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# Offsetting the Earnings Disincentive in Public Housing: Evidence from a Behaviorally Informed Field Intervention

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# Offsetting the Earnings Disincentive in Public Housing: Evidence from a Behaviorally Informed Field Intervention*


## Abstract

Income-based rents in public housing create an earnings disincentive. We collaborate with a public housing authority to design a behaviorally informed program that returns part of the rent induced by higher earnings to residents. Importantly, the program automatically enrolled households and was explicitly designed to make the increased payoff to working salient. Using a difference-in-differences approach, we estimate that annual household-head earnings rise 17% ($1,370/year) and public assistance falls 7.5%, with impacts on both intensive and extensive margins. These results provide evidence that an in-work benefit designed for salience can offset the earnings disincentive and affect follow-through labor market behavior.



## JEL classification

D91, I38, J22, R38

## Keywords

labor supply, in-work benefits, salience, public housing



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# 1 Introduction

Housing costs have risen rapidly, outpacing wage growth and straining affordability. In the United States, more than 90% of Americans live in counties where housing costs have increased faster than incomes over the past two decades; from 2010 to 2022, home prices rose 74% while average wages grew 54% (U.S. Department of the Treasury, 2024; Federal Housing Finance Agency, 2023; U.S. Bureau of Labor Statistics, 2023). Across the European Union, home prices increased 53% between 2015 and 2024, a large acceleration from the more gradual increases seen in previous decades (European Parliament, 2024). Affordability pressures are widespread: 61% of Americans report being "very concerned" about housing costs, nearly 50% of renters spend more than 30% of their income on rent, evictions affect more than two million U.S. households annually, and a record high of 770,000 people experienced homelessness on a single night in 2024 (Pew Research Center, 2025; U.S. Census Bureau, 2024; Collinson et al., 2024; U.S. Department of Housing and Urban Development, 2024*a*).

One of the primary policy tools to combat housing instability is public housing. In the U.S., 1.6 million people live in government-run public housing, with millions more living in voucher-supported housing and other forms of rental assistance (U.S. Department of Housing and Urban Development, 2024*b*). Rent is typically set at around 30% of income in order to make it affordable for low-income families (U.S. Department of Housing and Urban Development, 2025). However, research has identified an unintended consequence of living in public housing: it lowers labor force participation and earnings. Randomized housing lottery studies have consistently documented these effects, finding 6% decreases in labor force participation and 10% decreases in earnings in the U.S. (Jacob and Ludwig, 2012), and 8% and 13% decreases respectively in the Netherlands (Van Dijk, 2019). A longitudinal matching study using the Survey of Income and Program Participation similarly finds 15–17% decreases in earnings (Susin, 2005).

What causes these effects? One reason is that when rent is fixed as a proportion of monthly income, every additional dollar of earnings faces an effective marginal tax of 30% through higher rent. Not only is this an earnings disincentive in its own right, but it also interacts with the income phase-outs of other means-tested programs, meaning that low-income residents may lose access to other public assistance benefits as they earn more (also known as "benefits cliffs") (Moffitt, 2002; Altig et al., 2020). Recent work in economics studies how to mitigate this problem. This study incorporates two prominent suggestions: reducing the sensitivity of tenant payments to income (Dauth, Mense and Wrede, 2024; Zhang, 2025) and increasing awareness of program incentives (Chetty, Friedman and Saez,

2013; Kleven, 2024). However, previous work on in-work benefits—programs that provide low-income individuals with additional money for working—and the broader literature on behavioral change indicate that it is difficult for these programs to affect follow-through labor market behavior (Chetty, Friedman and Saez, 2013; Linos et al., 2022; Yang et al., 2022; Kleven, 2024; Dykstra, O'Flaherty and Whillans, 2025).

We study the first in-work benefit program, to our knowledge, that is explicitly designed to make follow-through salient. Specifically, we collaborate with a public housing authority to design a behaviorally informed program that returns a portion of rent increases to residents. This program, called the Rent-to-Save Pilot Demonstration (henceforth, RTS), places these returns into an escrow account, which residents receive as a lump sum cash transfer at the end of the program period. We target heads of household with outreach throughout the program to make these features salient. The program automatically enrolls all residents at two large family housing sites, removing the selection problem that complicates the evaluation of similar policy initiatives, which typically require individuals to actively opt in. We exploit this exogenous variation in program participation to compare the earnings trajectories of 223 automatically enrolled participants to 1,848 non-participants across 40 housing sites using a dynamic difference-in-difference design.

We find that the program significantly increases earnings. Specifically, we estimate that automatic enrollment into RTS leads to a 17% increase in annual household head earnings, representing approximately $1,370 in additional earnings per year relative to the control mean of $8,000. Given the 10–17% earnings reductions documented in prior work, this suggests that the RTS program successfully offsets the earnings disincentive inherent in income-based rent. These gains in earnings are accompanied by a 7.5% ($650) decrease in income from public assistance benefits, resulting in a 4% ($700) increase in total income per year relative to the control mean of $16,700. We find no evidence of spillover effects on other household adults, whose income remains unchanged. In addition, we find impacts on both the intensive and extensive margins, including a 6.4 percentage point increase in labor force participation among prior non-workers; this also comparably offsets the 6–8% decrease in labor force participation found in previous work. Finally, our welfare analysis shows that fiscal savings exceed program costs, so that the program generates a net savings for the government.

There remains sparse causal evidence that in-work benefits programs are effective, with much of the work dating to 1990s waiver-era programs (Eissa and Liebman, 1996; Eissa and Hoynes, 2004; Meyer and Rosenbaum, 2001; Card and Hyslop, 2005; Grogger and Karoly, 2005; Low et al., 2025). Indeed, the earlier evidence that showed labor supply effects has been called into question: Kleven (2024) finds that, apart from the 1993 reform, EITC

expansions are not associated with robust effects on employment, and that even the 1993 results may reflect concurrent welfare reform and macroeconomic conditions rather than the EITC itself. Experimental evidence points in the same direction: randomized trials of Paycheck Plus, an in-work benefits program for workers without dependent children, found at most modest employment effects in New York City and no significant effects in Atlanta (Miller et al., 2018; Yang et al., 2022). One explanation for these limited effects comes from Chetty, Friedman and Saez (2013), who use neighborhood-level tax data to show that earnings responses to the EITC are concentrated in neighborhoods where people actually understand the tax schedule—using the share of filers who bunch at EITC kink points as a proxy for local awareness—suggesting that understanding of the incentive, rather than the incentive alone, may drive behavior. Yet to our knowledge, no study has examined an in-work benefit program that is explicitly designed to make the incentive salient.

A separate literature shows that awareness and simplicity are first-order determinants of whether people take up the benefits available to them (Currie, 2006; Bettinger et al., 2012; Bhargava and Manoli, 2015; Herd and Moynihan, 2018; Finkelstein and Notowidigdo, 2019). However, there is limited evidence on effects on longer-run behavior that requires action beyond initial participation. Automatic enrollment, one of the most effective tools for increasing program participation, is successful for set-and-forget behaviors: behaviors where individuals take an action once and do not need to do anything further, such as enrolling in a retirement savings plan (Madrian and Shea, 2001; Chetty et al., 2014; Jachimowicz et al., 2019). However, there is growing evidence that simplifying initial participation does not on its own translate into follow-through behavior, including for in-work benefits (Guyton et al., 2017; Linos et al., 2022; Dykstra, O'Flaherty and Whillans, 2025).[1] While RTS uses automatic enrollment to prevent initial obstacles to participation, core features of the program were aimed at maintaining salience beyond enrollment. This paper provides evidence that a behaviorally informed program can affect follow-through labor market behavior, not just initial participation.

More broadly, we contribute to a growing literature on the design of housing assistance policy, including structural analyses of housing policy reform (Keane and Moffitt, 1998; Waldinger, 2021; Zhang, 2025) and reviews of housing assistance programs (Olsen, 2003; Collinson, Ellen and Ludwig, 2019), by showing that a behaviorally informed modification to the income-based rent structure can offset its earnings disincentive. There is very lit-

[1] Linos et al. (2022) find that six nudge interventions designed to increase EITC take-up had no effect on filing, and Guyton et al. (2017) find that IRS reminders increase EITC filing in the current year but effects do not persist the following year without additional reminders. More broadly, Dykstra, O'Flaherty and Whillans (2025) review evidence from tax, environmental, and civic domains showing that simplifying participation often fails to produce sustained behavioral change.

tle causal evidence on in-work benefits in the context of public housing specifically, with recent evaluations finding no effects on employment or earnings (Freedman, Verma and Vermette, 2023; Riccio et al., 2024). This represents a significant gap given the importance of public housing, the unique earnings disincentive created by income-based rent, and that this is generally a low-knowledge population with limited pre-existing awareness of program incentives.

The findings of this paper show that a policy intervention can counteract earnings disincentives in public housing, a context with otherwise high behavioral inertia and administrative friction. More broadly, our results suggest that a behaviorally informed in-work benefit can produce sustained labor market responses, even in a population where prior work would predict limited effects, with implications for the design of housing policy and in-work benefits in general.

# 2 Background

The Cambridge Housing Authority (CHA) launched the three-year RTS Pilot Demonstration in Cambridge, Massachusetts in 2016. CHA is a Moving to Work public housing agency, a status that provides selected public housing authorities with waivers of standard rules and funding flexibility to design and test new initiatives. CHA implemented the program in two large general-occupancy sites representative of the CHA housing portfolio. The program was embedded within routine public-housing operations, including within existing rent collection, with no changes to standard operating procedure other than implementation of the program. We launched the program in partnership with Compass Working Capital, a Boston-based non-profit that provides financial coaching. We collaborated with CHA and Compass on program design and implementation details, including running early focus groups; developing a behaviorally informed program; designing behaviorally informed account statements and other outreach; and contributing to survey design and collection.

## *2.1 The Rent-to-Save Program*

The RTS program automatically enrolled all resident households at two housing sites, who each received an escrow account. This account accrued funds through two mechanisms. First, every household received a monthly credit equal to 1% of their total rent regardless of income changes. Second, households whose incomes increased during the demonstration period received funds equal to 50% of any rent increase. To access their accumulated escrow accounts at the end of the program, households at both sites had to complete an exit survey.

In addition, residents at one site were also required to complete six months of financial coaching through Compass, with the option to request a waiver.

