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IZA DP No. 19088

November 2026

How Temporary Cash Transfers Affect Household Finances

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How Temporary Cash Transfers Affect Household Finances*

Abstract

We evaluate a guaranteed basic income experiment providing twelve monthly $500 transfers to very low-income US households. During the final month of payments, a survey index of financial distress declined by 0.18 SD, and an overall well-being index increased by 0.15 SD. By contrast, collections balances from credit reports show no change during transfers and likely increase for some recipients in the year after transfers end. These results appear to be driven by changes in financial decision-making including shifts away from informal borrowing and toward future returns.

JEL classification

I38, G51, H75

Keywords

guaranteed basic income, unconditional cash transfers, household finance, informal borrowing, financial distress, credit outcomes, scarcity

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* This research was financially supported by the Smith Richardson Foundation through a grant provided to the Wilson Sheehan Lab for Economic Opportunities (LEO) at the University of Notre Dame. We are deeply grateful to our implementing partners at the City of Rochester and the Rochester Black Community Focus Fund (BCFF) for their support of the evaluation and their valuable insight into the program implementation, and to Max Olander for research assistance. Any data provided herein does not constitute an official record of the New York State Unified Court System, which does not represent or warrant the accuracy thereof. The opinions, findings, and conclusions expressed in this publication are those of the authors and not those of the New York State Unified Court System, which assumes no liability for its contents or use thereof, or the City of Rochester. The evaluation was pre-registered with the American Economic Association RCT Registry (AEARCTR-0014150) and we follow the registered analysis plan below. Registration occurred on August 8, 2024 after random selection of participants but prior to receiving outcome data.

Low-income households often struggle to make ends meet, facing income volatility, minimal liquid savings, and frequent financial shocks. Many cope through informal financial arrangements—borrowing from family or friends, delaying bills, or relying on short-term workarounds that substitute for liquidity. Scarcity can shape well-being and financial decision-making by tightening short-run constraints and limiting attention to longer-run tradeoffs (Mullainathan and Shafir, 2013). In attempts to alleviate these pressures, dozens of experimental guaranteed basic income (GBI) and unconditional cash transfer pilots have launched in the U.S. in recent years with mixed conclusions. Some studies document increases in consumption and short-run improvements in financial well-being (Bartik et al., 2024; Liebman et al., 2022; West and Castro, 2023), but similar studies find limited effects on subjective distress, formal credit outcomes, or longer-run stability (Balakrishnan et al., 2024; Miller et al., 2024; Jacob et al., 2022). A central open question is how temporary income transfers affect households who begin with extremely limited liquid resources, juggle constantly changing financial obligations, and must rely heavily on informal coping mechanisms.

In this paper, we report results from a randomized controlled trial (RCT) of a city-run GBI pilot in Rochester, New York. Adults living in HUD-designated qualifying census tracts with household income at or below 185 percent of the federal poverty line applied to receive $500 per month for 12 months. We estimate experimental effects on financial distress and anxiety, overall well-being, spending, and employment using a follow-up survey fielded during the final month of payments, as well as formal credit and housing stability outcomes using quarterly administrative credit bureau records and administrative housing court records that span both the program year and the year after payments concluded.

Two features distinguish our setting from much of the recent U.S. evidence. First, Rochester applicants are exceptionally economically precarious—a population with very low incomes and limited attachment to formal financial products—closer to the intended targets of many ongoing municipal guaranteed income pilots than samples that, while low-income, retain more liquid assets (Bartik et al., 2024; Vivalt et al., 2024). Second, our study takes place in 2023–2024, after the acute phase of the COVID-19 pandemic, whereas several influential cash RCTs were implemented during 2020–2023 amid unusual policy supports and labor-market and price dynamics that could affect both baseline coping strategies and the marginal value of liquidity (Balakrishnan et al., 2024; Gennetian et al., 2024a).

The program improved measures of well-being during the year of cash transfers. Our primary pre-registered survey outcome is an index of financial distress and economic anxiety capturing prior economic anxiety, realized hardship, and anticipated hardship, along with a revealed-preference measure of short-run liquidity constraints. Receipt of GBI reduces this index by 0.18 standard deviations (s.d.). These improvements coincide with a 0.15 s.d. increase in an index of health and overall well-being.

On the other hand, more formal measures of household finances move less during the year of cash transfers. Our primary pre-specified credit report outcomes—balances in derogatory accounts and balances in collections—show no change. Households' reports about their balance sheets also show modest changes beyond the transfer itself. Reported income increases by $1.15 for every dollar transferred, which occurs alongside an increase in part-time employment. Consumption increases by only $0.32 per dollar of transfer, though this estimate is imprecise. Formal savings increase only $0.04 per transfer dollar.

The contrast between increased well-being and modest changes in traditional balance sheet measures can be explained by households responding to cash transfers on informal margins. For example, the control

group reports borrowing from family and friends 4.6 times more often than credit cards, and treatment cuts this type of informal borrowing in half. Similarly, treatment roughly halves the share of respondents who forgo a 100% financial return to take a smaller, immediate incentive from the research survey rather than waiting a few weeks for a larger payment. For a sample with very low income, cash transfers affect financial decision-making, and subsequently well-being, in informal ways.

Large treatment effects in survey data do not appear to be generated by survey response bias. Because follow-up survey response was modest and differential (53% in treatment vs. 30% in control), survey-based estimates could in principle be affected by selective attrition. However, respondents remain broadly balanced on baseline characteristics, survey results are robust to various re-weighting and bounding exercises, and results from administrative data change little if we limit analysis to the survey sample.

Finally, after cash transfers end, we find some evidence that finances deteriorate for a small number of households. Administrative credit records suggest that treatment increases average balances in collections during the year after payments were received, driven by the top decile of collections balances. Members of the treatment group also appear more frequently in court eviction filings in that year. Why these households experience negative outcomes is difficult to detect precisely because of the infrequency of tail outcomes; however, we observe suggestive increases in large financial obligations, such as student loans and mortgages, that could drive financial distress when cash transfers end.

Our results contribute to the emerging evidence on unconditional cash in three ways. First, we provide experimental evidence from a municipal GBI program implemented in a particularly economically disadvantaged, distinctly post-pandemic setting. In this context, we document a distinctive pattern of impacts during the cash transfer period: larger improvements in distress and well-being, comparatively modest changes in measured consumption, and no evidence of reduced employment. This pattern differs from several recent U.S. unconditional cash pilots (Bartik et al., 2024; Vivalt et al., 2024; Balakrishnan et al., 2024; Noble et al., 2025; Sauval et al., 2024).

Second, we provide evidence consistent with a mechanism centered on short-run liquidity constraints and informal financial adjustments. Welfare improvements coincide with changes in day-to-day financial decision-making rather than with changes in formal credit and savings margins. Using a real-stakes intertemporal choice in the follow-up survey, we show that the program relaxes short-run constraints, and we document a large reduction in informal borrowing from friends and family, suggesting that a key channel for cash among recipients with very low income is reduced reliance on informal borrowing.

Third, our results suggest some negative ways that repeated but temporary cash transfers can interact with financial decision-making in the face of scarcity. We find some evidence that a minority of households are worse off financially after the transfer ends, possibly because the temporary transfer induces large, irreversible financial decisions. Most feasible cash transfer programs have been temporary, and our results provide some caution in extrapolating from them to permanent basic income.

1 Design of the Experiment

1.1 Program Application and Eligibility

The City of Rochester collected online applications for their GBI pilot in June 2023 targeting adults in HUD-designated qualifying census tracts with income at or below 185% of the federal poverty line.[1] Applicants provided basic demographic information, a household roster, and self-attestation for program eligibility. The application also collected baseline measures of key outcomes such as the applicant's housing situation, monthly spending, financial stability, enrollment in public benefit programs, and a measure of mental well-being (Cohen et al., 1983). The City received 11,698 eligible applications from individuals living in 11,590 unique households. Only one individual in a household could be selected for the program, and the research team randomly selected one applicant from each household for potential program participation and subsequent analysis.

1.2 Random Assignment and Treatment

Program enrollment occurred across two cohorts during which 351 participants joined the program.[2] Participants were selected from a randomly ordered list until program slots were filled. During each cohort, the program administrator, the Black Community Focus Fund (BCFF), attempted to contact and enroll individuals on the list. In total, BCFF contacted 203 and 207 individuals to enroll the 175 and 176 program participants in cohorts 1 and 2, respectively. For our analysis, the treatment group ($N = 410$) includes individuals who BCFF attempted to contact. The remaining applicants ($N = 11,180$) who were not contacted and did not receive payments comprise the control group.

Program participants received $500 monthly for 12 months ($6,000 total). Recipients needed to have an active checking account, and the program actively helped participants open an account. All applicants could express interest in the City's financial counseling program. Those who expressed interest, both individuals in the treatment and control groups, were contacted by the City's Financial Empowerment Center with additional information about their free services.[3] Aside from these minor complementary services, treatment consisted of a simple $6,000 cash transfer.

1.3 Who Applies for a Basic Income Pilot?

The typical applicant to a GBI pilot differs from a typical low-income individual. The first three columns of Table 1 show average demographic characteristics and treatment-control differences for participants in the Rochester GBI Pilot. The next four columns report average characteristics from the Compton Pledge (Balakrishnan et al., 2024), OpenResearch Unconditional Cash (Bartik et al., 2024; Miller et al., 2024; Vivalt et al., 2024), Chelsea Eats (Liebman et al., 2022), and Baby's First Years (Noble et al., 2021;

[1] Per HUD guidelines, "Qualified Census Tracts must have 50% of households with incomes below 60% of the Area Median Gross Income (AMGI) or have a poverty rate of 25% or more." Program eligibility also required residency in Rochester for at least one year.

[2] See Appendix Table A-1 for program timeline.

[3] The center invited interested applicants to attend workshops at the local public library. While individual-level records are not available, the City estimates that 40–50 of the more than 11,000 applicants ended up attending a workshop. The workshops were topic-specific and covered savings, building credit, or decreasing debt.

Gennetian et al., 2024a,b; Noble et al., 2025) studies, respectively. Column (8) shows a representative sample of low-income adults from metropolitan areas in the 2023 American Community Survey (Ruggles et al., 2025). Compared to the 52% rate in the general population, pilot participants are much more likely to report being female: 81% in Chelsea Eats, 74% in Compton, 68% in Open Research, and nearly 100% in Baby's First Years. Cash pilot participants also tend to be 10–20 years younger and much more likely to be Black or Hispanic.

The average applicant in the Rochester GBI pilot lives a particularly economically precarious life. Columns (1) and (2) of Table 1 report control and treatment means for our study sample. Reported household incomes are very low—just over $17,000 annually, much lower than existing studies (other than Chelsea Eats). The study sample is even more heavily weighted toward Black and Hispanic participants than other cash transfer pilots. Appendix Table A-2 provides additional detail on a broader set of characteristics measured in the program application. Nearly 90% report being sometimes or often unable to afford food, 10% have unstable housing, and only 40% paid their full rent on time. Overall, we study cash transfers among a particularly economically precarious population.

Baseline characteristics also allow us to confirm the success of random assignment. Column (3) of Table 1 reports the difference in means between the two groups adjusting for the number of program applicants from the household. Also seeAppendix Table A-2 for balance tests along additional characteristics. Only a few characteristics are imbalanced at baseline. The treatment group is slightly more likely to be never married (79.5% vs. 75.0%), spends less on housing ($666 vs. $775), and spends less on healthcare ($23 vs. $31). Such differences, though statistically significant, are consistent with random assignment given their small magnitudes and the large number of tests.

1.4 Measuring Outcomes

We use three data sources to track the outcomes of applicants: follow-up surveys collected among a subset of applicants, quarterly credit records from a credit bureau, and matched eviction court records.

1.4.1 Survey

A follow-up survey took place 12 months after random assignment, just before the end of cash transfers. The survey team reached out via web survey to everyone in the treatment group and a random sample of the control group. Respondents chose whether to receive a $25 gift card within a week of completing the survey or a $50 gift at the end of a roughly 4-week waiting period. The treatment group was more likely to respond (53%; $N = 218$) than the control group (30%; $N = 300$). Attrition is not random (Appendix Table A-3), but balance changes little when limiting to the set of survey respondents (Appendix Table A-4). Our main results use pre-specified weights to adjust for attrition (see Appendix Section A.1.2). Section 2.5 discusses alternative adjustments for attrition bias in detail.

Our pre-specified primary survey outcome is an index of financial distress. The outcomes in this index are: distress experienced in the prior 12 months (being somewhat or often unable to afford food; being charged a late fee by a landlord; having utilities disconnected; being evicted or foreclosed upon), worries about distress during the previous 12 months (worries about eviction/foreclosure; worries about utilities disconnection), anticipated worries over the coming three months (expectations of material hardship; wor-

ries about future eviction/foreclosure; worries about future utilities disconnection), and an indicator for whether the respondent selected the smaller, more immediate survey incentive. The survey also covered many areas of participants' lives.[4]

1.4.2 Administrative Records

We use quarterly snapshots of credit reports provided by Experian from September 2022 through March 2026. Experian linked applicants using name, address, and date of birth. The overall match rate to credit data is high (68.3%) and is similar between the treatment group (70.5%) and control group (68.3%). See Appendix Section A.2.1 for more details on matching, weights, and baseline balance. We build a balanced panel in quarters relative to initial payment (see Appendix Table A-1) for the treatment group. For the control group, initial payment quarter is not defined so we include each control person twice, assigning a record to each cohort, and cluster on person. Our two primary pre-specified credit outcomes (total balance in collections; total balance in derogatory accounts) measure the amount of past-due debt an individual has on their credit report.

We supplement these credit records with matched eviction court records provided by the New York State Office of Court Administration (OCA). OCA provided the research team with eviction court records from 2021 through 2025, including information on the address of the filing and its disposition. The research team provided OCA with the roster of GBI study participants that OCA used to match to records using the soundex of the study participants first and last name. We identify eviction cases for study participants by matching on either soundex-matched names or addresses. Our primary outcome considers whether an individual had an eviction filing since GBI application.

1.5 Empirical Specification

This study is an RCT. Thus, we estimate program effects using the following "intent-to-treat" (ITT) model:

$$y_{it} = \alpha_0 + \beta_0 T_i + X_i \Gamma_0 + \mu_{0h(i)} + \epsilon_{0i} \tag{1}$$

where y_{it} is an outcome measure for person i observed in period t. For survey outcomes, we observe only one observation per person, but for administrative records we observe multiple time periods t per person and include two copies of each observation for the control group, as discussed above.[5] X_i represents a vector of observed characteristics for person i measured prior to random assignment. We pre-specified that the variables in X_i would include demographic information (age, age-squared, indicators for education level at baseline, gender, indicator for non-hispanic black, indicator for non-hispanic white, indicator for hispanic, indicators for marital status) and pre-randomization measures of the outcome variable, when available.[6] $\mu_{h(i)}$ is a vector of fixed effects for the number of program applicants within a household group,

[4] The survey instrument can be accessed here.

[5] Appendix Section A.2.2 describes how we construct weights for control group observations to maintain the relative importance of each cohort in the stacked dataset.

[6] Pre-randomization measures of the outcome are not available for late fees, utility disconnection, eviction, worry of eviction, worry of utility disconnection, worry of financial hardship, income categories, life ladder, physical health, relative physical health, and the financial well-being domain. For administrative credit data, we include lags for eight quarters before GBI payment.

which affects treatment probability. The key covariate in the analysis is the indicator for treatment T_i, which equals one if the program attempted to contact person i for potential program enrollment, and zero otherwise. We cluster standard errors by person. The coefficient estimate $\hat{\beta}_0$ provides the causal effect of being offered a spot in the GBI Pilot Program.

We also report "treatment-on-the-treated" (TOT) estimates that come from a two-stage model that instruments for the receipt of GBI payments (R_i) with the randomly assigned treatment status (T_i).

$$R_{it} = \alpha_1 + \beta_1 T_i + X_i \Gamma_1 + \mu_{1h(i)} + \epsilon_{1i} \tag{2}$$

$$y_{it} = \alpha_2 + \beta_2 R_{it} + X_i \Gamma_2 + \mu_{2h(i)} + \epsilon_{2i} \tag{3}$$

Because take-up for the program was high among survey respondents (95%), the ITT and TOT estimates are close in magnitude.

