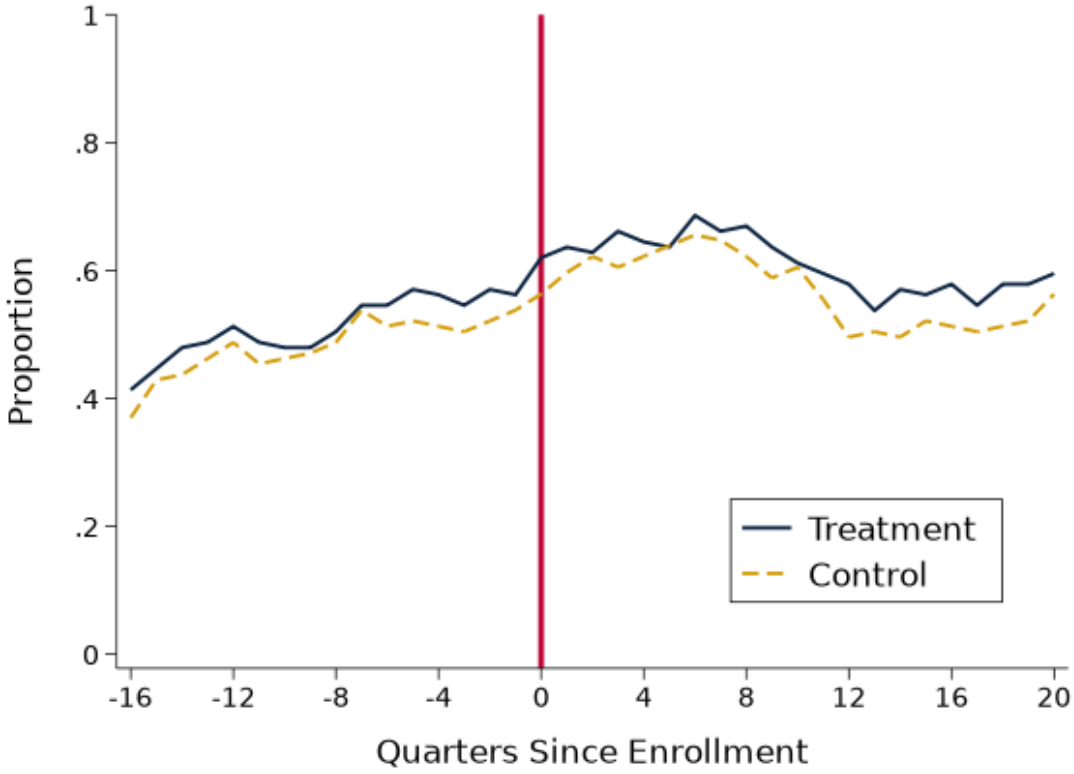
Notes: Outcomes measured used study data matched to credit reports from Experian. The sample is limited to people successfully match to an Experian record and have a non-missing credit score for quarters 3-5. Each plotted point comes from the coefficient on treatment in a regression of credit score in 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