### *2.2 Salience of the Program*

The RTS program was designed not only to alter the financial return to work, but to ensure that participants understood and internalized these altered incentives. Behavioral economics provides extensive evidence that the effects of economic incentives depend critically on whether individuals are aware of and attend to them (Chetty, Looney and Kroft, 2009; Taubinsky and Rees-Jones, 2018). In the context of public benefits, studies repeatedly document that eligible individuals fail to take up programs—or fail to respond to them behaviorally—due to information frictions, hassle costs, and inattention (Smeeding, Phillips and O'Connor, 2000; Phillips, 2001; Maag, 2005; Jones, 2010; Chetty, Friedman and Saez, 2013; Bhargava and Manoli, 2015; Nichols and Rothstein, 2016; Linos et al., 2022). Making incentives salient—that is, prominent and easy to understand at the moment of decision—can be as consequential for behavior as the incentive itself (Chetty, Friedman and Saez, 2013). These patterns are consistent with broader theories of administrative burden and benefit take-up that emphasize the role of defaults, information frictions, and transaction costs in shaping economic behavior (Currie, 2006; Herd and Moynihan, 2018; Dykstra, O'Flaherty and Whillans, 2025; Dykstra et al., 2025). Each core design feature of the RTS program was informed by this evidence.

*Automatic enrollment.* All residents at the two participating housing sites were automatically enrolled, rather than requiring an opt-in decision. The literature on defaults demonstrates that automatic enrollment profoundly shapes participation in economic programs. In the canonical example of retirement savings, automatic enrollment in 401(k) plans raises participation rates from roughly 40% to over 85%, with the vast majority of enrollees remaining at the default contribution rate (Madrian and Shea, 2001). Importantly, defaults do not operate simply by reducing paperwork: they exploit status quo bias and shape whether a program ever enters an individual's decision-making process. Chetty et al. (2014) distinguish between "active" and "passive" savers, finding that the majority of individuals are passive and that their behavior is governed largely by defaults rather than by active optimization over financial incentives. In our setting, automatic enrollment ensured that RTS was not filtered out at the participation stage, where closely related programs lose the vast majority of their potential participants (see Section 2.3).

*The universal 1% monthly credit.* Every enrolled household received a monthly credit equal to 1% of their total rent, regardless of whether their income changed. This feature

served several behavioral functions. First, it addressed what might be called a "cold start" problem common to incentive programs: many in-work benefit schemes only generate returns *after* an individual increases their earnings, meaning the incentive is invisible to those who have not yet changed their behavior. The 1% credit made the program tangible from the outset for all participants, including those not yet actively increasing their earnings. Second, once funds began accumulating, the growing balance likely activated endowment effects: the well-documented tendency for individuals to value assets they already possess more strongly than equivalent prospective gains (Kahneman, Knetsch and Thaler, 1990). Rather than weighing a hypothetical future reward, participants could observe a concrete and growing stake. Third, the monthly credit ensured that every quarterly statement displayed a nonzero and growing balance, creating a recurring point of engagement. Without this feature, households whose earnings had not changed would have received statements showing a zero balance, effectively a signal that the program was irrelevant to them.

*Escrow accounts and behaviorally informed quarterly statements.* Rather than reducing rent directly, the program deposited returns into separate escrow accounts. This leverages mental accounting, the tendency for individuals to treat money differently depending on how it is categorized or labeled (Thaler, 1999). By segregating the benefit into a distinct, visible account, the program made the reward psychologically salient in a way that a small reduction in monthly rent likely would not have achieved. Heads of household received quarterly account statements showing their growing balance, designed to be simple, easy to understand, and compelling (examples of the account statements, information sheets, and open house flyers can be found in Appendix C). The importance of reminders for sustaining attention to financial goals is well-established: Karlan et al. (2016) show that simple reminders substantially increase savings behavior, with the largest effects for reminders that reference specific goals. In the RTS context, each quarterly statement served as a goal-referenced reminder, displaying the accumulated balance and reinforcing its concrete connection to the participant's earnings decisions.

*Outreach through routine interactions and social networks.* Beyond the account statements, we conducted additional outreach: explanations of the program during standard income recertifications, mailed postcards, flyers distributed at housing sites, and on-site community meetings. We embedded program information within interactions that residents were already required to complete, such as the recertifications, reducing the marginal attention cost of learning about the program; this is consistent with evidence that in-person assistance at the point of decision is substantially more effective than providing equivalent information through other channels (Bettinger et al., 2012; Finkelstein and Notowidigdo, 2019). Finally, universal enrollment within the housing sites was intended to encourage in-

formation diffusion through residents' social networks within the buildings. Duflo and Saez (2003) find that social interactions are a primary channel through which information about retirement savings plans spreads, with individuals significantly more likely to enroll when their peers have enrolled. In the RTS setting, universal enrollment meant that the program could become common knowledge within a building.

### *2.3 Comparison to the Family Self-Sufficiency Program*

The RTS program builds on the federal Family Self-Sufficiency (FSS) program, which Congress created in 1990 among a suite of programs to promote economic independence for public housing residents. The program operates through partnerships between local housing authorities and Program Coordinating Committees—typically city agencies, colleges, universities, or financial service providers—and has become a permanent fixture of federal housing policy. Today, HUD sponsors FSS programs in over 600 housing authorities. Yet the program remains underutilized, with participation reaching only about 3% of eligible households.

RTS reimagined several key features of the traditional FSS model. Most fundamentally, it replaced FSS's opt-in structure with automatic enrollment, eliminating the need for households to make an affirmative decision to participate. The savings mechanisms also differed significantly. While FSS deposits the full rent increase from earnings growth into escrow accounts, RTS took an approach that provided all enrolled residents with funding but still incentivized earnings: all households received a universal 1% monthly credit regardless of income changes, plus 50% of any rent increases. The programs also operated on different timelines, three years for RTS versus five for FSS. Finally, the RTS incorporated regular, behaviorally informed account statements and targeted outreach to keep participants informed of their escrow balances. These design departures were largely motivated by the behavioral considerations detailed in Section 2.2.

## 3 Data and Empirical Strategy

### *3.1 Data*

We estimate the effect of automatic enrollment into the RTS program using administrative data from CHA. This data covers the universe of households who live in a CHA housing site from 2012 to 2019 and includes demographic and income data for every household member.[2]

[2]Residents are required to provide official documentation of income at each recertification—including pay stubs, W-2 forms, tax returns, and benefits statements—which CHA cross-checks against federal wage records from the National Directory of New Hires and the Social Security Administration.

We also have descriptive data from two surveys conducted in the years 2017 and 2019.[3]

In the administrative data, we observe 3,308 households living in 42 housing sites that appear at least once between 2012–2019. We make the following sample decisions: first, we keep all households that are in our sample for at least one pre-treatment year and at least one post-treatment year. Second, we drop households that move between treated and control housing sites over the sample period due to potential selection into or out of treatment. The sample we use for the main analysis is an unbalanced panel with 15,207 household-year observations living in 40 housing sites. The sample size is 10,896 household-year observations if we balance the panel (i.e., if we keep only 1,362 unique households that appear every year). However, we also present the results using the balanced panel, which are statistically robust and show similar effect sizes.

Our analysis focuses on the head of households. This is for two reasons: first, the escrow account is registered in their name, they conduct the income recertifications, and they alone receive the targeted outreach. Second, household heads' income as a share of total household income is large at 88%. We analyze spillover effects on non-head of household earnings in Section 4.1.2.

In 2015, the year before the RTS program started, CHA administrative data shows that the average head of household was 61 years old, lived in a two-person household, and paid $424 in rent.[4] Their income was $16,706, out of which $8,256 was earnings (i.e., income from labor). Using the 2017 survey data, we can also qualitatively characterize CHA residents' economic circumstances: using the Consumer Financial Protection Bureau's financial well-being score, most households cluster around scores of 45 to 55, which generally indicates limited liquid savings and a difficulty making ends meet (see Appendix B.2 for the full distribution). Only 20% reported having saved during the preceding year, whereas 71% had not (the remaining 9% did not answer).[5] Together, these figures indicate that CHA residents generally face income constraints and have difficulty building up assets.

Full descriptive statistics by treatment and control group can be found in Appendix A.1. While there are differences in income levels between treatment and control groups at baseline, our difference-in-differences approach only requires that income follows a similar trend across groups before the treatment. Appendix A.2 allows us to visually assess the plausibility of the parallel trend assumption, which holds for the pre-treatment period.

[3]For more information about these surveys, see Appendix B.

[4]This rent is much lower than the median rent over 2012-2016 in the area of $1,700 (Opportunity Insights and U.S. Census Bureau, 2018).

[5]Among those who failed to save, the main obstacles were medical expenses (33%) and day-to-day household bills (28%), followed by debt payments (21%), insufficient income (7%), and the cost of childcare (3%); 8% gave no specific reason.

### *3.2 Empirical Strategy*

Standard economic theory offers guidance on how to consider what determines an individual's labor supply decisions. In the simplest models, an individual makes trade-offs between leisure and work, subject to a feasibility constraint, based on their wage rate, the prevailing cost of goods, and their preferences. The RTS program enters this model by affecting the wage rate. Whereas unenrolled residents face an effective marginal tax of 30% on additional earnings, this is cut in half for residents who enroll. This makes additional work hours more attractive relative to leisure. Of course, many more factors are relevant to an individual making the choice to work more, including whether they are aware of and understand their wage rate and how earning more might affect their eligibility for other social programs (e.g., Murray, 1980; Leonesio, 1988; Moffitt, 2002; Van Dijk, 2019).

In order to estimate the average treatment effect of being automatically enrolled into the RTS program on income, we want to compare the income of enrolled households to the counterfactual where those same households were not enrolled. Since this counterfactual cannot be observed, we provide quasi-experimental evidence assuming that program assignment to housing sites is as good as random for any given household. We employ a dynamic difference-in-differences (DD) approach to analyze the relative evolution of outcomes while controlling for individual fixed effects and time trends. We estimate the following equation:

$$y_{ist} = \sum_{t=-P}^{T} \delta_t (Treat_s \times Time_t) + \gamma_t + \lambda_i + \epsilon_{ist} \tag{1}$$

where $y_{ist}$ denotes the outcome of individual $i$ in year $t$, living in housing site $s$. The variable $Treat_s$ equals 1 if individual $i$ was living in a housing site $s$ that automatically opened escrow accounts for residents. Indicator variables $Time_t$ measure the years relative to the start of the RTS program in 2016. The coefficients $\delta_0, \ldots, \delta_T$ capture the dynamic treatment effects, with each $\delta_t$ representing the effect of the program in year $t$. The coefficient $\delta_{-1}$ is normalized to zero, so that all effects are measured relative to 2015. The remaining pre-treatment coefficients $\delta_{-P}, \ldots, \delta_{-2}$ estimate anticipation effects in the years leading up to the program's implementation. $\gamma_t$ are year fixed effects, which control for time-varying factors that affect all individuals in the sample, while $\lambda_i$ are individual fixed effects, controlling for time-invariant individual characteristics. The error term is denoted as $\epsilon_{ist}$. We complement our event study analysis with static DD estimates that summarize the treatment effect across all post-treatment years. This approach uses the same specification but replaces the event study indicators with a single interaction term $Treat_s \times Post_t$, where $Post_t$ equals 1 from 2016 onward.

