The Effects of a Cash-Plus-Job-Training Program on Young Adults: Evidence from a Randomized Experiment in Los Angeles County

Marcella Cartledge*

November 2025

Click here for the latest available version of this paper.

Abstract

This paper evaluates a cash-plus-job-training program using a randomized controlled trial (RCT) design. The program targeted economically vulnerable young adults in Los Angeles County: 60 percent of participants were unhoused or housing-unstable at baseline, and their average annual income was less than $7,000. Individuals assigned to treatment received an unconditional cash transfer of $1,000 per month for three years and the option to participate in a structured job-training program. From nearly 2,000 eligible applicants, 300 individuals were randomly selected for treatment and 566 for control. Using six rounds of surveys and administrative data, I find sizable improvements: food, housing, and financial security indices increase by 0.2–0.3 standard deviations (SD); psychological well-being improves by 0.18 SD on the Kessler-6 scale; childbearing declines by nearly half; and felony arrests decline by 40 percent. Labor market outcomes improve substantially – employment rises by 7 percentage points (18 percent increase), monthly earnings increase by roughly $94 per month (30 percent increase), and job satisfaction rises by 0.21 SD. Instrumental variables estimates exploiting random assignment to caseworkers indicate that job training, conditional on cash, accounted for much of the program's overall impact, raising employment by 19 percentage points and monthly earnings by $240.

*I am grateful to my committee members Lee Lockwood, Jonathan Colmer, and Emma Harrington for their guidance and encouragement. I thank Johannes Haushofer, Sidhya Balakrishnan, Sewin Chan, Jonathan Morduch, and Sara Constantino for valuable advice and mentorship. I also thank Roberta Costa, Klea Kalia, and Alexander Jacobs at the Jain Family Institute for outstanding research support; the Los Angeles County Department of Public and Social Services for their partnership; and the survey implementation team at USC's Center for Economic and Social Research. I appreciate helpful comments from Peter Hull, Matt Pacenco, Jon Roth, Jesse Bruhn, Anna Aizer, Evan Rose, Greg Duncan, Ben Sprung-Keyser, Vittorio Bassi, Mark Duggan, and many others. I am also grateful to participants at the Cornell University Anti-Poverty Symposium and the University of Virginia seminar audiences, including Leora Friedberg, Ahnaf Rafi, Julie Mortimer, Amalia Miller, and John Pepper.

1 Introduction

The transition from adolescence to adulthood is among the most precarious stages of the life cycle. In the United States, individuals between the ages of 18 and 24 are both the poorest and least protected group within the social safety net. Their average earnings have fallen by roughly 20 percent relative to national averages over the past three decades; their poverty rate now exceeds that of both children and older adults; and their unemployment rate is nearly three times higher than that of prime-age workers (Barr and Malik, 2016; Bureau of Labor Statistics, 2020; Wimer et al., 2020). This period of economic fragility coincides with a pivotal juncture in human-capital formation, when young adults are expected to establish independent lives and secure attachment to the labor market. Yet the safety net provides little support at this stage. Most income-support policies such as the Earned Income Tax Credit or recent guaranteed-income pilots target parents or mid-career adults, while most job-training programs serve individuals who are already stably employed. As a result, many young adults, particularly those without college degrees, fall through the cracks at precisely the moment when these resources could most shape their long-term trajectories. Can cash support, especially when coupled with job training, increase employment and improve economic security for disadvantaged young people?

In partnership with the Los Angeles County Department of Public and Social Services (LA DPSS), I designed a randomized controlled trial (RCT) to evaluate the TAYportunity + Guaranteed Income program (hereafter TAYGI), an integrated cash-plus-job-training program for young adults. The program was conceived as part of LA County's broader effort to improve labor-market outcomes among "Transitional Age Youth" (TAY), defined as individuals aged 18–24 who are in economically precarious situations. From an applicant pool of nearly 2,000 individuals, 300 participants were randomly selected to receive an unconditional cash transfer of $1,000 per month for 36 months, along with the option to participate in a skills and job-training program implemented by LA DPSS. A control group of 566 individuals received neither benefit. The intervention targets both short- and long-term constraints to economic advancement: the cash transfer relaxes liquidity and material constraints that impede job search, mobility, and skill acquisition, while the job-training component facilitates human-capital investment that can raise productivity and foster stable employment. By combining these two forms of support, the program seeks to bridge the gap between temporary financial relief and durable labor-market attachment.

In recent years, policymakers have experimented with a wave of cash-transfer programs aimed at reducing economic insecurity and simplifying the welfare state. Experiments such as Baby's First Years ($333 per month to low-income mothers), the Compton Guaranteed Income Experiment ($500 per month to adults aged 21–55), and the OpenResearch Unconditional Support (ORUS) experiment ($1,000 per month for 36 months)

reflect renewed interest in direct cash support as a tool to promote well-being and opportunity. Evidence from these efforts has been mixed: while recipients experience greater financial stability and improved health, impacts on mental health, labor supply, and long-term self-sufficiency are typically modest (Balakrishnan et al., 2024; Bartik et al., 2024; Jaroszewicz et al., 2024; Miller et al., 2024; Vivalt et al., 2024). In parallel, a vast literature on job-training and active labor-market programs has produced similarly lukewarm results: participants gain skills and modest wage increases over time, but take-up rates are low and short-term impacts are often negligible (see for example, Barnow and Smith, 2015; Card et al., 2018). Against this backdrop, TAYGI provides a unique opportunity to test whether combining liquidity and human-capital interventions can yield larger, more persistent gains.

The TAYGI experiment reveals substantial and broad-based improvements. Treated participants were 7 percentage points more likely to be employed relative to a control mean of 39 percent and earned approximately $94 more per month on average — a 30 percent increase. They were also 15 percentage points more likely to be housed and experienced notable gains in food and financial security, corresponding to increases of 0.2 and 0.5 standard deviations, respectively.

Beyond labor-market outcomes, the program generated sizable improvements in social and psychological well-being. Participants reported higher job satisfaction and fewer search frictions, alongside gains of 0.2–0.5 standard deviations in food, housing, and financial security and a 0.2 standard-deviation increase in psychological well-being (Kessler-6). Felony arrests fell by 40 percent (control mean 9 percent) and births by 50 percent (control mean 16 percent). Spending increased by about $560 per month, spread across rent, groceries, utilities, transportation, and leisure, while asset accumulation doubled, driven by higher savings and durable-goods purchases. I observe no net rise in debt. Together, the results highlight how reducing liquidity and skill barriers jointly enhances employment, stability, and well-being.

To disentangle the contribution of the cash alone versus that of the job-training program conditional on cash, I exploit effectively random variation in the treatment group's assignment to "program liaisons," county caseworkers responsible for encouraging participation in training. Liaisons varied in their intensity of outreach and encouragement, and I use this variation to estimate the local average treatment effect (LATE) of training conditional on receiving cash and to decompose total effects into cash-only and cash-plus-training margins following the logic of multi-treatment instrumental variable frameworks developed by Mountjoy (2022) and Humphries et al. (2025).

I find that job-training participation accounts for much of the program's impacts. Participants induced into training are 19 percentage points more likely to be employed, work 26 additional hours per month, and earn about $250 more than those receiving only the cash transfer. They also accumulate greater savings and move less frequently, indicating improved economic stability. In contrast, unconditional cash transfers alone

generate short-term gains in expenditure and housing security but do not produce comparable improvements in employment or asset accumulation. One possible explanation for these patterns is that cash support and job training act as complementary policies: liquidity from cash transfers helps individuals overcome short-term financial constraints, enabling them to participate in job-training programs that generate more durable gains in employment and economic security.

The study population is highly disadvantaged. Ninety-three percent of applicants earned under $600 per month at baseline; 60 percent were unhoused or housing-insecure; and 28 percent were former foster-care youth. Fifty-seven percent identify as Black or African American and 30 percent as Hispanic or Latino. TAYGI thus provides a rare laboratory for studying how cash and skill investment interact among individuals on the margins of the labor market. I evaluate the program using six survey waves collected between November 2023 and June 2025, complemented by administrative arrest data scraped from the Los Angeles County Inmate Information Center. Survey completion rates exceeded 80 percent in every wave, with no significant differential attrition. Following McKenzie (2012), I pool repeated endlines to improve precision in this relatively small experiment and estimate both pooled and dynamic treatment effects.

This paper contributes to four related literatures: (1) cash transfers and guaranteed income, (2) job-training and active labor-market policies, (3) the emerging "cash-plus" paradigm, and (4) youth economic mobility. In the cash transfer space, programs in developed countries have shown positive short-run effects across a range of outcomes. Evidence from conditional cash transfer and casino dividends has shown that cash support leads to improved health (Akee et al., 2010; Forget, 2011), educational attainment and labor market outcomes, and lower criminality and recidivism (Akee et al., 2010; Bastian and Michelmore, 2018; Robins, 1985). New York City's conditional cash transfer program from 2007 to 2010 led to reduced financial hardship, and increased preventative dental visits (Riccio and Miller, 2016). Haushofer and Shapiro (2016) provide complementary evidence from rural Kenya, showing that unconditional transfers generate large increases in consumption, asset ownership, and psychological well-being in a developing-country setting. More recently, several guaranteed-income pilots across the United States have provided unconditional monthly transfers, but none have targeted young adults or combined income support with job training. These programs have produced mixed results: participants typically experience improved financial and physical health, but little change in mental health or long-term self-sufficiency, and labor supply often declines (Bartik et al., 2024; Jacob et al., 2022; Jaroszewicz et al., 2024; Miller et al., 2024; Vivalt et al., 2024; West and Castro, 2023). My paper builds on this work by providing the first experimental evidence that combining substantial unconditional cash transfers with skill-building opportunities can increase employment and earnings among young adults, showing that liquidity and human-capital investments together generate sustained improvements in economic mobility.

Second, this paper contributes to the extensive literature on job-training and active labor-market programs. Large federally funded initiatives such as the Job Training Partnership Act (JTPA), Workforce Investment Act (WIA/WIOA), Job Corps, and Trade Adjustment Assistance (TAA) typically generate modest earnings gains of 4–7 percent several years after participation (Barnow and Smith, 2015; Card et al., 2018; Schochet et al., 2008). In contrast, sector-focused and wrap-around models such as Project QUEST (Roder and Elliott, 2019) and WorkAdvance (Katz et al., 2022) produce sustained 20–30 percent increases in earnings. Complementary evidence shows that integrating soft-skills and placement support further boosts outcomes (Baird et al., 2022; Wheeler et al., 2022). Relative to this work, my study isolates the causal role of training participation within a cash-transfer environment. It shows that training effects are strongest when liquidity constraints are relaxed, identifying a mechanism for scaling sectoral-style gains among populations traditionally excluded from stable employment.

Third, this study advances the nascent "cash-plus" literature that explores complementarities between income support and human-capital interventions. Although evidence in this space is limited, results from these studies show promise for such policies. Sedlmayr et al. (2020) evaluates a randomized experiment in Uganda to demonstrate that an intervention in which individuals receive post-graduation training along with a cash benefit experience better outcomes relative to those who are only given cash. Roelen et al. (2017) advocates for cash-plus programs as a way to lengthen the impacts of cash and establishes a framework for policymakers to use when implementing such programs. TAYGI provides the first experimental evidence of a cash-plus approach targeted at young adults in a developed country context.

Finally, the paper contributes to the broader literature on youth economic mobility. By focusing on individuals at the threshold of labor-market entry, this paper highlights how early interventions can alter trajectories before disadvantage becomes entrenched. This work complements evidence from the Moving to Opportunity (MTO) experiment, which shows that relocating families to lower-poverty neighborhoods improved safety, mental health, and well-being in the short run (Gennetian et al., 2012; Jacob et al., 2014; Katz et al., 2001; Kling et al., 2007), and raised college attendance, earnings, and adult outcomes for children who moved at younger ages (Chetty et al., 2016). This paper extends this line of inquiry to the transition into adulthood, showing that targeted support at labor-market entry through integrated cash and training can similarly shift life-course trajectories before disadvantage becomes entrenched.

The rest of the paper proceeds as follows. Section 2 describes the experimental design and implementation. Section 3 details the data sources, survey structure, and empirical methodology. Section 4 presents the main intent-to-treat results. Section 5 explores mechanisms using instrumental-variable and decomposition analyses to distinguish the effects of cash-only support from those of cash combined with job training. Section 6 discusses the policy implications of integrating income and employment interventions for young adults at

the margins of the labor market, and Section 7 concludes.

2 Study Setting

2.1 Program Description

In May 2022, the Los Angeles County Department of Public Social Services (DPSS) announced the Transitional Age Youth Guaranteed Income (TAYGI) program, a three-year guaranteed-income and employment initiative targeting vulnerable transitional age youth (TAY) aged 18–24. The program was designed to evaluate whether sustained income support, paired with access to employment and training opportunities, could promote long-term economic stability and independence among youth receiving public assistance.

Each participant in the program received an unconditional cash transfer of $1,000 per month for 36 months, delivered through their existing Electronic Benefit Transfer (EBT) card. Payments were made monthly and without conditions on employment, education, or program participation. Participants were informed ex-ante of the exact amount, timing, and duration of transfers. In addition to the guaranteed income, participants were offered the opportunity to participate in the TAYportunity program, a complementary workforce initiative aimed at supporting young adults build the skills necessary to have strong careers.

The TAYportunity program is structured in three sequential phases. Participants first complete an orientation that introduces available supports and expectations. Those who finish orientation may progress to the second phase, which focuses on career readiness, soft-skill development, and motivational training. Upon completion, participants advance to Phase 3, selecting one of three employment pathways:

1. **Subsidized Employment:** A six-month full-time placement (40 hours per week at $15 per hour) with public and private employers across Los Angeles County including positions in parks and recreation, airport services at LAX, and the construction sector. This pathway provides immediate work experience, stable earnings, and exposure to structured workplace environments.

2. **Apprenticeship Opportunities:** Nine-month paid training programs in high-growth industries such as construction, aerospace, and bioscience. Developed in partnership with industry associations and labor unions, these apprenticeships provide skills training with on-the-job mentorship to facilitate transitions into long-term employment.

3. **Entrepreneurship Training Program:** A twelve-month business-development program for participants pursuing self-employment or small-enterprise creation. Participants receive individualized

coaching on business planning, licensing, and regulatory compliance, as well as guidance on accessing startup resources and capital.

Owing to the staggered enrollment process and capacity limitations of the TAYportunity program, participants initiated activities on a rolling basis. By the time of the first survey wave in November 2023, however, nearly all participants had completed the initial orientation phase.

2.2 Recruitment and Enrollment

Recruitment for the program was conducted through DPSS's General Relief Opportunities for Work (GROW) program[1], which serves roughly 6,000 participants. To qualify, individuals had to be 18–24 years old, reside in Los Angeles County, and be deemed "work-ready," meaning they possessed valid government identification, a Social Security number, and either a high-school diploma or GED, and had no felony convictions.

The program was advertised through online postings and direct outreach via text and email to GROW participants. Applications were accepted between June and July 2022. Prospective applicants were informed that limited slots were available and would be filled through a randomized lottery. The application, which I developed jointly with LA DPSS, was administered online and required roughly ten minutes to complete. In addition to screening questions for eligibility, the form collected information on demographic characteristics as well as baseline measures of some primary outcomes.

A total of 3,211 individuals applied to the program, of whom approximately 2,000 met the eligibility criteria. From this pool, 370 individuals were randomly assigned to the treatment group and 846 to the control group. Selected participants were contacted by LA DPSS and enrolled upon confirming their interest and eligibility, while control group members were notified that they had not been selected for the initial TAYGI rollout but might be invited to participate in future research or programs. In total, 300 treatment participants and 566 control participants were successfully reached and enrolled in the study[2]. Program enrollment occurred between August and December 2022.

Among the 370 individuals randomly assigned to the treatment group, all 300 who were successfully contacted by LA DPSS accepted and enrolled in the guaranteed income program. Within the treatment arm, 207 participants (69 percent) also opted to participate in the accompanying TAYportunity employment and training component. During the study period, 53 treatment participants discontinued receipt of cash transfers, primarily due to transitions into CalWORKs eligibility, relocation outside Los Angeles County, or incarceration.

[1]The program has since been renamed to the Skills and Training to Achieve Readiness Tomorrow (START!) program.
[2]Appendix E.1 discusses concerns regarding differential enrollment take up across treatment and control

Participant recruitment for the accompanying research study began in early 2023 after program enrollment had concluded. To minimize differential survey participation by treatment status, I initiated independent outreach to the study sample separately from LA DPSS's communications, identifying myself as a researcher working with LA DPSS to evaluate one of its programs. In addition to email invitations, I conducted extensive follow-up through phone calls, text messages, and video meetings to establish legitimacy and address participants' questions about the study. Concurrently, LA DPSS distributed a complementary email campaign noting its collaboration with a researcher to better understand clients' experiences and program impacts.

Because the study population is highly economically vulnerable, retaining contact proved challenging. Following coordinated outreach by both LA DPSS and myself, 300 treatment individuals and 566 control individuals were successfully reached. All contacted treatment individuals accepted and were enrolled in the TAYGI program. Ultimately, 195 treatment and 373 control participants consented to take part in the survey-based evaluation.

2.3 Study Sample

Applicants to the TAYGI program were racially diverse and economically vulnerable. Among all applicants, 57 percent identified as Black or African American and 30 percent as Hispanic or Latino. Roughly 28 percent were former foster youth, and 60 percent reported being unhoused or at immediate risk of losing housing. Financial vulnerability was widespread: 93 percent of applicants reported monthly earnings below $600. The program's $1,000 monthly transfer therefore provides a meaningful relaxation of liquidity constraints and enhances the statistical power to detect even modest behavioral responses in subsequent analyses.

Table 2 presents baseline characteristics for the experimental sample. At baseline, treatment and control groups are well balanced across demographic and labor-market variables. In the full sample, 56 percent of treatment and 59 percent of control participants identified as Black; mean age was 21.2 years in both groups. Roughly one in five participants had prior contact with the probation system, and about one-quarter had been in foster care. Baseline labor-market attachment was limited: over 80 percent were unemployed, and average weekly earnings were $76 in the control group and $88 in the treatment group, with mean hours worked of 5.2 and 4.8 hours, respectively.

To assess potential survey selection bias, the right-hand panel of Table 2 compares the subset of individuals who completed the baseline survey. Observable characteristics among survey completers remain balanced across treatment status. Mean differences in demographic and economic variables are small and statistically insignificant, confirming that randomization produced comparable treatment and control groups and that

subsequent survey participation was not systematically related to assignment status.