Many treatment effects we report are indices of several outcomes, which we compute as average standardized treatment effects. For a domain, like financial distress, with K outcomes, we estimate:

$$\hat{\tau} = \frac{1}{K} \sum_{k=1}^{K} \frac{\hat{\beta}^k}{\hat{\sigma}_k}, \tag{4}$$

where $\hat{\beta}^k$ is either the ITT or TOT estimate of the k^{th} outcome in the domain, and the control group standard deviation is $\hat{\sigma}_k$. We stack the data for all of the K outcomes in the domain and jointly estimate the ITT or TOT treatment effects in a single regression, clustering standard errors at the individual level.

2 Results

2.1 Income and Expenditures

Basic income increases total income by an amount similar to the actual transfer, with no evidence of crowd-out and even some increases in employment. Table 2 shows these results. The first row displays results for total monthly income, including income from work, the GBI program, and other sources. Columns (1) and (2) show that control and treatment participants have total monthly income of $1,455 and $1,948, on average, at 12-month follow-up. Column (3) estimates the ITT effect of being offered treatment, as in Equation (1). Being offered $500 of basic income per month increases total income by $515. Because of very high take-up of cash, TOT results in column (4) measuring the effect of actually receiving cash are only slightly larger, $547. To adjust for survey non-response bias, we also estimate specifications in columns (5) and (6) that re-weight the data using inverse propensity scores, which yields a TOT effect of $574. This value is slightly larger than the GBI payment, and indeed we also observe a 12 percentage point positive treatment effect on employment rates.[7] However, we cannot reject a $500 effect, which would imply neither crowd-in nor crowd-out of other income sources. In comparison, Vivalt et al. (2024) report declines in employment that crowd out 15% of the original transfer, an effect magnitude ($425) that is also within our 95% confidence interval.

[7] Employment effects are concentrated in part-time work. See Appendix Table A-7 for more detailed income and employment categories.

Income transfers likely lead to increased consumption, but any effect is relatively small and difficult to detect statistically. The third row of Table 2 reports that receiving basic income increases total spending by $162 per month. This effect is small, 8% of the control group mean and not statistically different from zero. It would imply a point estimate for the marginal propensity to consume of 0.32 out of the $500 monthly transfer, which is considerably smaller than that reported in Bartik et al. (2024). Some caution is necessary in comparing these results to other studies due to large standard errors in our survey sample for a skewed outcome like consumption. To the extent that consumption does increase, Appendix Table A-8 shows that these increases likely come from core expenditures like housing, utilities, and transportation.

2.2 During Cash Transfers: Main Results for Financial Distress and Well-Being

In our primary analysis, we find that people offered basic income show considerable declines in financial distress and economic anxiety while receiving the cash transfers. The fourth row of Table 2 displays our primary, pre-specified survey outcome: an index of financial distress. This index is measured at 12 months, just before the end of cash transfers. The control mean is mechanically zero for this average of z-scores. In the intent-to-treat specification, being offered basic income decreases the financial distress index by 0.2 s.d. ($p < 0.01$). Weighted and unweighted treatment effects are very similar, which increases confidence that survey results are reliable. Though, given lower survey response rates, we return to the question of non-response bias in more detail below.

Many areas of financial distress improve in response to income transfers. The financial distress index includes four pre-specified sets of outcomes: hardship experienced during the previous 12 months, worries about hardship during the previous 12 months, worries about future hardship over the next three months, and a real choice to delay survey incentive payments for a very high rate of return. Table 2 shows results for the four sub-indices of outcomes in the index. See Appendix Table A-9 for effects on individual outcomes included in each subindex. Reported experience of distress declines by 0.09 s.d., though this effect is not statistically significant. Measures of food scarcity drive any decline. Worries about housing and material distress, both over the past year and anticipation of the next three months, follow actual experiences. They show consistent declines with a 0.23 s.d. decline in past worries and a 0.24 s.d. decline in anticipated future worries. These summarize declines in many areas; for example, eviction worries over the past year decline by 14 percentage points and eviction worries about the next three months decline by 11 percentage points. Finally, while most outcomes in our primary survey outcome index rely on self-reports, the final outcome uses a consequential choice of the survey respondent. At the end of the survey, respondents were given the option of choosing a $25 gift card that they would receive within a week or a $50 gift card more than a month in the future.[8] Choosing to forgo a 100% monthly return provides a revealed preference measure of financial distress. Most control group respondents opted for the higher-valued gift card in the future—but 22% still selected the $25 gift card. Receiving basic income cuts this rate in half. Thus, the treatment changes financial distress measures, with improvements concentrated in self-reports of financial worries and real-stakes financial choices.

Many other survey outcomes improve besides our primary measure of financial distress. For example, returning to Table 2, the final row of survey outcomes shows a similarly large 0.15 s.d. increase in an

[8]The choice for the second cohort of survey respondents was for a $40 gift card in the future.

alternative index of respondent overall well-being based on measures of mental and physical health.[9]

On the other hand, our primary administrative data outcomes from credit reports show little sign of improvement during the year of cash transfers. Panel B of Table 2 shows no significant improvement in balances in derogatory accounts or balances in collections, our primary pre-specified administrative outcomes averaged over 0–4 quarters after GBI payment. Below, we return to the contrast between broad improvements for the treatment group alongside little change in interactions with formal credit.

2.3 After Cash Transfers: Main Results for Financial Distress

While we do not have survey records after the first year, administrative credit records suggest that primary outcomes worsen after the end of cash transfers. Estimates of treatment effects over time in Figure 1 indicate that collections and derogatory balances worsen in the year after cash transfers end. Appendix Figure A-3 and Appendix Figure A-4 show similar qualitative time patterns for other credit outcomes, such as credit scores. Panel C of Table 2 displays credit report outcomes for quarters 5–9. The point estimates of $211 for balances in derogatory accounts and $496 for balances in collections indicate worse debt situations than in quarters 0–4, though only the latter is statistically significant at $p < 0.10$ as standard errors are much larger after year one.

Any negative effects of cash transfers on financial stability are driven by tail outcomes affecting a relatively small number of people. Quantile regressions in Appendix Figure A-5 show that changes in mean collection balances originate in the right tail of the distribution, above the 90$^{\text{th}}$ percentile, where a small number of treated individuals see large balances in collections.[10] Supplementary administrative records on another extreme outcome, evictions, supports the interpretation that tail outcomes are worse in the second year. Appendix Figure A-6a and Panel D of Table 2 report how treatment affects the probability the household appears in a court eviction filing. Effects on evictions are positive but statistically insignificant in the first year, strengthening to a marginally significant 4.2 percentage points by 24 months, a 32% increase relative to the 13% control group mean. Overall, treatment leads to a noticeable increase in rare but extreme negative outcomes after cash transfers end.

2.4 Mechanisms

2.4.1 Informal Financial Decisions

The contrast between informal and formal financial arrangements helps explain different year-one results for survey measures of well-being and administrative credit reports. From results above, we know that treatment affects households' evaluation of their own situations because it changes how they respond to a real stakes financial decision about the timing of the survey incentive; however, this change does not appear in formal, year-one credit reports. Importantly, a similar contrast appears within survey measures of formal versus informal financial activity. Table 3 shows how the basic income treatment affects interactions with savings and debt. The top row displays a pre-specified index of financial well-being, which increases by a

[9] Appendix Table A-10 reports effects on the index components.

[10] Outliers also drive statistical imprecision in the means; outcomes winsorized at the 99$^{\text{th}}$ percentile show statistically stronger effects on means in Appendix Table A-11.

dramatic 0.33 s.d. The next four rows show a few selected elements from this index.[11] Treated households show large treatment effects on low-stakes outcomes: a 36 pp increase in making progress toward financial goals,[12] a 36 pp increase in having a checking account (a requirement to receive GBI payments), and a 19 pp increase in having any savings. But these steps add up to little formal savings. Only $242 out of the $6,000 income transfer appears in reported formal savings.

Instead, treatment has much larger effects on borrowing through informal channels. Panel B of Table 3 shows survey measures of respondents' reported borrowing by source. Consistent with administrative credit reports, survey responses indicate only small and statistically insignificant declines in borrowing from banks. Similarly, formal borrowing via credit cards may decline, but this effect is of modest size and not robust. On the other hand, borrowing via informal methods, particularly from family and friends, declines much more dramatically. Social connections are the most common borrowing source by far; while only 15% of the control group has borrowed from banks, 60% have borrowed from family and friends. Receiving basic income cuts this rate by 30 percentage points. In the short run, basic income improves welfare and allows most households to delever, but because most of their borrowing comes from informal sources like family and friends, these benefits appear neither in administrative credit records nor in survey measures of consumption.

2.4.2 Formal Borrowing and Tail Outcomes

We find suggestive evidence that, for a minority of households, receiving basic income encourages lumpy formal sector borrowing that increases the risk of rare negative outcomes after cash transfers end. The final two panels of Table 3 show administrative credit report measures for large-scale borrowing. In quarters 5-9, there are large but statistically imprecise increases in large debt items. For example, total debt balances excluding mortgage and auto loans increase by $1,300 (22%), a large but statistically insignificant value. This increase is almost entirely due to student debt and heavily driven by large outliers. Similarly, the prevalence of mortgages increases. The pattern in quarters 0–4 is similar but with generally smaller effects, except for stronger increases in auto loans.

These results are statistically imprecise and should be interpreted with caution, but they suggest that any negative financial outcomes that appear after cash transfers end are driven by large investments. We know that households changed how they managed their finances, with the treatment group making much different decisions about survey incentives just before the end of cash transfers. The available data suggest cash transfers induced large investments in education, housing, and transportation. We do not have long-term data to assess the eventual return to these investments. However, for some households, these new investments may have generated financial stress and the observed increase in collections in the year after cash transfers ended.

[11] See Appendix Table A-12 for all outcomes in the index.

[12] Applicants were asked about their top financial goals. Options included: save money for a goal; create a budget for my household; build or repair my credit; get out of debt; open a safe and affordable bank account; and other. Appendix Figure A-7 provides the share of treatment and control respondents who selected each goal. More than three-quarters of survey respondents selected "save toward a goal" at application, although budgeting, building/repairing credit, and getting out of debt were selected by roughly half. The follow-up survey prompted the respondents with their choice(s) from the application and asked whether they had made progress toward those specific goals.

2.5 Survey Response Bias

The contrast between survey measures of financial distress and administrative credit reports could instead be explained by survey bias. First, survey demand effects could be present. Respondents were aware that the follow-up survey was related to the GBI program. However, the large treatment effect on a real-stakes choice over survey incentives shows that treatment effects are not merely an issue of misreporting. Second, attrition bias could appear because survey response was modest and differential (53% in treatment, 30% in control). We demonstrate in several ways that our main results are robust to accounting for differential attrition.

First, administrative records give similar results in the full sample and when restricted to the sub-sample that responds to a survey. If survey respondents positively select on treatment, measured treatment effects should be more positive in the survey sub-sample. Appendix Figure A-8a and Appendix Figure A-9 show that our primary credit report and eviction outcomes change little when limited to the survey sub-sample.

Second, our main survey results change little when statistically adjusting for survey non-response in various ways. Table 4 shows treatment effects on our primary index of financial distress. Each row displays a different estimate of the TOT effect of receiving basic income. The first two rows replicate the unweighted and weighted effects of -0.21 s.d. and -0.18 s.d. from Table 2. The third row yields a similar effect of -0.19 s.d. when trimming the sample to observations with weights in common support of the treatment and control groups. The fact that the results change little with different re-weighting schemes suggests little scope for attrition bias based on observable characteristics. We can also account for attrition based on unobserved characteristics that are revealed by how difficult it is to survey participants. As in Behaghel et al. (2015), suppose that study participants vary in their cost of taking the survey, and treatment shifts that cost downward, inducing a greater response rate. Treatment effects can then be recovered by trimming 'unlikely' respondents from the treatment group. In our survey, study participants were contacted through a sequence of reminders; people who take more time to respond can reasonably be considered low probability/high cost responders. The fourth row shows the treatment effect estimated when trimming the slowest responders in the treatment group until response rates equalize across treatment and control. This procedure yields a treatment effect of -0.21 s.d.

Finally, we use tests for heterogeneous effects to examine if treatment effects would likely differ from our results if the full sample were available. To do this, we estimate a logit regression of survey response on baseline characteristics among the control group, and then we divide the full sample into equal groups of high, medium, and low response probability according to predicted values from the logit. We then estimate treatment effects in these three sub-groups within the survey sample. The final three rows of Table 4 show the results. Sample sizes of the three groups are not equal because, by definition, high-probability responders are over-represented in the sample with outcome data. Two facts are apparent. First, estimated treatment effects do not vary monotonically with predicted survey response probability: the largest effects are for low-probability responders, which limits the scope for attrition to explain our main results. Second, the simple average of the treatment effects for the three groups is 0.21, very similar to our main results. This simple average is informative because it gives equal weight to high, medium, and low probability responders, as would happen if one could observe outcomes for the full RCT sample.

3 Discussion

This paper explores the experimental impacts of a municipal basic income pilot in Rochester, NY. Eligible residents living in qualifying census tracts and earning below 185% of the federal poverty line applied for the chance to receive $500 per month for 12 months. Using a follow-up survey collected during the final month of payments, we find that the program substantially improved recipients' well-being. GBI receipt reduced a pre-registered index of financial distress and economic anxiety by 0.18 s.d. and increased overall well-being (self-reported physical and mental health) by 0.15 s.d. On the other hand, a quarterly panel of administrative credit reports shows null effects on collections balances during the period of the cash transfers and some indication of more distress after cash transfers end.

Given we report results for one of many basic income pilots, it is important to interpret our results in this broader context. In particular, we observe improvements in well-being during cash transfers that are large relative to prior studies. For example, the ORUS study (Miller et al., 2024) finds one-year effects of about 0.1 s.d. on the perceived stress score and a 0.2 point increase in life satisfaction. These same measures are components of our well-being index and increase by 0.43 s.d. and 0.62 points, respectively, in our sample (see Appendix Table A-10). For their most positive results, Liebman et al. (2022) find a 10.5 percentage point decline in not having enough to eat, compared with a 8.8 percentage point decline in our study. Balakrishnan et al. (2024) find a 0.36 s.d. decline in expectations about eviction, compared with a 0.22 s.d. decline in anticipated eviction worry in our study. Overall, we observe effects on well-being outcomes that tend to match the largest effects in other studies. Our results also differ from prior studies (Bartik et al., 2024; Vivalt et al., 2024) in finding smaller effects on consumption and more positive results for employment. Together, these results indicate that the effect of cash transfers will vary with context. The fact that our sample focuses on people who are poorer than most prior studies and occurs after the height of the COVID pandemic may explain these differences.

Our longer-term results generate questions for further research. We find some indication that, after cash transfers end, negative credit report outcomes become more common. A relatively small treatment group means we have limited power to detect changes in rare negative outcomes, and only two years of follow-up data means we cannot distinguish permanent financial distress from households willingly taking on some financial hardship in exchange for longer-run returns. Future work would do well to better understand how households manage the end of temporary cash transfers.

Most generally, our results stress the importance of considering how cash transfers interact with practical financial decision-making for people with very low income. Households cut borrowing from family and friends in half and become much less likely to forgo a 100% financial return over a period of weeks. At the same time, the evidence points to unsustainable formal borrowing in response to temporary transfers. Taken together, these findings demonstrate the importance of pairing survey measures of informal financial activity with formal credit records when evaluating cash transfer programs. Optimal cash transfer policies may look different when the poorest households operate primarily in informal financial environments and make financial decisions influenced by a context of scarcity (Mullainathan and Shafir, 2013).