The main assumption underlying equation 1 is that individuals residing in control housing sites represent an accurate counterfactual trend of treated residents had they not participated in the RTS program. The coefficients $\delta_{-P}, \ldots, \delta_{-2}$ test for pre-treatment relative trends. If these estimates are economically small and statistically indistinguishable from zero, it suggests that there is no selection on trends that bias our results.

To address potential serial correlation in our outcomes, we cluster standard errors by housing site in our main results. However, because we have only two treated housing sites, cluster-robust standard errors may be too small and thus lead us to overreject the null (Conley and Taber, 2011; MacKinnon and Webb, 2018, 2020; MacKinnon, Nielsen and Webb, 2023; Alvarez, Ferman and Wüthrich, 2025). To assess the possibility of overrejection, we also generate $p$-values using the permutation approach of Ferman and Pinto (2019). This method adjusts placebo estimates based on residual variance to account for heteroscedasticity due to differences in housing site size.[6] We arrive at similar conclusions with both inference methods.

# 4 The Effect of the Rent-to-Save Program on Income

## *4.1 Results*

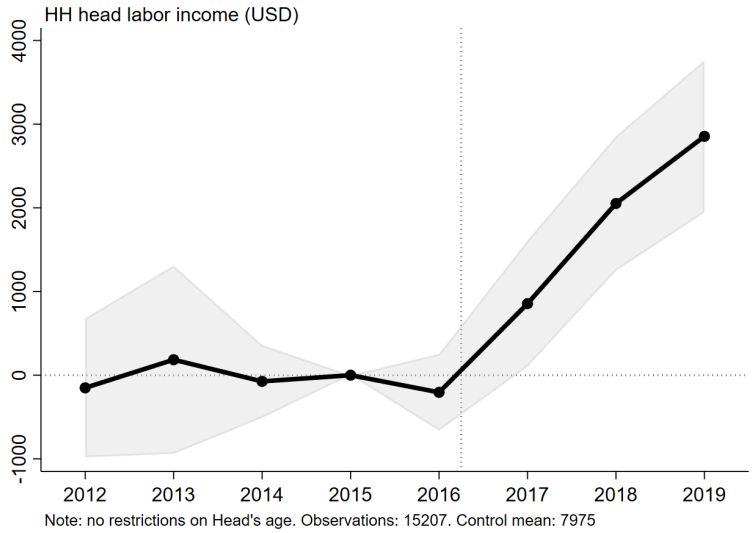
This section evaluates whether automatic enrollment in the RTS program alters labor market behavior among public housing residents. We treat annual earnings as the principal margin through which the program can affect behavior and estimate both dynamic and average treatment effects.

### 4.1.1 Effect on Head-of-Household Income

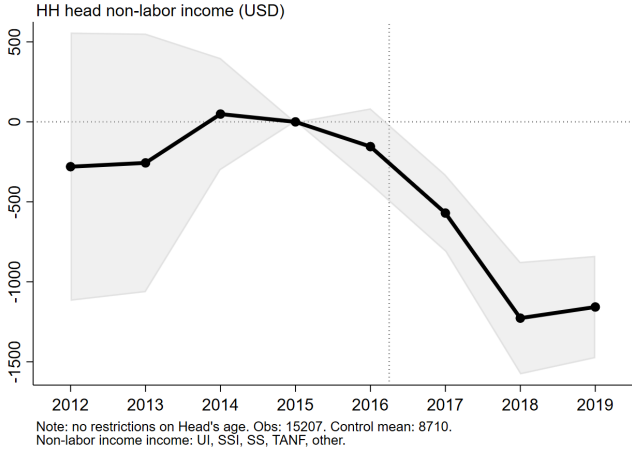
Figure 1 presents the main results for earnings (Panel 1a), non-labor income (Panel 1b), and total income (Panel 1c). Each of these figures plot the estimated $\delta_t$ coefficients from Equation 1 and the associated 95% confidence intervals. The coefficients represent the change in outcomes for individuals automatically enrolled in the RTS program relative to individuals

[6] With many treated and many control groups cluster-robust variance estimators (CRVE) at the group level are appropriate to allow for unrestricted intragroup correlation (Bertrand, Duflo and Mullainathan, 2004). With a small number of groups, it may be possible to obtain reliable inference using methods such as Wild Cluster Bootstrap (Cameron, Gelbach and Miller, 2008). However, these methods do not perform well when the number of treated groups is too small (MacKinnon and Webb, 2018). There are alternative inference methods that are valid with very few treated groups, but rely on some sort of homoskedasticity assumption in the group × time aggregate model (MacKinnon and Webb, 2020). This assumption would be too restrictive in our DD setting because the housing sites differ in size (see Appendix A.3). Thus, we implement Ferman and Pinto (2019)'s inference method that works in DD settings with few treated and many control groups in the presence of heteroskedasticity, e.g. variation in group sizes.

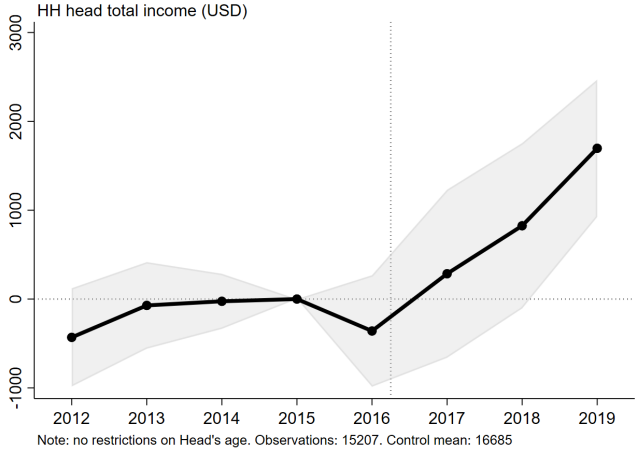
Figure 1: Effects of the Rent-to-Save program on head of household income




(a) Earnings




(b) Non-labor income




(c) Total income

Notes: These figures plot the dynamic estimates of Equation 1 and the associated 95% confidence intervals. The coefficients represent the change in outcomes for individuals automatically enrolled in the RTS program relative to individuals not automatically enrolled, with respect to the year immediately before the start of the program. Panel (a) shows changes in earnings; panels (b) and (c) show non-labor and total income respectively. All values are in USD and reflect differences relative to 2015, comparing those automatically enrolled in the RTS program to those not enrolled.

not automatically enrolled, with respect to the year immediately before the start of the program.

Prior to the RTS program, income trended similarly across the two groups: the coefficients

$\delta_{-t}$ are close to zero and not statistically significant. Starting the first year post-treatment (2017), we observe earnings increase among individuals in treated housing sites relative to non-treated housing sites. Post-treatment, earnings rise but this increase in income is partly offset by declines in non-labor income, leaving total income higher.

Table 1 presents the average difference-in-differences estimates for the three outcomes. We find a large increase in earnings associated with the RTS program with gains of about \$1,368 (17% relative to the control mean) each post-program year ($p_{\text{CR}} = 0.004$; $p_{\text{FP}} = 0.003$).[7] We find that the increase in earnings is offset by a decline in non-labor income—Social Security, Supplemental Security Income, and Temporary Assistance for Needy Families—of about \$653 (7.5% decrease, $p_{\text{CR}} = 0.005$; $p_{\text{FP}} = 0.009$), with a positive effect on total income of \$715 (4.3% increase, $p_{\text{CR}} = 0.048$; $p_{\text{FP}} = 0.046$). These estimates are robust to both standard cluster-robust inference and the permutation-based procedure of Ferman and Pinto (2019), which accounts for the small number of treated clusters. We also present the results using the balanced panel, which are statistically robust and show similar effect sizes, in Appendix A.4.[8]

Table 1: The effect of the Rent-to-Save program on head of household income

| | (1) Earnings | (2) Non-labor income | (3) Total income |
|---|---|---|---|
| $Treat_s \times Post_t$ | 1367.924*** | -653.180*** | 714.744** |
| | (443.459) | (216.847) | (349.758) |
| Observations | 15207 | 15207 | 15207 |
| Control mean | 7975.031 | 8710.283 | 16685.314 |
| % control mean | 17.153 | -7.499 | 4.284 |
| Cluster-robust $p$-values | 0.004 | 0.005 | 0.048 |
| Ferman–Pinto $p$-values | 0.003 | 0.009 | 0.046 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

We can interpret these earnings effects using an elasticity of earnings with respect to the

[7] Throughout, $p_{\text{CR}}$ denotes cluster-robust $p$-values and $p_{\text{FP}}$ denotes $p$-values from the Ferman and Pinto (2019) permutation test.

[8] We also test for heterogeneous treatment effects across the two housing sites. As in our main results, both sites individually show statistically significant increases in earnings and total income, along with significant decreases in non-labor income. When we formally test whether the treatment effects differ between sites, we find no significant differences for earnings or total income, but with different effects on non-labor income. The full table can be found in Appendix A.5.

net-of-tax rate, as in the elasticity of taxable income (ETI) literature (Gruber and Saez, 2002; Saez, Slemrod and Giertz, 2012; Chetty, 2012; Neisser, 2021). In our setting, income-based rent imposes an effective marginal tax of about 30%, so the pre-policy net-of-tax rate is roughly 0.70. The RTS program reduces this marginal tax to about 15%, raising the net-of-tax rate to about 0.85.[9] Combining this change with the estimated 17% increase in household-head earnings implies an ETI-style earnings elasticity of about 0.8, in line with high-knowledge settings in prior work (see Section 5 for a discussion).[10]

#### 4.1.2 Spillover Effects on Other Household Members

We next examine whether the RTS incentives spill over to other household members. Household heads generate the vast majority of their families' income—88% on average in CHA housing—and they alone conduct the income recertifications and receive the RTS program outreach. Does the program also affect the income of other household adults who were not directly targeted?

Table 2 tests whether other household adults adjust their earnings or non-labor income.[11] We find little evidence of such spillovers: the effect sizes are small and imprecisely estimated. The overall change in other household adults' income is also statistically indistinguishable from zero. These results reinforce the notion that the program's financial incentives are internalized almost exclusively by the household head (see Section 5 for a discussion).

### *4.2 Additional results*

#### 4.2.1 Extensive and intensive margin responses to the RTS program

The RTS program is associated with an increase in earnings among heads of household. A natural follow-up question is whether this increase reflects changes on the extensive margin (bringing non-earners into work), the intensive margin (increasing work hours), or both. Did the program lead more residents to enter the labor force? Did it encourage those already employed to work more hours? Although we do not directly observe employment status,

[9] We approximate the effective marginal rent rate before and after RTS as $\tau_0 \approx 0.30$ and $\tau_1 \approx 0.15$, based on the program rule that deposits 1% of total rent plus 50% of the rent increase mechanically generated by higher earnings into the escrow account. This implies a change in the net-of-tax rate from about $1-\tau_0 \approx 0.70$ to about $1-\tau_1 \approx 0.85$.