3 Data and Methods

3.1 Survey Data

I conducted six waves of follow-up surveys between November 2023 and June 2025. In each survey, I measured primary and secondary outcomes across employment, earnings, education, consumption, savings and debt, housing and food security, and psychological well-being. Appendix B presents the timeline of survey administration as well as details on the survey instrument. Following treatment randomization in August 2022, the first baseline survey was fielded in November 2023, with subsequent waves in March 2024, September 2024, December 2024, March 2025, and June 2025; surveys were approximately conducted every four to six months during the 36-month intervention window.

All surveys were administered online in partnership with the Center for Economic and Social Research (CESR) at the University of Southern California, which managed participant outreach, hosted the survey instrument, and implemented the follow-up protocol[3]. CESR contacted participants via personalized email invitations and issued reminder campaigns consisting of automated email nudges and targeted phone calls to maximize completion. Respondents could complete the survey using a mobile phone, tablet, or computer. In order to minimize respondent fatigue and to avoid high attrition across waves, surveys one through five were kept short and took approximately 20 minutes to comlete. Participants were compenated $20 for each survey complete in rounds one through three, and $30 per survey complete for rounds four and five. Survey wave 6 took approximately 40 minutes to complete, and individuals were paid $60 upon completion[4].

Response rates were consistently high across all six waves, ranging from 83 to 92 percent (Appendix A). Attrition is low and balanced: differences in completion rates between the treatment and control groups are never statistically significant ($p\text{-}value > 0.25$ in all waves). The overall response pattern is typical for short-interval longitudinal panels and suggests that attrition is unlikely to bias treatment estimates.

Each wave included identical modules on core outcomes to permit within-individual comparisons over time. The sixth survey added retrospective questions on job quality, benefit access, job search effort, and participation in specific TAYportunity training pathways. In addition to tracking behavioral and economic outcomes over time, the use of six repeated survey waves was an intentional design choice aimed at improving the precision of estimated treatment effects and increasing statistical power, following the guidance of McKenzie (2012).

[3]More details on CESR can be found in Appendix B.4

[4]In the pre-analysis plan, I stated I would pay individuals $20 for each survey. As the study went on, I had more money in the study budget than anticipated and opted to pay participants more for completing the survey to minimize attrition.

3.2 Arrest Records

I measure criminal-justice contact of individuals in the study using public booking records from the Los Angeles County Sheriff's Department Inmate Information Center (IIC). I programmatically extracted arrest-, booking-, and release-level records for the study window and harmonized dates, charge levels, release reasons, and case dispositions. Records were deterministically linked to study participants using exact matches on first and last name and date of birth; unmatched name hits were discarded. I restrict to arrests after randomization (August 2022) and within the study's age range.

3.3 Estimation Strategy

To estimate the causal effects of program assignment on participant outcomes, I employ McKenzie (2012), which increases statistical power by pooling information across multiple follow-up survey waves. This specification was pre-specified. Specifically, I estimate the following equation:

$$y_{it} = \beta_0 + \beta_1 T_i + y_{iB} + \sum_{t=1}^{T} \gamma_j \cdot D_j + X_i^{'}\delta + \varepsilon_i \tag{1}$$

where y_{it} denotes the outcome of interest for individual i at follow-up t, T_i is an indicator for assignment to the treatment group, and $y_{iB}$is the baseline value of the outcome (for outcomes I was able to obtain baseline measures of during recruitment). The vector D_j contains survey-wave dummies that absorb time-specific shocks common to all individuals, and X_i is a vector of demographic covariates. The term ε_{it} is the idiosyncratic error.

The coefficient of interest, β_1, represents the intent-to-treat (ITT) effect, or the average impact of being randomly offered participation in the TAYGI program, regardless of subsequent take-up of TAYportunity or other program components. Because random assignment ensures T_i is orthogonal to potential outcomes, β_1 identifies the causal effect of program assignemnt under standard assumptions. Finally, all specifications are estimated using heteroskedasticity-robust standard errors clustered at the individual level.

3.4 Outcome Variables

As part of the pre-analysis plan, I pre-specified six primary outcomes and four secondary outcomes. The outcomes were selected to align closely with those in Balakrishnan et al. (2024), a guaranteed-income study conducted in the same region, in order to facilitate direct comparison. Primary outcomes are denoted with an asterisk (*) and secondary outcomes with a dagger (†). The definitions below reproduce those in the registered pre-analysis plan.

Labor hours supplied per month*: measured as the total number of hours the respondent worked in the past month across formal employment, informal employment, and self-employment.

Income earned from work*[5]: measured as total earnings from formal employment, informal employment, and self-employment in the past month.

Educational attainment*: measured as the total count of academic or vocational credentials completed. Degrees and certificates contributing to this measure include high-school diploma or GED, associate degree, vocational certifications or licenses, bachelor's degree, and master's degree.

Food security†: measured as a standardized index of the following two binary variables: (a) whether or not the individual had to eat less than they felt they should do to cost in the past 30 days, and (b) whether or not the individual had to eat a lower quality diet than they should because of cost in the past 30 days.

Housing security†: measured as a standardized index of the following two items: (a) binary variable for housing status (housed or unhoused), and (b) a four-point likert scale variable capturing individuals' self-perceived likelihood of eviction.

Financial security*: measured as a standardized index of the following four binary variables: (a) whether or not the individual can cope with a $400 emergency bill using current resources or without going into debt, (b) whether or not the individual could pay all their bills in the past 30 days, (c) whether or not the individual could put money aside for the future in the past 30 days, and (d) whether or not the household could pay down debt in the past 30 days.

Psychological well-being*: measured using the Kessler-6 questionnaire of psychological distress (Kessler et, al., 1997).

Total expenditure*: measured as total monthly expenditure summed across the following categories: rent, food, utilities, other bills and debt payments, shopping expenses, leisure activities, education expenses, and transportation.

Total value of cash savings†[6]: measured as the self-reported dollar amount of total cash savings.

Total value of debt† : measured as the sum of self-reported balances across mortgage debt, vehicle loans, medical debt, legal bills, credit-card debt, loans from friends and family, student debt, and other miscellaneous debts.

[5] In the pre-analysis plan, this variable is named "income earned in the labor market in the past month". The variable is identical in content.

[6] In the pre-analysis plan, this variable is named "total value of savings". The variable is identical in content.

Additionally, I pre-registered the following outcomes as exploratory analysis: employed or working, measured using a binary variable that is equal to one if the individual worked via formal employment, informal employment, self-employment; satisfaction with employment situation, measured using a five-point Likert scale; a binary variable denoting whether or not the respondent has been looking for work in the past month; a binary variable measuring whether or not the respondent would like to be working more than their current situation; the number of residential moves in the past 90 days; and a time use module that measures weekly hours spent asleep, studying or on education, performing unpaid labor, engaging in leisure, working for pay, and on transportation.

I also examine several variables that were not formally pre-specified but provide complementary evidence on program mechanisms and participant behavior. These include measures of employment type (full-time and part-time), employment benefits, health outcomes such as doctor visits and childbearing, asset accumulation, and administrative records of arrests. Although these variables were not part of the pre-registered set, they enrich the interpretation of the main results by capturing intermediate adjustments in labor quality, health, and criminal justice involvement that help explain the broader economic and behavioral impacts of the intervention.

4 RCT Results

The intervention generated substantial improvements across a broad set of outcomes, including labor market engagement, economic well-being, and broader indicators of household stability. Here, I present treatment effects in standardized units to provide a consistent measure of effect size across outcomes; in the remainder of the section, I discuss each domain in greater detail and report the corresponding effects in their original units. Figure 1 summarizes the standardized treatment effects on employment-related outcomes. Relative to the control group, treated participants were 0.15 standard deviations (*p-value 0.02*) more likely to be employed, 0.08 standard deviations (*p-value 0.22*) higher in hours worked per month, and 0.13 standard deviations (*p-value 0.07*) higher in income from work. They also reported a 0.21 standard deviation (*p-value 0.00*) increase in employment satisfaction. Together, these effects indicate that the intervention meaningfully increased both the extensive and intensive margins of labor supply, suggesting that cash and job-training support jointly reduced barriers to work and improved the quality of employment among a population with low baseline labor market attachment.

Figure 2 displays estimated effects on measures of economic security and subjective well-being. Treatment participants experienced significant gains in financial stability, with increases of 0.51 standard deviations (*p-value 0.00*) in financial security and 0.20 standard deviations (*p-value 0.01*) in food security. Housing

outcomes also improved: the treatment group was 0.33 standard deviations (*p-value 0.00*) more likely to be housed, 0.26 standard deviations (*p-value 0.00*) higher on the housing security index, and 0.11 standard deviations (*p-value 0.09*) lower in the likelihood of eviction. These effects were accompanied by a 0.18 standard deviation (*p-value 0.01*) improvement in psychological well-being, consistent with reduced financial stress and greater day-to-day stability. For expenditure and savings outcomes, treated individuals exhibited a 0.21 standard deviation (*p-value 0.00*) increase in total expenditure and a 0.35 standard deviation (*p-value 0.00*) increase in cash savings. Treatment individuals experienced no significant change in debt burden. Collectively, these results suggest that the intervention enhanced both liquidity and economic resilience, helping participants meet immediate needs while improving their longer-term financial position.

Finally, the effects on behavioral and life-course outcomes point to potential downstream benefits of improved stability. As shown in Figures 3 and 4, treatment participants exhibited significantly lower rates of felony arrest and a lower likelihood of having given birth during the study period. Relative to a control mean of 9 percent, the likelihood of a felony arrest fell by 4.4 percentage points (*p-value 0.03*), while effects on overall arrest rates were small and statistically insignificant. The probability of having a child declined from 15 percent in the control group to 8 percent (*p-value 0.04*) among treatment participants. These patterns are consistent with reductions in exposure to risk and delayed fertility decisions. While these effects are estimated with less precision, they align with the broader interpretation that financial stability and structured engagement through job training can reduce vulnerability to crisis-driven behaviors and improve longer-term life trajectories. The remainder of this section explores these results in greater detail and examines additional outcomes to provide a more complete picture of the intervention's overall effects.

4.1 Employment Outcomes and Educational Attainment

4.1.1 Employment, Hours and Earnings

The intervention generated meaningful improvements across several dimensions of participants' labor market engagement. As shown in Table 1, individuals in the treatment group were 7 percentage points (*p-value 0.02*) more likely to be employed or working relative to the control mean of 39%. They also supplied an additional 5.7 (*p-value 0.16*) hours of labor per month relative to control (mean 40.04) and earned roughly 30% more in monthly income – $424 on average compared to $326 in the control group. Although the hours effect is imprecisely estimated, the joint pattern of higher employment and earnings suggests that participants not only entered work at higher rates but also accessed somewhat better-paying positions. These results indicate that the program increased labor-market attachment without evidence of displacement from formal to informal work.

4.1.2 Full-Time and Gig Employment

The intervention also affected the type of employment participants engaged in. Table 7 shows treatment individuals were 6 percentage points (*p-value 0.02*) more likely to be employed full-time relative to the control mean of 12%. In contrast, rates of part-time or gig employment were statistically indistinguishable across groups, as was the prevalence of multiple-job holding. Taken together, these findings imply that the program primarily facilitated transitions from part-time or irregular work into full-time employment, consistent with improved job matching and reduced financial pressure to accept temporary jobs.

4.1.3 Job Satisfaction and Search

Subjective labor-market outcomes also improved. As reported in Table 8, treatment participants rated their overall satisfaction with their employment situation 0.27 points higher (*p-value 0.00*) than the control mean of 2.33 on a five-point scale, and they were 6 percentage points more satisfied with their working hours (*p-value 0.03*) relative to the control mean of 19%.

To explore potential mechanisms, the endline survey (survey 6) collected additional measures on commute time and job benefits. Details of the construction of the benefits index can be found in Appendix B. I find no statistically significant differences between treatment and control for work commute times (TE -2.11 minutes, *p-value -0.98*) or employment benefits (TE 0.17 standard deviations, *p-value 0.81*). Because these data are only observed at endline, statistical power is limited, but the null findings suggest that higher job satisfaction is more likely explained by improved pay or job stability than by shorter commutes or greater access to benefits.

Finally, I find evidence that the program changed job search behavior. Treatment individuals were 6 percentage points (*p-value 0.02*) less likely to report actively looking for a new job relative to the control mean of 79%. In individuals' current or most recent job search, treatment and control participants did not report differences in the amount of hours they spent searching for a new job, but treatment individuals did submit 18.17 (*p-value 0.067*) fewer applications that theen control mean of 46.48 applications. These findings are consistent with greater employment stability and improved job quality reducing the need for continued search activity.

4.1.4 Educational Attainment

Table 9 shows that the program increased educational engagement. Treatment participants were 7 percentage points (*p-value 0.03*) more likely to be enrolled in an educational or training program relative to a control mean of 21%, representing a roughly one-third increase. In contrast, there is no detectable impact on the

total number of diplomas or certificates earned. These findings suggest that the TAYGI program primarily influenced ongoing engagement in education or training, rather than the completion of additional credentials within the short study window. Consistent with the program's emphasis on connecting participants to workforce pathways, the results indicate that participants responded to the intervention by re-entering or remaining in educational settings, but measurable gains in credential attainment had not yet materialized by the time of measurement.

4.2 Security and Well-being

4.2.1 Food, Housing and Financial Security

The intervention had sizable impacts on several dimensions of individuals' well-being and security. Table X reports standardized indices for food, housing, and financial security. Food security rose 0.21 (*p-value 0.00*) standard deviations (SD). There were two components that entered the index, and the treatment effects of both indicators are shown in Table 10. The first variable is a binary indicator for whether the respondent experienced a time in the past 30 days where they were not able to eat enough because of cost, to which 77% of individuals in the control group responded affirmatiely. Treatment individuals were 10 percentage points (*p-value 0.03*) less likely to experience hunger in the past 30 days. The other index component is a binary indicator for whether or not the respondent needed to eat lower-quality food than desired because of cost. The treatment group experienced a five percentage point (*p-value 0.09)* decrease in this measure relative to the control mean of 81%.

As can be seen in Table 1, individuals in the treatment group also experienced an increase in housing security of 0.26 SD (*p-value 0.00*) relative to control. The housing security variable is a standardized index comprised of (1) a binary variable for whether or not the respondent is housed, and (2) a variable that asks the respondent to evaluate their likelihood of eviction using a four-point likert scale. As shown in Table 10, the treatment group experiences a 15 percentage point increase in housing rate relative to the control group, which has a mean housing rate of 56% (i.e., 44% of control individuals are unhoused compared to 29% of the treatment group). Individuals in the treatment group are seven percentage points less likely to feel they are likely to be evicted (*p-value 0.08*) than the control group. Finally, respondents were also asked to report how many times they moved in the past 90 days and how many adults lived in the household (including themselves) across all surveys. I did not observe any significant differences across treatment and control for these two measures.

Treatment individuals also experienced a large increase in financial security of 0.31 SD (*p-value 0.00*) relative to control individuals. The financial security index is comprised of four variables for which I report

effects for individually in Table 10. The first is a binary variable which measures whether or not the respondent was able to pay their bills over the past 30 days. Relative to the control mean of 16%, individuals in the treatment group are more than twice as able to pay their bills with a treatment effect of 18 percentage points (*p-value 0.02*). The second input into the index is a binary variable asking if the respndent had an ability to pay off debt in the past 30 days. 16% of control individuals were able to make payments on their debt, compared to 41% of treatment individuals. The third input into the financial security index a binary variable which measures individuals' ability to handle an emergency $400 expense in a secure way, i.e. using a method that does not require them to take on more debt such as cash, a debit card, or a credit card with a statement paid off in full at month's end. Indviduals in the treatment group are 10 percentage points (*p-value 0.02*) more likely to be able to handle this emergency expense relative to the control group, which has a mean probability of 12%. The final variable that is an input into this index is a binary variable that asks whether individuals have been able to set money aside for the future over the past 30 days. The treatment effect for this outcome is 8 percentage points relative to the control mean of 6%.

4.2.2 Psychological Measures

Table 11 shows outcomes related to psychological and physical well-being. I measure psychological outcomes across all six survey waves using the Kessler-6 scale for pyschological distress, which is measured on a five-point scale in which a higher number corresponds with better psychological well-being. Across all survey waves, the control mean is 3.09, and the pooled effect for treatment individuals is 0.26 (*p-value 0.04*).

In the endline survey (Survey 6), I collected several other measures of self-reported psychological measures which are shown in Table 13. The first measure is the GAD-2 Anxiety Scale, which is measured on a four-point scale in which a higher score indicates higher levels of anxiety. Individuals had a 0.21 (*p-value 0.04*) lower anxiety score relative to their control counterparts, who had a mean score of 2.32. Individuals also reported measures of where they felt they were on Cantril's Ladder, a 10-point scale in which zero is the worst possible life for the person, and 10 is the best possible life. Individuals in the treatment and control groups did not report significant differences in where on the ladder they felt they were in August 2022 and the present (July 2025), but the treatment group's perception of future position on the ladder is about a half-point higher (TE 0.44, *p-value 0.09*) than the control group's (mean 7.34). Finally, individuals in the study completed the Short Grit Scale 5-point index. There were no economically or statistically signficant differences between treatment and control indvidiuals' perceived levels of grit (TE 0.03*, p-value 0.81*).

4.2.3 Physical Well-being Outcomes

All measures of physical health were collected only in the endline survey, and therefore I am not well-powered to detect small effects, but I do not find any significant physical health outcomes. Treatment participants were no more or less likely to attend preventative (TE 0.06, *p-value 0.37*), dental (TE -0.10, *p-value 0.85*) or eye (0.13, *p-value 0.20*) doctor visits relative to the control group. I also find no evidence that individuals in the treatment group were less likely to forgo medical care due to cost (TE -0.04, *p-value 0.37*).

4.3 Income and Expenditure

4.3.1 Income

The intervention produced sizable impacts on participants' income and financial activity. As shown in Table 12, individuals in the treatment group experienced an average increase of $581.18 (*p-value 0.00*) in total income relative per month to control. This effect is driven primarily by two components. First, treatment participants earned $97.03 (*p-value 0.06*) more from labor income as discussed in the previous subsection, a modest but meaningful gain given the control group's mean monthly earnings of $327.25. Second, participants receive an additional $502.74 per momth in public benefits (*p-value 0.00*) relative to the control mean of $187.25. This gain is of course largely comprised of the cash transfers from the GI + TAYportunity program. Treatment individuals experience a very small gain in additional income from other miscellaenous sources (TE 8.17, *p-value 0.04*) relative to control (mean $10.74). Income in the form of transfers from family and friends did not differ economically or statistically significantly between treatment and control.