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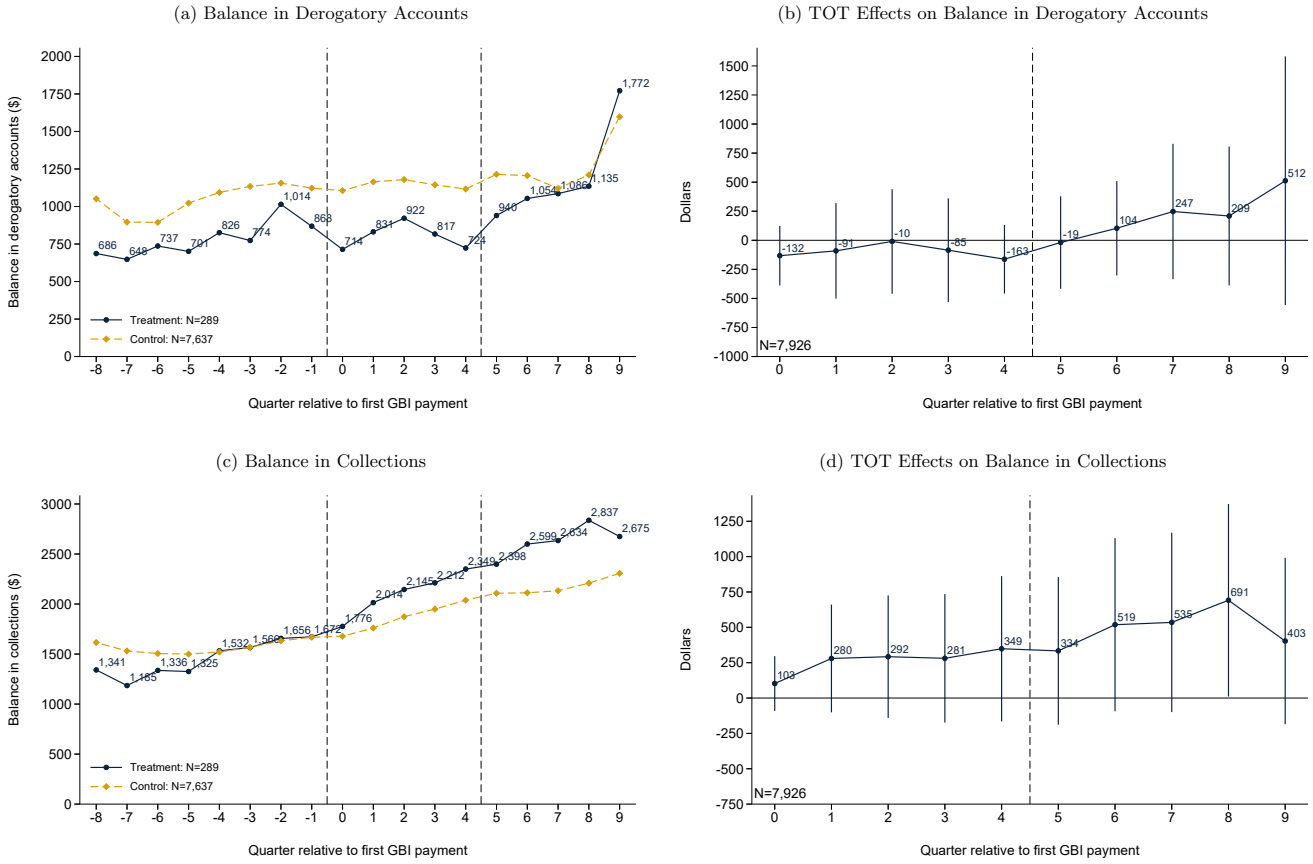
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Figure 1: Credit Report Outcome Trends and Treatment Effects over Time



Notes: Data source is administrative credit attributes from a large credit bureau. The sample includes 7,926 study participants with a balanced panel of credit records from the 8th quarter before application through the 9th quarter following application. Panels (a) and (c) plot the average debt balance in derogatory accounts and in collections, respectively, over time relative to the first GBI payment. Quarter 0 represents the quarter in which the first GBI payment was sent, and is thus a different calendar quarter for each program cohort. Treatment (solid line, navy circles) and control (dashed line, gold diamonds) groups are based on an individual's randomly assigned treatment status. Panels (b) and (d) report the effect of GBI receipt (treatment-on-the-treated (TOT) estimates) on quarterly derogatory debt and collections debt, respectively. Navy circles report the estimates from a specification that controls for baseline demographics, as well as the eight pre-period observations of the outcome, using a weighted sample that accounts for observable selection into credit matching. Vertical solid navy lines report the 95-percent confidence interval around the estimate.

Table 1: Comparative Baseline Characteristics & Study Details

	Rochester GBI Pilot			Compton	OpenResearch	Chelsea Eats	Baby's First Years	ACS
	Control Mean	Treatment Mean	Adjusted T-C	Control Mean	Control Mean	Control Mean	Control Mean	Mean ≤ 100% FPL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline Characteristics								
Gender								
Female	0.749	0.710	-0.039^{+}	0.74	0.678	0.809	1.00	0.522
Male	0.242	0.283	0.041^{+}		0.319			0.478
Other/missing	0.009	0.007	-0.002					0.000
Education								
Less than high school	0.219	0.183	-0.035^{+}				0.245	0.185
High school diploma or GED	0.457	0.480	0.024				0.500	0.337
Some college	0.199	0.220	0.020				0.170	0.276
College degree or more	0.125	0.117	-0.008	0.17	0.205		0.083	0.201
Other Demographics								
Age	40.8	39.9	-0.8	34.9	30.0		26.8	47.3
Kids in household	0.413	0.407	-0.006		0.571		1.000	0.154
Married	0.058	0.056	-0.002		0.222		0.21	0.199
Single (never married)	0.750	0.795	0.045*				0.42	0.534
Race								
Black, Non-Hispanic	0.680	0.654	-0.026	0.26	0.305	0.026	0.40	0.170
White, Non-Hispanic	0.087	0.098	0.011	0.01	0.481	0.088	0.11	0.550
Hispanic	0.180	0.198	0.018	0.68	0.214	0.895	0.41	0.198
Other/missing	0.054	0.051	-0.003				0.09	0.124
Baseline Housing & Transit								
Mainly drives	0.424	0.427	0.003					0.149
Mainly uses public transportation	0.234	0.227	-0.007					0.019
Rents home	0.726	0.727	0.001					0.303
Owns home	0.096	0.088	-0.008		0.137			0.269
Household income	$17,329	$16,932	-$411	$26,308	$29,895	$17,143	$22,470	$24,927
Baseline Employment								
Currently employed	0.452	0.420	-0.033	0.58	0.586	0.361	0.571	0.257
Currently employed full time	0.215	0.200	-0.015			0.262		0.160
Panel B: Study Details								
N (T, C)		410; 11,180		695; 1,402	1,000; 2,000	2,213; 1,402	400; 600	288,374
Payment started		Sep 2023		Feb 2021	Nov 2020	Nov 2020	May 2018	
Payment duration (years)		1		2	3	0.75	6.33	
Payment amounts (T, C)		$500, $0		$500 (avg.), $0	$1,000, $50	$200–$400, $0	$333, $20	
Follow-up timing		12 months		18 months	18 months, 30 months	18 months	Annually for 1–8 years	
Experiment location(s)		Rochester, NY		Compton, CA	IL, TX	Chelsea, MA	New York, NY; New Orleans, LA; Omaha, NE; Minneapolis-St. Paul, MN	

Notes: Rochester GBI Data come from applicants' baseline applications. The Rochester GBI sample includes one applicant from each of the 11,590 household groups. ACS ≤ 100% FPL sample includes U.S. adults in metropolitan areas at or below 100% of the Federal Poverty Level using the 2023 American Community Survey (Ruggles et al., 2025). Rochester GBI household income is winsorized at the 99th percentile, and negative reported values are changed to zero. All variables for the BFY sample are reported for the mothers rather than their children. **0.01, *0.05, $^{+}$0.10 significance levels

Table 2: Effect of GBI Payments on Survey and Administrative Data Outcomes

	Unweighted				Weighted			
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Survey Outcomes								
Monthly household income	$1,455	$1,948	$515**	$547**	$540**	$574**	39%	518
	(1,368)	(1,280)	(102)	(107)	(104)	(108)		
Currently employed	0.463	0.537	0.100*	0.106**	0.110**	0.117**	25%	518
	(0.499)	(0.500)	(0.039)	(0.041)	(0.041)	(0.043)		
Monthly spending	$1,948	$1,980	$102	$109	$151	$162	8%	437
	(1,348)	(1,313)	(129)	(135)	(163)	(171)		
Financial distress & anxiety standardized treatment effect	0.000	-0.196	-0.197**	-0.209**	-0.166**	-0.176**		518
	(0.538)	(0.510)	(0.046)	(0.048)	(0.050)	(0.052)		
Experienced distress, last 12 months (STE)	0.000	-0.130	-0.125*	-0.133*	-0.082	-0.087		518
	(0.623)	(0.629)	(0.056)	(0.059)	(0.062)	(0.065)		
Worried, last 12 months (STE)	0.000	-0.266	-0.275**	-0.292**	-0.220**	-0.234**		518
	(0.874)	(0.868)	(0.077)	(0.081)	(0.085)	(0.088)		
Anticipate worries, next 3 months (STE)	0.000	-0.227	-0.230**	-0.244**	-0.222**	-0.236**		518
	(0.624)	(0.548)	(0.052)	(0.055)	(0.054)	(0.057)		
Selected 25 dollar survey incentive	0.217	0.124	-0.094**	-0.099**	-0.104**	-0.111**	-51%	518
	(0.413)	(0.330)	(0.033)	(0.034)	(0.032)	(0.034)		
Overall well-being standardized treatment effect	0.000	0.184	0.156**	0.166**	0.144*	0.153**		518
	(0.618)	(0.651)	(0.055)	(0.057)	(0.057)	(0.059)		
Panel B: In-Program credit outcomes (Average of Q0 through Q4)								
Balance in derogatory accounts	$1,142	$801	-$93	-$106	-$84	-$96	-8%	7,926
	(4,241)	(1,903)	(117)	(134)	(121)	(139)		
Balance in collections	$1,860	$2,099	$215	$246	$227	$261	14%	7,926
	(3,194)	(4,038)	(159)	(182)	(161)	(185)		
Panel C: Post-Program credit outcomes (Average of Q5 through Q9)								
Balance in derogatory accounts	$1,270	$1,197	$135	$155	$183	$211	17%	7,926
	(4,499)	(2,523)	(148)	(170)	(172)	(197)		
Balance in collections	$2,187	$2,629	$418+	$479+	$432+	$496+	23%	7,926
	(3,547)	(5,023)	(252)	(289)	(246)	(283)		
Panel D: Administrative Eviction Records Outcomes								
Evicted within 12 months	0.072	0.085	0.013	0.015			21%	11,590
	(0.259)	(0.280)	(0.014)	(0.016)				
Evicted within 24 months	0.130	0.166	0.036+	0.042+			32%	11,590
	(0.337)	(0.372)	(0.018)	(0.021)				
Household Application, Survey Wave FEs			X	X	X	X		
Full Baseline Controls			X	X	X	X		

Notes: Data come from the GBI program application, a 12-month follow-up survey, and an administrative credit bureau. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise. Administrative credit outcomes control for eight measurements of each outcome from the eight quarters before beginning the GBI program. Eviction results use the full GBI study sample, so propensity weights are not possible to construct. The percent changes for Panel D in Column (7) use the unweighted TOT effects in Column (4).
**0.01, *0.05, +0.10 significance levels

Table 3: Effect of GBI Payments on Saving and Borrowing

	Unweighted				Weighted			
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Financial wellbeing								
Standardized treatment effect	-0.017	0.461	0.319**	0.339**	0.307**	0.326**		518
	(0.549)	(0.764)	(0.044)	(0.046)	(0.043)	(0.045)		
Selected outcomes from index								
Made progress toward goals	0.363	0.706	0.335**	0.356**	0.341**	0.362**	100%	518
	(0.482)	(0.456)	(0.042)	(0.044)	(0.045)	(0.047)		
Has checking account	0.563	0.881	0.320**	0.340**	0.336**	0.357**	63%	518
	(0.497)	(0.325)	(0.036)	(0.038)	(0.038)	(0.040)		
Has any savings	0.117	0.303	0.175**	0.186**	0.174**	0.185**	158%	518
	(0.322)	(0.461)	(0.035)	(0.037)	(0.037)	(0.039)		
Savings amount	$127	$429	$252**	$268**	$228**	$242**	191%	518
	(697)	(1,251)	(88)	(92)	(71)	(74)		
Panel B: Borrowing methods								
Applied for bank loan	0.147	0.124	-0.022	-0.023	-0.025	-0.026	-18%	518
	(0.354)	(0.330)	(0.031)	(0.033)	(0.032)	(0.034)		
Applied for credit card	0.130	0.083	-0.041	-0.044	-0.056*	-0.060*	-46%	518
	(0.337)	(0.276)	(0.027)	(0.028)	(0.025)	(0.026)		
Applied for payday or title loan	0.117	0.060	-0.050*	-0.053*	-0.059*	-0.062*	-54%	518
	(0.322)	(0.237)	(0.025)	(0.026)	(0.025)	(0.026)		
Asked friends or family	0.603	0.344	-0.261**	-0.277**	-0.279**	-0.296**	-49%	518
	(0.490)	(0.476)	(0.043)	(0.045)	(0.045)	(0.048)		
Panel C: In-program credit outcomes (Average of Q0 through Q4)								
Number of inquiries (last 3 months)	0.387	0.393	0.015	0.017	0.010	0.012	3%	7,926
	(0.423)	(0.445)	(0.023)	(0.026)	(0.023)	(0.026)		
Total debt excluding mortgage/auto loans	$7,059	$8,077	$476	$547	$347	$399	6%	7,926
	(14,404)	(17,997)	(509)	(584)	(429)	(493)		
Student loan balance	$4,737	$5,290	$192	$220	$67	$77	2%	7,926
	(13,199)	(16,409)	(466)	(535)	(388)	(446)		
Credit score	585.3	589.3	-0.6	-0.7	-1.2	-1.4	-0%	7,926
	(75.6)	(74.3)	(2.3)	(2.6)	(2.4)	(2.7)		
Has an auto loan/lease	0.344	0.358	0.019	0.022	0.017	0.020	6%	7,926
	(0.446)	(0.451)	(0.016)	(0.019)	(0.016)	(0.019)		
Has a mortgage	0.085	0.092	0.007	0.008	0.008	0.009	10%	7,926
	(0.271)	(0.277)	(0.007)	(0.007)	(0.006)	(0.007)		
Panel D: Post-program credit outcomes (Average of Q5 through Q9)								
Number of inquiries (last 3 months)	0.344	0.365	0.024	0.028	0.035	0.040	12%	7,926
	(0.396)	(0.421)	(0.023)	(0.026)	(0.024)	(0.028)		
Total debt excluding mortgage/auto loans	$5,845	$7,830	$1,482+	$1,701+	$1,132	$1,300	22%	7,926
	(14,321)	(20,903)	(878)	(1,006)	(722)	(828)		
Student loan balance	$3,430	$5,155	$1,327	$1,522	$980	$1,126	33%	7,926
	(13,110)	(19,631)	(828)	(949)	(672)	(772)		
Credit score	589.7	588.6	-5.0	-5.7	-5.3	-6.1	-1%	7,926
	(76.0)	(80.1)	(3.4)	(3.9)	(3.6)	(4.2)		
Has an auto loan/lease	0.340	0.341	0.003	0.004	0.002	0.003	1%	7,926
	(0.446)	(0.447)	(0.022)	(0.025)	(0.022)	(0.025)		
Has a mortgage	0.094	0.107	0.013	0.015	0.013	0.015	16%	7,926
	(0.280)	(0.299)	(0.010)	(0.012)	(0.009)	(0.011)		
Household Application, Survey Wave FEs			X	X	X	X		
Full Baseline Controls			X	X	X	X		

Notes: Data come from the GBI program application, a 12-month follow-up survey, and an administrative credit bureau. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise. Administrative credit outcomes control for measurements of each outcome during each of the eight quarters prior to program start. Savings amount is winsorized at the 99^{th} percentile.

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

Table 4: Effect of GBI Payments on Financial Distress and Anxiety, Adjusting for Survey Non-Response

	Standardized Treatment Effect	N
	(1)	(2)
Unweighted TOT	-0.209**	518
	(0.048)	
Weighted TOT	-0.176**	518
	(0.052)	
Weighted TOT, common support	-0.191**	497
	(0.054)	
Bound, difficult-to-survey	-0.208**	422
	(0.062)	
Weighted TOT probability subgroups		
Low survey response probability	-0.303**	101
	(0.113)	
Medium survey response probability	-0.160^{+}	179
	(0.085)	
High survey response probability	-0.188**	238
	(0.068)	

Notes: Data come from the GBI program application and a 12-month follow-up survey. Column (1) shows the standardized treatment effect estimate for the Financial Distress and Anxiety outcome domain. Column (2) shows the total number of observations included in each regression. Each row represents a different regression specification. "Weighted TOT, common support" restricts the sample to respondents whose probabilities of completing the follow-up survey given observed baseline characteristics include members of both the treatment and control groups. "Bound, difficult-to-survey" trims the latest-responding treatment group members until the treatment group's response rate matches that of the control group's. "Weighted TOT probability subgroups" split the 1,410 possible survey respondents into three different subgroups based on their likelihoods of completing the follow-up survey. These subgroups are then restricted to only the participants in each group who actually completed the follow-up survey. Heteroskedasticity-robust standard errors in parentheses.

