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

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Abstract

Income-based rents, common in public housing, create an earnings disincentive. We study a policy designed to counteract this effect by returning part of the rent induced by higher earnings to residents. Importantly, the program automatically enrolled households and used a behaviorally informed design 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 use of public assistance falls 7.5%, suggesting a salient intervention can successfully offset the earnings disincentive found in prior work. We document employment impacts on both the intensive and extensive margins.

Keywords: public housing; labor supply; in-work benefits; salience

JEL codes: I38, J22, R38

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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 in public housing 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, this income-based rent structure creates an unintended consequence: it reduces labor force participation and earnings among residents. 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 effectively faces a 30% marginal tax rate through higher rent payments. 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") (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).

This paper examines whether a policy intervention that involves a salient approach to mitigating the earnings disincentive in income-based rents can be effective in public housing. Specifically, we study the Rent-to-Save Pilot Demonstration (henceforth, RTS) at the Cambridge Housing Authority in Cambridge, M.A. We collaborated with the housing authority to design a behaviorally informed program that returns a portion of rent increases to residents. This money is placed into an escrow account, which residents receive as a lump sum cash transfer at the end of the program period. Heads of household receive targeted outreach throughout the program to make these features salient. All residents at two large family housing sites were automatically enrolled, 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 total 4% ($700) increase in overall income per year relative to the control mean of $16,700. We find no evidence of spillover effects on non-heads of household, 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.

Our analysis contributes to several literatures. First, we add to the growing body of work on the causal effects of housing assistance on economic outcomes, which includes studies of the Moving to Opportunity experiment (Kling, Liebman and Katz, 2007; Ludwig et al., 2013; Chetty, Hendren and Katz, 2016), eviction effects (Collinson et al., 2024), and public housing voucher programs (Jacob and Ludwig, 2012; Van Dijk, 2019). Second, we contribute to policy reform evaluations in housing, complementing structural analyses (Keane and Moffitt, 1998; Waldinger, 2021; Zhang, 2025) and reviews of housing assistance (Olsen, 2003; Collinson, Ellen and Ludwig, 2019). Third, we provide evidence on the causal effects of income support on labor supply, joining research on lottery winners (Imbens, Rubin and Sacerdote, 2001; Cesarini et al., 2017; Golosov et al., 2024), cash transfers (Banerjee et al., 2017; Vivalt et al., 2024; Bartik et al., 2024; Balakrishnan et al., 2024), and the Earned Income Tax Credit (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Eissa and Hoynes, 2004; Nichols and Rothstein, 2016; Kleven, 2024).

There remains sparse causal evidence on in-work benefits programs—programs that provide low-income individuals with additional money for working—with much of the earlier 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). While earlier evidence suggested that EITC-style programs were effective, recent work calls into question whether this was due confounding factors taking place at the same time, making it important to study current-era programs (Chetty, Friedman and Saez, 2013; Kleven, 2024). Most importantly, there is very little causal evidence of in-work benefits programs in the context of public housing specifically (Moulton, Freiman and Lubell, 2021; Verma et al., 2017; Freedman, Verma and Vermette, 2023), representing a significant gap given the importance of public housing and the unique earnings disincentive created by income-based rent. In addition, while the literature shows that awareness and simplicity are first-order determinants of benefit take-up (Currie, 2006; Bettinger et al., 2012; Herd and Moynihan, 2018; Finkelstein and Notowidigdo, 2019), we show that an in-work benefit explicitly designed for salience can affect follow-through labor market behavior beyond initial take-up.

The findings of this paper show that a well-designed policy can counteract earnings disincentives. We demonstrate that a behaviorally informed earnings-return program can successfully change labor market behavior among public housing residents, a context with otherwise high behavioral inertia and administrative friction. These results provide guidance for the design of in-work benefits programs as well as a new tool for the design of housing policy.

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 that were chosen to be representative of the CHA housing portfolio. The program was embedded within routine public-housing operations, including with existing rent collection. It was launched in partnership with Compass Working Capital, a Boston-based non-profit that provides financial coaching. We collaborated with CHA and Compass on study implementation details, including running early focus groups, developing survey questions, coordinating door-to-door surveys, and designing behaviorally informed account statements and outreach.