4.3.2 Expenditure

Turning to expenditure, the intervention increased participants' spending by an average of $563 (*p-value 0.00*) relative to control. Disaggregated results indicate that higher expenditures are spread across multiple categories: rent, food consumed outside the home, utilities, bills, shopping, leisure, groceries, and transportation all increase significantly for treatment participants. For example, treatment households spent an additional $198.80 per month on rent, $32.38 on groceries, $22.26 on utilities, 437.75 on bills, and $29.59 on transportation (*all p-values* 0.00). Increases are also visible in discretionary categories such as shopping (TE 14.59, *p-value 0.00*), leisure (TE 7.41, *p-value 0.02*) and food outside the home (TE 25.37, *p-value 0.00*), as well as assistance to friends an family (TE $11.49, *p-value 0.00*).

4.4 Assets and Debt

4.4.1 Savings and Asset Accumulation

The intervention had sizable impacts on participants' asset holdings, while also influencing debt patterns. Table 13 shows that treatment individuals accumulated an additional $149.06 in total cash savings relative to the control group *(p-value 0.00)*. This effect represents a large increase given that control participants reported an average savings balance of just $94.10.

In the endline survey (Wave 6), I collected responses from study participants on their asset purchases since August 2022, along with the price they paid for each asset. Table 14 summarizes the asset purchases of both the treatment and control groups. Over the course of the three-year intervention, treatment individuals accumulated $886.71 (*p-value 0.06*) more in assets relative to the control mean of $968.62. I obtain measures of total net worth at endline by summing Wave 6 cash savings and asset purchases and find that treatment individuals have more than double the net worth (TE $1150.69, *p-value 0.02*) of those in the control group (mean $1,063.28).

4.4.2 Debt

Turning to debt outcomes in Table 13, the results show that on average, there is no statistically significant difference in total debt between treatment and control individuals. For both groups total debt balances increased over time: the treatment mean level of debt in survey wave one was $3,825.97 and it increased steadily to $7240.78 at endline.

Examining individuals' debt composition, it can be seen that treatment individuals carry $877.12 more vehicle debt (*p-value 0.02*) relative to control. At the same time, there were no statistically significant differences in student debt, medical debt, credit card balances, borrowing from friends and family, or legal and miscellaneous debts. These results indicate that increased indebtedness was not broad-based but concentrated in specific categories—particularly durable goods. This may reflect greater access to credit or a higher willingness to take on debt when coupled with improved income and cash balances.

4.5 Time Use

Table 15 shows the results for the study's primary time-use outcomes, including hours of sleep, paid work, education, unpaid labor, leisure, and transportation. Individuals in the treatment group slept 3.94 more hours per week (*p-value 0.00*) than the control group who reported a mean of 33.95 hours of sleep per week. Treatment individuals also spent 1.30 additional hours on education (*p-value 0.06*) relative to the control group, who on average spent 4.19 hours per week on educational endeavors. Time spent working rose by 1.42

hours relative to the control mean of 10.01 hours, though this estimate is imprecise (*p-value 0.*22). Unpaid labor fell by 1.29 hours relative to control (mean 7.11) hours (*p-value 0.15*), while leisure (0.06 hours) and transportation (0.55 hours) did not differ meaningfully from control.

4.6 Life-course and Behavioral Outcomes

4.6.1 Arrests

Table 16 reports the effects of the intervention on arrest outcomes using administrative records that cover the full sample (N $\approx$ 1,216). The intent-to-treat (ITT) estimates show a 3.6 percentage-point decline in the probability of any felony arrest (*p-value 0.03*) relative to a control mean of nine percent. The corresponding local average treatment effect (LATE) of participation in job-training conditional on cash is a 4.4 percentage-point reduction (*p-value 0.03*). In this case, the LATE is estimated by instrumenting actual program take-up with randomized treatment assignment, capturing the causal effect of participating in the intervention among those induced to enroll by the offer of treatment. In contrast, the effect on the broader measure of any arrest is smaller and statistically insignificant (ITT = -1.9 percentage points; LATE = -2.4 percentage points). The point estimates for the number of felony arrests over the intervention period also fall by 0.047 in the ITT and 0.058 in the LATE (*p-value* $<$ *0.10* for both) whereas total arrest counts show no meaningful movement.

These findings suggest that the combined cash-plus-training intervention had a meaningful deterrent effect on serious criminal-justice outcomes, reductions in felony arrests, while leaving overall arrest incidence unchanged. The absence of a detectable effect on total arrests implies that the effect is concentrated on more serious offenses, consistent with the broader pattern of program-driven improvements in earnings, stability, and well-being documented earlier. The concentration of effects on felony arrests may also reflect the fact that individuals who become incarcerated are disqualified from continued participation in the intervention and are therefore directly incentivized to avoid serious offenses.

4.6.2 Child-bearing

Individuals in the treatment group were nearly half as likely to have a child relative to the control group. 15% of individuals in the control group either gave birth or had a partner who gave birth during the intervention period, compared to only 8% (*p-value 0.04*) of individuals in the treatment group. Importantly, this difference is not driven solely by female respondents – in fact, the drop is more statistically significant for men than women (Figure 4). This decline may reflect an increased ability to afford and access contraception, which enables participants to better avoid unintended pregnancies, or it may indicate that increased employement or the TAYportunity program raised the opportunity cost of parenthood during this stage of life.

5 Mechanisms Driving Results

The results discussed in Section 4 are the treatment effects of the bundled intervention – monthly cash transfers and the *option* to participate in the TAYportunity program. Given the mixed evidence on cash transfers and job-training programs in the US to date, it is useful to know to whether the effects I am observing are a result of the cash transfers in and of themselves, or if they are due to the combination of cash transfers and TAYportunity program. To disentangle the relative contributions of the cash and employment components of the intervention, I first use an instrumental-variables (IV) approach to estimate the causal effect of participation in TAYportunity conditional on receiving the guaranteed income. This step identifies the incremental impact of the employment and training program among individuals who already benefit from the liquidity provided by monthly transfers. Using the methods proposed in Humphries et al. (2025), I then use a simple decomposition framework, which uses the bundled treatment estimates and IV estimates as inputs, to recover the implied effects of the cash-only treatment. I am then able to compare outcomes across three margins: the effects of the bundled treatment, the effects of TAYportunity conditional on receiving cash, and the effects of receiving only the cash transfers. In sections 5.1 and 5.2, I discuss the IV and cash decomposition methodologies, and I present the results from these methods in section 5.3.

5.1 IV Approach to Measure Effects of TAYportunity

To better understand the mechanisms driving the observed treatment effects, I begin by estimating the marginal effect of job-training participation among individuals who received cash transfers. Because individuals who choose to participate in the TAYportunity program may differ systematically from those who do not, simple comparisons of participants and non-participants would yield biased estimates of program effects. To address this selection issue, I employ an instrumental variable (IV) approach to obtain estimates of the effects from participating TAYportunity program conditional on receiving the cash transfers[7]. This analysis is therefore restricted to individuals who were randomized into the treatment group and offered the cash transfers, and it estimates the marginal effect of job-training participation on top of the cash.

5.1.1 Instrument Construction

As described in Section 2, indivduals randomized into the treatment group were assigned a TAYportunity Liaison whose purpose was to encourage the individuals to participate in the TAYportunity program and to answer any questions they may have along the way. Assignment to liaisons was effectively random;

[7]Because all individuals in the treatment group receive the monthly cash transfers, I cannot measure the effect of the TAYportunity program in the absence of cash assistance, but I am able to obtain effects of the program conditional on receiving cash as well as estimates for the impact of solely the cash transfers.

participants were assigned in order of enrollment, which itself tracked the randomized treatment list. There are seven program liaisons, and each liaison was assigned an average of 50 participants. Table 16 in Appendix A denotes the exact number of participants assigned to each liaison.

Although all liaisons were instructed to encourage treatment individuals to participate in the TAYportunity program, they differed in how persistently they followed up with their assigned individuals and how strongly they encouraged them to participate in the job-training. This effectively random assignment along with varying rates of encouragement across liaisons create a classic "judge IV" setting, where participants are as-if randomly paired with decision-makers who vary in stringency, generating quasi-experimental variation in take-up (see Aizer and Doyle, 2015 and Dobbie et al., 2018). I create a liaison encouragement intensity measure by calculating the leave-one-out average of individuals per liaison that attended the TAYportunity orientation (stage one of the job-training program discussed in Section 2). The instrument is therefore defined as:

$$Z_i = \frac{1}{N_{\ell(i)} - 1} \sum_{j \neq i, j \in G_{\ell(i)}} A_j$$

where $A_i \in \{0, 1\}$ is an indicator of whether or not individual i attended the TAYportunity orientation, $\ell(i)$ is the liaison to whom individual i was assigned, $G_{\ell(i)}$is the set of all individuals assigned to liaison $\ell(i)$, and $N_{\ell(i)}$is the number of elements in set $G_{\ell(i)}$. Figure X below shows the distribution of liaison encouragement intensities.

5.1.2 Estimation

Having defined the instrument, I next estimate the causal effect of TAYportunity participation using a two-stage least squares (2SLS) framework. The estimation proceeds in two steps. In the first stage, I relate individual participation in the TAYportunity program to the randomly assigned liaison's encouragement intensity, capturing the extent to which variation in liaison behavior predicts take-up among otherwise comparable individuals. In the second stage, I estimate the impact of predicted participation on key outcomes of interest, conditional on receiving the monthly cash transfers. The two-stage least squares (2SLS) specification is the following:

$$A_i = \pi_0 + \pi_1 Z_i + X_i^{'} \pi_2 + \alpha_t + \alpha_i + u_{it} \tag{2}$$

$$Y_{it} = \tau_0 + \tau_1 A_i + X_i^{'} \tau_2 + \alpha_t + \alpha_i + \varepsilon_{it} \tag{3}$$

where Y_{it} is the outcome of interest, X_i is a vector of controls, α_t is time fixed effects, α_i is the individual random effect, and ε_i and u_i are the error terms. Standard errors are clustered at the individual level. Identification in this setting requires the following assumptions:

A1: Relevance: Assignment to more encouraging liaisons increases the probability of training. This condition is empirically supported: individuals paired with liaisons exhibiting higher encouragement intensity are significantly more likely to attend the orientation and subsequently enroll in the program. The first-stage F-statistic of 361.27[8] comfortably exceeds conventional thresholds (Staiger and Stock, 1997).

A2: Exclusion: Conditional on covariates and fixed effects, liaison encouragement intensity affects outcomes only through its impact on an individual's participation in the TAYportunity program. In other words, assignment to a more encouraging liaison should not directly alter employment, income, or well-being except via increased take-up of job-training opportunities. This assumption is plausible because all liaisons were instructed to provide the same information and resources; their only systematic difference lies in the degree of follow-up and encouragement provided, not in access to other program benefits or administrative discretion that could directly affect outcomes.

A3: Monotonicity: The monotonicity assumption requires that assignment to a more encouraging liaison weakly increases the probability of participating in the TAYportunity program for all individuals, i.e., there are no "defiers" whose participation probability decreases when paired with a more encouraging liaison. In addition, it requires that the ranking of liaisons by their level of encouragement is consistent across individuals: a liaison who is generally more encouraging must be more encouraging for *every type* of participant, not only for specific subgroups. In this context, the assumption rules out heterogeneity such as a liaison being particularly motivating for participants of the same race or background but less so for others. Based on variation in liaison behavior on observable dimensions, and the fact that assignments were effectively random, this monotonicity condition is plausible in practice.

A4: Stable Unit Treatment Value Assumption (SUTVA): SUTVA requires that one individual's outcomes are unaffected by the treatment status or liaison assignment of others. This condition is likely to hold in this context because interactions between participants are limited: individuals were contacted separately, assigned distinct liaisons, and participated in job-training placements that were administered independently.

Together, these assumptions identify a local average treatment effect (LATE) of TAYportunity participation among liaison compliers; that is, individuals whose decision to engage in the job-training program was influenced by the degree of encouragement provided by their assigned liaison. The LATE thus captures the

[8]Following Hull (2017), the adjusted first-stage F-statistic in my setting is 51.61. This adjustment takes into account that my liaison encouragement measure is contructed using the data from seven liaisons, which means the observed F-statistic from the first-stage regression is seven times the size of the true F-statistic.

causal impact of participating in TAYportunity for the subset of treated individuals whose take-up behavior responds to variation in liaison stringency, holding constant the receipt of guaranteed income. The resulting estimates can therefore be interpreted as internally valid causal effects for the marginal participant induced into training by stronger liaison encouragement.

5.2 Decomposition to Obtain Cash Effects

To isolate the causal effect of the unconditional cash transfer component of the TAYGI program, I extend the instrumental-variables framework presented in Section 5.1 using a modified version of the sequential-choice model developed by Humphries et al. (2025). This framework provides a formal structure for modeling sequential treatment decisions under limited instruments, allowing me to map the two-stage nature of the TAYGI program onto a set of margin-specific causal effects. Building on this framework, I derive a decomposition that expresses the overall intent-to-treat (ITT) effect as the sum of the causal effect of receiving the cash transfer and the incremental effect of training participation conditional on cash.

5.2.1 Notation

To employ this framework, I define participants in the study as being members of one of three exhaustive and mutually exclusive groups: the control group (0), the members of the treatment group that have elected not to do job-training and therefore receive only cash (C), and the members of treatment group who have opted into TAYportunity and are therefore receiving cash and job-training (J). The treatment group is defined as $T \equiv \{C \cup J\}$. For each outcome of interest, the potential outcome for an individual i of group $k \in \{0, C, J\}$ is $Y_i(k)$.

As stated above, there are two margins along which I am interested in estimating effects: (1) control vs. cash only (no job-training) and (2) cash-only vs. cash-plus-training. Therefore I want to estimate the following margin-specific causal effects:

$$\tau_{C|0} \equiv \mathbb{E}\left[Y(C) - Y(0) \,|0 \to C\right] \tag{4}$$

$$\tau_{J|C} \equiv \mathbb{E}\left[Y(J) - Y(C) \,|C \to J\right] \tag{5}$$

Two instruments generate exogenous variation across these treatment states: the first, which I denote in this section as Z_1, is simply the random assignment of individuals into the treatment or control group for the TAYGI program. The second, denoted as Z_2, is the liaison encouragement measure defined in the previous section.

5.2.2 Sequential Choice Structure

The experimental setting is most consistent with the sequential choice model discussed in Humphries et. al. (2025), in which participants move through the TAYGI program in two stages. Participants in the study are first randomized into control (0) or treatment (T), and subsequently, members of the treatment group may opt into the TAYportunity program (J) or not (C). The key assumption satisfied under this framework[9] is conditional pairwise monotonicity (CPM), which requires that toggling one instrument moves individuals in only one direction along any given margin (no defiers), but it does not prohibit simultaneous flows along different margins when the instrument is not margin-specific. Therefore, in my case, $Z_2$is margin-specific so toggling Z_2 induces flows only from state $C \to J$, but Z_1 is not treatmnet specific,meaning that toggling Z_1 induces both $0 \to C$ and $0 \to J$ flows.

Let $\mu^{(n)}_{j \to k}$denote the share of individuals who move from state j to k for $j, k \in \{0, C, J\}$when instrument Z_n for $n \in \{1, 2\}$ is toggled. Also let p_T denote the share of individuals that are in group T, and let p_J denote the share of individuals in the sample that are in group J. Then, the changes in treatment shares induced by each Z_n are:

$$\Delta p^{(1)}_T = \mu^{(1)}_{0 \to C} + \mu^{(1)}_{0 \to J} \tag{6}$$

$$\Delta p^{(1)}_J = \mu^{(1)}_{0 \to J} \tag{7}$$

$$\Delta p^{(2)}_J = \mu^{(2)}_{C \to J} \tag{8}$$

5.2.3 Identification Under Limited Instruments

For any instrument $Z_n$for $n \in \{1, 2\}$, the reduced-form effect on the outcome can be expressed as a weighted average of margin-specific treatment effects:

$$\Delta Y^{(n)} = \tau_{C|0}\mu^{(n)}_{0 \to C} + \tau_{J|0}\mu^{(n)}_{0 \to J} + \tau_{J|C}\mu^{(n)}_{C \to J} \tag{9}$$

Because Z_2 is margin-specific, I am able to rely on the standard result from Humphries et. al. to identify $\tau_{J|C}$. This instrument induces no flows from the control group into either of the treatment groups, i.e. $\mu_{0 \to C} = \mu_{0 \to J} = 0$, and therefore, toggling Z_2 allows me to cleanly identify $\tau_{J|C}$ via the simple Wald ratio:

$$\tau_{J|C} = \left. \frac{\Delta Y}{\Delta p_J} \right|_{Z_2} \tag{10}$$

Note that $\tau_{J|C}$ here is equivalent to the τ_1 obtained in section 5.1.

[9]This framework also relies on the standard IV assumptions.

The case for Z_1 is less straightforward because the first-stage randomization Z_1 is not treatment-specific. Toggling Z_1 induces flows across two margins: $\mu^{(1)}_{0\to C}, \mu^{(1)}_{0\to J} \geq 0$. Therefore, in this case, the change in outcome Y is:

$$\Delta Y^{(1)} = \tau_{C|0}\mu^{(1)}_{0\to C} + \tau_{J|0}\mu^{(1)}_{0\to J} \tag{11}$$

Because the experiment contains only a single randomization lever, equation 11 is under-identified: there are two unknown causal effects but only one independent source of exogenous variation. Random assignment provides a one-dimensional instrument that shifts both margins together in equal proportions. Therefore I don't have the type of variation necessary to decomponse effects on this margin into separate cash and training-specific components.

5.2.4 The Additivity Restriction

In order to obtain a fully identified system, I need to impose one final assumption – additivity:

$$\tau_{J|0} = \tau_{C|0} + \tau_{J|C} \tag{12}$$

This assumption states that the total effect of moving directly from control to the cash + training state $(0 \to J)$ equals the sum of the cash-only effect $(0 \to C)$ and the incremental training effect conditional on cash $(C \to J)$. Plugging equation X into X yields:

$$\Delta Y^{(1)} = \tau_{C|0}\underbrace{\left(\mu^{(1)}_{0\to C} + \mu^{(1)}_{0\to J}\right)}_{\Delta p^{(1)}_T} + \tau_{J|C}\underbrace{\mu^{(1)}_{0\to J}}_{\Delta p^{(1)}_J} \tag{13}$$

and thus, the cash-only effect can be recovered as:

$$\tau_{C|0} = \frac{\Delta Y^{(1)} - \tau_{J|C}\Delta p^{(1)}_J}{\Delta p^{(1)}_T} \tag{14}$$

Because all individuals assigned to treatment receive the cash transfer, $\Delta p^{(1)}_T = 1$ and the share of individuals opting into training $\Delta p^{(1)}_J$ is simply equal to the proportion of individuals in my sample that I observe taking up the training, which I denote using p. Equation X therefore simplifies to:

$$ITT = \tau_{C|0} + p\tau_{J|C} \tag{15}$$

This decomposition expresses the intent-to-treat (ITT) effect as the sum of the average effect of receiving the cash transfer and the incremental effect of training weighted by its take-up rate.