[10] We compute $\varepsilon \equiv \Delta \ln y / \Delta \ln(1-\tau) \approx \ln(1.17)/\ln(0.85/0.70) \approx 0.8$.

[11] For this part of the analysis, we drop households whose only member is the head (8,502 household-year observations), and we add as a sample restriction that the other adults in the household appear in the sample as many years as the head of the households. We also exclude members under age 18 and full-time students. These restrictions result in a sample size of 2,799 household-member-year observations which correspond to 369 unique observations of other household adults.

Table 2: The effect of the Rent-to-Save program on income of other adults in the household

| | (1) Earnings | (2) Non-labor income | (3) Total income |
|---|---|---|---|
| $Treat_s \times Post_t$ | 101.213 | 113.869 | 215.082 |
| | (602.707) | (123.531) | (620.649) |
| Observations | 2799 | 2799 | 2799 |
| Control mean | 10864.959 | 2644.599 | 13509.558 |
| % control mean | 0.932 | 4.306 | 1.592 |
| Cluster-robust $p$-values | 0.868 | 0.364 | 0.731 |
| Ferman–Pinto $p$-values | 0.858 | 0.400 | 0.704 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3) for other household adults. All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

we use positive annual earnings as a proxy, and separate our sample by whether residents ever had positive annual earnings in the pre-treatment years. These results are presented in Table 3.

Focusing first on those heads of household who never had positive earnings in the pre-treatment years, we observe that the program raises the probability of having any earnings by 6.4 percentage points ($p_{\text{CR}} = 0.001$; $p_{\text{FP}} = 0.002$). This effect represents more than a three-fold increase over the control mean. The unconditional earnings effect for this subgroup is \$1,313 per year, roughly 4.4 times the control mean, reflecting the combined entry of new earners and any changes in earnings among those who remain employed. The estimate is precise under cluster-robust inference ($p_{\text{CR}} = 0.000$) and remains significant under the Ferman–Pinto permutation procedure ($p_{\text{FP}} = 0.015$).

Turning to the heads of households who did have positive earnings in the pre-treatment years, we observe that the probability of having any earnings increases by 8.8 percentage points, an 11.2% rise over the control mean. This effect is statistically robust across both inference procedures ($p_{\text{CR}} = 0.001$; $p_{\text{FP}} = 0.002$). Their annual earnings rise by \$1,429, a 7% gain relative to the control mean of \$19,893. The estimate is marginally significant with cluster-robust inference ($p_{\text{CR}} = 0.067$) but becomes imprecise under the Ferman–Pinto permutation test ($p_{\text{FP}} = 0.148$), suggesting moderate but not definitive evidence of higher earnings. The corresponding event studies, presented in Appendix A.6, suggest that both extensive and intensive margins may have contributed to the observed earnings gains. The dynamic estimates of the effect of RTS on earnings show no pre-trends. However, there

Table 3: The effect of the Rent-to-Save program on extensive and intensive margins

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Non-Workers | | Workers | |
| | Employment | Earnings | Employment | Earnings |
| $Treat_s \times Post_t$ | 0.064*** | 1312.935*** | 0.088*** | 1429.405* |
| | (0.018) | (263.322) | (0.023) | (756.698) |
| Observations | 8967 | 8967 | 6240 | 6240 |
| Control mean | 0.018 | 297.363 | 0.790 | 19893.169 |
| % control mean | 361.712 | 441.525 | 11.174 | 7.185 |
| Cluster-robust $p$-values | 0.001 | 0.000 | 0.001 | 0.067 |
| Ferman–Pinto $p$-values | 0.002 | 0.015 | 0.002 | 0.148 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on extensive and intensive margin changes in employment (columns 1 and 3, respectively) and earnings (columns 2 and 4, respectively). As we do not directly observe employment status, we use having positive earnings as a proxy. All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

appears to be a pre-trend in the probability of employment for prior workers in the year 2012 (see panel A.6c). Hence, we cannot know whether the RTS program had an effect on continued labor force participation for those residents who were already working.

These results show increases in both labor force participation and earnings among those who had previously not worked. Among those already attached to the labor market, we find a statistically robust increase in the probability of continued employment, though a pre-trend in the event study tempers this finding. The corresponding earnings effect for prior workers is positive but only marginally significant. More generally, however, we note that these results should be interpreted through the limitation that we use positive annual earnings as a proxy for employment status. As such, we cannot observe the duration, stability, or quality of employment, and the decomposition into extensive and intensive margin responses is necessarily approximate.

### 4.2.2 Results Restricted by Age

In our full sample, the average age of heads of households is 61 years old. While we estimate our main results using the unrestricted sample, policymakers may also want to understand the effect of the RTS program on only the traditional working age population.[12] We thus also analyze our results using our main difference-and-differences specification on a sample

[12]In 2024, the labor force participation rate among older Americans was 27.1% for U.S. workers 65-to-74 years old and 8.6% for 75 and older (U.S. Bureau of Labor Statistics, 2025).

restricted to heads of households under 67 years old in 2015, the retirement age in the U.S.[13] This reduces our sample size from 15,207 to 9,413.

In this restricted sample, we estimate a \$1,392 (11%) increase in earnings and a \$814 (13%) decrease in non-labor income. These estimates are statistically significant ($p_{\text{CR}} = 0.012$; $p_{\text{FP}} = 0.028$ for earnings; $p_{\text{CR}} = 0.001$; $p_{\text{FP}} = 0.001$ for non-labor income) and similar in size to those using the full sample, though the percent changes are slightly different because the younger sample has higher earnings and lower non-labor income. The estimated overall effect on total income is an increase of \$578 (3%), which is similar in size to the full sample treatment estimates but not significant ($p_{\text{CR}} = 0.217$; $p_{\text{FP}} = 0.207$). The table of results can be found in Appendix A.9 and the corresponding event studies in Appendix A.10.[14]

### 4.2.3 Heterogeneous Effects by Gender

In our full sample, 66% of head of households are female. Do the effects of the program differ by gender? To study this question, we present separate difference-in-differences estimates for households headed by women and men.

We observe a similar earnings rise for both groups: \$1,492 for women ($p_{\text{CR}} = 0.005$; $p_{\text{FP}} = 0.004$) and \$1,517 for men ($p_{\text{CR}} = 0.063$; $p_{\text{FP}} = 0.027$), representing 18% and 21.5% of their respective control means. Women, however, see a larger fall in public assistance: \$823 (10%, $p_{\text{CR}} = 0.000$; $p_{\text{FP}} = 0.011$) versus \$445 (5%) for men, the latter of which is not statistically significant ($p_{\text{CR}} = 0.517$; $p_{\text{FP}} = 0.114$). In total, this means that women only see a net increase in total income of \$669 (4%), which is not statistically distinguishable from zero ($p_{\text{CR}} = 0.145$; $p_{\text{FP}} = 0.123$), while men experience a net gain of roughly \$1,070 (6.5% increase, $p_{\text{CR}} = 0.001$; $p_{\text{FP}} = 0.090$). The table of these results can be found in Appendix A.12.

In sum, the program expands earnings similarly for both genders, yet the offsetting loss of public assistance is much larger for female heads; this means that their net improvement in total income is modest (and imprecisely estimated), whereas male heads retain most of their additional earnings and realize a higher overall gain.

### 4.2.4 Escrow Accounts and Exit Survey

The RTS program deposited the returned earnings from participants into escrow accounts. Using administrative data and the RTS exit survey, we can provide descriptive evidence on

[13] Appendix A.7 shows the age distribution of the head of households in both samples. Appendix A.8 shows descriptive statistics by treatment status in the age-restricted sample.

[14] We also re-estimate these results on this restricted sample using a balanced panel, which shows similar results. This table can be found in Appendix A.11.

final account balances and the intended use of funds. The exit survey was administered in 2019 as a program exit requirement. 80% of treated households completed it (for more information about the survey, see Appendix B).

The mean final escrow balance was $1,360, which is equal to about one month's total income for residents in CHA housing. There was a wide distribution in final balances: the 90th percentile reached $3,647 and the largest single balance was $10,000, but half of all accounts closed with less than $600 (the full distribution of amounts can be found in Appendix A.13a).

Program participants planned to use this money in a variety of ways. In the exit survey, the largest share (37%) planned to use the money for everyday household bills such as food or medicine, indicating the financial precarity of some of the study participants. 22% intended to set aside the funds for debt repayment. Smaller but still notable groups hoped to create an emergency savings account (13%), cover children's school or college costs (12%), save toward home ownership (8%), purchase a car (6%), or add to retirement savings (2%). Taken together, 37% planned on strengthening their financial position by reducing their debt or increasing their savings, while another 26% planned on using the escrow account to invest in education or a durable good. Finally, when asked to identify the program's goals, 92% of respondents were able to correctly identify at least one.

#### 4.2.5 Welfare Analysis

Our results suggest that the RTS program successfully counteracts the earnings disincentive typically associated with income-based rent: evidence from prior work using randomized housing lotteries finds that moving into public housing reduces earnings by 10–13% (Jacob and Ludwig, 2012; Van Dijk, 2019), while evidence using a matching approach finds reductions of 15–17% (Susin, 2005). The magnitude of our estimated 17% earnings increase suggests RTS may fully offset this earnings distortion.

To formalize the welfare implications, we use the marginal value of public funds (MVPF) framework of Hendren and Sprung-Keyser (2020), which compares beneficiaries' willingness-to-pay (WTP) for the policy to its net cost to the government. We define the WTP conservatively as the direct cash transfer to beneficiaries: the average escrow payout of $1,360 per person.[15] On the fiscal side, the program generated average per-person savings of $3,278 over three years, comprising $1,960 in reduced public assistance outlays (accruing to federal and state budgets) and $1,318 in higher net rent revenue to CHA after funding the escrow.

[15]We do not include the residents' increases in total income in this calculation because under standard envelope-theorem assumptions, policy-induced changes in labor earnings do not enter WTP, as the utility gain from additional income is offset by the disutility of additional effort at the margin.

These estimates are conservative in that they omit additional income and payroll tax revenue generated by the earnings increase, which would further reduce the net cost to government.

CHA reports their administrative costs as $437 per person per year, representing additional staffing and administrative resources for the program, which was otherwise embedded in routine public housing operations. Over three years, this totals $1,311 per person, well below the fiscal savings. Financial coaching, offered at only one site, cost an additional $2,419 per person per year, covering program staff, workshops, and credit report fees, plus a prorated share of organizational overhead.