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

How Temporary Cash Transfers Affect Household Finances

Priti Kalsi, David Phillips, Ryan Swanson, and Patrick S. Turner

A Online Appendix

A.1 Follow-up Survey Data Appendix

A.1.1 Survey Procedures

The research team contracted with NORC at the University of Chicago to collect a web-based follow-up survey. The survey was collected during two waves in 2024, roughly 12 months after payments began for the two program cohorts. The initial survey collection period began on August 5, 2024, and continued through September 18, 2024. The second survey collection period began on October 7, 2024, and continued through November 16, 2024.[13] During each collection period, the treatment group from the corresponding program cohort was contacted via text, email, and phone to complete a survey. An additional 500 control group applicants were also randomly selected to receive survey invitations during each collection period (for a total of 1,000 potential control group survey respondents). Respondents were provided a gift card incentive for their participation. During the survey, respondents were asked whether they would prefer receiving a $25 gift card within a week of completing the survey or receiving a $50 gift at the end of a roughly 4-week waiting period.[14]

A.1.2 Response Rates, Selection, and Treatment-Control Balance

We sent survey invitations to all 410 treatment group applicants and a random sample of 1,000 control group applicants. When estimating program effects on outcomes measured using survey data, we restrict our sample to individuals who completed the entire follow-up survey. Among the 1,410 applicants who were sent survey invitations, 518 completed the follow-up survey. The response rate was higher in the treatment group (53.2%) than in the control group (30.0%). The survey was carried out over two waves. The first cohort of 203 treatment group applicants, along with 500 randomly selected control group applicants were surveyed beginning in August 2024. The second cohort of 207 treatment group applicants, along with the remaining 500 randomly selected control groups applicants, were surveyed beginning in October 2024. Response rates were slightly higher in the second round of surveying (40.7%) relative to the first round of surveying (32.7%).

Table A-3 reports average baseline characteristics of those who were invited but did not respond to the follow-up survey (column 1) and those who responded to the follow-up survey (column 3). Standard deviations for each variable are reported in columns (2) and (4) for the non-respondents and respondents, respectively. The magnitude of the difference in average characteristics is reported in column (5). In general, respondents are more likely to identify as female, have higher levels of completed education, are a little younger (39.1 vs. 41.6), and are more likely to have children in the household. As discussed above, most applicants identify as Black, non-Hispanic, and we see no meaningful differences in the likelihood of being Black between the two groups (67.6% vs. 66.0%). However, respondents are more likely to be White (13.1% vs. 7.1%) and less likely to be Hispanic (14.3% vs. 21.7%) relative to their non-respondent peers. Respondents are also economically advantaged at baseline: they are more likely to drive a car, rent their home, earn more income, and be employed.

To account for selective survey response, we construct a set of inverse propensity weights to use in our analysis of outcomes from this data source. Separately by treatment assignment, we use a logit specification to predict survey response based on the application characteristics reported in Table A-3. The resulting attrition weight is the inverse of the predicted probability an individual completed the follow-up survey, and we top code the weights at the 99$^{\text{th}}$ percentile. Figure A-1 reports the distributions of predicted completion probabilities for the treatment and control groups.

Application characteristics remain balanced between the treatment group and the control group among the 518 follow-up survey respondents. Table A-4 reports average baseline characteristics of respondents

[13] For both survey waves, the initial survey invitation was sent prior to the transfer of the final GBI payment. More than 80% of respondents completed the survey prior to the transfer of the final month's payment.

[14] Due to budget constraints, the larger, delayed gift card offered during the second survey wave was $40.

in the control group (column 1) and treatment group (column 2). In columns (3) and (5), we both report the unweighted and weighted difference in average characteristics, respectively. In testing across 43 characteristics in the unweighted sample, only baseline housing spending has a t-statistic with magnitude greater than 2. A joint test of balance has a p-value of 0.04. Weighting the sample by the inverse propensity weights described above yields a sample that is strongly balanced by construction, with a joint test p-value of 0.88.

A.1.3 Cleaning Income Responses

The following section describes all income data cleaning decisions, and the reasoning for each decision. During wave one of the survey, many respondents contacted BCFF and the City saying they experienced technical difficulties completing the income questions. These answers are therefore noisier than those in other sections of the survey, so our changes to respondents' income answers deserve careful attention.

First, we make two general changes. For participants who had not given an answer to one or more income questions, we replace their missing amounts with zero. Note that none of these participants had left all income questions missing. Then, we multiply any one-or-two-digit numbers by 1,000, assuming these participants had intended to type in thousands. This multiplication also catches participants who typed income amounts with three decimal places, likely intending to have used commas instead.

We adjust reported GBI incomes based on what we know for certain: which respondents enrolled in the program. Some GBI participants we know received all payments reported $500 for their yearly GBI amounts. This number is how much participants received each month, so we multiply $500 by 12 to get their yearly GBI income of $6,000.

Some other GBI recipients reported yearly payments other than $500 or $6,000. Figure A-2 shows that many respondents in the treatment group also reported receiving $5,000 in GBI payments, or none at all. We also change these respondents' reported GBI amounts to $6,000. If they report GBI amounts higher than $6,000, we do not change their answers because they may have received additional payments from other GBI programs.

We annualize the reported incomes of several other participants. For three participants, their reported household incomes are their separate income streams multiplied by 12. This discrepancy suggests they had reported yearly household incomes, but monthly separate income streams. We therefore also annualize their separate income streams. Since we know some participants reported monthly GBI payments, those participants could have also reported monthly numbers for other income sources. For enrolled respondents who entered any other income sources less than $1,000 and also reported $500 for their GBI payments, we annualize the other income sources too.

After cleaning each separate income stream, we re-calculate each participant's household income as the sum of each stream. This end goal was also built into our validation for this survey question. Finally, we winsorize all cleaned spending amounts at the 99th percentile to reduce the impact of outliers on our results.

A.2 Administrative Credit Data Appendix

A.2.1 Match Rates, Selection, and Treatment-Control Balance

We attempted to link all GBI program applicants to quarterly credit records from Experian using names, dates of birth, and addresses collected in the initial program application. When estimating program effects on credit outcomes, we restrict our sample to individuals who linked to a balanced panel of credit records during the 8 quarters before the start of GBI payment and the 9 quarters after. For the treatment group, we define quarter 0 as the quarter of the first GBI payment: Q3 2023 for Cohort 1 and Q4 2023 for Cohort 2. Because the control group did not receive a GBI payment, we allow for them to potentially be in the panel twice—once as a comparison for Cohort 1 and once as a comparison for Cohort 2—defining quarter

0 accordingly for each instance. Among the 11,590 applicants, 7,926 individuals matched to this balanced panel for an overall match rate of 68.3%. Match rates are similar between the treatment group (70.5%) and the control group (68.3%).

Table A-5 reports average baseline characteristics of those who did not (column 1) and did match (column 3) to the full credit panel. In general, matched applicants are more likely to be female, have higher levels of completed education, and are more likely to have kids in the household. They also are a little more economically advantaged relative to their unmatched peers. They are more likely to drive.

We construct weights to account for select matching as before. We estimate a logit of credit report matching on the variables in Table A-3, which we estimate by treatment group and (for the control group) wave and winsorize as before. Second, we apply weights to adjust for including members of the control group twice. As noted in the main text, we include members of the control group twice, once for each cohort with relative time measured for that cohort.

Application characteristics remain relatively balanced between the treatment group and the control group among the 7,926 applicants who matched to the credit panel. Table A-6 reports average baseline characteristics of respondents in the control group (column 1) and treatment group (column 2). In columns (3) and (5), we both report the unweighted and weighted difference in average characteristics, respectively. In the unweighted sample, the treatment group is more likely to be male, is slightly more educated, is more likely to be Hispanic, and is less likely to be Black. Balance in the weighted sample (column 5) is a little better, though the treatment group is still more likely to be male, and we do not reject the null hypothesis that all of the characteristics are unrelated to treatment assignment.

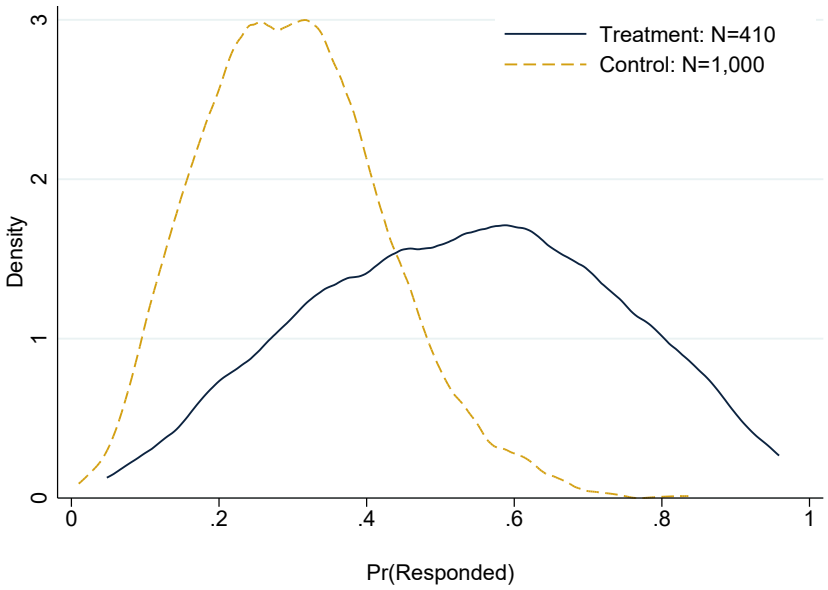
A.2.2 Stacking & Attrition Weights

To maintain the relative importance of each cohort in administrative records (i.e., Cohort 2 has slightly more individuals in the treatment group), we construct a "stacking weight" for each observation. Specifically, for an individual i assigned to cohort q, we construct the following weight:

$$w_{iq} = \begin{cases} 1 & \text{if } T_i = 1 \\ \frac{N_q^{T=1}/N^{T=1}}{N_q^{T=0}/N^{T=0}} & \text{if } T_i = 0 \end{cases}$$

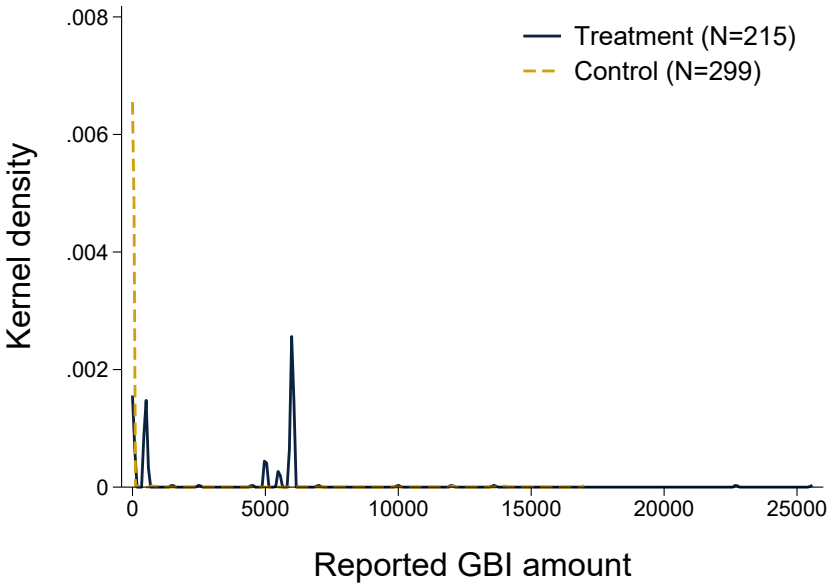
where the numerator of the control group weight is the number of treatment group individuals in the cohort relative to the total number of treatment group individuals and the denominator is the number of control group individuals providing data for that cohort relative to the total number of control group individuals who contribute data in either or both panels. Because a few individuals in the control group are only matched to a balanced panel for the first cohort or the second cohort, the denominator of the weight is close to but not exactly 1. When weighting to account for both the stacking of the data and observable selection, we multiply the stacking and attrition weights and normalize the product to sum to one within the treatment group and the control group.

Figure A-1: Predicted Survey Response Probability, By Treatment Group



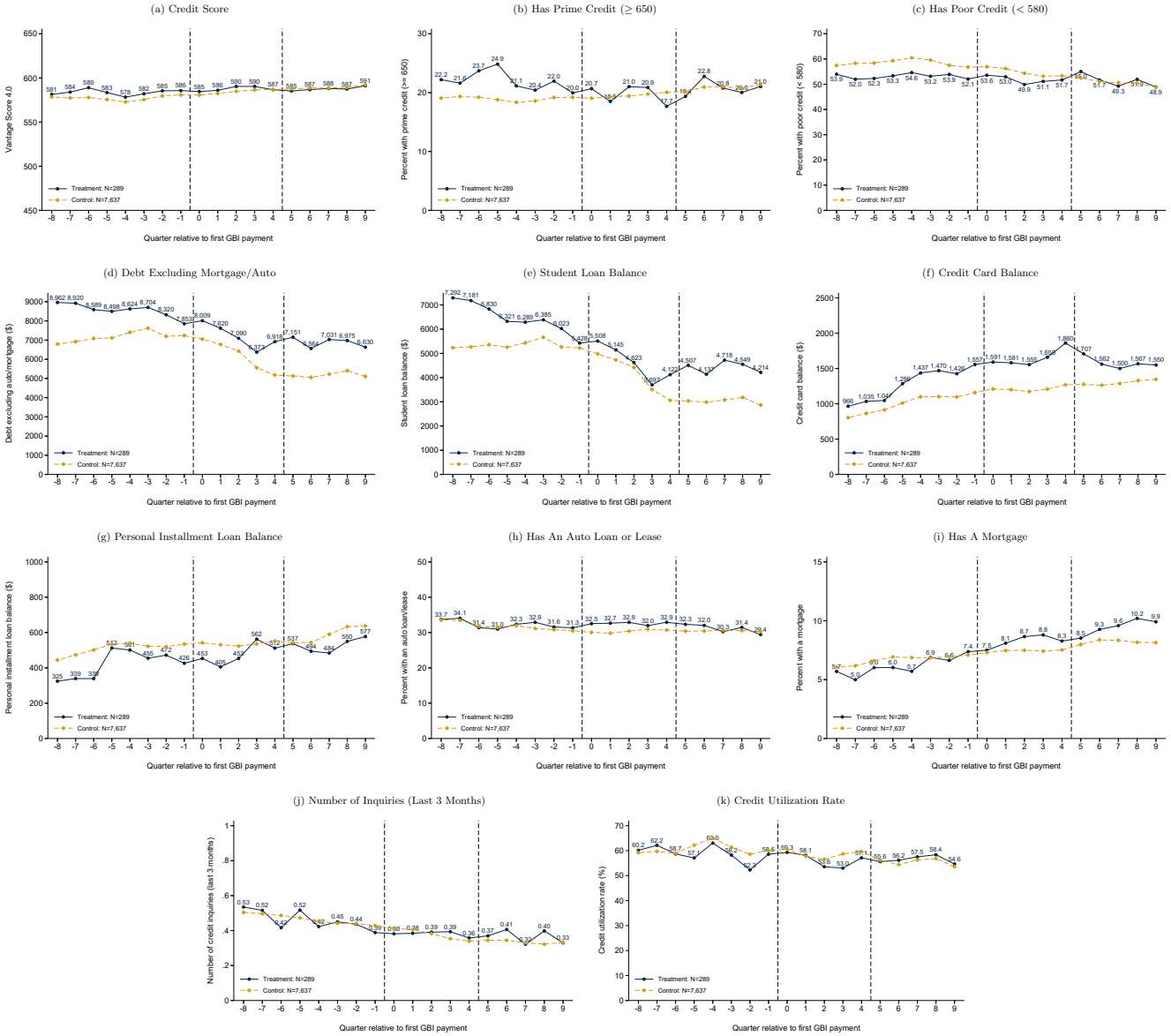
Notes: Data come from the GBI program application. The sample is restricted to the 1,410 individuals who NORC contacted for the 12-month follow-up survey.