5.3 Results

I this section, I report the estimated treatment effects from three complementary specifications: intent-to-treat (ITT), instrumental-variables local average treatment effects (LATE), and the derived cash-only effects obtained from the decomposition described in Section 5.2. Columns (1) and (2) present the ITT and LATE estimates, respectively, while column (3) reports the implied effects of receiving the unconditional cash transfer in the absence of job-training.

5.3.1 Employment and Earnings

Table 3 shows the three-margin panel of treatment effects for employment and earnings outcomes. Individuals who participated in job-training experienced a large increase in employment outcomes relative to those who received only cash. They were 19 percentage points more likely to be employed (*p-value 0.00*), worked an additional 26 hours in the past 30 days (*p-value 0.02*), and earned roughly $253 more from employment in that period (*p-value 0.04*). These effects represent a considerable amplification of the impacts observed in the ITT estimates, suggesting that job-training substantially boosted labor market engagement above and beyond the provision of cash. Even though the cash-only effects on employment outcomes are statistically insignificant, it is notable that the point estimates are negative across all three measures: employment status (–0.05), hours worked (–11.4), and earnings (–$69), suggesting that unconditional cash transfers alone did not stimulate labor supply and may have modestly reduced short-term work effort relative to the control group. Consistent with this pattern, satisfaction with employment and working hours rise among job-training participants (although not statistically signficant) but remain flat among cash-only recipients.

5.3.2 Consumption, Savings and Debt

Table 14 shows treatment effects for spending and savings across all three margins. Participants who participated in TAYportunity accumulated an additional $264 in total savings (*p-value 0.02*), a significant increase over cash-only recipients. While the ITT estimates already suggest improvements in overall cash savings, the LATE results indicate that job-training deepened these financial gains by fostering higher earnings and therefore higher savings. Turning to expenditure, the unconditional transfers alone increase total spending by $646 (*p-value 0.04*) but do not lead to higher savings, suggesting that the additional liquidity was largely used for consumption smoothing rather than asset accumulation. In contrast, the job-training program does not lead to significant increases in total expenditure. Finally, neither job-training nor the cash transfers yield statistically significant effects on participants' debt burdens.

5.3.3 Economic Security and Well-being

The results in Table 17 indicate that both the cash and job-training components of the TAYGI program contributed to improvements in economic security and well-being. The ITT estimates indicate substantial and statistically significant gains in perceived food security (0.20 SD, *p-value 0.00*), and though neither the LATE or cash-only estimates on this outcome are significant, the point estimates indicate that this effect are driven both by the job-training program as well as the cash transfers themselves. Gains in financial security are driven primarily by the job-training program – the LATE estimate yields a 0.57 SD increase (*p-value 0.07*) in financial security. The cash-only effect on the measure, although smaller and imprecisely estimated (0.17 SD, *p-value 0.47*), points to moderate improvements in short-term material stability, which is consistent with the role of guaranteed income in easing liquidity constraints.

Housing and mobility outcomes reveal an interesting pattern. The ITT estimates show that the bundled treatment led to a 0.18 s.d. (*p-value 0.02*) increase in housing security, which was driven primarily by the cash transfers, which on their own led to a 0.15 s.d. increase (*p-value 0.10*) increase in housing security. Job-training did not have a significant effect on the measure, but participants were significantly less likely to move residences: the number of moves in the past 90 days fell by 3.3 (*p-value 0.02*) relative to cash-only participants. This suggests that job-training may have indirectly improved housing stability, not by raising housing security perceptions directly, but by helping individuals stabilize living arrangements through higher and more consistent earnings. Finally, job-training participants reported a 0.23 point increase in psychological well-being on the Kessler-6 scale (*p-value 0.06*) relative to cash-only individuals, indicating reduced psychological distress resulting from the TAYportunity program.

5.3.4 Time Use

Turning to the time-use outcomes shown in Table 18, job-training substantially reallocated participants' weekly activities. Those whole participated in TAYportunity spent an additional 6.51 hours working per week (*p-value 0.02*), 6.82 more hours sleeping (*p-value 0.00*), and 5.10 more hours on leisure (*p-value 0.01*). There is no evidence of changes in time allocated to education, transportation, or unpaid labor. Individuals who took up only the cash part of the intervention experienced 3.29 fewer leisure hours (*p-value 0.04*) per week, but did not experience any other statistically significant difference in time allocation. These results imply that job-training increased both productive and restorative activities without displacing non-market time, perhaps by improving the regularity and structure of participants' daily schedules.

6 Discussion

6.1 Overview of Main Findings

The experimental evidence from the TAYportunity + Guaranteed Income (TAYGI) program shows that combining sustained unconditional cash assistance with structured employment and training opportunities can generate broad and durable gains across economic, psychological, and social dimensions. Eighteen months after random assignment, treated young adults are substantially more likely to be employed, work more hours, and earn higher labor income. They also exhibit lower financial stress, greater housing and food security, improved mental health, and reduced contact with the criminal-justice system. The pattern of effects points to a unified mechanism: predictable income relieved liquidity and other constraints that had previously limited labor-market engagement, while access to skills and job-training raised the expected return to work. The simultaneous relaxation of both financial and structural barriers translated into sustained improvements in stability and self-sufficiency rather than short-lived consumption responses.

The magnitude of these effects is notable given the population's baseline disadvantage. Employment rises by nearly seven percentage points from a control mean of 0.39, monthly hours by more than 12 percent, and labor earnings by roughly 30 percent. Housing-security and food-security indices improve by 0.18 and 0.20 standard deviations, respectively, and psychological-distress scores fall by 8.5 percent. Felony arrests decline materially, suggesting that the program's benefits extended beyond labor markets to encompass social and behavioral outcomes. Together, these findings indicate that sustained liquidity combined with credible pathways to work can meaningfully alter both short-run behavior and longer-run trajectories for young adults facing overlapping financial and opportunity constraints.

6.2 Labor Supply Elasticities Relative to Other Cash Transfer Studies

To better contextualize my results in the literature, I translate the observed changes into implied income elasticities of labor supply and then benchmark the elasticities against those found in other guaranteed income studies in developed countries. Using control-group means of 39% employment participation, 40.2 hours worked per month, and the $1,000 monthly transfer as the exogenous income shock, the aggregate elasticities are +0.24 on the extensive margin and +0.17 on the intensive margin. Disaggregating by treatment arm reveals sharp heterogeneity: the cash-only group exhibits modest negative elasticities (–0.17 and –0.38 respectively), consistent with the classical income effect, whereas the cash + training group shows strongly positive elasticities (+0.65 and +0.86 respectively), implying that employment and earnings rose nearly proportionally with the transfer value.

Table 6 situates TAYGI's elasticities relative to both incentive-based and unconditional cash transfer settings. The lottery and pure guaranteed income experiments yield negative elasticities, reflecting pure wealth effects: recipients treat the transfer as non-labor income and reduce labor supply. Interestingly, the cash-only treatment for my setting results in similarly negative elasticities, both on intenstive and extensive margins. It is worth noting that the negative elasticity I find on the extensive margin is similar to that of the OpenResearch Unconditional Support (ORUS) study (Vivalt et al., 2024), since that intervention is the same as the cash component of the TAYGI program. In contrast, the EITC and the bundled TAYGI program both generate positive elasticities by tying liquidity to participation or by pairing income support with opportunities for human capital investment. Despite its unconditional structure, the cash plus job-training nature of TAYGI produces effects similar in magnitude to the work-contingent EITC, underscoring how non-pecuniary constraints can be as binding as formal work incentives.

6.3 Economic Security, Well-Being and Crime Relative to Other Cash Transfer Studies

The TAYGI results reveal broad improvements in material security, psychological well-being, and criminal-justice outcomes, distinguishing this experiment from the mixed record of prior unconditional cash programs. The estimated gains of 0.2–0.3 standard deviations in food, housing, and financial security contrast with the smaller and often transitory effects observed in U.S. guaranteed-income pilots, where increases in subjective well-being and spending were not accompanied by measurable reductions in hardship or insecurity (Balakrishnan et al., 2024; Bartik et al., 2024; Jacob et al., 2022; Jaroszewicz et al., 2024; Miller et al., 2024). In TAYGI, the durability of security gains across six survey waves suggests that pairing transfers with structured training altered not only liquidity but also the underlying income process. This pattern is consistent with the longer-run improvements in health and schooling documented in the EITC and Alaska Permanent Fund contexts (Akee et al., 2010; Bastian and Michelmore, 2018; Forget, 2011), yet the persistence here arises in a population without dependents or prior labor-market attachment, underscoring the importance of concurrent skill investment.

Psychological well-being improved by roughly 0.25 points on the Kessler-6 scale and anxiety declined by 0.2 points, echoing the stress reductions reported in earlier cash experiments but exceeding their magnitude. Whereas prior U.S. pilots found muted effects on mental health once material hardship was accounted for (Vivalt et al., 2024; West and Castro, 2023), TAYGI's design appears to have translated financial relief into genuine reductions in distress. The combination of predictable income and engagement in training likely increased perceived control and future orientation—mechanisms absent from purely pecuniary transfers.

The decline in felony arrests (-3.6 percentage points) also contrasts with earlier evidence. Casino-dividend and EITC studies found small or statistically fragile effects on crime (Akee et al., 2010), while evaluations of job-training programs such as Job Corps documented moderate reductions largely attributable to increased employment opportunities (Barnow and Smith, 2015).

6.4 Connections to Job-Training Evidence

The magnitude of the TAYGI effects are similar to those seen in some of the most successful U.S. sectoral employment initiatives in the short-run. Short-term evaluations of Project QUEST (Roder and Elliott, 2019), Year Up (Fein and Dastrup, 2022), and WorkAdvance (Katz et al., 2022) report short-term increases of 4–10 percentage points in employment participation and 20–53% percent earnings gains within two years of randomization. Meanwhile, most federally funded WIA/WIOA programs yield only a 2–4 p.p. increase in employment and 4–7 percent increases in earnings (Schochet et al., 2008). TAYGI achieves effects of comparable or greater magnitude in a shorter horizon and with a younger, more precarious population.

Two design features help explain the short-run success of the TAYportunity program. First, the guaranteed-income component lowered the up-front costs of participation by allowing recipients to more easily afford transportation and forgone earnings. Second, the training pathways improved matching between employers and participants, generating a partially mechanical increase in employment through program placements. This interpretation is supported by evidence of reduced job-search effort: treated individuals submitted roughly half as many job applications as control participants during their most recent job search.

Although the program's short-run impacts are substantial, its ultimate success will depend on whether these gains persist in the longer term. Long-term evaluations of the sectoral job-training programs discussed above report sustained 10–30 percent earnings gains several years post-randomization, while most federally funded WIA/WIOA programs yield only 4–7 percent increases. Future research is needed to determine where TAYGI falls on that spectrum. As it stands, the TAYGI results provide compelling evidence for a "cash-plus" policy approach in which income support serves not as a substitute for human-capital investment, but as its enabling complement.

7 Conclusion

This paper provides the first experimental evidence from a high-income context on the combined effects of guaranteed income and job training for economically marginalized young adults. Using a randomized controlled trial of Los Angeles County's TAYportunity + Guaranteed Income (TAYGI) program, I find that unconditional cash transfers paired with optional job-training pathways substantially improved participants'

economic security, well-being, and stability. Employment increased by roughly 18 percent and earnings by 30 percent, accompanied by sizable gains in food, housing, and financial security and reductions in psychological distress and felony arrests. These effects were driven primarily by participants who engaged in the job-training component, indicating that liquidity support and human-capital investment operated as complements rather than substitutes.

Decomposing the treatment effects reveals that the unconditional cash transfers alone provided immediate relief from financial strain, raising consumption, rent payments, and housing stability; however, they did not meaningfully increase employment or long-term asset accumulation. By contrast, participation in the job-training program conditional on cash generated larger and more persistent improvements in labor-market outcomes, savings, and well-being. These results suggest that income support can serve as an enabling complement to human-capital programs, allowing participants to overcome liquidity and fixed-cost barriers to skill acquisition and labor-market entry.

The findings have important policy implications. Integrating income and employment interventions can yield returns that neither policy achieves alone, particularly for young adults navigating the transition to economic independence. As policymakers in the United States and elsewhere consider expansions of guaranteed-income pilots, the TAYGI results highlight the potential of a "cash-plus" approach that simultaneously addresses short-term security and long-term mobility. Future research should examine the persistence of these effects after transfers end and explore how program design, e.g. training quality, liaison engagement, or timing of cash support, shapes the relationship between short-run support and long-term human capital investment.

References

Aizer, A., & Doyle, J. (2015). Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges. *Quarterly Journal of Economics*, *130*(2), 759–803.

Akee, R., Copeland, W., Keeler, G., Angold, A., & Costello, E. J. (2010). Parents' Incomes and Children's Outcomes: A Quasi-experiment Using Transfer Payments from Casino Profits. *American Economic Journal: Applied Economics*, *2*(1), 86–115. https://doi.org/10.1257/app.2.1.86

Baird, M. D., Enberg, J., & Gutierrez, I. A. (2022). RCT evidence on differential impact of US job training programmes by pre-training employment status. *Labour Economics*, *75*(102140).

Balakrishnan, S., Chan, S., Constantino, S., Haushofer, J., & Morduch, J. (2024). Household Responses to Guaranteed Income: Experimental Evidence from Compton, California. *NBER Working Paper*, (33209).

Barnow, B., & Smith, J. (2015). Employment and Training Programs. In R. A. Moffitt (Ed.), *Economics of Means-Tested Transfer Programs in the United States, Volume 2* (pp. 127–234). University of Chicago Press / NBER. https://www.nber.org/books-and-chapters/economics-means-tested-transfer-programs-united-states-volume-2/employment-and-training-programs

Barr, C., & Malik, S. (2016). Revealed: the 30-year economic betrayal dragging down Generation Y's income. *The Guardian*.

Bartik, A. W., Rhodes, E., Broockman, D. E., Krause, P. K., Miller, S., & Vivalt, E. (2024). The Impact of Unconditional Cash Transfers on Consumption and Household Balance Sheets: Experimental Evidence from Two US States. *NBER Working Paper*.

Bastian, J., & Michelmore, K. (2018). The long-term impact of the earned income tax credit on children's education and employment outcomes. *Journal of Labor Economics*, *36*(4), 1127–1163. https://doi.org/10.1086/697477

Bureau of Labor Statistics. (2020). Labor force statistics by age and employment status. https://www.bls.gov

Card, D., Kluve, J., & Weber, A. (2018). What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations. *Journal of the European Economic Association*, *16*(3), 894–931. https://doi.org/10.1093/jeea/jvx028

Chetty, R., Hendren, N., & Katz, L. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, *106*(4), 855–902.

Dobbie, W., Goldin, J., & Yang, C. (2018). The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. *American Economic Review*, *108*(2), 201–240.

Fein, D., & Dastrup, S. (2022). *Benefits that last: Long-term impact and cost-benefit findings for year up* (tech. rep. No. 77). Office of Planning, Research and Evaluation.

Forget, E. L. (2011). The Town with No Poverty: The Health Effects of a Canadian Guaranteed Annual Income Field Experiment. *Canadian Public Policy*, *37*(3), 283–305. https://doi.org/10.3138/cpp.37.3.283

Gennetian, L., Sciandra, M., Sanbonmatsu, L., Ludwig, J., Katz, L., Duncan, G., Kling, J., & Kessler, R. (2012). The Long-Term Effects of Moving to Opportunity on Youth Outcomes. *Cityscape*, *14*, 137–68.

Haushofer, J., & Shapiro, J. (2016). The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya. *Quarterly Journal of Economics*, *131*(4), 1973–2042. https://doi.org/10.1093/qje/qjw025

Hull, P. (2017). *Examiner designs and first-stage f statistics: A caution* (tech. rep.). Brown University.

Humphries, J., Ouss, A., Stavreva, K., Stevenson, M., & van Dijk, W. (2025). Conviction, Incarceration, and Recidivism: Understanding the Revolving Door. *NBER Working Paper*, (32894).

Jacob, B., Kapustin, M., & Ludwig, J. (2014). Human Capital Effects of Anti-Poverty Programs: Evidence from a Randomized Housing Voucher Lottery. *NBER Working Paper*, (20164).

Jacob, B., Pilkauskas, N., Rhodes, E., Richard, K., & Shaefer, L. (2022). The COVID-19 Cash Transfer Study II: The Hardship and Mental Health Impacts of an Unconditional Cash Transfer to Low-Income Individuals. *National Tax Journal*, *75*(3).

Jaroszewicz, A., Jachimowicz, J., Hauser, O., & Jamison, J. (2024). How Effective is (More) Money? Randomizing Unconditional Cash Transfer Amounts in the US. *SSRN*.

Katz, L., Hendra, R., Roth, J., & Schaberg, K. (2022). Why Do Sectoral Employment Programs Work? Lessons from WorkAdvance. *Journal of Labor Economics*, *40*(S1).

Katz, L., Kling, J., & Liebman, J. (2001). Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment. *Quarterly Journal of Economics*, *116*(2), 607–654.

Kling, J., Liebman, J., & Katz, L. (2007). Experimental Analysis of Neighborhood Effects. *Econometrica*, *75*(1), 83–119.

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more t in experiments. *Journal of Development Economics*, *99*(2), 210–221.

Miller, S., Rhodes, E., Bartik, A. W., Broockman, D. E., Krause, P. K., & Vivalt, E. (2024). Does Income Affect Health? Evidence from a Randomized Controlled Trial of a Guaranteed Income. *NBER Working Paper*, (32711).

Mountjoy, J. (2022). Community Colleges and Upward Mobility. *American Economic Review*, *112*(8), 2580–2630.

Riccio, J. A., & Miller, C. (2016). New York City's First Conditional Cash Transfer Program: What Worked, What Didn't. *SSRN Working Paper*.

Robins, P. K. (1985). A Comparison of the Labor Supply Findings from the Four Negative Income Tax Experiments. *The Journal of Human Resources*, *20*(4), 567–582.