The net cost of the program without coaching is negative, meaning the program more than pays for itself while delivering positive value to beneficiaries: the fiscal savings of $3,278 per person exceed the administrative costs of $1,311.[16] Including coaching costs, the MVPF for the coaching site is 0.26, below the typical range for adult-targeted programs.[17] However, we do not detect significant earnings differences across sites; if anything, labor market effects were directionally weaker where coaching was provided (see Appendix A.5).

# 5 Discussion

Prior work gives reason to expect that an earnings-return program would not produce the effects we find. In-work benefits have not consistently been shown to produce earnings responses; where earnings responses do appear, they are concentrated among subgroups with high pre-existing knowledge of the incentive schedule (Chetty, Friedman and Saez, 2013; Kleven, 2024). More broadly, simplifying program participation through tools like automatic enrollment has been most effective for set-and-forget behaviors, and there is growing evidence that it does not on its own translate into follow-through behavioral change (Guyton et al., 2017; Linos et al., 2022; Dykstra, O'Flaherty and Whillans, 2025). That the RTS program produced sustained labor market effects in a low-income population characterized by high behavioral inertia is itself suggestive that the salience strategy was effective.

However, several additional pieces of evidence support this interpretation. First, as described in Section 4.2.4, 80% of treated households completed the exit survey, and 92% of respondents correctly identified at least one of the program's goals, demonstrating high levels of engagement with and understanding of the program.

[16] Our welfare calculations are based on the three-year program period. The earnings effects we estimate are largest in the final program year, suggesting that awareness of and engagement with the program grew over time and that escrow balances would have been larger had the program continued. If these behavioral effects persist or continue to grow beyond the program horizon, the welfare gains would be correspondingly larger.

[17] Hendren and Sprung-Keyser (2020) document that adult-targeted programs typically have MVPFs between 0.5 and 2.

Second, as shown in Section 4.1.2, the effects of the program appear only among heads of household—the individuals in our study with the escrow account registered in their name, who conduct the income recertifications, and who receive the targeted program outreach—and not among other household adults. While we cannot rule out other explanations, this finding is consistent with the interpretation that the program's behavioral design and communication were important for its effects.

Third, the magnitude of the earnings response is itself informative about the role of salience. Canonical ETI estimates for broad taxpayer populations typically lie in the range of 0.1–0.4 (e.g., Saez, Slemrod and Giertz, 2012; Neisser, 2021). At the same time, substantially larger earnings elasticities have been estimated for specific subgroups. Taxable-income elasticities for high-income and high-knowledge earners are often in the range of 0.5–1.0 (e.g. Gruber and Saez, 2002; Saez, Slemrod and Giertz, 2012); Chetty, Friedman and Saez (2013) estimate intensive-margin earnings elasticities of about 0.31 (phase-in) and 0.14 (phase-out) on average in the United States, rising to roughly 0.84 and 0.29, respectively, in neighborhoods in the top decile of EITC knowledge. Our implied elasticity of about 0.8 is comparable to these high-knowledge estimates, despite being estimated for a low-income public housing population. The fact that our population exhibits an elasticity comparable to the most informed subgroups in prior work is suggestive evidence that the RTS program's behaviorally informed design was successful in raising attention to a level typically only observed among populations with high knowledge of the incentive schedule.

# 6 Conclusion

Income-based rent in housing—which, in principle, makes rent affordable for low-income households—also creates an earnings disincentive: rent is set at about 30% of income, meaning that these households face an effective marginal tax of 30 cents on each additional dollar of earnings. This paper provides causal evidence that a behaviorally informed earnings-return program can counteract this disincentive.

We use longitudinal data from the Cambridge Housing Authority and leverage quasi-random assignment to the Rent-to-Save program to compare automatically enrolled residents to a comparison group not exposed to the program. We find that the RTS program increases annual head of household earnings by 17% ($1,368), with a 7.5% ($653) decrease in public assistance and a 4% ($715) increase in total income. Given that prior work documents 10–17% earnings reductions from entering public housing, this suggests the program successfully offsets the earnings disincentive inherent in income-based rent. We also find that the program is self-financing: the administrative costs of $1,311 per person are well below the fiscal savings

generated by higher rent revenue and reduced public assistance.

We present suggestive evidence that the program's behaviorally informed design—automatic enrollment, a universal 1% credit, escrow accounts, quarterly statements, and targeted outreach—were important to its effects. While we cannot test the salience mechanism directly, prior work finds limited evidence that in-work benefits produce robust labor supply responses; where effects do appear, they are concentrated among those with high knowledge of the tax schedule. Our evidence is consistent with this interpretation: exit surveys show high levels of engagement with and understanding of the program; effects appear exclusively among heads of household who directly received the targeted outreach; and the implied earnings elasticity of 0.8 is comparable to the most informed subgroups in prior work, despite being estimated for a low-income public housing population.

This suggests that implementation details—how incentives are structured, communicated, and to whom—matter for whether programs achieve their intended behavioral effects. Features such as automatic enrollment, visible savings vehicles, and behaviorally informed outreach may offer a practical blueprint for policymakers seeking to design in-work benefit programs in public housing and in other settings where follow-through is important.

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# Appendix

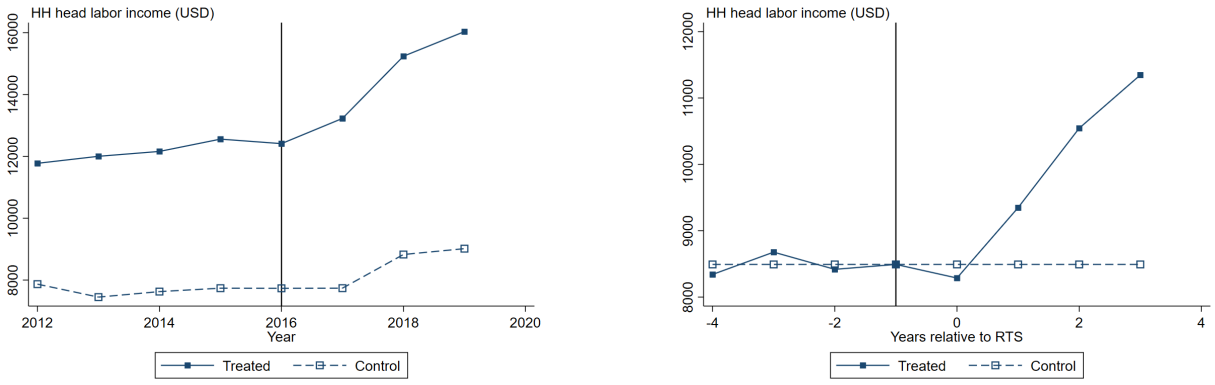
# A Additional Tables and Figures

Table A.1: Descriptive statistics 2015

| | Control | Treatment | p-value |
|---|---|---|---|
| **HoH demographic characteristics** | | | |
| Head of HH's age | 62 (15) | 51 (15) | <0.001 |
| Female | 0.65 (0.5) | 0.77 (0.4) | <0.001 |
| White | 0.51 (0.5) | 0.35 (0.5) | <0.001 |
| | | | |
| **Head of household income** | | | |
| Any earnings (dummy) | 0.32 (0.47) | 0.48 (0.50) | <0.001 |
| Total income | 16,420 (12,439) | 19,071 (14,696) | 0.003 |
| Earnings | 7,737 (14,535) | 12,557 (16,420) | <0.001 |
| Non-labor income | 8,683 (7,775) | 6,514 (8,243) | <0.001 |
| Share of HH income | 0.89 (0.24) | 0.83 (0.29) | <0.001 |
| | | | |
| **Household characteristics** | | | |
| Total income (Household) | 20,970 (20,374) | 25,776 (21,623) | <0.001 |
| HH members with income | 1.2 (0.54) | 1.4 (0.63) | <0.001 |
| Household size | 1.8 (1.3) | 2.8 (1.4) | <0.001 |
| Years lived in public housing | 11.3 (10) | 15.5 (11) | <0.001 |
| | | | |
| **Rental Unit characteristics** | | | |
| Bedrooms | 1.4 (1.1) | 2.4 (0.9) | <0.001 |
| Rent | 414 (314) | 506 (379) | <0.001 |
| | | | |
| Observations | 1,848 (89.2%) | 223 (10.8%) | |

Notes: This table presents descriptive statistics using the CHA administrative data from 2015, one year before the RTS program started.

Figure A.2: Visual pre-trends assessment

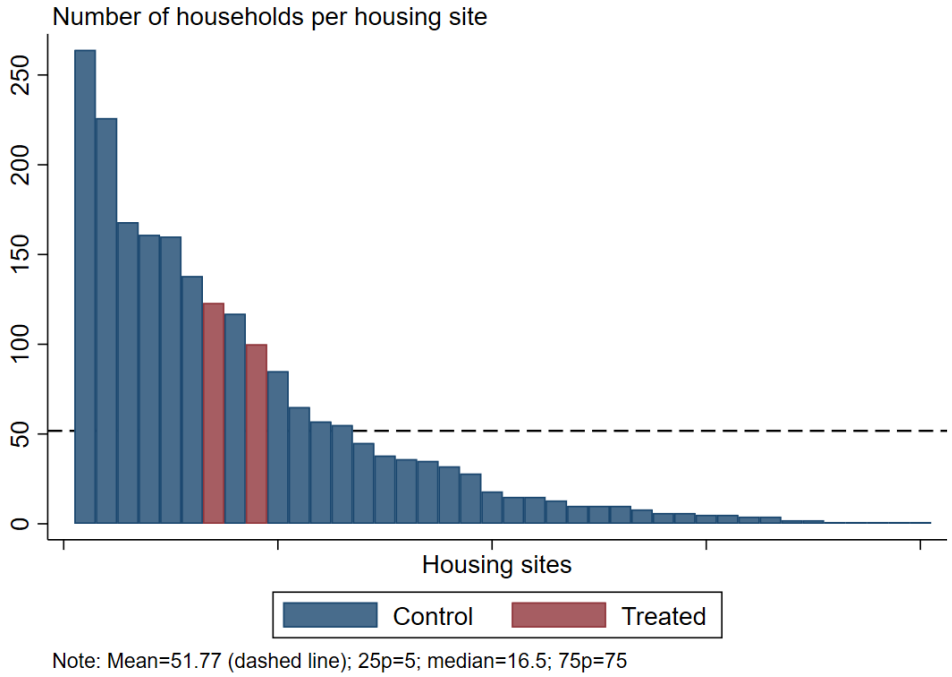



(a) Plotted means

(b) Predicted values

Notes: This figure allows us to visually check the plausibility of the difference-in-differences parallel trend assumption. Panel (a) plots mean head of household earnings across treated and control groups. Panel (b) plots the predicted values.

Figure A.3: Distribution of cluster size




Notes: This figure shows the distribution of housing sites' size across treatment and control groups.