Figure A-2: Distribution of GBI Income Before Data Cleaning



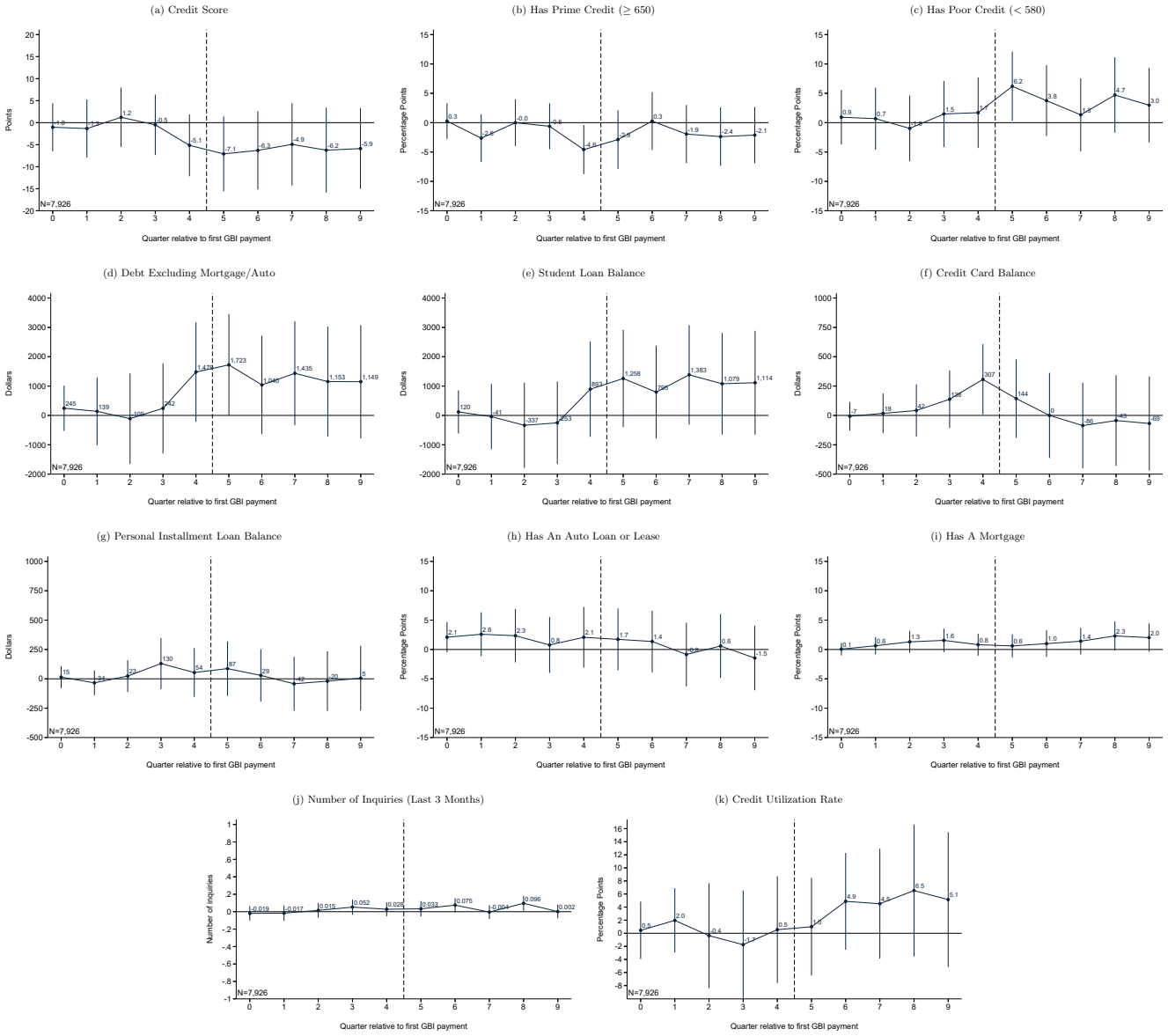
Notes: Data come from raw, annualized follow-up survey responses to a question about income from the GBI program. Kernel density plots are plotted by random treatment assignment with a bandwidth of 50.

Figure A-3: Trends in Additional Credit Outcomes, By Treatment Status



Notes: Data source is administrative credit attributes from a large credit bureau. The sample includes 7,926 study participants with a balanced panel of credit records from the 8^{th} quarter before the first GBI payment through the 9^{th} quarter following. Each panel plots the average of the outcome given by the panel label over time relative to the first GBI payment. Quarter 0 represents the quarter in which the first GBI payment was sent, and is thus a different calendar quarter for each program cohort. Treatment (solid line, navy circles) and control (dashed line, gold diamonds) groups are based on an individual's randomly assigned treatment status.

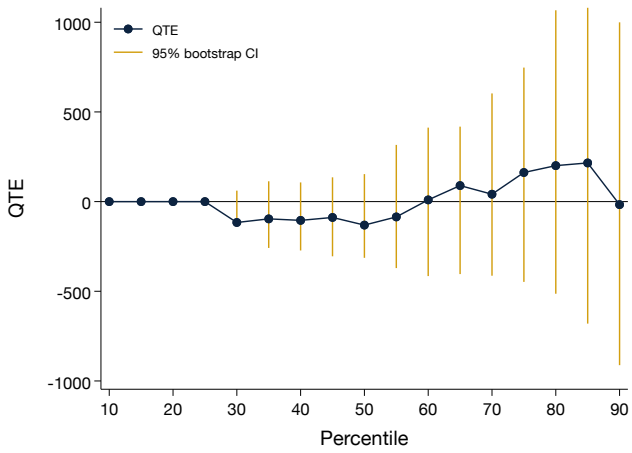
Figure A-4: Effects of GBI Receipt on Additional Credit Outcomes, By Treatment Status



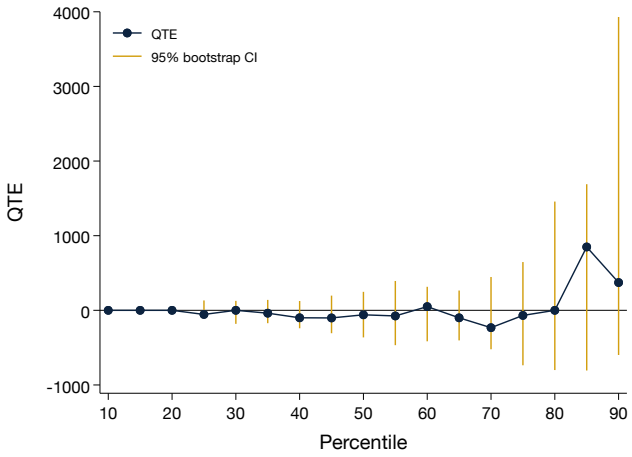
Notes: Data source is administrative credit attributes from a large credit bureau. The sample includes 7,926 study participants with a balanced panel of credit records from the 8th quarter before the first GBI payment through the 9th quarter following application. Each panel reports the effect of GBI receipt (treatment-on-the-treated (TOT) estimates) on the outcome given by the panel label. Navy circles report the estimates from a specification that controls for baseline demographics, as well as the eight pre-period observations of the outcome, using a weighted sample that accounts for observable selection into credit matching. Vertical navy lines report the 95-percent confidence interval around the estimate.

Figure A-5: Unadjusted Quantile Treatment Effects on Primary Credit Outcomes

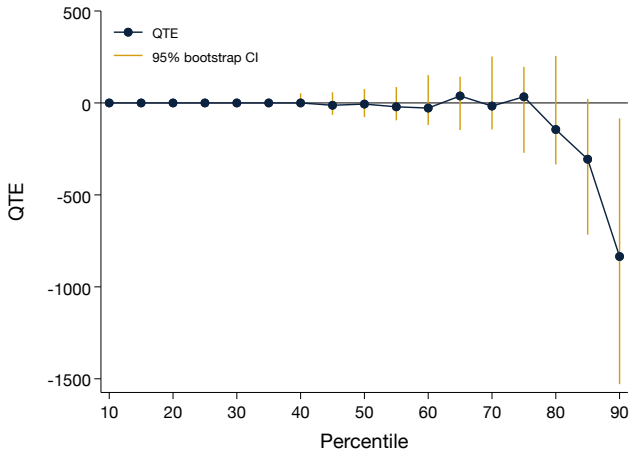
(a) Balance in Collections, Q0–Q4



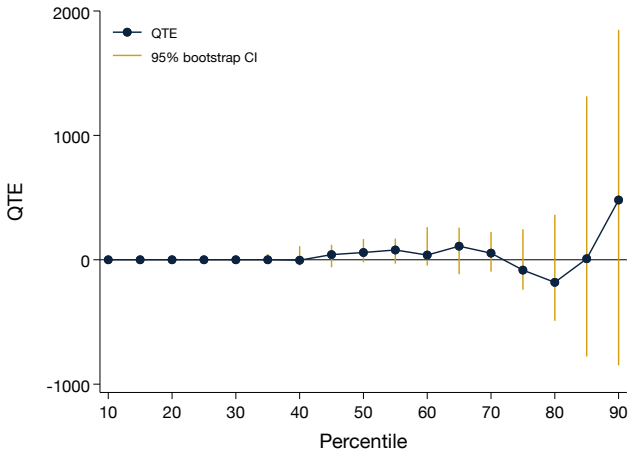
(b) Balance in Collections, Q5–Q9



(c) Balance in Derogatory Accounts, Q0–Q4



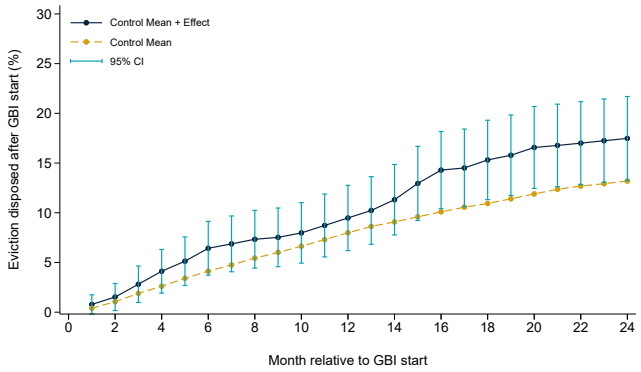
(d) Balance in Derogatory Accounts, Q5–Q9



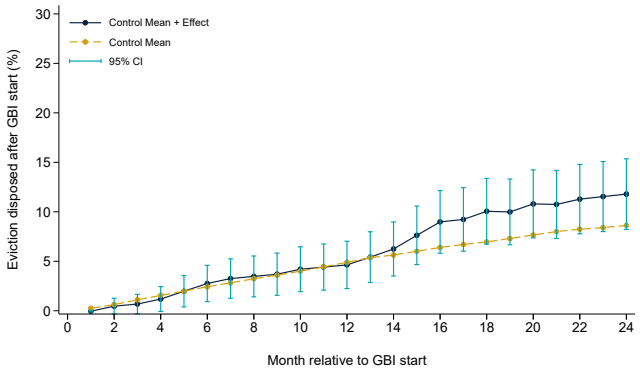
Notes: Data source is administrative credit attributes from a large credit bureau. The sample includes 7,926 study participants who matched to the balanced panel of credit records. Each panel reports unadjusted quantile treatment effects of assignment to GBI on debt balances averaged over the quarters indicated by the panel label. Quarter 0 represents the quarter in which the first GBI payment was sent, and is thus a different calendar quarter for each program cohort. Navy circles report differences between the corresponding quantiles of the weighted treatment and control outcome distributions, evaluated at five-percentile increments from the 10^{th} through the 90^{th} percentile. Estimates do not adjust for baseline covariates. Vertical gold lines report pointwise 95-percent percentile-bootstrap confidence intervals based on 1,000 replications, resampling individuals within treatment assignment groups and keeping repeated observations for each individual together.

Figure A-6: Evictions, By Treatment Status, Per Court Record Match Type, Full Sample

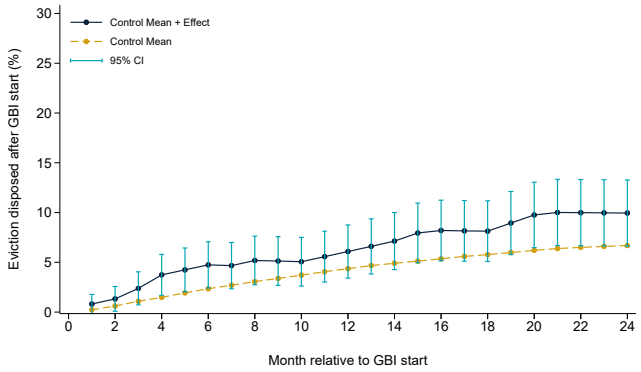
(a) Matches Eviction Filing Address or Name (Main Results)



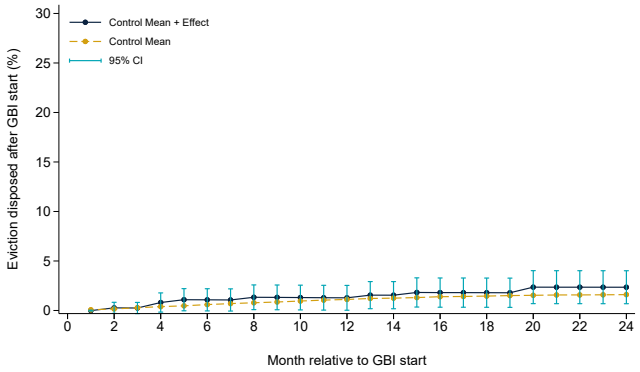
(b) Matches Eviction Filing Address



(c) Matches Eviction Filing Name

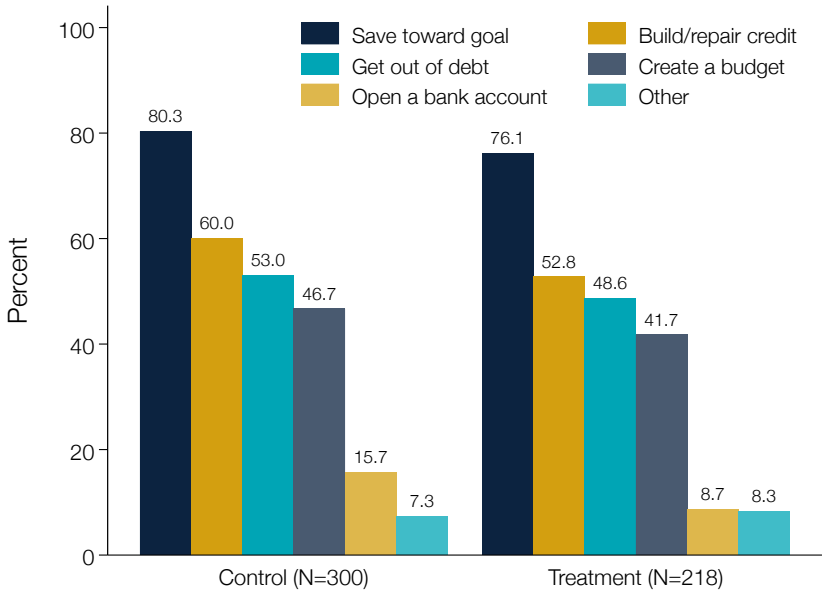


(d) Matches Eviction Filing Address And Name



Notes: Data source is administrative eviction records from the New York State Office of Court Administration. The sample includes all 11,590 study participants. Each panel plots the control group mean (dashed gold line) and the TOT effect added onto the control mean (solid navy line) on evictions disposed over time relative to the first GBI payment. Each panel's name shows how the GBI study sample was matched to the eviction records. Month 0 represents the month in which the first GBI payment was sent, and is thus a different calendar month for each program cohort. Only eviction cases where the initial filing occurred after GBI start are considered.

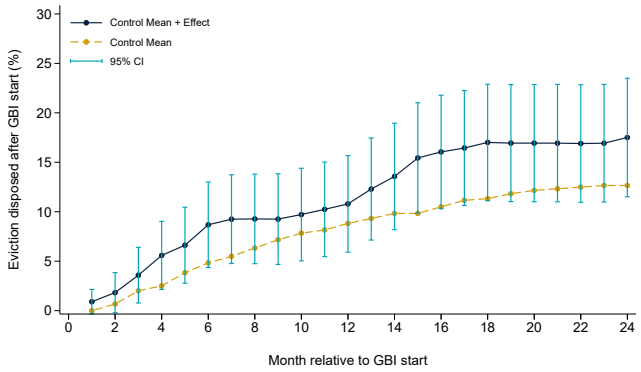
Figure A-7: Main Financial Goals Selected in GBI Application



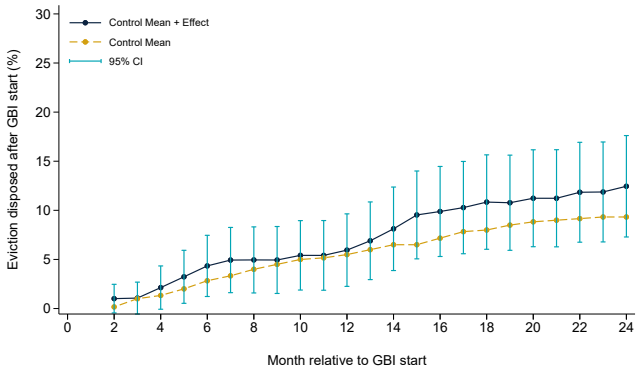
Notes: Data come from the GBI program application. The sample is restricted to the 518 individuals who responded to the 12-month follow-up survey. Applicants could select multiple financial goals that they had. The height of each vertical bar depicts the share of each group that selected that goal.