Roder, A., & Elliott, M. (2019). Project quest evaluation report. https://economicmobilitycorp.org/stimulating-opportunity/

Roelen, K., Devereux, S., Abdulai, A.-G., Martorano, B., Palermo, T., & Ragno, L. P. (2017). *How to Make "Cash Plus" Work: Linking Cash Transfers to Services and Sectors* (tech. rep. Innocenti Working Paper 2017-10). UNICEF Office of Research-Innocenti. https://www.unicef-irc.org/publications/pdf/IWP_2017_10.pdf

Schochet, P. Z., Burghardt, J., & McConnell, S. (2008). Does Job Corps Work? Impact Findings from the National Job Corps Study. *American Economic Review*, *98*(5), 1864–1886. https://ies.ed.gov/ncee/wwc/Docs/InterventionReports/WWC_Jobcorps_040108.pdf

Sedlmayr, R., Shah, A., & Sulaiman, M. (2020). Cash-plus: Poverty impacts of alternative transfer-based approaches. *Journal of Development Economics*, *144*(102418). https://doi.org/10.1016/j.jdeveco.2019.102418

Staiger, D., & Stock, J. H. (1997). Instrumental Variables with Weak Instruments. *Econometrica*, *65*(3), 557–586.

Vivalt, E., Rhodes, E., Bartik, A. W., Broockman, D. E., Krause, P. K., & Miller, S. (2024). The Employment Effects of a Guaranteed Income: Experimental Evidence from Two U.S. States. *NBER Working Paper*.

West, S., & Castro, A. (2023). Impact of Guaranteed Income on Health, Finances, and Agency: Findings from the Stockton Economic Empowerment Demonstration. *Journal of Urban Health*, *100*, 227–244. https://doi.org/10.1007/s11524-023-00723-0

Wheeler, L., Garlick, R., Johnson, E., Shaw, P., & Gargano, M. (2022). LinkedIn(to) Job Opportunities: Experimental Evidence from Job Readiness Training. *American Economic Journal: Applied Economics*, *14*(2), 101–125.

Wimer, C., Nam, J., Garfinkel, I., Kaushal, N., Waldfogel, J., & Fox, L. (2020). Young adult poverty in historical perspective: The role of policy supports and early labor market experiences. *Social Science Research*, *86*(102390).

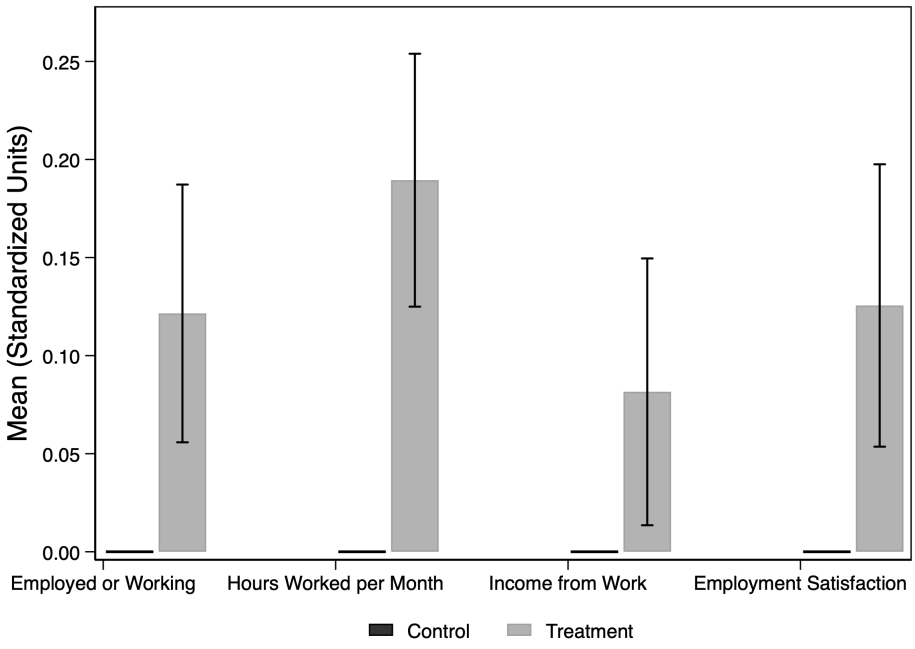
Figures and Tables

Table 1: Sample Balance

	Full Sample			Survey Completers		
Variable	**C**	**T**	**t-stat**	**C**	**T**	**t-stat**
Black	0.59	0.56	1.22	0.53	0.50	1.21
Hispanic/Latino	0.03	0.02	1.21	0.04	0.03	1.05
Female	0.60	0.56	1.06	0.62	0.59	1.35
Age	21.20	21.14	0.25	21.35	21.32	0.13
Probation	0.16	0.18	1.42	0.18	0.18	0.22
Foster care	0.24	0.22	0.67	0.27	0.24	1.12
Hours worked/wk	5.19	4.78	0.72	8.63	8.25	0.45
Unemployed	0.83	0.85	0.94	0.78	0.79	0.31
Earnings/wk	76.27	88.34	1.15	92.85	103.47	0.82

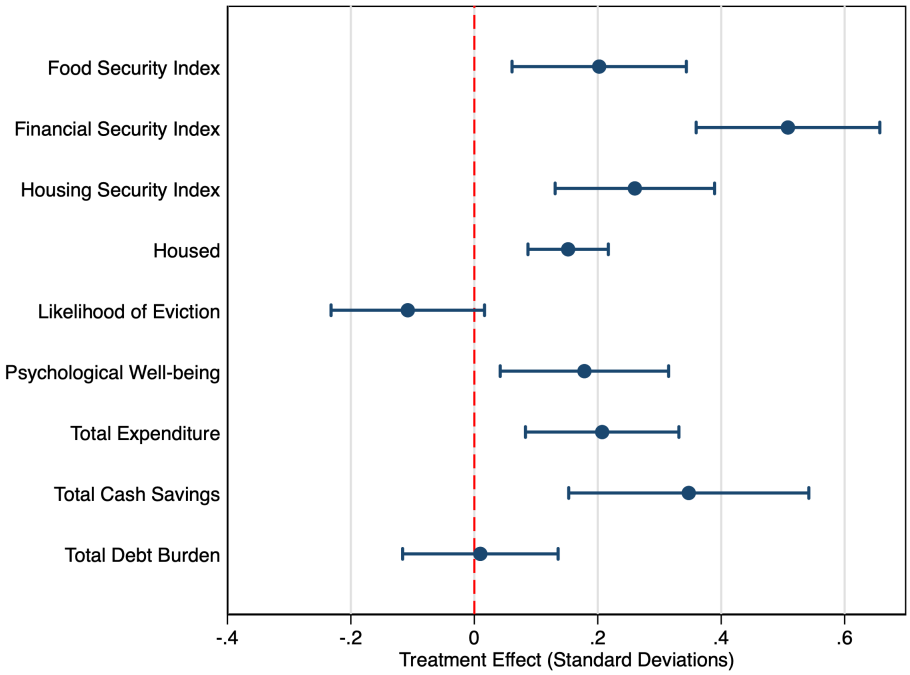
Note: This table reports baseline means for the treatment and control groups and tests for balance across all pre-treatment covariates. Column (1) reports mean values for the control group, column (2) reports mean values for the treatment group, and column (3) reports the difference between the two for the full sample. Columns (4), (5) and (6) report the same measures for the survey subsample. P-values in parentheses correspond to two-sided tests of equality of means. All variables are measured prior to random assignment. No differences are statistically significant after accounting for multiple testing.

Figure 1: Employment Outcomes



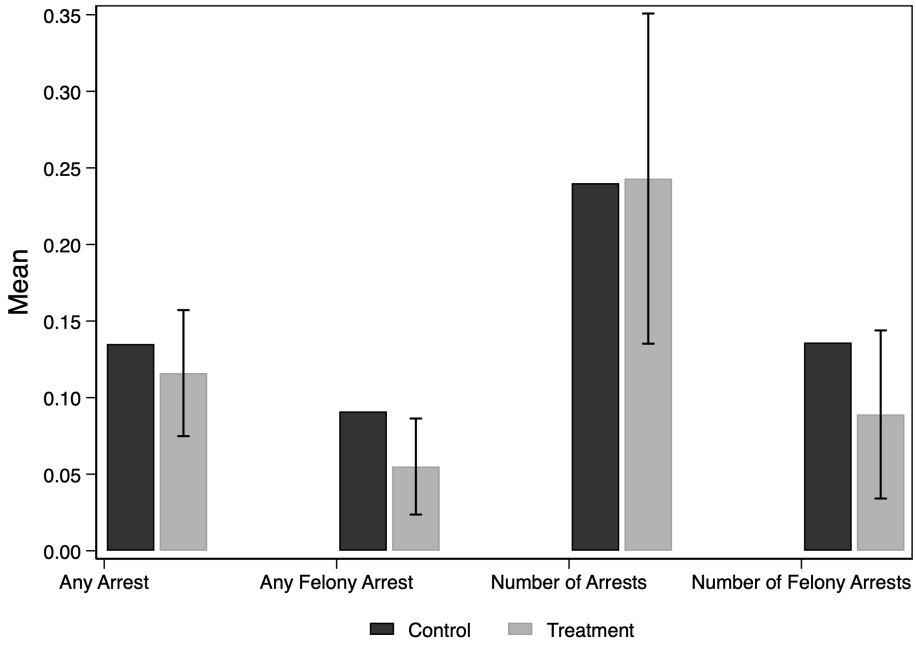
Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

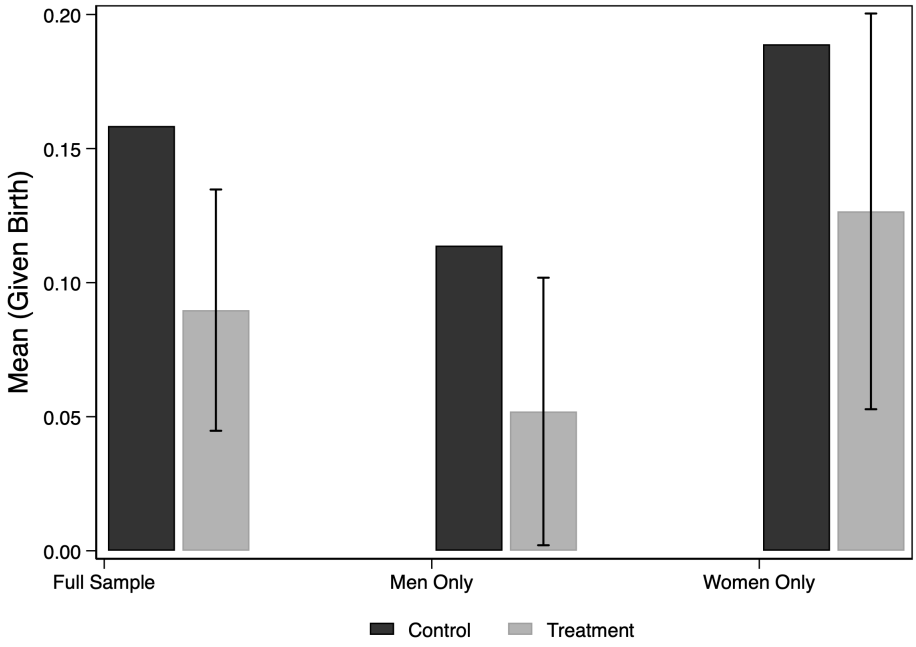
Figure 2: Economic Security and Well-being Outcomes



Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 3: Outcomes on Risky Behaviors





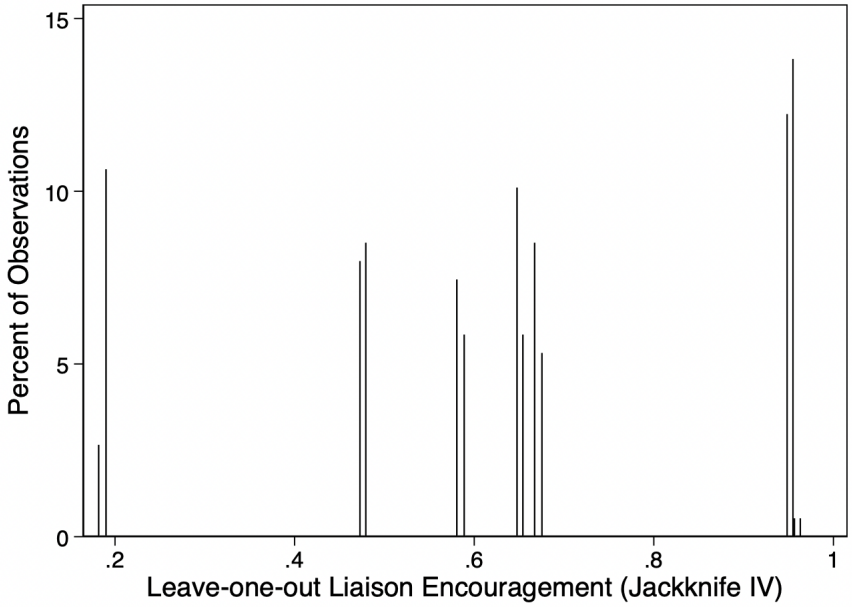
Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Savings, Assets and Debt

Outcome	**Control Mean**	**Treatment Effect**	**N**
Total Cash Savings	**94.10**	**149.06*** **	**2700**
	(429.01)	**(42.58)**	
Value of Assets Bought since 2022 (Survey 6 Only)	867.04	980.22**	489
	(2831.17)	(425.89)	
Total Debt	**5652.53**	**132.52**	**2700**
	(13567.17)	**(871.98)**	
Student Debt	2644.41	-668.75	2700
	(8404.55)	(505.60)	
Vehicle Debt	1113.91	877.12**	2700
	(4479.47)	(354.51)	
Medical Debt	158.11	104.99	2700
	(987.50)	(85.07)	
Legal Debt	96.97	-33.56	2700
	(626.76)	(32.15)	
Credit Card Debt	608.77	-51.66	2700
	(1671.78)	(108.45)	
Debt to Friends and Family	175.90	-19.06	2700
	(776.97)	(52.43)	
Other Debt	236.90	-73.52	2700
	(1214.93)	(76.94)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Distribution of Liaison Encouragement Measures Across Sample



Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 3: Treatment Effect Decomposition – Employment Outcomes

	Treatment Effects				
Outcome	**ITT (OLS)**	**LATE (IV)**	**Cash Only**	**Cash p-value**	**N**
Employed or Working	0.07**	0.19***	-0.05	0.382	930
	(0.03)	(0.07)	(0.05)		
Hours Worked Past 30 Days	5.27	26.02**	-11.44	0.196	930
	(4.32)	(11.90)	(8.78)		
Income Earned from Work	93.81*	252.92**	-68.55	0.469	930
	(52.02)	(122.51)	(94.29)		
Satisfaction with Employment Situation	0.25***	0.64	-0.15	0.571	930
	(0.08)	(0.40)	(0.27)		
Looking for New Job	-0.06**	-0.07	-0.02	0.852	929
	(0.03)	(0.12)	(0.08)		
Satisfaction with Working Hours	0.06***	0.13	-0.02	0.825	930
	(0.02)	(0.14)	(0.09)		
Level of Education Completed	0.01	0.21	-0.13	0.243	930
	(0.04)	(0.15)	(0.11)		

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. LATE sample comprised of 71 control and 123 treatment individuals. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 4: Treatment Effect Decomposition – Expenditure, Savings and Debt

	Treatment Effects				
Outcome	**ITT (OLS)**	**LATE (IV)**	**Cash Only**	**Cash p-value**	**N**
Total Expenditure	563.69***	-129.86	646.42**	0.042	930
	(172.40)	(407.60)	(313.34)		
Total Cash Savings	149.96***	264.02**	-26.42	0.806	930
	(42.58)	(110.59)	(82.78)		
Total Debt	132.52	4213.24	-2572.11	0.280	930
	(871.98)	(3429.12)	(2367.69)		

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. LATE sample comprised of 71 control and 123 treatment individuals. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 5: Labor Supply Elasticities Across Cash Transfer Studies

Study/Context	**Type of Transfer**	**Extensive-Margin Elasticity**	**Intensive-Margin Elasticity**
TAYGI (Bundled)	$1,000/month for 36 months + option for job-training	+0.24	+0.17
TAYGI Training Conditional on Cash Transfers	Job-training while receiving $1,000/month for 36 months	+0.65	+0.86
TAYGI Cash-Only	$1,000/month for 36 months	-0.17	-0.38
Lottery (Sweden) Cesarini et al. 2017	Lump-sum windfall	-0.05 to -0.10	-
Lottery (US) Golosov, Graber, Mogstad & Novgorodsky 2023	Lump-sum windfall	-0.4	-
OpenResearch / OpenAI GI (Vivalt et al. 2024)	$1,000/month for 36 months	-0.16	-0.15
Compton Pledge (Balakrishnan et al. 2024)	Average of $500/month for 24 months	-0.47	-

Appendix

A Supplementary Figures and Tables

Table 6: Survey Response Rates

	Total	**Control**	**Treatment**	**p-value**
Survey 1	92%	92%	92%	**0.861**
Survey 2	89%	89%	89%	**0.877**
Survey 3	85%	84%	85%	**0.750**
Survey 4	83%	83%	83%	**0.924**
Survey 5	87%	88%	84%	**0.254**
Survey 6	83%	81%	83%	**0.456**

Table 7: Employment Participation and Earnings

Outcome	**Control Mean**	**Treatment Effect**	**N**
Employed or Working	**0.39**	**0.07****	**2700**
	(0.48)	**(0.03)**	
Hours Worked (30 Days)	**40.23**	**5.27**	**2700**
	(63.89)	**(4.32)**	
Income Earned from Work	**327.25**	**93.81***	**2700**
	(752.08)	**(52.02)**	
Employed Full-Time	0.12	0.06**	2700
	(0.33)	(0.02)	
Employed Part-Time	0.16	-0.03*	2700
	(0.36)	(0.02)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Employment Satisfaction and Job Search

Outcome	Control Mean	Treatment Effect	N
Employment Satisfaction (4-point scale)	**2.33**	**0.25*** **	**2700**
	(1.24)	**(0.08)**	
Satisfaction Working Hours (Binary)	**0.19**	**0.06*** **	**2700**
	(0.19)	**(0.02)**	
Looking for New Job (Binary)	**0.79**	**-0.06** **	**2700**
	(0.41)	**(0.03)**	
Wave 6 Work Amenity Outcomes			
Work Commute (Minutes)	12.67	-2.17	489
	(23.39)	(2.11)	
Hours Spent in Last Job Search	36.45	-3.06	489
	(129.57)	(10.99)	
Applications Submitted in Last Job Search	49.10	-21.18**	489
	(150.23)	(10.14)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Educational Attainment