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 rent contribution regardless of income changes. Second, households whose incomes increased during the demonstration period received funds equal to 50% of any rent increase. Residents could qualify for a waiver based on old age or disability. 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 details and outreach were designed to promote awareness among the two enrolled housing sites. First, automatic enrollment and the automatic 1% monthly credit meant that all study participants received funds, giving everyone an immediate stake in the program. Second, participants' credits were deposited into escrow accounts, a design choice intended to make the returns to working concrete and visible. Heads of household received quarterly statements showing their growing balance, with behaviorally informed design to be simple, easy to understand, and compelling. Third, we conducted other kinds of outreach: mailed postcards, flyers distributed at housing sites, on-site community meetings, and explanations of the program during standard income recertifications. Examples of the information sheets, account statements, and open house flyers can be found in Appendix C. Finally, universal enrollment inside housing sites encouraged spillover effects through social networks inside the buildings. Since this was a program that required follow-through action to be successful, the goal was to make program understandable and visible to residents.

Evidence shows that awareness and simplicity are key drivers of the take-up of in-work and other social benefits. Studies of the EITC repeatedly document limited knowledge of the program and frictions in claiming among eligibles (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). Complementary evidence from other policy initiatives demonstrates that making benefits salient and lowering hassle costs increases take-up: for example, for both university enrollment and SNAP participation, streamlined information and in-person assistance increases take-up (Bettinger et al., 2012; Finkelstein and Notowidigdo, 2019). These patterns are consistent with broader theories of administrative burden and classic reviews on benefit take-up that emphasize defaults, information frictions, and transaction costs (Currie, 2006; Herd and Moynihan, 2018; Dykstra, O'Flaherty and Whillans, 2025). In our setting, RTS coupled automatic enrollment with regular, behaviorally informed communications; at exit, 92% of participants correctly identified at least one of the study's goals, demonstrating high program understanding. Taken together, this paper offers early causal evidence that an in-work benefit designed for salience can affect labor-market behavior past initial take-up.

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, as detailed in Section 2.2, the RTS program focused heavily on outreach and awareness, including through the automatic 1% monthly credit, which is not a standard part of the FSS model.

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 basic income and demographic data for every household member. We also have descriptive data from two surveys conducted in the years 2017 and 2019.[1]

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. Third, we drop households whose participation in the program was waived due to disability or old age. Fourth, we retain the 45 treated households (20% of the treated group) who do not ultimately access their escrow accounts, allowing us to estimate the population-level effect of automatic enrollment rather than effects only among households that follow through. 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 households 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 account statements, program notifications, and financial coaching. 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.[2] Their income was $16,706, out of which $8,256 was labor earnings. 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

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

[2] 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).

savings and 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).[3] 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.

3.2 Empirical Strategy

Economic theory offers guidance on how to consider what determines an individual's earnings and 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 rate of 30% on additional earnings, this rate 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; perhaps most important in this context is whether they receive benefits from other social programs, and how earning more might affect their eligibility for those 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

[3] 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.

$Treat_s$ 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 δ_t representing the effect of the program in year t. The coefficients $\delta_{-P}, \ldots, \delta_{-1}$ estimate the anticipation effects in the years leading up to the program's implementation. γ_t are year fixed effects, which control for time-varying factors that affect all individuals in the sample, while λ_i are individual fixed effects, controlling for time-invariant individual characteristics. The error term is denoted as ϵ_{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 $\delta_{-P}, \ldots, \delta_{-1}$ in equation 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 a permutation approach that adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size (Ferman and Pinto, 2019). We arrive to similar conclusions with both inference methods.[4]

[4]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 $\times$ time aggregate model (MacKinnon and Webb, 2020). This assumption would be too restrictive in our DD setting because the housings 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.

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

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 Results

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 overall income (Panel 1c). Each of these figures plot the estimated δ_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 not automatically enrolled, with respect to the year immediately before the start of the program.