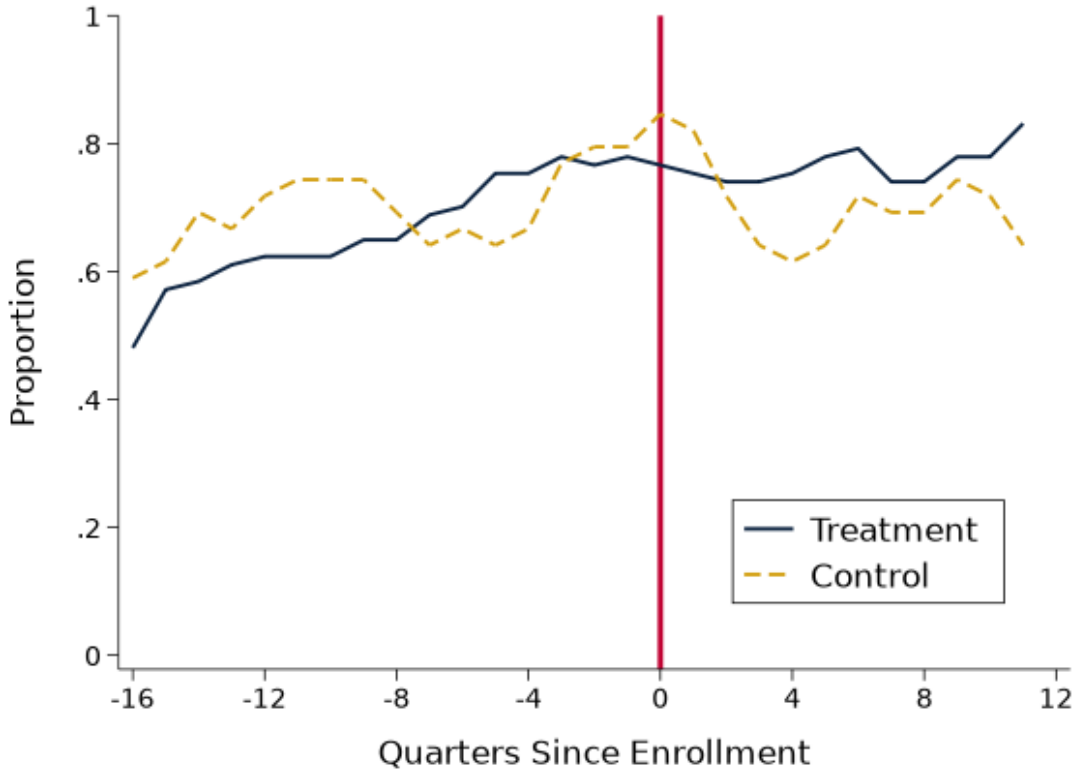
Notes: Data comes from dots-mixed/Simon executive control task on the one year follow-up survey. The sample includes all study participants who respond to the survey. The graph displays average response times in milliseconds, winzorized at the 5th and 95th percentiles. The first 20 trials were practice 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.

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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.10: Detailed Employment Outcomes, One Year Survey

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Employed	271	0.55	0.58	0.52	0.11*
					(0.06)
Hours Worked per Week	271	19.31	19.39	19.22	2.74
					(2.23)
Hourly Wage (Simple)	148	12.51	12.54	12.48	-0.04
					(1.34)
Hourly Wage (Dynamic)	147	12.30	11.92	12.75	-1.11
					(1.61)
Total Household Income ($)	258	14,695	13,161	16,353	-2,560
					(1,997)

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 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.11: Outcomes in Many Domains, One Year Survey, Narrower Sample

	(1) Sample Size	(2) Full Sample	(3) Treatment	(4) Control	(5) Adj. Diff.
Main Outcomes					
Quarterly Earnings Increase	182	0.47	0.46	0.48	0.06
					(0.08)
High Home Quality	182	0.31	0.29	0.34	0.06
					(0.08)
Improvement in Primary Goal	182	0.45	0.50	0.39	0.19**
					(0.08)
Improvement in Primary Goal (Bridge Tool)	182	0.46	0.42	0.51	-0.06
					(0.08)
Bridge Outcomes					
All Children Enrolled in School	182	0.84	0.85	0.82	0.06
					(0.07)
Increased Health	182	0.23	0.23	0.23	0.06
					(0.08)
Increased Social Networks	182	0.52	0.50	0.56	-0.06
					(0.08)
Increased Education or Enrolled	182	0.47	0.45	0.49	-0.11
					(0.08)
Increased Net Assets	182	0.38	0.38	0.39	-0.06
					(0.09)

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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.12: 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.13: 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.14: 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 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. 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.15: 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.16: 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 Religous 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.17: 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.18: 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.

Table A.19: 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 ($\geq$ 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 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. Statistical significance at the 10, 5, and 1 percent levels are denoted by *, **, and ***, respectively.