Outcome	Control Mean	Treatment Effect	N
Number of Diplomas or Certificates Earned	**0.92**	**0.01**	**2700**
	(0.65)	**(0.04)**	
Currently Enrolled in Educational Program	0.21	0.07**	2700
	(0.40)	(0.03)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Economic Security Outcomes

Outcome	Control Mean	Treatment Effect	N
Food Security	**0.00**	**0.20*** **	**2700**
	(1.00)	**(0.07)**	
Did Not Eat Enough	0.77	-0.10***	2700
	(0.42)	(0.03)	
Low Quality Food	0.81	-0.05*	2700
	(0.39)	(0.03)	
Housing Security	**0.00**	**0.26*** **	**2700**
	(1.00)	**(0.06)**	
Housed (Binary)	0.56	0.15***	2700
	(0.49)	(0.03)	
Likelihood Eviction (4-point scale)	1.74	-0.07*	2700
	(0.94)	(0.06)	
Times Moved 90 Days	3.28	-0.77	2700
	(10.01)	(0.63)	
Adults in Household	1.96	0.05	2700
	(1.64)	(0.11)	
Financial Security	**0.00**	**0.53*** **	**2700**
	(1.00)	**(0.08)**	
Ability to Pay Bills (Binary)	0.16	0.18***	2700
	(0.36)	(0.03)	
Ability to Pay Debt (Binary)	0.23	0.06***	2700
	(0.30)	(0.02)	
Ability to Save for Future (Binary)	0.06	0.08***	2700
	(0.24)	(0.02)	
Ability to Handle Emergency (Binary)	0.12	0.10***	2700
	(0.33)	(0.02)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Psychological and Physical Well-being

Outcome	**Control Mean**	**Treatment Effect**	**N**
Psychological Well-being (K6 Index)	**3.09**	**0.26****	**2700**
	(1.46)	**(0.10)**	
Wave 6 Pyschological Measures			
Anxiety (GAD-2 Index)	2.42	-0.17*	489
	(1.08)	(0.10)	
Non-Cognitive Skills (Short Grit Scale)	3.10	0.00	489
	(1.30)	(0.13)	
Past Life Satisfaction (Cantril Ladder)	4.55	-0.18	489
	(2.92)	(0.30)	
Current Life Satisfaction (Cantril Ladder)	5.09	0.39	489
	(2.69)	(0.25)	
Anticipated Life Satsifaction (Cantril Ladder)	7.34	0.48*	489
	(2.83)	(0.26)	
Wave 6 Physical Health Measures			
Preventative Doctor Visits since August 2022	1.52	0.05	489
	(3.00)	(0.28)	
Dentist Visits since August 2022	1.25	-0.02	489
	(5.97)	(0.49)	
Eye Doctor Visits since August 2022	0.33	0.14	489
	(0.82)	(0.10)	
Individual or Partner Gave Birth since August 2022	0.16	-0.07**	489
	(0.37)	(0.03)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Income and Expenditure

Outcome	**Control Mean**	**Treatment Effect**	**N**
Total Income Past 30 Days	611.06	581.18***	2700
	(1240.57)	(76.25)	
Income Earned from Work	**327.25**	**93.81***	**2700**
	(752.08)	**(52.72)**	
Income from Public Benefits	187.25	502.74**	2700
	(251.70)	(30.88)	
Income from Friends and Family	21.58	-4.33	2700
	(76.72)	(4.06)	
Income from Other Sources	10.74	8.17**	2700
	(60.62)	(4.10)	
Total Expenditure	**1342.26**	**563.09*** **	**2700**
	(2718.13)	**(172.40)**	
Rent	286.75	203.25***	2700
	(515.14)	(41.83)	
Groceries	184.77	32.45***	2700
	(155.97)	(10.25)	
Education	14.91	5.74	2700
	(73.08)	(5.26)	
Payments to Friends and Family	13.65	13.25***	2700
	(53.66)	(4.08)	
Debt Payments	543.97	111.31	2700
	(2464.75)	(151.32)	
Food Outside the Home	57.97	25.23***	2700
	(91.54)	(6.06)	
Utilities	45.29	22.12***	2700
	(106.70)	(7.39)	
Bills	62.44	38.44***	2700
	(91.00)	(6.57)	
Subscriptions	9.36	5.94***	2700
	(23.16)	(1.66)	
Shopping	33.22	16.77***	2700
	(78.96)	(5.35)	
Leisure	11.21	7.83**	2700
	(40.17)	(3.23)	
Transportation	50.67	33.52***	2700
	(101.52)	(7.40)	
Other	13.59	7.19*	2700
	(71.31)	(3.88)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Asset Purchases Across Treatment and Control

	Control Purchases	Average Price (C)	Treatment Purchases	Average Price (T)
Washing Machine	1.07%	$662.50	3.61%	$282.00
Dryer	0.80%	$76.66	3.61%	$212.00
Dishwasher	0.27%	$10.00	1.20%	$50.00
Microwave	5.90%	$68.18	11.45%	$81.42
Vacuum	6.43%	$90.75	11.45%	$105.63
Home Entertainment	7.51%	$439.00	9.64%	$480.50
Gaming Console	6.70%	$346.48	9.64%	$393.06
Gym Equipment	4.02%	$90.80	9.04%	$101.00
Air Conditioner	3.49%	$262.31	7.23%	$194.17
Jewelry	4.02%	$755.20	7.83%	$532.69
Musical Instrument	1.07%	$116.25	3.01%	$1,200.00
Power Tools	2.41%	$320	4.82%	$488.75
Real Estate	0.00%	-	0.00%	-
Computer	8.31%	$684.10	16.27%	$701.44
Cell Phone	23.06%	389.58	33.73%	$660.43
Vehicle	8.31%	$5,896.77	15.06%	$7,824.00
Motorcycle	0.00%	-	0.00%	-
Bicycle	1.88%	$152.14	4.22%	$136.43
Other	3.22%	$595.00	6.02%	$755.00
None	46.38%	-	40.36%	-

Table 14: Time Use

Outcome	Control Mean	Treatment Effect	N
Time Spent Sleeping Past 7 Days	**33.95**	**3.94*** **	**2700**
	(20.26)	**(1.27)**	
Time Spent Working Past 7 Days	**10.06**	**1.32**	**2700**
	(15.97)	**(1.08)**	
Time Spent on Education Past 7 Days	**4.23**	**1.30***	**2700**
	(9.94)	**(0.69)**	
Time Spent on Unpaid Labor Past 7 Days	**7.12**	**-1.27**	**2700**
	(15.39)	**(0.89)**	
Time Spent on Transportation Past 7 Days	**6.99**	**0.58**	**2700**
	(11.42)	**(0.66)**	
Time Spent on Leisure Past 7 Days	**4.40**	**0.04**	**2700**
	(10.63)	**(0.69)**	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Arrest Outcomes

Outcome	**Control Mean**	**ITT Effect**	**LATE Effect**	**N**
	Mean	**Effect**		
Any Arrest	0.135	-0.019	-0.024	2700
		(0.021)	(0.025)	
Any Felony Arrest	0.091	-0.036**	-0.044**	2700
		(0.016)	(0.020)	
Number of Arrests	0.240	0.003	0.000	2700
	(0.055)		(0.067)	
Number of Felony Arrests	0.136	-0.047*	-0.058*	2700
		(0.028)	(0.035)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 1,216 unique individuals in the dataset; 846 control individuals and 370 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Liaison Assignment Summary

	Individuals Per Liaison
Liaison 1	58
Liaison 2	57
Liaison 3	45
Liaison 4	48
Liaison 5	47
Liaison 6	51
Liaison 7	50

Table 17: Treatment Effect Decomposition – Security and Well-being Outcomes

	Treatment Effects				
Outcome	**ITT (OLS)**	**LATE (IV)**	**Cash Only**	**Cash p-value**	**N**
Food Security (Standardized Index)	0.20***	0.14	0.11	0.439	930
	(0.07)	(0.20)	(0.14)		
Financial Security (Standardized Index)	0.53***	0.57*	0.17	0.472	930
	(0.08)	(0.34)	(0.23)		
Housing Security (Standardized Index)	0.18***	0.05	0.15*	0.098	930
	(0.06)	(0.10)	(0.09)		
Housed (Binary Outcome)	0.15***	0.16**	0.05	0.358	930
	(0.03)	(0.06)	(0.05)		
Likelihood of Eviction (4-Point Scale)	-0.08	0.08	-0.13*	0.075	930
	(0.63)	(1.49)	(1.15)		
Number of Times Moved Past 90 Days	-6.72	-3.33**	1.42	0.218	930
	(0.06)	(0.08)	(0.07)		
Psychological Well-being (Kessler-6 Index)	0.26**	0.23**	0.11	0.363	930
	(0.10)	(0.11)	(0.13)		

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. LATE sample comprised of 71 control and 123 treatment individuals. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 18: Treatment Effect Decomposition – Time Use Outcomes

	Treatment Effects				
Outcome	**ITT (OLS)**	**LATE (IV)**	**Cash Only**	**Cash p-value**	**N**
Time Spent Sleeping Past 7 Days	3.94***	6.82***	-0.59	0.775	930
	(1.27)	(1.87)	(2.10)		
Time Spent Working Past 7 Days	1.52	6.51**	-2.86	0.196	930
	(1.08)	(2.97)	(2.10)		
Time Spent on Education Past 7 Days	1.30*	0.51	0.97	0.246	930
	(0.69)	(0.73)	(0.83)		
Time Spent on Unpaid Labor Past 7 Days	-1.27	1.77	-2.41	0.515	930
	(0.89)	(5.58)	(3.69)		
Time Spent on Leisure Past 7 Days	0.04	5.10**	-3.29**	0.041	930
	(0.69)	(2.23)	(1.50)		
Time Spent on Transportation Past 7 Days	0.58	0.35	0.35	0.691	930
	(0.66)	(0.90)	(0.88)		

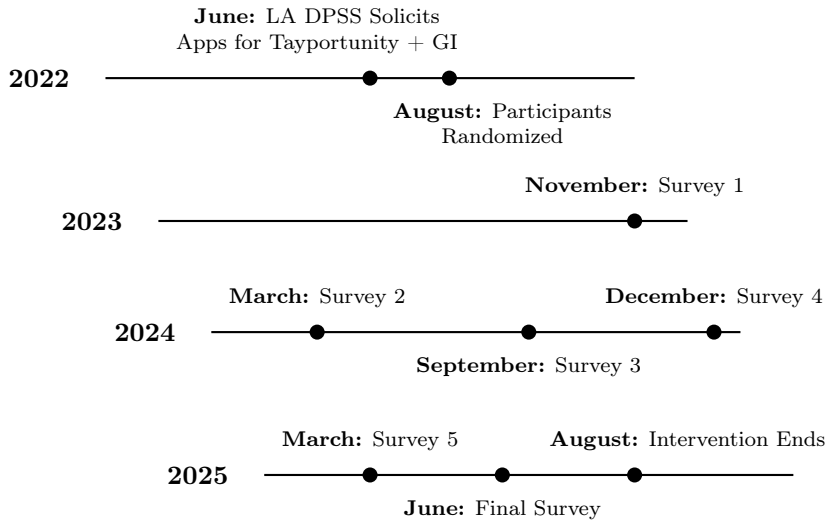
Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. LATE sample comprised of 71 control and 123 treatment individuals. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

B Additional Study Details

B.1 Survey Timeline

The figure below provides an overview of the implementation timeline for the TAYGI program and accompanying research activities. It summarizes key milestones in program rollout, survey fielding, and data collection across the study period.

Figure 5: Project Timeline



B.2 Interaction with Existing Welfare Programs

The $1,000 per month cash transfer is not treated as taxable income for US federal income tax purposes; it is considered a gift. Therefore, the transfer receipt has no bearing on recipients' eligibility for subsidized health care, including Medicaid. The Los Angeles Department of Public Social Services (LA DPSS) managed participants' access to state benefits to ensure participating in the treatment program did not cause them to lose access to other programs (e.g. CalFRESH). However, individuals who became eligible for CalWORKS (California's version of WIC) were not allowed to take up both programs and needed to choose which they preferred to continue in. LA DPSS is also providing treatment group participants access to financial and benefits counseling so that they can ensure their transfers do not interfere with their other benefits unnecessarily.

B.3 Project Funding

The Los Angeles County Department of Public and Social Services (LA DPSS) funded and implemented the TAYGI program. In addition to covering all program costs, LA DPSS provided research funding totaling

approximately $450,000 over five years, which was used primarily to support participant incentives and payments to the survey implementation partner (USC CESR, discussed below). The Jain Family Institute (JFI) served as the fiscal and administrative host for the project, managing the research contract with LA DPSS and handling all associated financial logistics, including disbursement of research funds, subcontracts, and compliance reporting. This structure ensured that the project's financial management and contracting activities are carried out through an established 501(c)(3) research organization with the necessary infrastructure to administer multi-year public-sector grants.

B.4 Additional Details on the Survey Implementation Team

The Center for Economic and Social Research (CESR) at the University of Southern California served as the survey implementation partner for the TAYGI project. CESR was responsible for programming and hosting the six survey waves on their secure web-based survey platform, ensuring compliance with data security and confidentiality requirements. They coordinated participant outreach, including mass text and email reminder campaigns, to maximize response rates across survey waves. CESR also managed participant incentive distribution, providing payments through multiple modalities: CashApp, Venmo, prepaid debit cards, and Amazon gift cards, depending on participant preference and accessibility.

B.5 Qualitative Study of the GI + TAYportunity Program

In addition to the project presented in this paper, a separate research team, led by Professor Ajay Chaudry (Research Scholar, New York University Wagner Graduate School of Public Services), is conducting a qualitative study of the TAYGI program. To be clear, this is not a mixed-methods study; it is two studies operating in parallel evaluating the same program. For the qualitative portion of the study, 30 individuals were randomly selected from the RCT sample to participate in in-depth longitudinal interviews over 36 months in order to gain a deep understanding of how the program impacts the lives, well-being, and futures of the participants. A longitudinal qualitative study with a subset of participants facilitates more open-ended responses and the pursuit of topics as they emerge, rather than following survey scripts. They will also allow deeper understandings of heterogeneity – how different groups experience participation in the program differently (Gibson-Davis and Duncan 2005). The longitudinal study design consists of six waves of in-depth interviews with approximately 30 individuals: 24 individuals receiving guaranteed income, and 6 from the control group, who applied but were not randomly selected to receive payments.

C Dynamic Treatment Effects

This appendix presents the dynamic intent-to-treat (ITT) effects of the intervention across six survey waves conducted between November 2023 and July 2025. While the study is underpowered to detect modest short-term effects at individual survey waves, the evolution of treatment–control differences over time offers valuable insight into the persistence and timing of program impacts.

C.1 Employment Outcomes and Educational Attainment

C.1.1 Employment and Earnings Dynamics

Figures 7–9 plot dynamic treatment effects on employment status, work hours, and monthly labor earnings. Employment gains emerged quickly: by the first follow-up wave, treated individuals were roughly 4-5 percentage points more likely to be employed, and this gap widened slightly and remained stable through the endline. Treatment employment rates fluctuated between 43-49 percent across waves, compared with 37-41 percent in the control group.

Earnings followed a similar upward trajectory, with divergence intensifying over time. The treatment–control gap in monthly work income expanded from $32 at baseline to over $140 by the final wave, indicating both growing labor-market attachment and progression into higher-paying jobs. By contrast, treatment effects on total hours worked were less monotonic. While the treatment group consistently worked more hours than controls, the gap oscillated around 5-6 hours per month without a clear upward trend, suggesting that higher earnings primarily reflected transitions to better-paying employment rather than increases in total hours supplied.

Figure 6: Employment Participation Across Survey Waves

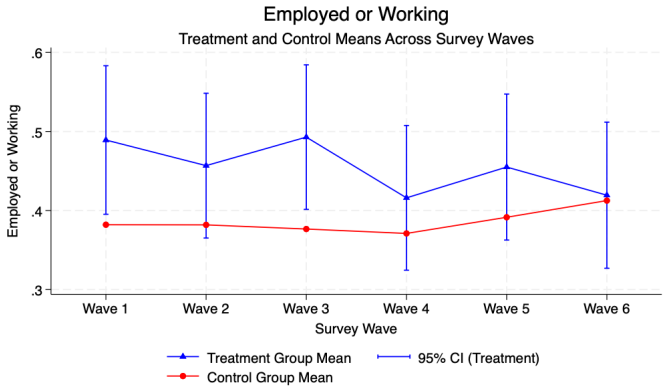


Figure 7: Working Hours Across Survey Waves

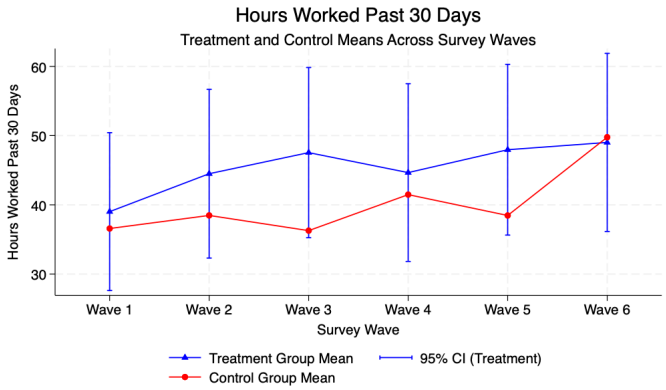
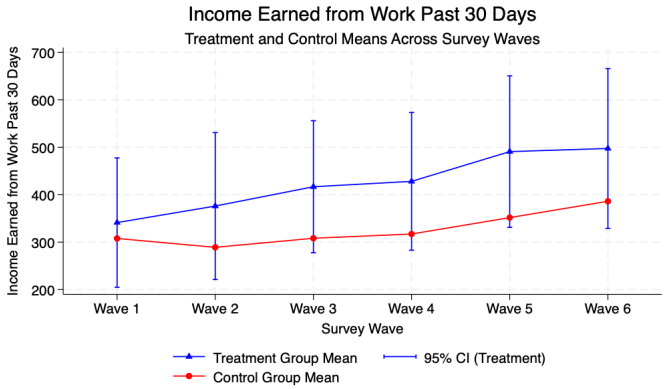


Figure 8: Work Income Across Survey Waves



C.1.2 Job Satisfaction and Search

Figures 5 and 6 show that treatment participants reported persistently higher satisfaction with both employment and working hours across nearly all survey waves. Improvements were evident from the first post-treatment survey and remained stable thereafter. Consistent with higher job satisfaction, Figure 7 shows that treatment individuals were consistently less likely to be engaged in active job search throughout the intervention. This sustained reduction in search activity suggests that the program improved job quality and employment stability rather than merely increasing employment incidence.