Table A.4: Effect of the Rent-to-Save program on head of household's income - Balanced Panel

| | Earnings | Non-labor income | Total income |
|---|---|---|---|
| $Treat_s \times Post_t$ | 1546.564*** | -664.340** | 882.225** |
| | (424.862) | (266.198) | (340.748) |
| Control mean | 9182.300 | 8306.813 | 17489.113 |
| % control mean | 16.843 | -7.998 | 5.044 |
| Cluster-robust $p$-values | 0.001 | 0.017 | 0.013 |
| Ferman–Pinto $p$-values | 0.001 | 0.000 | 0.020 |
| Observations | 10896 | 10896 | 10896 |

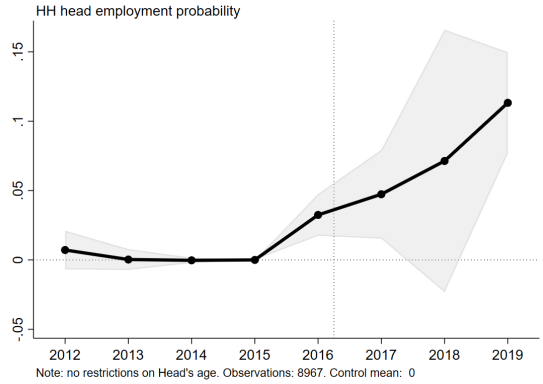
Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

Table A.5: The effect of the Rent-to-Save program on head of household income by housing site

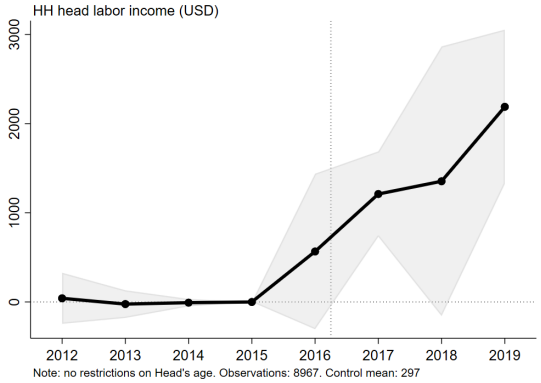
| | (1) Earnings | (2) Non-labor income | (3) Total income |
|---|---|---|---|
| $HousingSite_1 \times Post_t$ | 1580.037*** | -836.674*** | 743.364** |
| | (432.082) | (151.182) | (353.284) |
| $HousingSite2 \times Post_t$ | 1110.989** | -430.913*** | 680.077* |
| | (445.342) | (131.235) | (373.608) |
| Control mean | 7975.031 | 8710.283 | 16685.314 |
| % Housing Site 1 of control | 19.812 | -9.606 | 4.455 |
| % Housing Site 2 of control | 13.931 | -4.947 | 4.076 |
| $p$-value: Housing Site 1 = Housing Site 2 | 0.155 | 0.006 | 0.732 |
| Observations | 15207 | 15207 | 15207 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). This specification includes one dummy for each treatment arm: Housing Site 1 did not receive financial coaching; Housing Site 2 received financial coaching. All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses.

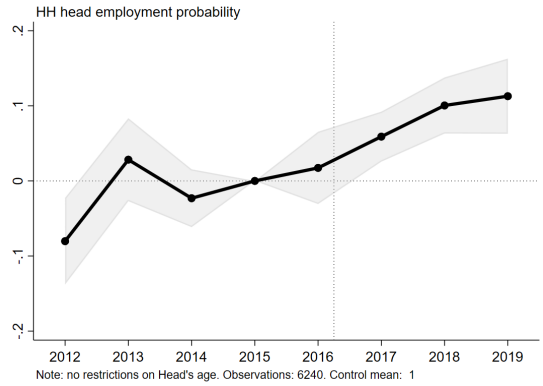
Figure A.6: Earnings and employment changes on the extensive and intensive margin



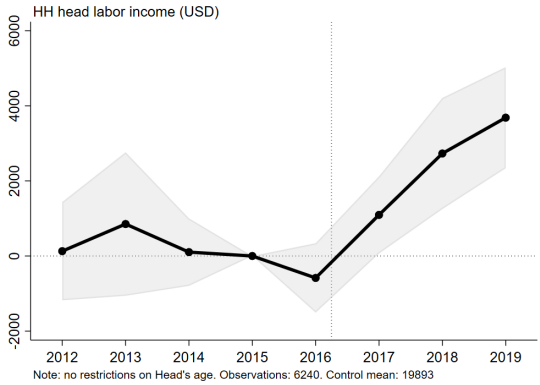

(a) Employment - prior non-workers




(b) Earnings - prior non-workers



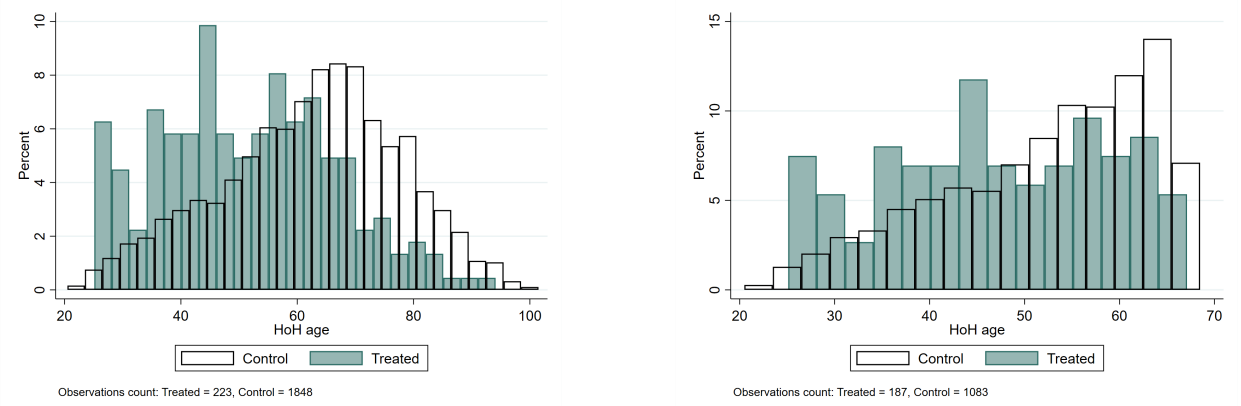

(c) Employment - prior workers




(d) Earnings - prior workers

Notes: These figures plot the dynamic estimates of Equation 1 and the associated 95% confidence intervals. The coefficients represent the change in outcomes for individuals automatically enrolled in the RTS program relative to individuals not automatically enrolled, with respect to the year immediately before the start of the program. Panel (a) and panel (b) show the change in employment probability and earnings, respectively, for those who were not working at baseline. Panel (c) and panel (d) show the change in employment probability and earnings, respectively, for those who were working at least one pre-treatment year. All values are in USD and reflect differences relative to 2015, comparing those automatically enrolled in the RTS program to those not enrolled.

Figure A.7: Age distribution across samples




(a) Main sample

(b) Less than 67

Notes: This figure shows the age distribution across treated and control groups in our main sample (panel a) and our restricted sample of younger head of households (panel b).

Table A.8: Descriptive statistics 2015 - HoH<67 years old

| | Control | Treatment | p-value |
|---|---|---|---|
| **HoH demographic characteristics** | | | |
| HoH's age | 52 (11) | 47 (12) | <0.001 |
| Female | 0.66 (0.5) | 0.8 (0.4) | <0.001 |
| White | 0.5 (0.5) | 0.3 (0.5) | <0.001 |
| | | | |
| **Head of household income** | | | |
| Any earnings (dummy) | 0.49 (0.5) | 0.56 (0.5) | 0.083 |
| Total income | 18,221 (14,411) | 19,760 (15,220) | 0.181 |
| Earnings | 12,352 (16,978) | 14,518 (16,988) | 0.107 |
| Non-labor income | 5,869 (7,112) | 5,242 (7,944) | 0.274 |
| Share of HH income | 0.86 (0.27) | 0.83 (0.30) | 0.148 |
| | | | |
| **Household characteristics** | | | |
| Total income (Household) | 24,630 (24,164) | 26,302 (21,674) | 0.375 |
| HH members with income | 1.3 (0.62) | 1.4 (0.62) | 0.051 |
| Household size | 2.3 (1.5) | 3.1 (1.4) | <0.001 |
| | | | |
| **Rental Unit characteristics** | | | |
| Bedrooms | 1.8 (1.2) | 2.6 (0.8) | <0.001 |
| Rent | 470 (353) | 519 (386) | 0.082 |
| | | | |
| Observations | 1,083 (85.3%) | 187 (14.7%) | |

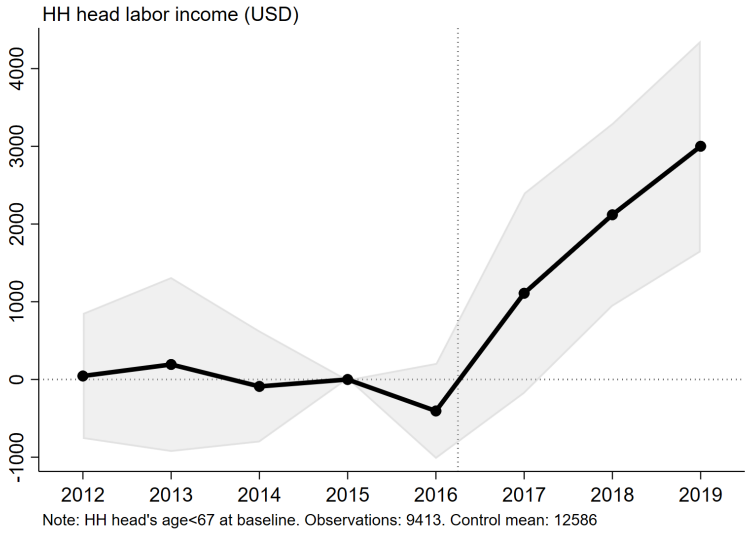
Notes: This table presents descriptive statistics using the CHA administrative data from 2015, one year before the RTS program started.