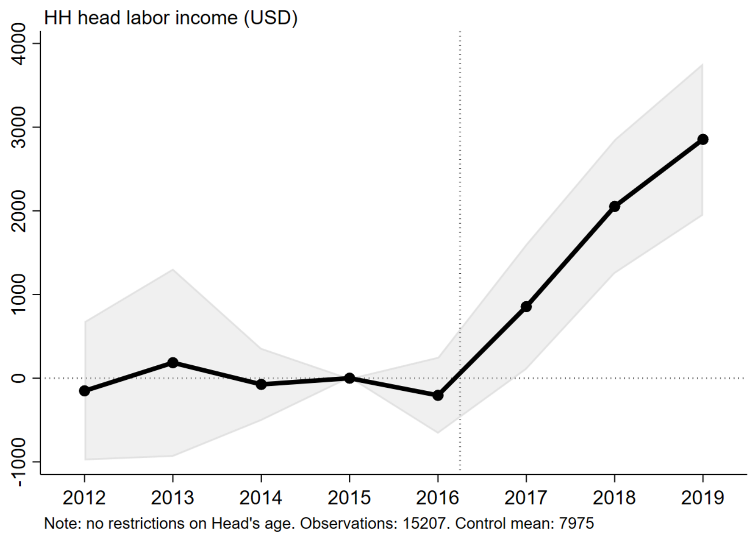
Prior to the RTS program, income trended similarly across the two groups: the coefficients δ_{-t} are close to zero and not statistically significant. Starting the first year post-treatment (2017), we observe earnings increase among individuals in treated housing sites relative to non-treated housing sites. Post-treatment, earnings rise but this increase in income is partly offset by declines in non-labor transfers, leaving total household income higher overall.

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. We find that the increase in earnings is offset by a decline in non-labor income—Social Security, Supplemental Security Income, Temporary Assistance for Needy Families—of about \$653 (7.5%), with an overall positive effect on total income of \$715 (4.3%). These estimates are robust to the permutation-based inference procedure of Ferman and Pinto (2019); the main results remain statistically significant when accounting for site-level clustering with heterogeneous cluster sizes and a 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.[5]

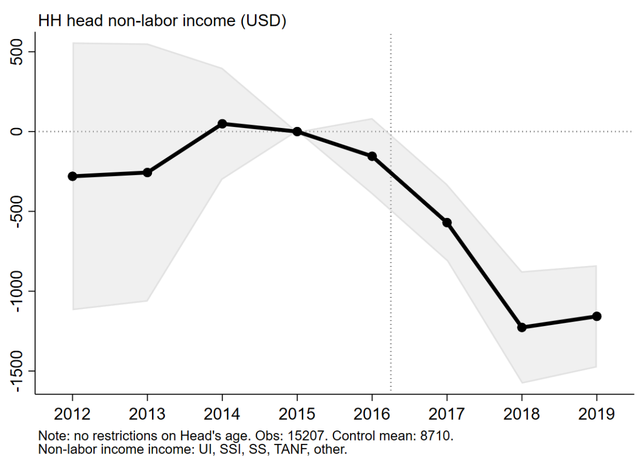
We interpret these earnings effects using an elasticity of earnings with respect to the net-of-tax rate, as in the elasticity of taxable income (ETI) literature (Gruber and Saez, 2002;

[5] 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.

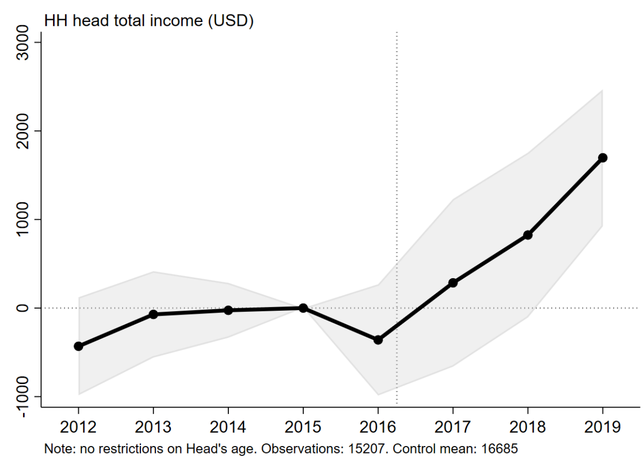
Figure 1: Effects of the Rent-to-Save program on head of household income



(a) Earnings



(b) Non-labor income



(c) Overall 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.

Saez, Slemrod and Giertz, 2012; Chetty, 2012; Neisser, 2021). In our setting, income-based rent imposes an effective marginal "rent tax" of about 30%, so the pre-policy net-of-tax rate is roughly 0.70, and the RTS program reduces this rent tax to about 15%, raising the

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 Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

net-of-tax rate to about 0.85.[6] Combining this change with the estimated 17% increase in household-head earnings implies an ETI-style earnings elasticity with respect to the net-of-tax rate of about 0.8, comparable to individuals in other high-knowledge settings in prior work.[7]

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 account statements, program notifications, and financial counseling. Does the program also affect the

[6] 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 $1-\tau_1 \approx 0.85$.