Figure A-8: Evictions, By Treatment Status, Per Court Record Match Type, Follow-Up Survey Sample

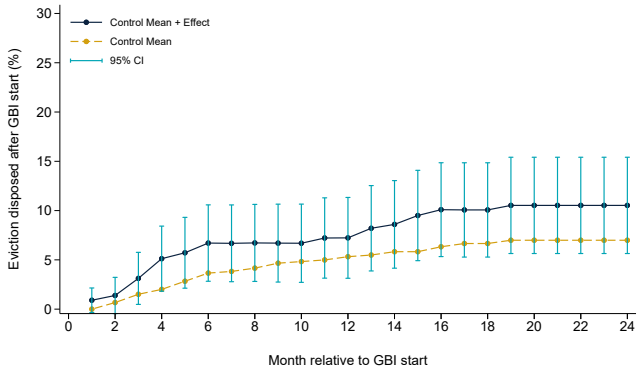
(a) Matches Eviction Filing Address Or Name



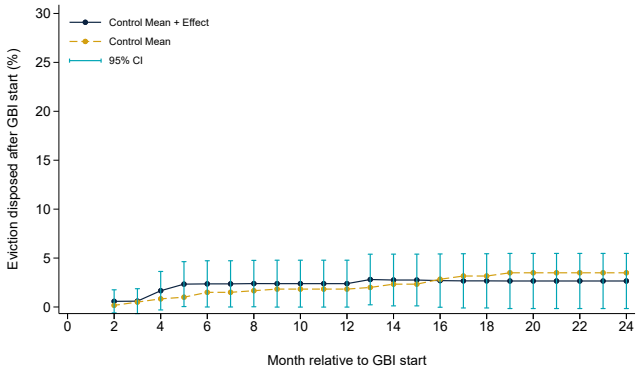
(b) Matches Eviction Filing Address



(c) Matches Eviction Filing Name

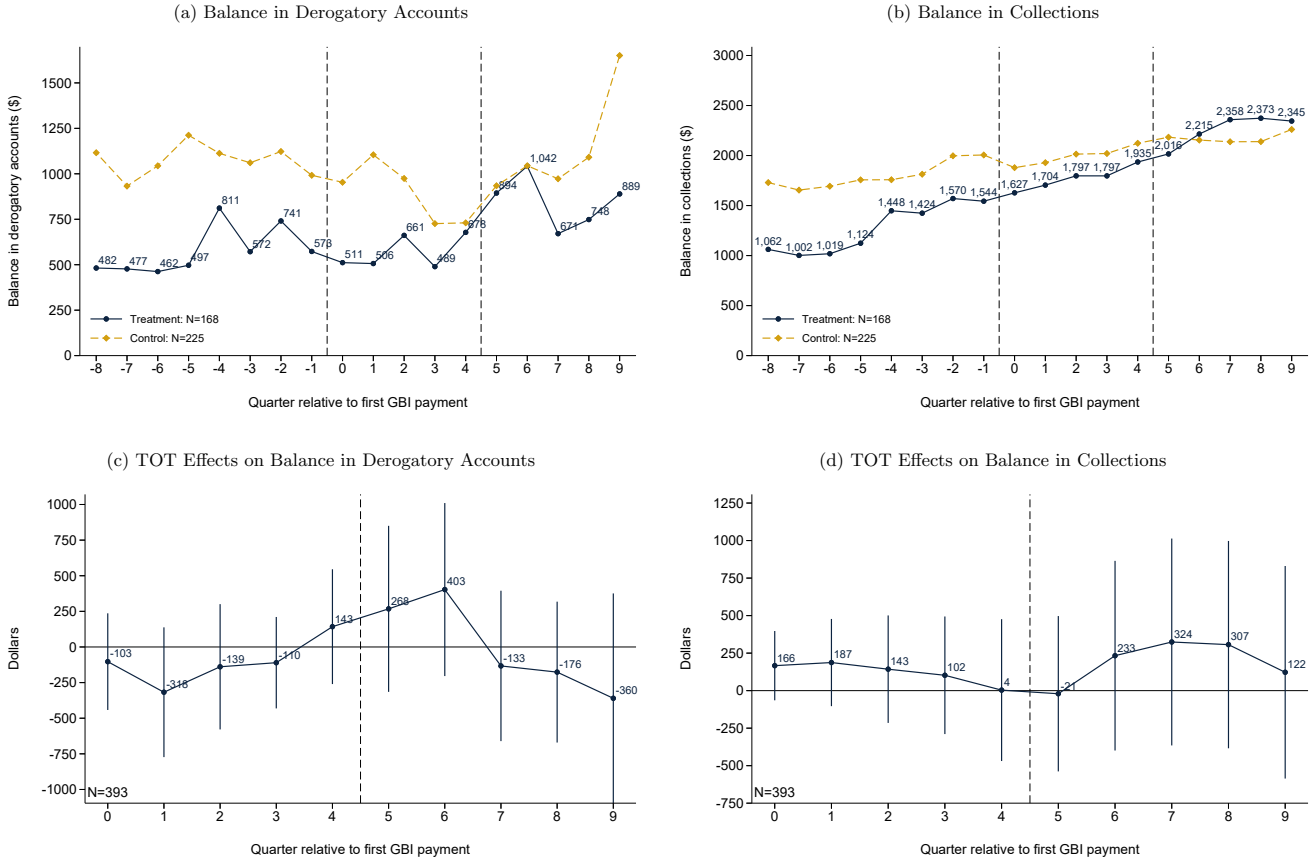


(d) Matching Eviction Filing Address And Name



Notes: Data source is administrative eviction records from the New York State Office of Court Administration. The sample includes all 518 follow-up survey respondents. Each panel plots the control group mean (dashed gold line) and the TOT effect added onto the control mean (solid navy line) on evictions disposed over time relative to the first GBI payment. Each panel's name shows how the GBI study sample was matched to the eviction records. Month 0 represents the month in which the first GBI payment was sent, and is thus a different calendar month for each program cohort. Only eviction cases where the initial filing occurred after GBI start are considered.

Figure A-9: Balance in Derogatory Accounts and Collections, By Treatment Status, Follow-up Survey Respondents



Notes: Data source is administrative credit attributes from a large credit bureau. The sample includes 393 follow-up survey respondents with a balanced panel of credit records from the 8th quarter before the first GBI payment through the 9th quarter following application. Panels (a) and (b) plot the average debt balance in derogatory accounts and in collections, respectively, over time relative to the first GBI payment. Quarter 0 represents the quarter in which the first GBI payment was sent, and is thus a different calendar quarter for each program cohort. Treatment (solid line, navy circles) and control (dashed line, gold diamonds) groups are based on an individual's randomly assigned treatment status. Panels (c) and (d) report the effect of GBI receipt (treatment-on-the-treated (TOT) estimates) on quarterly derogatory debt and collections debt, respectively. Navy circles report the estimates from a specification that controls for baseline demographics, as well as the four pre-period observations of the outcome, using a weighted sample that accounts for observable selection into credit matching. Vertical solid navy lines report the 95-percent confidence interval around the estimate.

Table A-1: Program Timeline

	Application Window	Program Enrollment	First Payment	Final Payment
Cohort 1	Week of 6/22/2023	7/17–8/18/2023	9/15/2023	8/15/2024
Cohort 2	Week of 6/22/2023	9/18–11/20/2023	12/1/2023	11/1/2024

Notes: This table summarizes the application, enrollment, and payment timeline for the City of Rochester GBI Pilot Program.

Table A-2: Applicant Characteristics and Baseline Balance

	Full Sample		Control		Treatment				Diff./	
	Mean	SD	Mean	SD	Mean	SD	(5) – (3)	t-stat	SD	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Gender										
Nonbinary	0.006	(0.076)	0.006	(0.077)	0.002	(0.049)	-0.004	-1.39	-0.05	11,590
Male	0.243	(0.429)	0.242	(0.428)	0.283	(0.451)	0.041	1.81	0.10	11,590
Female	0.748	(0.434)	0.749	(0.434)	0.710	(0.454)	-0.039	-1.71	-0.09	11,590
Other	0.001	(0.028)	0.001	(0.027)	0.002	(0.049)	0.002	0.70	0.06	11,590
Missing gender	0.003	(0.050)	0.003	(0.050)	0.002	(0.049)	-0.000	-0.02	-0.00	11,590
Education										
Less than high school	0.217	(0.412)	0.219	(0.413)	0.183	(0.387)	-0.035	-1.82	-0.09	11,590
High school diploma or GED	0.457	(0.498)	0.457	(0.498)	0.480	(0.500)	0.024	0.95	0.05	11,590
Some college	0.200	(0.400)	0.199	(0.400)	0.220	(0.414)	0.020	0.96	0.05	11,590
College degree or more	0.125	(0.331)	0.125	(0.331)	0.117	(0.322)	-0.008	-0.51	-0.03	11,590
Other Demographics										
Age	40.8	(14.0)	40.8	(14.0)	39.9	(13.6)	-0.8	-1.24	-0.06	11,590
Kids in household	0.413	(0.492)	0.413	(0.492)	0.407	(0.492)	-0.006	-0.23	-0.01	11,590
Divorced	0.086	(0.280)	0.086	(0.281)	0.076	(0.265)	-0.011	-0.80	-0.04	11,590
Married	0.057	(0.233)	0.058	(0.233)	0.056	(0.230)	-0.002	-0.14	-0.01	11,590
Separated	0.055	(0.228)	0.056	(0.229)	0.034	(0.182)	-0.022	-2.33	-0.09	11,590
Single (never married)	0.752	(0.432)	0.750	(0.433)	0.795	(0.404)	0.045	2.21	0.10	11,590
Widow	0.033	(0.178)	0.033	(0.178)	0.024	(0.154)	-0.008	-1.08	-0.05	11,590
Missing marital status	0.017	(0.131)	0.017	(0.131)	0.015	(0.120)	-0.003	-0.46	-0.02	11,590
Race										
Black, Non-Hispanic	0.679	(0.467)	0.680	(0.467)	0.654	(0.476)	-0.026	-1.11	-0.06	11,590
White, Non-Hispanic	0.087	(0.282)	0.087	(0.282)	0.098	(0.297)	0.011	0.72	0.04	11,590
Hispanic	0.180	(0.384)	0.180	(0.384)	0.198	(0.399)	0.018	0.92	0.05	11,590
Multiracial	0.012	(0.111)	0.012	(0.110)	0.015	(0.120)	0.002	0.38	0.02	11,590
Non-Black, Non-White, Non-Multiracial	0.015	(0.121)	0.015	(0.122)	0.010	(0.098)	-0.005	-1.07	-0.04	11,590
Missing race	0.026	(0.161)	0.026	(0.161)	0.027	(0.162)	0.000	0.05	0.00	11,590
Baseline Financial Distress										
Sometimes/often unable to afford food	0.876	(0.329)	0.877	(0.329)	0.859	(0.349)	-0.018	-1.05	-0.06	11,590
Baseline Housing & Transit										
Mainly drives	0.424	(0.494)	0.424	(0.494)	0.427	(0.495)	0.003	0.14	0.01	11,590
Mainly uses public transportation	0.234	(0.423)	0.234	(0.423)	0.227	(0.419)	-0.007	-0.34	-0.02	11,590
Rents home	0.726	(0.446)	0.726	(0.446)	0.727	(0.446)	0.001	0.02	0.00	11,590
Owns home	0.096	(0.294)	0.096	(0.294)	0.088	(0.283)	-0.008	-0.56	-0.03	11,590
Rents or owns home	0.822	(0.382)	0.822	(0.382)	0.815	(0.389)	-0.007	-0.38	-0.02	11,590
Has unstable housing situation	0.093	(0.290)	0.093	(0.290)	0.100	(0.300)	0.007	0.49	0.03	11,590
Paid full rent on time	0.417	(0.493)	0.417	(0.493)	0.422	(0.495)	0.005	0.18	0.01	9,528
Household income	$17,314	(13,136)	$17,329	(13,160)	$16,932	(12,487)	-$411	-0.65	-0.03	11,590
Baseline Spending										
Housing	$772	(1,266)	$775	(1,281)	$666	(744)	-$108	-2.80	-0.08	11,590
Utilities	$188	(242)	$188	(241)	$190	(260)	$2	0.16	0.01	11,590
Food	$351	(302)	$352	(304)	$329	(249)	-$23	-1.84	-0.08	11,590
Transportation	$250	(288)	$250	(288)	$244	(291)	-$6	-0.42	-0.02	11,590
Healthcare	$30	(84)	$31	(85)	$23	(73)	-$8	-2.10	-0.09	11,590
Childcare	$43	(127)	$43	(127)	$40	(120)	-$2	-0.41	-0.02	11,590
Other	$170	(452)	$170	(452)	$167	(439)	-$3	-0.15	-0.01	11,590
Baseline Overall Wellbeing										
Perceived stress score (0 to 40)	21.378	(6.603)	21.388	(6.590)	21.102	(6.944)	-0.290	-0.83	-0.04	11,590
Participated in education/training program	0.079	(0.270)	0.079	(0.270)	0.078	(0.269)	-0.001	-0.07	-0.00	11,590
Baseline Employment										
Currently employed	0.451	(0.498)	0.452	(0.498)	0.420	(0.494)	-0.033	-1.32	-0.07	11,590
Currently employed full time	0.215	(0.411)	0.215	(0.411)	0.200	(0.400)	-0.015	-0.77	-0.04	11,590
Joint test of balance (p-value)							0.115			

Notes: Data come from applicants' baseline GBI applications. The sample includes one applicant from each of the 11,590 household groups. Columns (1) and (2) report each variable's mean and standard deviation for the full sample of applicants. Columns (3) and (4) report those statistics for only the applicants assigned to the control group, while columns (5) and (6) report those statistics for the treatment group. Column (7) reports the difference in each variable's control and treatment group means. If the t-stat in column (8) is high, then we are less confident in our hypothesis that the difference is zero. Column (9) divides the difference by the control group's standard deviation to show economic significance: whether the difference is large enough to have a real-world effect. Column (10) shows the number of applicants in each variable's sample. Income and spending variables are winsorized at the 99th percentile, and negative reported values are changed to zero.