Figure 9: Employment Satisfaction Across Survey Waves

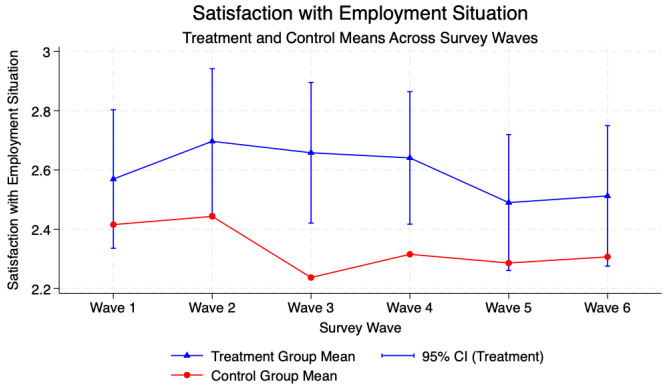


Figure 10: Working Hours Satisfaction Across Survey Waves

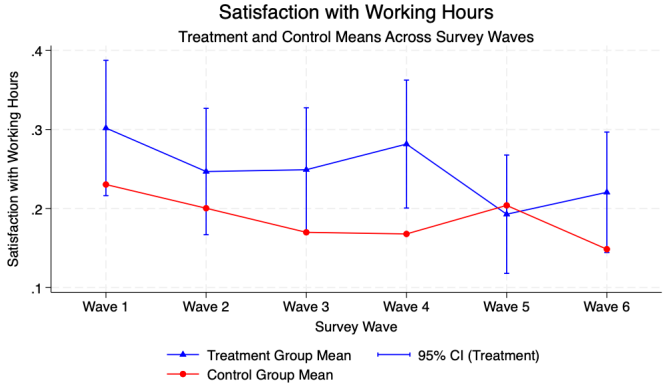
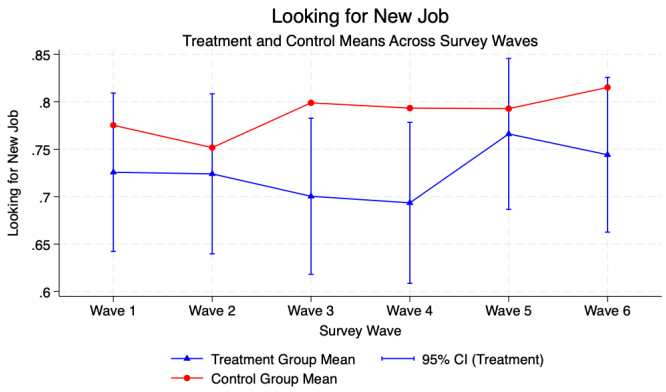


Figure 11: Probability of Looking for New Job Across Survey Waves

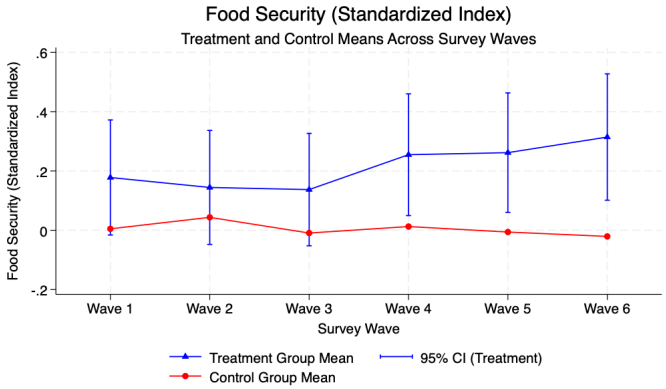


C.2 Security and Well-being

C.2.1 Food, Housing and Financial Security

Figure 8 illustrates dynamic improvements in food security. Gains were modest in the early survey waves but accelerated during the final year of the intervention. The pooled treatment effect rose from 0.14 standard deviations (SD) across waves 1-3 to 0.28 SD across waves 4-6, indicating that households increasingly stabilized food consumption as transfers accumulated and budgeting behavior adjusted over time.

Figure 12: Food Security Across Survey Waves



Figures 9 and 10 plot dynamic treatment effects on housing outcomes. Housing security improved gradually, with divergence appearing in wave 3 and persisting through the endline. The share of treatment participants in stable housing increased from 8 percentage points above control in the first survey to 17 points by wave 6. The index-based measure mirrors this pattern, rising from roughly 0.09 SD in early waves to 0.23 SD in later ones. These patterns suggest that housing stabilization required several months of accumulated financial security and adjustment.

Figure 13: Housing Security Across Survey Waves

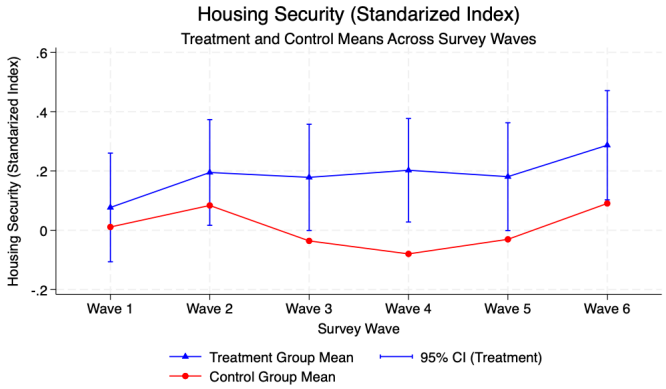
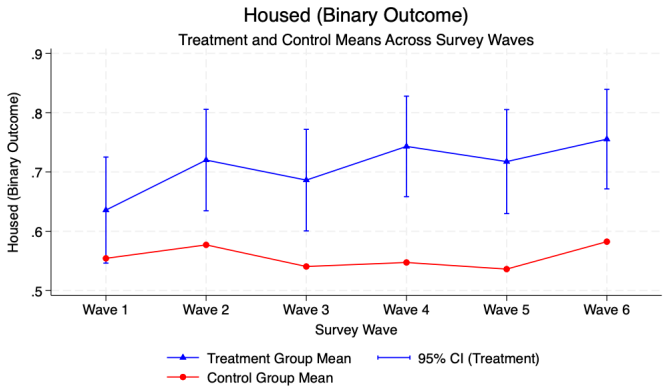


Figure 14: Proportion of Individuals Housed Across Survey Waves



Dynamic results for the financial security index (Figures 11–12) reveal immediate and sustained improvements from the program's inception. Treatment effects appeared by the first follow-up survey and remained large throughout, consistent with the direct liquidity injection from monthly cash transfers. For most component variables (ability to pay bills, manage emergencies, and save) treatment-control gaps remained steady or widened modestly over time, underscoring that households maintained elevated financial resilience rather than experiencing only transient relief.

Figure 15: Financial Security Across Survey Waves

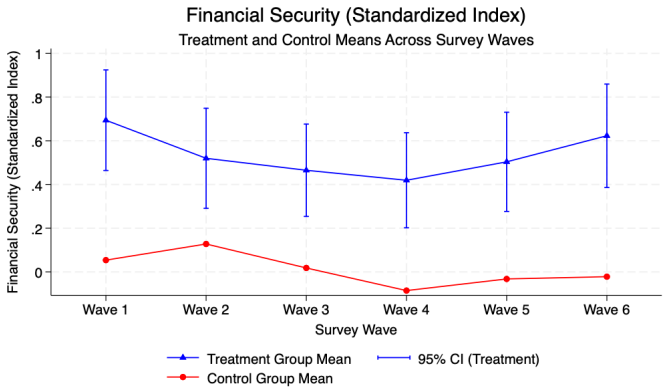
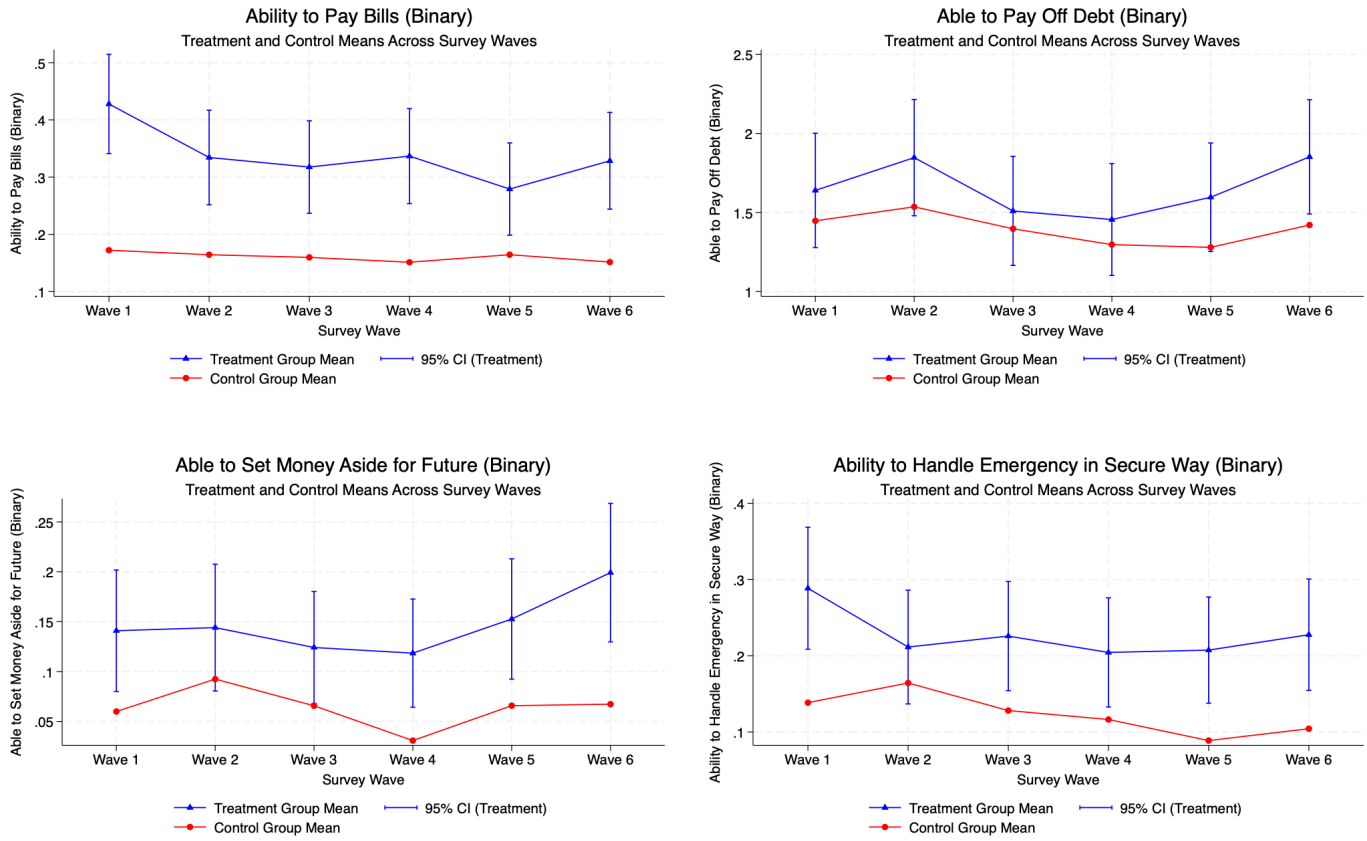


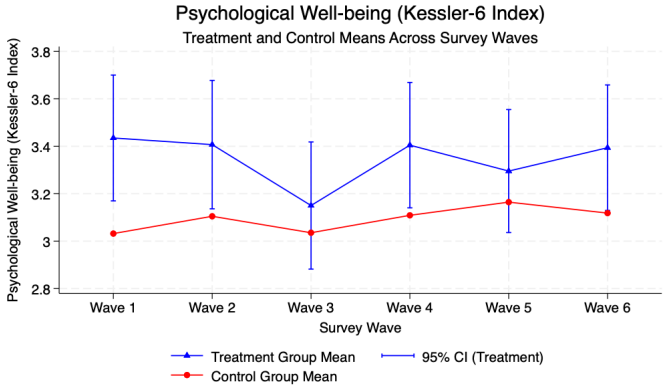
Figure 16: Various Financial Outcomes Across Survey Waves



C.2.2 Psychological Measures

Figure 13 shows the evolution of psychological well-being as measured by the Kessler-6 index. Differences in mental-health scores emerged immediately and persisted with little attenuation through the final survey wave. Treated individuals reported consistently higher well-being (≈ 0.25 SD on average), suggesting that reductions in financial strain and improved stability translated into sustained psychological benefits rather than short-lived improvements.

Figure 17: Financial Security Across Survey Waves



C.3 Income, Expenditure, and Savings

C.3.1 Income Dynamics

Figures 14-16 decompose the evolution of total income into labor earnings and public benefits. Total income among treatment participants remained roughly constant around $1,200 per month throughout the study, while control incomes rose modestly from $550 to $700. This aggregate stability masks important compositional changes: as shown in Figures 15 and 16, treatment participants' earnings increased steadily from $340 to nearly $500 per month, while public-benefit income declined from $785 to $607. This substitution pattern suggests a gradual shift from transfer-based to labor-based income sources as participants strengthened their attachment to the labor market.

Figure 18: Total Income Across Survey Waves

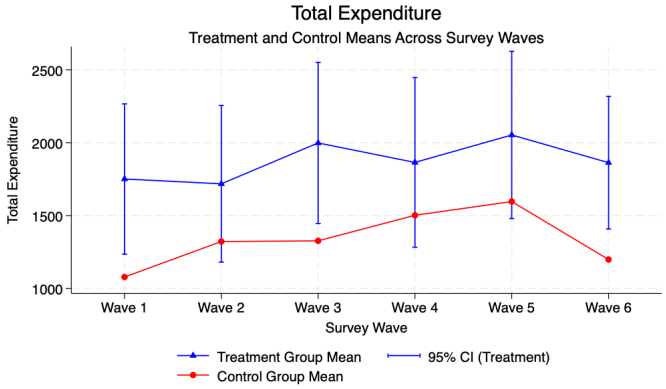


Figure 19: Income from Work Across Survey Waves

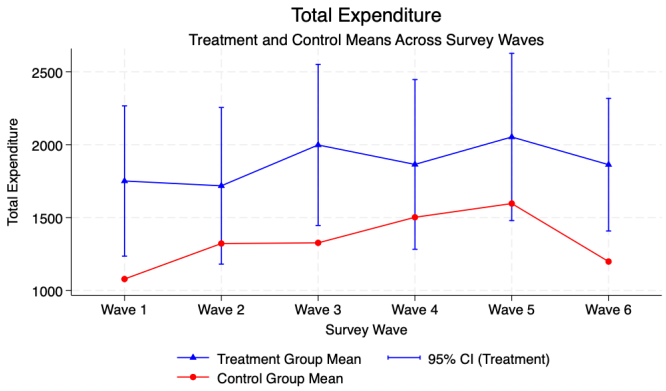
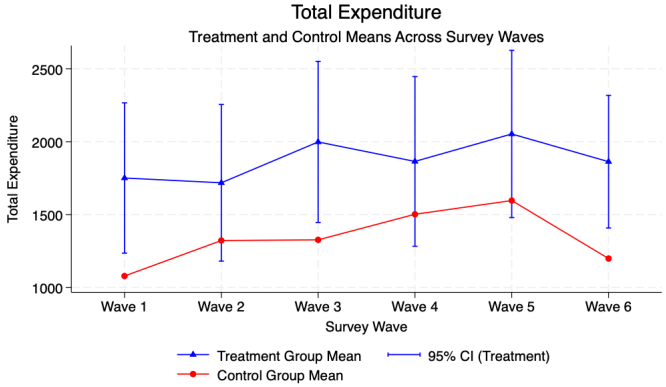


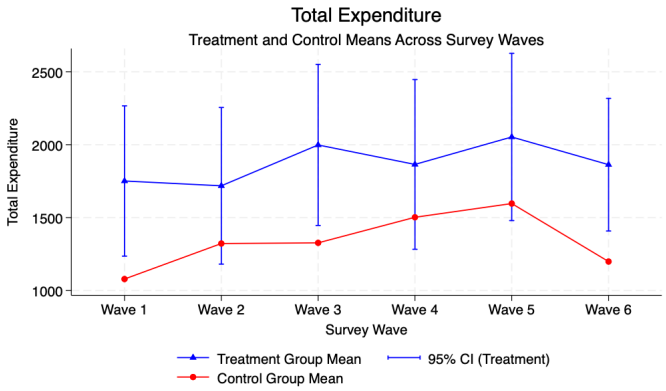
Figure 20: Income from Public Benefits Across Survey Waves



C.3.2 Expenditure

Figure 17 depicts the dynamic evolution of total monthly expenditures. The treatment-control gap opened immediately and remained wide throughout, consistent with permanent income smoothing in response to predictable transfers. While expenditure levels fluctuated across waves, the relative gap was stable, indicating sustained increases in consumption rather than short-term spikes.

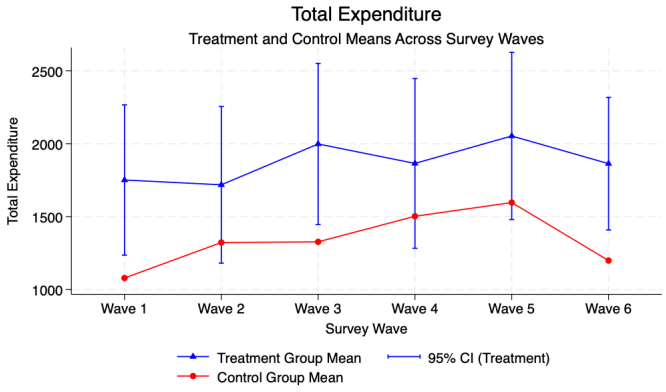
Figure 21: Income from Work Across Survey Waves



C.3.3 Savings Behavior

Savings dynamics, shown in Figure X, followed the opposite trajectory: treated individuals accumulated cash balances rapidly after program onset, reaching approximately $200 by wave 1 and more than $330 by wave 6, compared to roughly $100 for controls. The late-period rise in savings, particularly in waves 5 and 6, suggests forward-looking behavior as participants anticipated the program's conclusion.