[7] We compute $\varepsilon \equiv \Delta \ln y / \Delta \ln(1-\tau) \approx \ln(1.17)/\ln(0.85/0.70) \approx 0.8$. Canonical ETI estimates for broad taxpayer populations typically lie in the range of 0.2–0.4 (e.g. Saez, Slemrod and Giertz, 2012; Neisser, 2021). At the same time, substantially larger earnings elasticities have been estimated for subgroups facing particularly strong incentives or high knowledge of the schedule: for example, 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, and taxable-income elasticities for high-income or top earners around major tax reforms are often in the 0.4–0.8 range (e.g. Kleven and Schultz, 2014; Miao, Selin and Söderström, 2024). In this context, our implied elasticity of about 0.8 sits toward the upper end of ETI-based earnings responses—comparable to high-knowledge and high-income settings—despite being estimated for a low-income public housing population.

income of spouses and other adult members of the household, those who did not directly receive the program information?

Table 2 tests whether spouses or other adults in the households adjust their earnings or transfer receipt.[8] 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.

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 Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall income (column 3) from other adults in the household (excluding 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

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, we use positive annual earnings as a proxy. We separate our sample by whether by

[8] 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 spouses and other household adults.

whether residents ever had positive annual earnings in the pre-treatment years and analyze employment and earnings responses. 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. This effect represents more than a three-fold increase over the control mean. The corresponding gain in annual earnings for these new labor market entrants is $1,313, roughly 4.4 times that of the control mean. The estimate is precise under cluster-robust inference and remains significant when we apply the Ferman–Pinto permutation procedure.

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 Rent-to-Save program on extensive and intensive margin changes in earnings (columns 1 and 3, respectively) and employment (columns 2 and 4, respectively). As we do not directly observed 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

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. Finally, their annual labor income rises by $1,429, a 7% gain relative to the control mean of $19,893. The estimate is marginally significant with cluster-robust inference ($p = 0.067$) and becomes imprecise under the Ferman–Pinto permutation test ($p = 0.148$), suggesting moderate but not definitive evidence of higher earnings per worker. 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 appears to be a pre-trend in the in probability of employment along for prior workers in the year 2012 (see panel A.6c). Hence, we cannot know whether the RTS program had an effect on 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, as well as increased earnings among those already attached to the labor market. For this latter group, we also find positive but less definitive evidence of continued employment. However, these results should be interpreted with caution given the indirect measure of employment, which prevent us from understanding the kind and quality of employment being obtained.

4.2.2 Results Restricted by Age

In our full sample, the average age of heads of households is 61 years old, with 39% being 67 or older. While we estimate our main results using the unrestricted sample, policymakers may also want to understand the effect of the RTS program on the traditional working age population.[9] We thus also analyze our results using our main difference-and-differences specification on a sample restricted to heads of households under 67 years old in 2015.[10] This reduces our sample size from 15,207 to 9,413.

In this restricted sample, we estimate a $1,392 (11%) increase in labor income and a $814 (13%) decrease in non-labor income. These estimates are statistically significant and similar in size to those using the full sample, though the percent changes are slightly different because the younger sample earns more in labor income and less in 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. The table of results can be found in Appendix A.9 and the corresponding event studies in Appendix A.10.[11]

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 presents separate difference-in-differences estimates for households headed by women and men.

Earnings rise by similar amounts for both groups: $1,492 for women and $1,517 for men,

[9]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). In addition, as described in 3.1, the households in our unrestricted sample are those who did not obtain a waiver for disability or old age.

[10]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.

[11]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.

representing 18% and 21.5% of their respective control means. Among women, however, this gain is accompanied by an $823 (10%) fall in public assistance, so the implied $669 increase in total income is small and not statistically distinguishable from zero. Male heads see a smaller, statistically insignificant reduction in non-labor income ($445), leaving a net gain of roughly $1,070 in total income (6.5%), significant at the one-percent level. In sum, the program expands earnings similarly for both genders, yet the offsetting loss of transfer income 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 substantially higher overall gain. The full table of these results can be found in Appendix A.12.