Table A.9: Effect of the Rent-to-Save program on younger head of household's income (age<67)

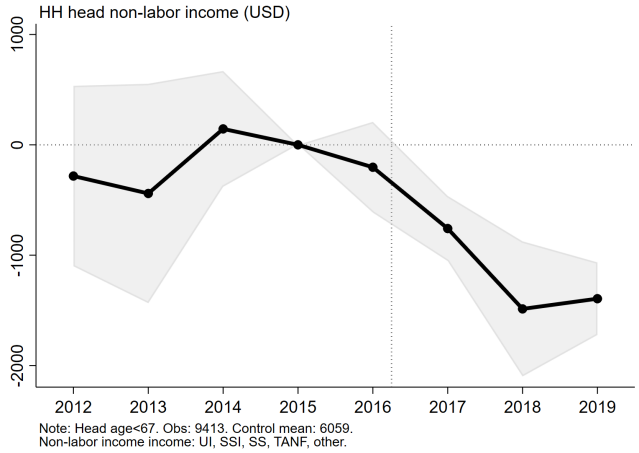
| | (1) Earnings | (2) Non-labor income | (3) Total income |
|---|---|---|---|
| $Treat_i \times Post_t$ | 1392.132** | -814.178*** | 577.954 |
| | (525.886) | (216.909) | (460.415) |
| Control mean | 12586.032 | 6058.673 | 18644.704 |
| % control mean | 11.061 | -13.438 | 3.100 |
| Cluster-robust $p$-values | 0.012 | 0.001 | 0.217 |
| Ferman–Pinto $p$-values | 0.028 | 0.001 | 0.207 |
| Observations | 9413 | 9413 | 9413 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

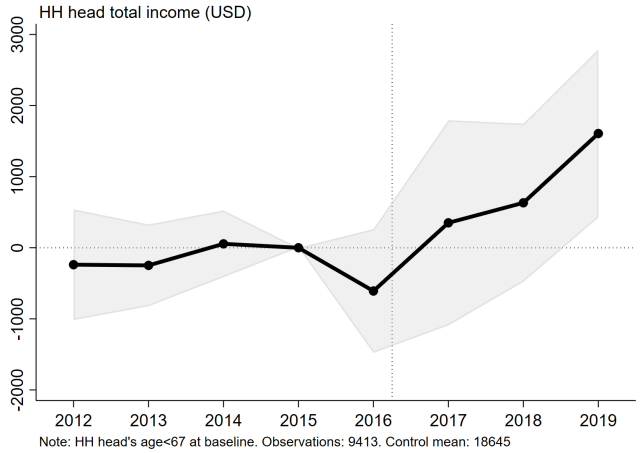
Figure A.10: Effect of the Rent-to-Save program on younger head of household's income (age<67)




(a) Earnings




(b) Non-labor income




(c) Total income

Notes: These figures plot the dynamic estimates of Equation 1 and the associated 95% confidence intervals for a restricted sample of younger workers at baseline. The coefficients represent the change in outcomes for individuals automatically enrolled in the RTS program relative to individuals not automatically enrolled, with respect to the year immediately before the start of the program. Panel (a) shows changes in earnings; panels (b) and (c) show non-labor and total income respectively. All values are in USD and reflect differences relative to 2015, comparing those automatically enrolled in the RTS program to those not enrolled.

Table A.11: Effect of the Rent-to-Save program on younger head of household's income (age<67) - Balanced Panel

| | Earnings | Non-labor income | Total income |
|---|---|---|---|
| $Treat_s \times Post_t$ | 1459.661*** | -845.047*** | 614.614 |
| | (473.417) | (253.409) | (444.436) |
| Control mean | 14276.315 | 5627.486 | 19903.801 |
| % control mean | 10.224 | -15.016 | 3.088 |
| Cluster-robust $p$-values | 0.004 | 0.002 | 0.175 |
| Ferman–Pinto $p$-values | 0.038 | 0.001 | 0.233 |
| Observations | 6896 | 6896 | 6896 |

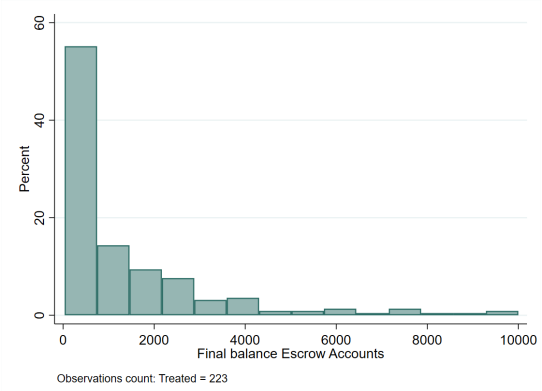
Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

Table A.12: The effect of the Rent-to-Save program on head of household income by gender

| | (1) Earnings | (2) Non-labor income | (3) Total income |
|---|---|---|---|
| Panel A: Female heads of household | | | |
| $Treat_s \times Post_t$ | 1491.739*** | -822.720*** | 669.020 |
| | (507.157) | (157.744) | (450.097) |
| Observations | 10077 | 10077 | 10077 |
| Control mean | 8465.091 | 8333.193 | 16798.284 |
| % control mean | 17.622 | -9.873 | 3.983 |
| Cluster-robust $p$-values | 0.005 | 0.000 | 0.145 |
| Ferman–Pinto $p$-values | 0.004 | 0.011 | 0.123 |
| Panel B: Male head of household | | | |
| $Treat_s \times Post_t$ | 1517.422* | -444.918 | 1072.504*** |
| | (784.469) | (679.141) | (285.754) |
| Observations | 5102 | 5102 | 5102 |
| Control mean | 7065.442 | 9424.743 | 16490.185 |
| % control mean | 21.477 | -4.721 | 6.504 |
| Cluster-robust $p$-values | 0.063 | 0.517 | 0.001 |
| Ferman–Pinto $p$-values | 0.027 | 0.114 | 0.090 |

Notes: This table presents difference-in-differences estimates of the average treatment effect of the RTS program on earnings (column 1), non-labor income (column 2), and total income (column 3). Panel A shows estimates for female heads of household and Panel B for male heads. All regressions include household head fixed effects and year fixed effects. Standard errors clustered at site level in parentheses. The last two lines present cluster-robust $p$-values and $p$-values from the Ferman and Pinto (2019) permutation test.

Figure A.13: Escrow accounts balance




(a) Escrow Accounts

Notes: This figure shows the distribution of escrow account balances at the end of the program.

# B Survey data

Two household surveys were conducted during the program period. The first, carried out in 2017, was intended to serve as a "baseline" instrument but was fielded roughly one year after the intervention had already begun, meaning it does not capture true pre-treatment conditions. Its coverage is uneven: only 48% of treated heads of household (108 of 223) and 17% of control heads (314 of 1,860) responded, raising concerns about non-response bias and differential selection. Moreover, respondents in the 2017 survey cannot be reliably linked to the administrative panel used in the main analysis, so the survey cannot be used for longitudinal outcomes.

The second survey, administered in 2019 as an exit requirement, reached an 80% response rate among treated participants but provides no information for the control group. This survey can be matched to the administrative data.

Descriptive information from the 2017 data to characterize residents' financial well-being can be found in Section 3.1. Information from the exit survey, including study participants' planned use of money, can be found in Section 4.2.4. In the following section we show additional information from the 2017 survey. All descriptive statistics reported should be interpreted with the limitations mentioned in mind.

## *B.1 Descriptive Statistics from the 2017 Survey*

Table B.1: Survey 2017 - Descriptive Statistics

| | Control | Treated | *p*-value |
|---|---|---|---|
| *Demographics* | | | |
| Age | 63.815 (14.594) | 55.056 (15.121) | <0.001 |
| Female | 0.640 (0.481) | 0.694 (0.463) | 0.307 |
| *Financial behavior* | | | |
| Invested last year | 0.158 (0.366) | 0.284 (0.453) | 0.005 |
| Saved last year | 0.213 (0.410) | 0.227 (0.421) | 0.769 |
| Lowered debt last year | 0.370 (0.484) | 0.400 (0.492) | 0.604 |
| CFPB well-being score | 48.658 (14.869) | 46.419 (12.365) | 0.213 |
| Observations | 314 (74.4%) | 108 (25.6%) | |

Notes: This table shows descriptive statistics from the 2017 Survey done across treatment and control households.

Given the low response rate of the 2017 survey—48% of treated heads of household and only 17% of control heads—the summary statistics in Table B.1 should be interpreted

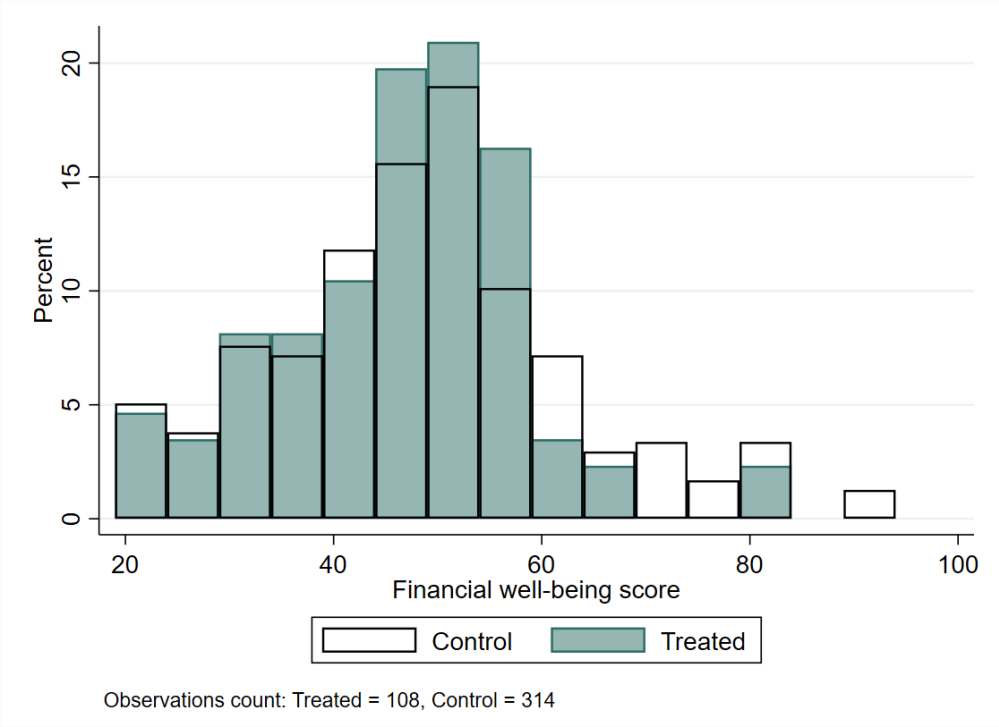
cautiously. Among those who did respond, treated participants are on average younger than controls, while the share of women is similar in both groups. Self-reported financial behavior differs only on one dimension: investment activity is higher in the treated group (28%) than the control group (16%), whereas the proportions who saved (23%) or reduced debt (40%) in the prior year are statistically indistinguishable. Finally, average CFPB financial-well-being scores are similar— 46.4 for treated households and 48.7 for controls—suggesting equivalent levels of financial resilience among survey respondents early in the program.