Table A-3: Follow-up Survey Response Selection

	Non-response		Response				Diff./	
	Mean	SD	Mean	SD	(3) – (1)	t-stat	SD	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender								
Nonbinary	0.008	(0.088)	0.008	(0.088)	-0.000	-0.03	-0.00	1,410
Male	0.280	(0.449)	0.199	(0.400)	-0.082	-3.55	-0.18	1,410
Female	0.709	(0.455)	0.788	(0.409)	0.080	3.38	0.18	1,410
Other	0.001	(0.033)	0.000	(0.000)	-0.001	-1.00	-0.03	1,410
Missing gender	0.002	(0.047)	0.006	(0.076)	0.004	0.96	0.07	1,410
Education								
Less than high school	0.244	(0.430)	0.145	(0.352)	-0.099	-4.68	-0.23	1,410
High school diploma or GED	0.481	(0.500)	0.440	(0.497)	-0.041	-1.48	-0.08	1,410
Some college	0.168	(0.374)	0.263	(0.440)	0.094	4.07	0.25	1,410
College degree or more	0.107	(0.309)	0.153	(0.360)	0.046	2.42	0.15	1,410
Other Demographics								
Age	41.6	(14.8)	39.1	(12.3)	-2.4	-3.27	-0.16	1,410
Kids in household	0.374	(0.484)	0.473	(0.500)	0.098	3.57	0.20	1,410
Divorced	0.090	(0.286)	0.077	(0.267)	-0.012	-0.76	-0.04	1,410
Married	0.066	(0.249)	0.050	(0.219)	-0.016	-1.26	-0.06	1,410
Separated	0.044	(0.205)	0.046	(0.210)	0.003	0.22	0.01	1,410
Single (never married)	0.753	(0.431)	0.788	(0.409)	0.034	1.46	0.08	1,410
Widow	0.030	(0.171)	0.027	(0.162)	-0.003	-0.36	-0.02	1,410
Missing marital status	0.017	(0.129)	0.012	(0.107)	-0.005	-0.83	-0.04	1,410
Race								
Black, Non-Hispanic	0.660	(0.474)	0.676	(0.469)	0.015	0.58	0.03	1,410
White, Non-Hispanic	0.071	(0.256)	0.131	(0.338)	0.060	3.52	0.24	1,410
Hispanic	0.217	(0.413)	0.143	(0.350)	-0.074	-3.58	-0.18	1,410
Multiracial	0.012	(0.110)	0.017	(0.131)	0.005	0.73	0.05	1,410
Non-Black, Non-White, Non-Multiracial	0.013	(0.115)	0.006	(0.076)	-0.008	-1.50	-0.07	1,410
Missing race	0.026	(0.159)	0.027	(0.162)	0.001	0.14	0.01	1,410
Baseline Financial Distress								
Sometimes/often unable to afford food	0.879	(0.326)	0.888	(0.316)	0.009	0.53	0.03	1,410
Baseline Housing & Transit								
Mainly drives	0.416	(0.493)	0.477	(0.500)	0.062	2.26	0.13	1,410
Mainly uses public transportation	0.217	(0.413)	0.236	(0.425)	0.018	0.76	0.04	1,410
Rents home	0.702	(0.458)	0.763	(0.426)	0.060	2.49	0.13	1,410
Owns home	0.102	(0.303)	0.091	(0.288)	-0.010	-0.64	-0.03	1,410
Rents or owns home	0.804	(0.397)	0.853	(0.354)	0.050	2.43	0.13	1,410
Has unstable housing situation	0.093	(0.291)	0.098	(0.298)	0.005	0.32	0.02	1,410
Paid full rent on time	0.439	(0.497)	0.412	(0.493)	-0.027	-0.91	-0.06	1,159
Household income	$16,420	(13,377)	$18,887	(12,710)	$2,450	3.42	0.18	1,410
Baseline Spending								
Housing	$744	(1,330)	$899	(1,576)	$168	2.06	0.13	1,410
Utilities	$199	(272)	$186	(200)	-$11	-0.89	-0.04	1,410
Food	$346	(302)	$370	(284)	$26	1.62	0.09	1,410
Transportation	$254	(298)	$265	(295)	$11	0.66	0.04	1,410
Healthcare	$32	(93)	$25	(71)	-$7	-1.54	-0.07	1,410
Childcare	$39	(127)	$42	(119)	$2	0.36	0.02	1,410
Other	$179	(485)	$189	(438)	$14	0.56	0.03	1,410
Baseline Overall Wellbeing								
Perceived stress score (0 to 40)	21.159	(6.908)	21.867	(6.663)	0.712	1.91	0.10	1,410
Participated in education/training program	0.058	(0.234)	0.102	(0.303)	0.044	2.83	0.19	1,410
Baseline Employment								
Currently employed	0.404	(0.491)	0.488	(0.500)	0.085	3.09	0.17	1,410
Currently employed full time	0.191	(0.393)	0.228	(0.420)	0.037	1.62	0.09	1,410
Joint test of balance (p-value)					0.000			

Notes: Data come from GBI program applications. The table reports baseline characteristics of program applicants by whether or not they completed the follow-up survey. Columns (1) and (2) report each variable's mean and standard deviation among the 892 applicants who were contacted by NORC but who did not complete the survey. Columns (3) and (4) report these statistics for the 518 survey respondents. Column (5) reports the difference between the respondent and non-respondents means. Column (6) reports the t-statistic from the null hypothesis that the difference is zero. Column (7) divides the difference by the standard deviation of the variable among the non-respondents. Column (8) shows the number of applicants in each variable's sample. Income and spending variables are winsorized at the 99th percentile, and negative reported values are changed to zero.

Table A-4: Applicant Characteristics and Baseline Balance, Follow-Up Survey Respondents

			Unweighted		Weighted		
	Control Mean	Treatment Mean	Diff.	t-stat	Diff.	t-stat	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender							
Nonbinary	0.010	0.005	-0.005	-0.74	0.006	0.47	518
Male	0.193	0.206	0.013	0.36	0.040	0.87	518
Female	0.790	0.784	-0.005	-0.15	-0.041	-0.89	518
Other	0.000	0.000	0.000	.	0.000	.	518
Missing gender	0.007	0.005	-0.002	-0.32	-0.005	-0.65	518
Education							
Less than high school	0.170	0.110	-0.060	-1.96	-0.030	-0.66	518
High school diploma or GED	0.440	0.440	0.000	0.00	0.014	0.29	518
Some college	0.240	0.294	0.054	1.35	0.018	0.52	518
College degree or more	0.150	0.156	0.006	0.19	-0.002	-0.09	518
Other Demographics							
Age	39.2	39.1	-0.1	-0.13	-0.6	-0.42	518
Kids in household	0.507	0.427	-0.080	-1.81	-0.098	-2.12	518
Divorced	0.073	0.083	0.009	0.39	0.016	0.62	518
Married	0.057	0.041	-0.015	-0.80	-0.008	-0.35	518
Separated	0.060	0.028	-0.033	-1.84	-0.034	-1.68	518
Single (never married)	0.763	0.821	0.058	1.61	0.037	0.89	518
Widow	0.033	0.018	-0.015	-1.09	-0.005	-0.26	518
Missing marital status	0.013	0.009	-0.004	-0.45	-0.006	-0.69	518
Race							
Black, Non-Hispanic	0.670	0.683	0.014	0.33	-0.006	-0.14	518
White, Non-Hispanic	0.137	0.124	-0.013	-0.43	0.002	0.07	518
Hispanic	0.143	0.142	-0.001	-0.04	0.006	0.14	518
Multiracial	0.017	0.018	0.002	0.14	0.003	0.26	518
Non-Black, Non-White, Non-Multiracial	0.003	0.009	0.006	0.80	0.003	0.49	518
Missing race	0.030	0.023	-0.007	-0.50	-0.007	-0.53	518
Baseline Financial Distress							
Sometimes/often unable to afford food	0.907	0.862	-0.044	-1.53	-0.039	-1.14	518
Baseline Housing & Transit							
Mainly drives	0.497	0.450	-0.047	-1.06	0.003	0.07	518
Mainly uses public transportation	0.220	0.257	0.037	0.97	-0.006	-0.15	518
Rents home	0.773	0.748	-0.025	-0.67	-0.024	-0.52	518
Owns home	0.087	0.096	0.010	0.37	0.003	0.11	518
Rents or owns home	0.860	0.844	-0.016	-0.50	-0.020	-0.52	518
Has unstable housing situation	0.097	0.101	0.004	0.16	0.011	0.33	518
Paid full rent on time	0.415	0.408	-0.007	-0.15	-0.010	-0.20	442
Household income	$19,125	$18,561	-$555	-0.49	-$664	-0.56	518
Baseline Spending							
Housing	$1,033	$715	-$318	-2.52	-$204	-2.41	518
Utilities	$189	$182	-$7	-0.38	-$11	-0.61	518
Food	$389	$343	-$46	-1.88	-$32	-1.26	518
Transportation	$268	$260	-$8	-0.32	-$12	-0.42	518
Healthcare	$25	$23	-$2	-0.33	-$6	-0.81	518
Childcare	$44	$39	-$5	-0.50	-$5	-0.42	518
Other	$183	$197	$14	0.34	-$42	-0.94	518
Baseline Overall Wellbeing							
Perceived stress score (0 to 40)	22.193	21.417	-0.774	-1.30	-0.626	-0.97	518
Participated in education/training program	0.090	0.119	0.029	1.06	0.004	0.16	518
Baseline Employment							
Currently employed	0.513	0.454	-0.059	-1.33	-0.012	-0.24	518
Currently employed full time	0.233	0.220	-0.013	-0.35	0.000	0.01	518
Joint balance tests (p-values)			0.043		0.877		

Notes: Data come from applicants' baseline GBI applications. The sample includes respondents to the 12-month follow-up survey. Columns (1) and (2) report average characteristics among the control group and treatment group, respectively. Columns (3) and (4) report the strata-adjusted treatment-control difference using an unweighted sample and the t-statistic of the test of the null hypothesis that the difference is zero. Columns (5) and (6) report the same using a weighted sample to account for observable selection into survey response. Column (7) reports the number of applicants in each variable's sample. Income and spending variables are winsorized at the 99th percentile, and negative reported values are changed to zero.

Table A-5: Applicant Characteristics and Baseline Balance, Credit Data Match vs. Non-match

	Non-match		Match				Diff./	
	Mean	SD	Mean	SD	(3) – (1)	t-stat	SD	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gender								
Nonbinary	0.009	(0.096)	0.004	(0.065)	-0.005	-2.86	-0.05	11,590
Male	0.345	(0.476)	0.196	(0.397)	-0.149	-16.51	-0.31	11,590
Female	0.641	(0.480)	0.797	(0.402)	0.157	17.16	0.33	11,590
Other	0.001	(0.029)	0.001	(0.028)	-0.000	-0.11	-0.00	11,590
Missing gender	0.004	(0.064)	0.002	(0.042)	-0.002	-2.02	-0.04	11,590
Education								
Less than high school	0.296	(0.457)	0.181	(0.385)	-0.115	-13.24	-0.25	11,590
High school diploma or GED	0.481	(0.500)	0.447	(0.497)	-0.034	-3.45	-0.07	11,590
Some college	0.156	(0.363)	0.220	(0.415)	0.064	8.47	0.18	11,590
College degree or more	0.067	(0.250)	0.152	(0.359)	0.085	14.75	0.34	11,590
Other Demographics								
Age	40.2	(14.5)	41.0	(13.7)	0.9	3.08	0.06	11,590
Kids in household	0.347	(0.476)	0.443	(0.497)	0.096	9.93	0.20	11,590
Divorced	0.073	(0.260)	0.092	(0.289)	0.019	3.46	0.07	11,590
Married	0.047	(0.213)	0.062	(0.241)	0.015	3.30	0.07	11,590
Separated	0.052	(0.221)	0.056	(0.231)	0.005	1.08	0.02	11,590
Single (never married)	0.777	(0.416)	0.740	(0.439)	-0.037	-4.41	-0.09	11,590
Widow	0.035	(0.184)	0.031	(0.174)	-0.004	-1.06	-0.02	11,590
Missing marital status	0.015	(0.123)	0.018	(0.134)	0.003	1.19	0.02	11,590
Race								
Black, Non-Hispanic	0.670	(0.470)	0.683	(0.465)	0.013	1.35	0.03	11,590
White, Non-Hispanic	0.098	(0.297)	0.083	(0.275)	-0.015	-2.63	-0.05	11,590
Hispanic	0.178	(0.383)	0.181	(0.385)	0.003	0.34	0.01	11,590
Multiracial	0.014	(0.116)	0.012	(0.108)	-0.002	-0.79	-0.02	11,590
Non-Black, Non-White, Non-Multiracial	0.017	(0.128)	0.014	(0.118)	-0.003	-1.01	-0.02	11,590
Missing race	0.023	(0.151)	0.028	(0.165)	0.004	1.41	0.03	11,590
Baseline Financial Distress								
Sometimes/often unable to afford food	0.878	(0.328)	0.876	(0.330)	-0.002	-0.32	-0.01	11,590
Baseline Housing & Transit								
Mainly drives	0.250	(0.433)	0.504	(0.500)	0.254	27.96	0.59	11,590
Mainly uses public transportation	0.314	(0.464)	0.196	(0.397)	-0.118	-13.32	-0.25	11,590
Rents home	0.703	(0.457)	0.737	(0.440)	0.035	3.85	0.08	11,590
Owns home	0.034	(0.182)	0.124	(0.329)	0.089	18.73	0.49	11,590
Rents or owns home	0.737	(0.440)	0.861	(0.346)	0.124	15.06	0.28	11,590
Has unstable housing situation	0.148	(0.355)	0.068	(0.251)	-0.080	-12.30	-0.23	11,590
Paid full rent on time	0.424	(0.494)	0.415	(0.493)	-0.010	-0.88	-0.02	9,528
Household income	$13,679	(12,097)	$18,995	(13,257)	$5,322	21.35	0.44	11,590
Baseline Spending								
Housing	$662	(1,204)	$822	(1,290)	$158	6.45	0.13	11,590
Utilities	$151	(239)	$205	(242)	$54	11.39	0.23	11,590
Food	$319	(307)	$366	(299)	$46	7.62	0.15	11,590
Transportation	$169	(236)	$287	(302)	$119	22.96	0.50	11,590
Healthcare	$25	(77)	$33	(87)	$7	4.59	0.10	11,590
Childcare	$32	(110)	$47	(134)	$16	6.64	0.14	11,590
Other	$116	(368)	$196	(484)	$79	9.76	0.22	11,590
Baseline Overall Wellbeing								
Perceived stress score (0 to 40)	21.667	(6.558)	21.244	(6.620)	-0.420	-3.19	-0.06	11,590
Participated in education/training program	0.069	(0.253)	0.084	(0.277)	0.015	2.95	0.06	11,590
Baseline Employment								
Currently employed	0.352	(0.478)	0.497	(0.500)	0.145	14.98	0.30	11,590
Currently employed full time	0.144	(0.351)	0.248	(0.432)	0.104	13.72	0.30	11,590
Joint test of balance (p-value)					0.000			

Notes: Data come from applicants' baseline applications. The table compares applicants who did and did not match to a balanced panel of credit outcomes. Columns (1) and (2) report each variable's mean and standard deviation for applicants who did not match to the credit panel. Columns (3) and (4) report those statistics for the applicants who did match to the credit panel. Column (5) reports the difference in each variable's matched and non-matched group means. Column (6) reports the t-statistic from the null hypothesis that the difference is zero. Column (7) divides the difference by the standard deviation of the variable among the applicants who did not match to the credit panel. Column (8) shows the number of applicants in each variable's sample. Income and spending variables are winsorized at the 99th percentile, and negative reported values are changed to zero.

Table A-6: Applicant Characteristics and Baseline Balance, Credit Data Sample

	Control Mean	Treatment Mean	Unweighted Diff.	Unweighted t-stat	Weighted Diff.	Weighted t-stat	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender							
Nonbinary	0.004	0.003	-0.001	-0.24	-0.000	-0.07	7,926
Male	0.194	0.232	0.038	1.51	0.035	1.20	7,926
Female	0.799	0.765	-0.035	-1.36	-0.032	-1.08	7,926
Other	0.001	0.000	-0.001	-2.45	-0.001	-2.39	7,926
Missing gender	0.002	0.000	-0.002	-3.74	-0.002	-3.65	7,926
Education							
Less than high school	0.182	0.149	-0.033	-1.54	-0.039	-1.53	7,926
High school diploma or GED	0.446	0.443	-0.004	-0.12	0.024	0.78	7,926
Some college	0.219	0.270	0.051	1.92	0.022	0.93	7,926
College degree or more	0.153	0.138	-0.014	-0.69	-0.007	-0.39	7,926
Other Demographics							
Age	41.1	39.9	-1.2	-1.49	-0.9	-0.98	7,926
Kids in household	0.444	0.436	-0.008	-0.26	-0.009	-0.31	7,926
Divorced	0.093	0.076	-0.017	-1.05	-0.011	-0.65	7,926
Married	0.062	0.073	0.011	0.71	0.000	0.04	7,926
Separated	0.057	0.042	-0.016	-1.30	-0.017	-1.47	7,926
Single (never married)	0.738	0.779	0.041	1.63	0.043	1.73	7,926
Widow	0.032	0.021	-0.011	-1.28	-0.005	-0.52	7,926
Missing marital status	0.019	0.010	-0.008	-1.34	-0.010	-1.86	7,926
Race							
Black, Non-Hispanic	0.684	0.633	-0.051	-1.77	-0.025	-0.86	7,926
White, Non-Hispanic	0.082	0.097	0.015	0.84	0.008	0.44	7,926
Hispanic	0.180	0.215	0.035	1.41	0.017	0.74	7,926
Multiracial	0.012	0.010	-0.002	-0.26	-0.001	-0.14	7,926
Non-Black, Non-White, Non-Multiracial	0.014	0.014	-0.000	-0.03	-0.001	-0.17	7,926
Missing race	0.028	0.031	0.003	0.32	0.002	0.18	7,926
Baseline Financial Distress							
Sometimes/often unable to afford food	0.876	0.865	-0.011	-0.54	-0.022	-0.95	7,926
Baseline Housing & Transit							
Mainly drives	0.506	0.481	-0.025	-0.83	-0.001	-0.03	7,926
Mainly uses public transportation	0.196	0.194	-0.002	-0.08	-0.003	-0.12	7,926
Rents home	0.737	0.744	0.006	0.24	-0.000	-0.01	7,926
Owns home	0.125	0.104	-0.021	-1.14	-0.013	-0.73	7,926
Rents or owns home	0.862	0.848	-0.015	-0.68	-0.013	-0.53	7,926
Has unstable housing situation	0.067	0.090	0.023	1.36	0.027	1.30	7,926
Paid full rent on time	0.414	0.433	0.019	0.58	0.016	0.49	6,827
Household income	$19,030	$18,451	-$574	-0.76	-$332	-0.43	7,926
Baseline Spending							
Housing	$828	$696	-$131	-2.61	-$113	-2.55	7,926
Utilities	$206	$190	-$16	-1.14	$2	0.13	7,926
Food	$368	$327	-$40	-2.93	-$29	-1.95	7,926
Transportation	$288	$279	-$9	-0.48	-$5	-0.32	7,926
Healthcare	$33	$25	-$8	-1.69	-$8	-1.97	7,926
Childcare	$48	$44	-$4	-0.49	-$2	-0.34	7,926
Other	$196	$187	-$8	-0.31	-$2	-0.07	7,926
Baseline Overall Wellbeing							
Perceived stress score (0 to 40)	21.251	21.128	-0.119	-0.28	-0.112	-0.26	7,926
Participated in education/training program	0.084	0.087	0.003	0.17	-0.004	-0.23	7,926
Baseline Employment							
Currently employed	0.499	0.436	-0.063	-2.12	-0.031	-1.01	7,926
Currently employed full time	0.249	0.215	-0.034	-1.39	-0.020	-0.82	7,926
Joint balance tests (p-values)			0.075		0.413		

Notes: Data come from applicants' baseline GBI applications. The sample includes individuals who matched to the balanced credit panel. Columns (1) and (2) report average characteristics among the control group and treatment group, respectively. Columns (3) and (4) report the strata-adjusted treatment-control difference using an unweighted sample and the t-statistic of the test of the null hypthesis that the difference is zero. Columns (5) and (6) report the same using a weighted sample to account for observable selection into the credit sample. Column (7) reports the number of applicants in each variable's sample. Income and spending variables are winsorized at the 99th percentile, and negative reported values are changed to zero.