Figure 22: Income from Work Across Survey Waves



D Heterogeneity Analysis

As pre-specified, I estimated effects across the following dimensions of heterogeneity: sex, race, and educational attainment[10]. The econometric specification for this exercise is:

$$y_{iSt} = \beta_0 + \beta_1 T_i + \beta_2 T_i \times H_i + \gamma' X_i + \delta y_{iB} + \theta S_t + \varepsilon_{it}$$

[10] Although it is not clearly dictated in the pre-analysis plan, I treat the procession of a GED or highschool diploma as the differentiating factor in my sample for educational attainment. Because of this population's economic status, not enough individuals in my sample attend college to warrant heterogeneity analysis on that dimension.

where H_i is an indicator variable for the dimension of heterogeneity. Again, y_i is an outcome measured at baseline B or at the survey round S; T_i indicates a dummy for being in the treatment group; t takes on values $1 : n$ depending on the survey round; S_t is a dummy variable which takes on a value of 1 for survey round t; and X is a vector of control variables, which includes baseline demographics.

Across demographic subgroups, the intervention produced broadly positive but heterogeneous impacts on employment, security, and financial outcomes. Treatment effects on employment are concentrated among men, Black participants, and those without a high school diploma or GED, each showing statistically significant gains in employment rates and hours worked. In contrast, participants with a high school diploma and women display smaller or insignificant labor market responses, suggesting stronger labor supply responses among individuals with lower baseline attachment to the labor market. Improvements in financial security and psychological well-being are evident across several subgroups, particularly among Black participants and those without a high school diploma, consistent with the program's liquidity and stress-relief channels. Finally, increases in total expenditure and savings are most pronounced among men and Black participants, while effects on time use indicate modest shifts toward greater productive activity, particularly work and education time among groups experiencing the largest employment gains. Taken together, these patterns suggest that the program's impacts are strongest for individuals facing greater structural barriers to employment and financial stability. Understanding the mechanisms driving these heterogeneous effects, whether through differences in baseline constraints, program engagement, or local labor market conditions, remains an important area for future research.

Table 19: ITT Estimates on Pre-Registered Outcomes by Sex

	Men		Women	
	Control mean	Treatment effect	Control mean	Treatment effect
	(1)	(2)	(3)	(4)
Panel A: Employment Outcomes				
Employed or Working	0.35	0.16***	0.41	-0.01
	(0.47)	(0.05)	(0.49)	(0.04)
Hours Worked Past 30 Days	39.09	16.46**	40.98	-5.12
	(63.02)	(6.45)	(64.48)	(5.85)
Income Earned from Work	288.93	250.16***	352.80	-67.30
	(689.86)	(84.38)	(790.14)	(64.93)
Satisfaction with Employment Situation	2.26	0.51***	2.38	0.03
	(1.22)	(0.12)	(1.25)	(0.11)
Panel B: Security and Well-being				
Food Security (Standardized Index)	-0.00	0.20*	0.01	0.18*
	(0.98)	(0.10)	(1.01)	(0.10)
Financial Security (Standardized Index)	0.02	0.64***	0.01	0.37***
	(1.03)	(0.11)	(0.97)	(0.11)
Housing Security (Standardized Index)	0.03	0.22**	-0.01	0.28***
	(1.00)	(0.10)	(0.99)	(0.09)
Psychological Well-being (Kessler-6 Index)	3.17	0.12	3.04	0.35**
	(1.46)	(0.15)	(1.46)	(0.14)
Panel C: Expenditure, Savings and Debt				
Total Expenditure	1034.32	898.07***	1547.55	274.35
	(1857.44)	(227.15)	(3148.65)	(249.86)
Total Cash Savings	114.11	195.25***	80.75	116.74**
	(493.17)	(68.73)	(379.94)	(59.29)
Total Debt	4674.15	848.22	6304.79	-782.89
	(11924.95)	(1228.94)	(14528.35)	(1279.07)
Panel D: Time Use				
Time Sleeping Past 7 Days	34.78	2.06	33.39	5.97***
	(20.82)	(1.82)	(19.86)	(1.76)
Time Working Past 7 Days	9.77	4.11**	10.25	-1.28
	(15.76)	(1.61)	(16.12)	(1.46)
Time Spent on Education Past 7 Days	3.42	1.15	4.77	1.37
	(8.75)	(0.84)	(10.64)	(1.08)
Time Spent on Unpaid Labor Past 7 Days	6.61	-1.74	7.47	-0.53
	(15.09)	(1.13)	(15.58)	(1.42)
Time Spent on Leisure Past 7 Days	5.55	-0.65	3.63	0.52
	(11.99)	(1.05)	(9.55)	(0.86)
Time Spent on Transportation Past 7 Days	6.52	0.97	7.31	0.42
	(10.96)	(0.97)	(11.70)	(0.93)

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,543 (1,062 control and 481 treatment) are female, and 1,157 (708 control and 449 treatment) are not. There are 567 unique individuals in the dataset; 318 who are female (281 control and 102 treatment), and 249 who are not (157 control and 92 treatment). Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: ITT Estimates on Pre-Registered Outcomes by Race

	Non-Black		Black	
	Control mean	Treatment effect	Control mean	Treatment effect
	(1)	(2)	(3)	(4)
Panel A: Employment Outcomes				
Employed or Working	0.42	0.04	0.36	0.10**
	(0.49)	(0.05)	(0.48)	(0.04)
Hours Worked Past 30 Days	49.72	-0.40	33.44	9.96*
	(69.79)	(6.74)	(58.41)	(5.61)
Income Earned from Work	467.14	-9.45	227.22	182.01***
	(934.23)	(84.66)	(567.96)	(61.44)
Satisfaction with Employment Situation	2.38	0.19	2.30	0.31***
	(1.27)	(0.13)	(1.21)	(0.10)
Panel B: Security and Well-being				
Food Security (Standardized Index)	-0.06	0.28**	0.05	0.13
	(0.93)	(0.11)	(1.04)	(0.10)
Financial Security (Standardized Index)	-0.00	0.56***	0.02	0.45***
	(0.98)	(0.11)	(1.00)	(0.10)
Housing Security (Standardized Index)	0.17	0.18*	-0.11	0.30***
	(1.03)	(0.11)	(0.95)	(0.08)
Psychological Well-being (Kessler-6 Index)	3.04	0.33**	3.13	0.18
	(1.44)	(0.15)	(1.47)	(0.14)
Panel C: Expenditure, Savings and Debt				
Total Expenditure	1705.22	24.09	1082.69	1013.68***
	(3103.35)	(230.42)	(2372.90)	(240.69)
Total Cash Savings	161.98	100.89	45.55	189.38***
	(582.70)	(68.27)	(259.66)	(53.38)
Total Debt	6630.02	-376.02	4953.52	697.48
	(13456.49)	(1237.92)	(13609.25)	(1181.26)
Panel D: Time Use				
Time Sleeping Past 7 Days	37.24	3.50*	31.59	4.15**
	(19.27)	(1.85)	(20.63)	(1.73)
Time Working Past 7 Days	12.43	-0.10	8.36	2.49*
	(17.45)	(1.68)	(14.60)	(1.40)
Time Spent on Education Past 7 Days	4.40	1.70	4.11	0.92
	(10.41)	(1.08)	(9.60)	(0.90)
Time Spent on Unpaid Labor Past 7 Days	8.05	-1.74	6.46	-0.91
	(15.79)	(1.50)	(15.06)	(1.10)
Time Spent on Leisure Past 7 Days	5.23	-0.96	3.80	0.98
	(11.24)	(0.98)	(10.14)	(0.92)
Time Spent on Transportation Past 7 Days	7.58	0.95	6.57	0.30
	(11.73)	(1.01)	(11.17)	(0.85)

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,533 (1,032 control and 501 treatment) are black, and 1,167 (738 control and 429 treatment) are non-black. There are 567 unique individuals in the dataset; 324 who are black (220 control and 104 treatment), and 243 who are not (153 control and 90 treatment). Data has been winsorized at 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: ITT Estimates on Pre-Registered Outcomes by High School Diploma/GED Attainment

	No HS/GED		HS/GED	
	Control mean	Treatment effect	Control mean	Treatment effect
	(1)	(2)	(3)	(4)
Panel A: Employment Outcomes				
Employed or Working	0.30	0.11*	0.42	0.07*
	(0.46)	(0.06)	(0.49)	(0.04)
Hours Worked Past 30 Days	35.46	-2.97	41.89	7.85
	(59.81)	(7.00)	(65.20)	(5.08)
Income Earned from Work	268.56	67.76	347.74	120.02**
	(698.33)	(93.05)	(769.16)	(61.21)
Satisfaction with Employment Situation	2.24	0.21	2.36	0.27***
	(1.32)	(0.15)	(1.21)	(0.09)
Panel B: Security and Well-being				
Food Security (Standardized Index)	0.14	0.10	-0.04	0.23***
	(1.11)	(0.14)	(0.95)	(0.08)
Financial Security (Standardized Index)	0.05	0.56***	-0.00	0.46***
	(0.98)	(0.14)	(1.00)	(0.09)
Housing Security (Standardized Index)	0.08	0.25**	-0.02	0.24***
	(1.04)	(0.12)	(0.98)	(0.07)
Psychological Well-being (Kessler-6 Index)	2.95	0.42**	3.14	0.16
	(1.50)	(0.18)	(1.44)	(0.11)
Panel C: Expenditure, Savings and Debt				
Total Expenditure	1127.24	462.22	1417.32	573.03***
	(2010.71)	(306.30)	(2922.03)	(198.92)
Total Cash Savings	83.53	34.80	97.78	194.34***
	(403.01)	(68.73)	(437.82)	(54.01)
Total Debt	2858.40	1586.69	6627.93	110.94
	(9129.05)	(1203.59)	(14684.75)	(1052.22)
Panel D: Time Use				
Time Sleeping Past 7 Days	34.64	6.38***	33.70	2.81*
	(20.89)	(1.97)	(20.04)	(1.46)
Time Working Past 7 Days	8.86	-0.74	10.47	1.96
	(14.95)	(1.75)	(16.30)	(1.27)
Time Spent on Education Past 7 Days	2.88	3.45***	4.70	0.83
	(8.15)	(1.26)	(10.46)	(0.80)
Time Spent on Unpaid Labor Past 7 Days	7.76	-2.05	6.90	-0.86
	(16.83)	(1.48)	(14.85)	(1.06)
Time Spent on Leisure Past 7 Days	3.13	1.93*	4.84	-0.49
	(9.18)	(1.06)	(11.06)	(0.81)
Time Spent on Transportation Past 7 Days	6.42	2.33*	7.20	-0.12
	(11.59)	(1.35)	(11.35)	(0.74)

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,981 (1,312 control and 669 treatment) with a GED or high school diploma, and 719 (458 control and 261 treatment) without. There are 567 unique individuals in the dataset; 420 with a GED/high school diploma (281 control and 139 treatment), and 147 without (92 control and 55 treatment). Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Robustness of Results

E.1 Sample Enrollment and Differential Take-Up

At baseline, 1,216 individuals were randomly assigned into treatment and control groups, with 370 assigned to treatment and 846 assigned to control. Of these, 300 treatment participants and 566 control participants ultimately enrolled in the survey study. This differential enrollment rate arose not from systematic selection on individual characteristics, but from logistical challenges inherent to working with a highly mobile and economically precarious population. Substantial time elapsed between initial randomization and survey enrollment, during which many individuals changed phone numbers or email addresses, making re-contact difficult. This problem was particularly acute for the control group, who did not maintain ongoing communication with the Los Angeles County Department of Public and Social Services (DPSS) after randomization, unlike treatment individuals who remained in contact through program implementation.

While this imbalance in enrollment rates warrants attention, it is unlikely to threaten the internal validity of the experimental estimates. The observed differential participation reflects differential reachability rather than selective attrition on unobservables correlated with treatment effects. Moreover, to the extent that selection occurred, it likely biases estimated treatment effects toward zero: control individuals who were successfully located and enrolled may represent a more stable and connected subset of the original control group, thereby reducing the contrast between treatment and control outcomes. As such, the estimated intent-to-treat effects reported in the main text are best interpreted as conservative lower-bound estimates of the program's true impacts.

E.2 FWER-Corrected ITT Estimates

To address concerns about multiple hypothesis testing within each family of outcomes, we implement Family-Wise Error Rate (FWER) corrections using the Westfall-Young step-down procedure. This method controls the probability of making at least one Type I error across multiple tests within each outcome group. I apply the correction separately to each conceptual family of outcomes (e.g., main outcomes, income sources, expenditure categories, debt types, employment/education, security measures, and time use), preserving the meaningful groupings in the analysis. I follow techniques similar to those used in Haushofer and Shapiro (2016).

Table 22: FWER-Corrected Employment and Education Outcomes

	(1) Control mean (std. dev)	(2) Treatment effect	(3) N
Panel A: Employment Outcomes			
Employed or Working	0.39	0.072**	2700
	(0.48)	(0.032)	
Hours Worked Past 30 Days	40.23	5.268	2700
	(63.89)	(4.322)	
Income Earned from Work	327.25	93.809*	2700
	(752.08)	(52.019)	
Satisfaction with Employment Situation	2.33	0.255***	2700
	(1.24)	(0.079)	
Satisfaction with Working Hours	0.19	0.062***	2700
	(0.38)	(0.024)	
Looking for New Job	0.79	-0.062**	2697
	(0.41)	(0.025)	
Panel B: Education Outcomes			
Level of Education Completed	0.92	0.006	2700
	(0.65)	(0.044)	
Currently Enrolled in Educational Program	0.21	0.069**	2699
	(0.40)	(0.027)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 23: FWER-Corrected Security and Well-being Outcomes

	(1) Control mean (std. dev)	(2) Treatment effect	(3) N
Panel A: Food Security			
Food Security (Standardized Index)	0.00	0.202***	2698
	(1.00)	(0.072)	
Not Able to Eat Enough in Past 30 Days (Binary)	0.77	-0.101***	2700
	(0.42)	(0.029)	
Consumed Low-Quality Food Due to Cost Past 30 Days (Binary)	0.81	-0.046*	2698
	(0.39)	(0.027)	
Panel B: Housing Security			
Housing Security (Standardized Index)	0.00	0.260***	2700
	(0.99)	(0.066)	
Housed (Binary Outcome)	0.56	0.152***	2700
	(0.49)	(0.033)	
Likelihood of Eviction	2.20	-0.075*	2700
	(0.69)	(0.044)	
Number of Times Moved Past 90 Days	3.28	-0.773	2700
	(10.01)	(0.634)	
Panel C: Financial Security			
Financial Security (Standardized Index)	0.01	0.508***	2698
	(1.00)	(0.076)	
Ability to Pay Bills (Binary)	0.16	0.177***	2700
	(0.36)	(0.027)	
Able to Set Money Aside for Future (Binary)	0.06	0.081***	2700
	(0.24)	(0.018)	
Able to Pay Off Debt (Binary)	0.39	0.088***	2698
	(0.49)	(0.030)	
Ability to Handle Emergency in Secure Way (Binary)	0.12	0.104***	2700
	(0.33)	(0.023)	
Panel D: Psychological Well-being			
Psychological Well-being (Kessler-6 Index)	3.09	0.260**	2696
	(1.46)	(0.102)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 24: FWER-Corrected Income and Expenditure

	(1) Control mean (std. dev)	(2) Treatment effect	(3) N
Panel A: Income			
Total Income	611.06	581.181***	2700
	(1240.57)	(76.252)	
Income Earned from Work	327.25	97.029*	2700
	(752.08)	(52.715)	
Income from Public Benefits	187.25	502.739***	2700
	(251.70)	(30.879)	
Support form Family and Friends	21.58	-4.331	2700
	(76.72)	(4.056)	
Any Other Income	10.74	8.168**	2700
	(60.62)	(4.097)	
Panel B: Expenditure			
Total Expenditure	1342.26	563.094***	2700
	(2718.13)	(172.399)	
Rent	287.57	198.796***	2700
	(520.84)	(41.178)	
Utilities	45.56	22.258***	2700
	(106.36)	(7.257)	
Bills	62.81	37.748***	2700
	(91.94)	(6.418)	
Groceries	184.98	32.281***	2700
	(154.84)	(9.682)	
Food Outside	56.97	25.372***	2700
	(88.74)	(5.600)	
Subscriptions	9.39	5.630***	2700
	(22.93)	(1.608)	
Shopping	32.79	14.588***	2700
	(73.74)	(4.868)	
Leisure	11.42	7.414**	2700
	(40.00)	(3.085)	
Education	14.48	5.358	2700
	(71.07)	(4.612)	
Transportation	51.74	29.590***	2700
	(101.83)	(7.208)	
Payments to Family and Friends	13.28	11.485***	2700
	(50.45)	(3.676)	
Debt Payments	486.62	118.487	2700
	(2288.11)	(132.925)	
Other Expenses	36.69	28.780***	2700
	(138.26)	(7.451)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 25: FWER-Corrected Savings and Debt Outcomes

	(1) Control mean (std. dev)	(2) Treatment effect	(3) N
Panel A: Savings			
Total Cash Savings	94.10	149.060***	2700
	(429.01)	(42.584)	
Panel B: Debt			
Total Debt	5652.53	132.520	2700
	(13567.17)	(871.977)	
Student Debt	2644.41	-668.746	2700
	(8404.55)	(505.604)	
Vehicle Debt	1113.91	877.116**	2700
	(4479.47)	(364.513)	
Medical Debt	158.11	104.988	2700
	(987.50)	(85.065)	
Legal Debt	96.97	-33.564	2700
	(626.76)	(32.148)	
Credit Card Debt	608.77	-51.662	2700
	(1671.78)	(108.454)	
Debt Owed to Friends and Family	175.90	-19.061	2700
	(776.97)	(52.431)	
Any Other Debt	236.90	-73.518	2700
	(1214.93)	(76.942)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26: FWER-Corrected Employment and Education Outcomes

	(1) Control mean (std. dev)	(2) Treatment effect	(3) N
Time Sleeping Past 7 Days	33.95	3.942***	2700
	(20.26)	(1.270)	
Time Working Past 7 Days	10.06	1.317	2700
	(15.97)	(1.081)	
Time Spent on Education Past 7 Days	4.23	1.302*	2700
	(9.94)	(0.690)	
Time Spent on Unpaid Labor Past 7 Days	7.12	-1.271	2700
	(15.39)	(0.888)	
Time Spent on Leisure Past 7 Days	4.40	0.042	2700
	(10.63)	(0.689)	
Time Spent on Transportation Past 7 Days	6.99	0.575	2700
	(11.42)	(0.661)	

Note: Sample comprised of 2,700 total observations across 6 waves of data collection: 1,770 control and 930 treatment. There are 568 unique individuals in the dataset; 373 control individuals and 195 treatment. Data has been winsorized at the 1 and 99 percent levels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$