### *B.2 Financial Well-Being Scores from the 2017 Survey*

Figure B.2 plots the CFPB financial-well-being score at the 2017 survey date for the 108 treated and 314 control respondents. In both groups the bulk of the distribution lies between the "medium-low" (38–49) and "medium-high" (50–57) CFPB ranges: most households cluster around scores of 45–55, indicating limited liquid savings and persistent difficulty making ends meet, but at least some automated saving among the higher scorers. Only a small share of either group registers below 30 (the "very low" category associated with acute hardship), and an equally small right-tail reaches into the "high" bracket (58–67) or beyond. These patterns suggest that, despite differences in survey response rates, the financial well-being of respondents in the two arms was broadly comparable in 2017, with most households entering the programme in a financially fragile, though not extreme, position.[18]

[18]The information on the development of the ranges and the facts about typical experiences comes from the national Financial Well-Being Survey. For more information, see the CFPB's website: consumerfinance.gov/practitioner-resources/financial-well-being-resources

Figure B.2: Financial well-being scores distribution 2017




Notes: This figure plots the CFPB financial-well-being score at the 2017 survey date for the 108 treated and 314 control respondents. The information on the development of the ranges and the facts about typical experiences comes from the national Financial Well-Being Survey. For more information, see the CFPB's website: consumerfinance.gov/practitioner-resources/financial-well-being-resources

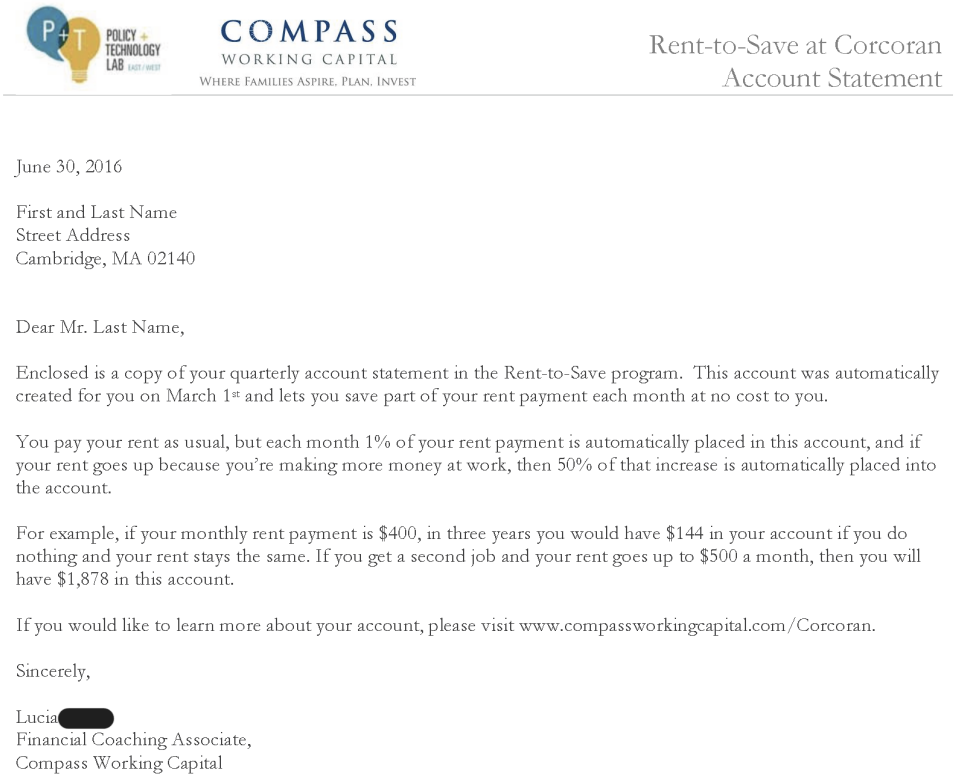
# C Outreach Materials

A variety of outreach was conducted during the program period to promote awareness and understanding of the program, as described in Section 2.2. Below are examples of these materials, including the account statements sent quarterly to residents.

Figure C.1: Information Sheet


Notes: This figure shows the information sheet sent to each household describing the program in an infographic.

Figure C.2: Letter



POLICY + TECHNOLOGY LAB EAST / WEST

COMPASS
WORKING CAPITAL
WHERE FAMILIES ASPIRE, PLAN, INVEST

Rent-to-Save at Corcoran
Account Statement

June 30, 2016

First and Last Name
Street Address
Cambridge, MA 02140

Dear Mr. Last Name,

Enclosed is a copy of your quarterly account statement in the Rent-to-Save program. This account was automatically created for you on March 1st and lets you save part of your rent payment each month at no cost to you.

You pay your rent as usual, but each month 1% of your rent payment is automatically placed in this account, and if your rent goes up because you're making more money at work, then 50% of that increase is automatically placed into the account.

For example, if your monthly rent payment is $400, in three years you would have $144 in your account if you do nothing and your rent stays the same. If you get a second job and your rent goes up to $500 a month, then you will have $1,878 in this account.

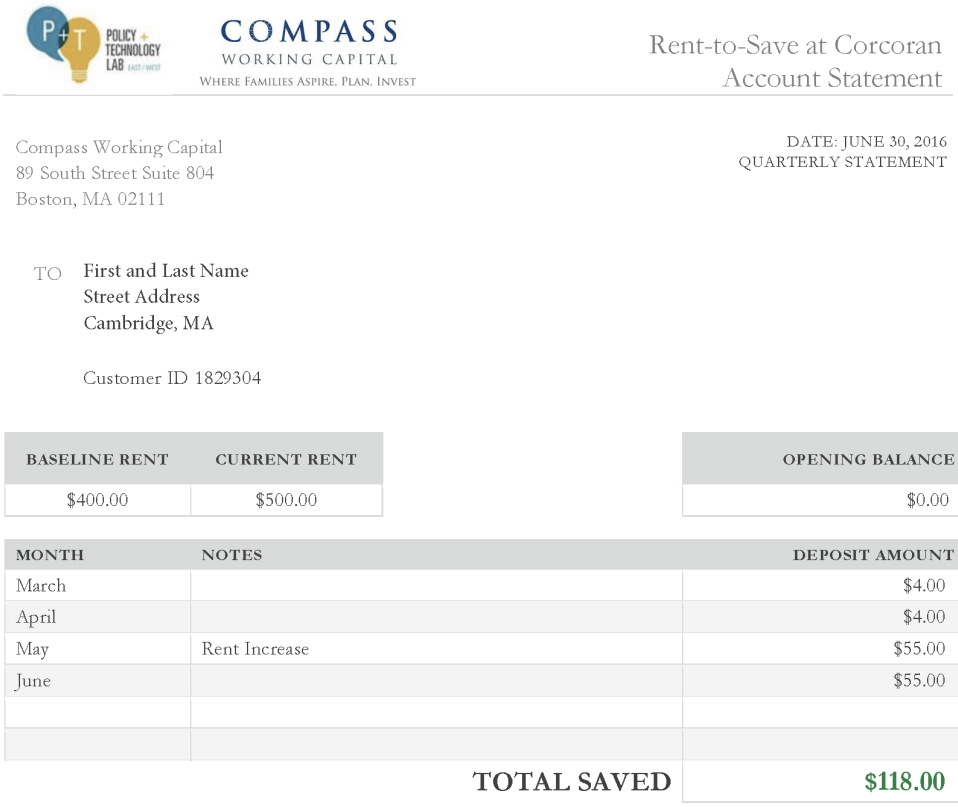
If you would like to learn more about your account, please visit www.compassworkingcapital.com/Corcoran.

Sincerely,

Lucia [redacted]
Financial Coaching Associate,
Compass Working Capital

Notes: This figure shows the letter sent to each household describing the program.

Figure C.3: Account Statement



COMPASS
WORKING CAPITAL
WHERE FAMILIES ASPIRE, PLAN, INVEST

Rent-to-Save at Corcoran
Account Statement

Compass Working Capital
89 South Street Suite 804
Boston, MA 02111

DATE: JUNE 30, 2016
QUARTERLY STATEMENT

TO First and Last Name
Street Address
Cambridge, MA

Customer ID 1829304

| BASELINE RENT | CURRENT RENT |
|---|---|
| $400.00 | $500.00 |

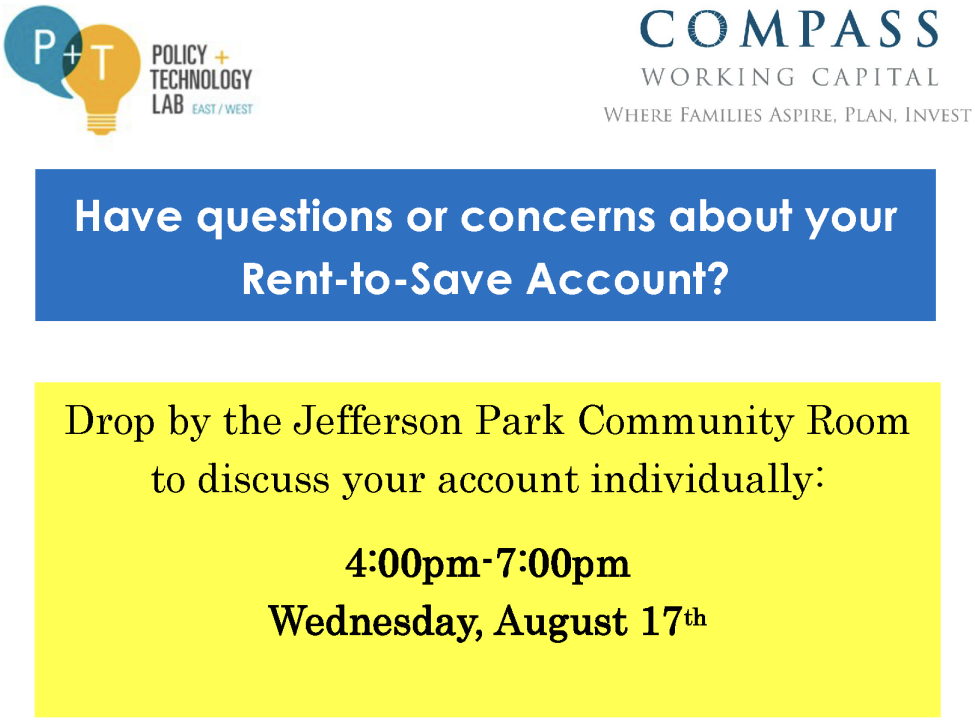
| OPENING BALANCE |
|---|
| $0.00 |

| MONTH | NOTES | DEPOSIT AMOUNT |
|---|---|---|
| March | | $4.00 |
| April | | $4.00 |
| May | Rent Increase | $55.00 |
| June | | $55.00 |
| | | |
| | | |
| | TOTAL SAVED | $118.00 |

For more information about your financial goals account statement, please visit:
www.compassworkingcapital.com/Corcoran

Notes: This figure shows the account statement sent to each study participant that displays their escrow account history and current balance.

Figure C.4: Open House Flyer




Notes: This figure shows an example of a flyer distributed at a treated housing site to advertise an information session about the RTS program.