Table A-7: Effect of GBI Payments on Annual Income and Employment

	Unweighted				Weighted		
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary outcome							
Total household income	$17,460	$23,371	$6,182**	$6,569**	$6,480**	$6,891**	39%
	(16,411)	(15,357)	(1,228)	(1,285)	(1,249)	(1,298)	
Income sources							
Earned	$13,984	$13,059	-$1,173	-$1,246	-$779	-$828	-6%
	(16,480)	(15,625)	(1,327)	(1,384)	(1,244)	(1,301)	
Rental	$209	$433	$244+	$259+	$290+	$308+	148%
	(1,206)	(1,868)	(146)	(152)	(153)	(160)	
Investment	$1.000	$0.917	-$0.128	-$0.136	-$0.719	-$0.764	-76%
	(9.966)	(9.556)	(1.013)	(1.059)	(1.138)	(1.191)	
Public assistance	$762	$738	-$3	-$3	-$114	-$121	-16%
	(3,129)	(2,698)	(261)	(273)	(289)	(302)	
GBI program(s)	$147	$5,804	$5,577**	$5,923**	$5,614**	$5,967**	4,057%
	(1,250)	(1,644)	(140)	(103)	(157)	(120)	
Other	$1,920	$2,839	$1,035*	$1,099*	$1,030+	$1,095+	57%
	(5,343)	(6,422)	(517)	(539)	(601)	(626)	
Employment							
Currently employed	0.463	0.537	0.100*	0.106**	0.110**	0.117**	25%
	(0.499)	(0.500)	(0.039)	(0.041)	(0.041)	(0.043)	
Currently employed full time	0.300	0.298	0.000	0.000	-0.003	-0.003	-1%
	(0.459)	(0.459)	(0.037)	(0.039)	(0.038)	(0.040)	
Household Application, Survey Wave FEs			X	X	X	X	
Full Baseline Controls			X	X	X	X	
Observations	300	218	518	518	518	518	

Notes: Data come from the GBI program application and a 12-month follow-up survey. The sample includes all 518 individuals who completed the follow-up survey. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise. $^{**}0.01$, $^{*}0.05$, $^{+}0.10$ significance levels

Table A-8: Effect of GBI Payments on Monthly Spending

	Unweighted				Weighted		
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary outcome							
Total spending	$1,948	$1,980	$102	$109	$151	$162	8%
	(1,348)	(1,313)	(129)	(135)	(163)	(171)	
Spending categories							
Housing	$747	$789	$61	$65	$81	$87	12%
	(545)	(529)	(52)	(55)	(54)	(57)	
Utilities	$199	$209	$12	$13	$21	$22	11%
	(238)	(278)	(25)	(26)	(34)	(35)	
Food	$443	$384	-$30	-$32	$9	$10	2%
	(432)	(475)	(48)	(50)	(60)	(63)	
Transportation	$264	$301	$49^{+}	$52^{+}	$56*	$60*	23%
	(307)	(311)	(27)	(28)	(28)	(29)	
Healthcare	$33.78	$29.86	-$1.21	-$1.29	$4.36	$4.65	14%
	(84.70)	(89.91)	(8.68)	(9.08)	(10.67)	(11.19)	
Childcare	$54.49	$38.41	-$10.61	-$11.35	-$12.47	-$13.33	-24%
	(163.54)	(152.47)	(14.01)	(14.68)	(15.32)	(16.00)	
Other	$207	$228	$29	$31	$18	$20	10%
	(538)	(502)	(47)	(49)	(63)	(67)	
Household Application, Survey Wave FEs			X	X	X	X	
Full Baseline Controls			X	X	X	X	
Observations	259	178	437	437	437	437	

Notes: Data come from the GBI program application and a 12-month follow-up survey. The sample includes all 437 individuals who completed the spending module in the follow-up survey. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise.
$^{**}0.01$, $^{*}0.05$, $^{+}0.10$ significance levels

Table A-9: Effect of GBI Payments on Components of Financial Distress Index

	Unweighted				Weighted		
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary outcome							
Standardized treatment effect	0.000	-0.196	-0.197**	-0.209**	-0.166**	-0.176**	
	(0.538)	(0.510)	(0.046)	(0.048)	(0.050)	(0.052)	
Distress experienced in last 12 months							
Subdomain standardized treatment effect	0.000	-0.130	-0.125*	-0.133*	-0.082	-0.087	
	(0.623)	(0.629)	(0.056)	(0.059)	(0.062)	(0.065)	
Sometimes/often unable to afford food	0.803	0.697	-0.100**	-0.106**	-0.083*	-0.088*	-11%
	(0.398)	(0.461)	(0.038)	(0.039)	(0.038)	(0.040)	
Charged late fee by landlord	0.390	0.353	-0.040	-0.043	-0.032	-0.034	-9%
	(0.489)	(0.479)	(0.043)	(0.045)	(0.046)	(0.048)	
Utilities disconnected	0.290	0.229	-0.059	-0.063	-0.033	-0.035	-12%
	(0.455)	(0.421)	(0.040)	(0.041)	(0.044)	(0.046)	
Evicted or foreclosed	0.110	0.096	-0.012	-0.013	0.006	0.006	5%
	(0.313)	(0.296)	(0.028)	(0.029)	(0.032)	(0.033)	
Worried at all in last 12 months							
Subdomain standardized treatment effect	0.000	-0.266	-0.275**	-0.292**	-0.220**	-0.234**	
	(0.874)	(0.868)	(0.077)	(0.081)	(0.085)	(0.088)	
Eviction or foreclosure	0.617	0.463	-0.158**	-0.168**	-0.134**	-0.143**	-23%
	(0.487)	(0.500)	(0.044)	(0.046)	(0.048)	(0.050)	
Utilities disconnection	0.650	0.546	-0.108*	-0.115*	-0.079+	-0.084+	-13%
	(0.478)	(0.499)	(0.043)	(0.045)	(0.046)	(0.048)	
Anticipated worries over next 3 months							
Subdomain standardized treatment effect	0.000	-0.227	-0.230**	-0.244**	-0.222**	-0.236**	
	(0.624)	(0.548)	(0.052)	(0.055)	(0.054)	(0.057)	
Material hardship	0.257	0.147	-0.109**	-0.115**	-0.121**	-0.129**	-50%
	(0.438)	(0.355)	(0.036)	(0.038)	(0.038)	(0.040)	
Eviction or foreclosure	0.387	0.284	-0.103*	-0.109*	-0.099*	-0.105*	-27%
	(0.488)	(0.452)	(0.042)	(0.044)	(0.044)	(0.047)	
Utilities disconnection	0.460	0.349	-0.116**	-0.123**	-0.079+	-0.084+	-18%
	(0.499)	(0.478)	(0.043)	(0.045)	(0.047)	(0.050)	
Prefers present to future funds							
Selected 25 dollar survey incentive	0.217	0.124	-0.094**	-0.099**	-0.104**	-0.111**	-51%
	(0.413)	(0.330)	(0.033)	(0.034)	(0.032)	(0.034)	
Household Application, Survey Wave FEs			X	X	X	X	
Full Baseline Controls			X	X	X	X	
Observations	300	218	518	518	518	518	

Notes: Data come from the GBI program application and a 12-month follow-up survey. The sample includes all 518 individuals who completed the follow-up survey. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise. $^{**}0.01$, $^{*}0.05$, $^{+}0.10$ significance levels

Table A-10: Effect of GBI Payments on Overall Well-Being

	Unweighted				Weighted		
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary outcome							
Standardized treatment effect			0.156**	0.166**	0.144*	0.153**	
			(0.055)	(0.057)	(0.057)	(0.059)	
Mental health							
Perceived stress score (from 0 to 40) (R)	20.82	17.66	-2.83**	-3.00**	-2.85**	-3.03**	-15%
	(7.02)	(7.80)	(0.61)	(0.63)	(0.66)	(0.69)	
Life satisfaction ladder (from 0 to 10)	5.10	5.77	0.67**	0.71**	0.59*	0.62*	12%
	(2.29)	(2.27)	(0.21)	(0.22)	(0.23)	(0.24)	
Physical health							
Excellent/very good self-reported health	0.230	0.261	0.017	0.018	0.019	0.020	9%
	(0.422)	(0.440)	(0.038)	(0.040)	(0.041)	(0.043)	
Somewhat/much better health compared to last year	0.363	0.468	0.093*	0.098*	0.096*	0.102*	28%
	(0.482)	(0.500)	(0.044)	(0.046)	(0.048)	(0.050)	
Career readiness							
Participated in education/training program	0.187	0.142	-0.058+	-0.062+	-0.073*	-0.077*	-41%
	(0.390)	(0.350)	(0.031)	(0.032)	(0.031)	(0.032)	
Household Application, Survey Wave FEs			X	X	X	X	
Full Baseline Controls			X	X	X	X	
Observations	300	218	518	518	518	518	

Notes: Data come from the GBI program application and a 12-month follow-up survey. The sample includes all 518 individuals who completed the follow-up survey. Outcomes marked with "(R)" are reverse-coded when constructing the standardized treatment effect. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise.
**0.01, *0.05, +0.10 significance levels

Table A-11: Post-Program Effect of GBI Payments on Average Credit Outcomes (Q5 through Q9), Winsorized at 99th Percentile

	Raw Data			Winsorized (99th)		
	Unweighted		Weighted	Unweighted		Weighted
	Control Mean (1)	Treatment Mean (2)	TOT Effect Full Controls (3)	Control Mean (4)	Treatment Mean (5)	TOT Effect Full Controls (6)
Balance in collections	\$2,187	\$2,525	\$496^{+}	\$2,125	\$2,455	\$492^{+}
	(3,547)	(5,023)	(283)	(3,152)	(4,552)	(256)
Balance in derogatory accounts	\$1,247	\$1,139	\$211	\$1,101	\$1,119	\$156
	(4,499)	(2,523)	(197)	(2,440)	(2,410)	(180)
Credit card balance	\$1,453	\$1,733	-\$11	\$1,360	\$1,717	\$119
	(3,808)	(3,933)	(174)	(3,100)	(3,850)	(169)
Personal installment loan balance	\$667	\$591	\$12	\$592	\$561	\$35
	(2,880)	(2,414)	(116)	(2,155)	(2,174)	(110)
Revolving debt balance	\$2,022	\$2,362	\$94	\$1,816	\$2,339	\$355
	(6,491)	(5,604)	(279)	(4,566)	(5,471)	(259)
Student loan balance	\$3,430	\$5,155	\$1,126	\$3,032	\$4,962	\$1,458^{+}
	(13,110)	(19,631)	(772)	(9,271)	(18,252)	(767)
Total balance (excl. auto/mortgage)	\$5,845	\$7,830	\$1,300	\$5,434	\$7,624	\$1,674*
	(14,321)	(20,903)	(828)	(10,733)	(19,514)	(823)
Number of individuals	7,637	289	7,926	7,637	289	7,926

Notes: Data come from the GBI program application and administrative credit data. The sample includes all 7,926 individuals who matched to the balanced panel of credit outcomes. Columns (1) through (3) report results using the credit records. Columns (4) through (6) report results after winsorizing outcomes at the 99th percentile. Standard deviations are reported below means in parentheses, and heteroskedasticity-robust standard errors are reported below effect estimates in parentheses.
**0.01, *0.05, $^{+}$0.10 significance levels

Table A-12: Effect of GBI Payments on Financial Well-Being

	Unweighted				Weighted		
	Control Mean	Treatment Mean	ITT Effect Full Controls	TOT Effect Full Controls	ITT Effect Full Controls	TOT Effect Full Controls	Percent Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Primary outcome							
Standardized treatment effect			0.319**	0.339**	0.307**	0.326**	
			(0.044)	(0.046)	(0.043)	(0.045)	
Financial progress							
Made progress toward goals	0.363	0.706	0.335**	0.356**	0.341**	0.362**	100%
	(0.482)	(0.456)	(0.042)	(0.044)	(0.045)	(0.047)	
Has checking account	0.563	0.881	0.320**	0.340**	0.336**	0.357**	63%
	(0.497)	(0.325)	(0.036)	(0.038)	(0.038)	(0.040)	
Has savings account	0.170	0.220	0.042	0.045	0.030	0.031	18%
	(0.376)	(0.415)	(0.034)	(0.036)	(0.032)	(0.033)	
Has any savings	0.117	0.303	0.175**	0.186**	0.174**	0.185**	158%
	(0.322)	(0.461)	(0.035)	(0.037)	(0.037)	(0.039)	
Has more savings than last year	0.090	0.266	0.166**	0.176**	0.155**	0.165**	183%
	(0.287)	(0.443)	(0.034)	(0.035)	(0.035)	(0.036)	
Savings amount	$127	$429	$252**	$268**	$228**	$242**	191%
	(697)	(1,251)	(88)	(92)	(71)	(74)	
Financial strain							
Paid any overdraft fees (R)	0.323	0.443	0.128**	0.136**	0.155**	0.165**	51%
	(0.468)	(0.498)	(0.045)	(0.047)	(0.047)	(0.049)	
Rolled over payday loan (R)	0.077	0.061	-0.014	-0.014	-0.011	-0.012	-15%
	(0.267)	(0.240)	(0.024)	(0.025)	(0.025)	(0.026)	
Applied for payday or title loan (R)	0.117	0.060	-0.050*	-0.053*	-0.059*	-0.062*	-54%
	(0.322)	(0.237)	(0.025)	(0.026)	(0.025)	(0.026)	
Household Application, Survey Wave FEs			X	X	X	X	
Full Baseline Controls			X	X	X	X	
Observations	300	218	518	518	518	518	

Notes: Data come from the GBI program application and a 12-month follow-up survey. The sample includes all 518 individuals who completed the follow-up survey. Outcomes marked with "(R)" are reverse-coded when constructing the standardized treatment effect. Columns (1) and (2) report the means for control and treatment group respondents. Columns (3) and (5) report intent-to-treat (ITT) effects from Equation (1) in Section 1.5 using an unweighted sample and a weighted sample that accounts for observable attrition, respectively. Columns (4) and (6) similarly report the treatment-on-the-treated (TOT) estimates where treatment assignment is used to instrument for GBI receipt. Column (7) reports the percent change of the TOT effect in column (6) relative to the control group mean. Column (8) reports the number of observations included in each regression between the treatment and control groups. Values in parentheses are standard deviations in columns (1) and (2) and heteroskedasticity-robust standard errors otherwise. Savings amount is winsorized at the 99^{th} percentile.
$^{**}0.01$, $^{*}0.05$, $^{+}0.10$ significance levels