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 final account balances, the intended use of funds, and participant perceptions of the accounts.[12]

The mean final escrow balance was $1,360, which is equal to about one month's total income for residents in CHA housing. This represents a substantial amount of savings for this population. That being said, 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). Thus, while many participants accumulated a significant amount of assets, there was considerable heterogeneity in the final balance. Some of this could be due to the program ending after three years: we saw the strongest earnings growth in the last year of the program, suggesting that the escrow balances would have become larger if the program ended later.

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.

[12] The exit survey was administered in 2019 as a program exit requirement. 80% of treated households completed it. More information about the survey can be found in Appendix B.

Finally, comments from the exit survey provide insight into how participants perceived the RTS program, with the escrow account broadly top of mind. Some stressed the program's effortless quality: "Very good. I didn't even have to think about it. I would never have been able to save that on my own." Others described plans for the money: "The money was building up and, at the end of it all, it's real! It's going to go for my kids, which is huge." Still others emphasized how the program helped them save despite financial hardships:

> "I live paycheck to paycheck because I have a lot of bills. I don't have any other savings. I don't spend money on any other things. I never had the chance. It's a good program. Plus they hold the money for you so you don't spend it."

Notably, 92% of respondents were correctly able to identify at least one of the program's goals, providing further evidence that study participants were aware of and understood the program. Altogether, the exit survey suggests the escrow played an important role in making the returns to working both concrete and salient to residents.

4.2.5 Welfare Analysis

Our results suggest that the RTS program successfully counteracts the earnings disincentive typically associated with income-based rent. Using evidence from randomized housing lotteries, prior research 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 labor earnings distortion.

To formalize the welfare implications, we use the Hendren–Sprung-Keyser MVPF, which compares beneficiaries' willingness-to-pay (WTP) for the policy to its net cost to the government (Hendren and Sprung-Keyser, 2020). We use envelope-theorem logic to take the WTP as the average cash delivered via escrow plus the increase in total income over the three years of the program, equal to $3,504 per person. On the fiscal side, the policy generated an average per-person savings of $3,278, including the sum of reduced public assistance outlays for three years plus higher net rent revenue to CHA after funding the escrow.

CHA administrative costs were $437 per person per year, representing additional staffing and administrative resources for the program, which is otherwise embedded in routine public housing operations. Over three years, this totals $1,311 per person, well below 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. 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).

Hendren and Sprung-Keyser (2020) document that adult-targeted programs typically have MVPFs between 0.5 and 2, with cash and tax-credit transfers to low-income adults clustered near 1. Including coaching costs, the MVPF for the coaching site is 0.66. This means that the MVPF for the financial coaching version of the RTS program is still within the typical range for adult-targeted programs, but substantially lower than the core program. Without coaching, the program constitutes a Pareto improvement: it delivers \$3,504 in beneficiary value while generating net savings for the government.

5 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 effectively face a 30 cent tax on each additional dollar of earnings. This paper delivers evidence that a salient, behaviorally informed earnings-return program that reduces this earnings disincentive can lead to improvements in labor market outcomes.

We use longitudinal data from the Cambridge Housing Authority and leverage quasi-random assignment to the Rent-to-Save program. This data allows us to compare residents automatically enrolled in the program to a comparison group of public housing residents not exposed to the program, finding that the RTS program increases annual head of household earnings by \$1,368 while reducing social assistance by \$653. Over the period of our study, the typical family received a gross financial benefit of about \$3,504, including escrow accounts and net increases in income. At the same time, the program provided a large fiscal benefit: CHA received an average increase in rental income of \$1,318 per person after funding escrow accounts, and use of public assistance decreased by \$1,960 per person over the study period. Administrative costs were \$1,311, well below these fiscal savings, meaning that the program is self-financing and generates net savings for the government.

The RTS program, beyond altering the net return to work, also used design and outreach strategies informed by behavioral science: automatic enrollment, escrow accounts, frequent account statements, reminders embedded in routine bureaucratic interactions, on-site communications, and encouraging spillover effects through social networks inside the buildings. By reducing information and hassle costs and keeping the reward visible over time, these implementation details likely increased the perceived payoff to additional work relative to the default rent schedule. While we cannot isolate salience as a mechanism, several findings suggest it was important: exit surveys indicate participants were aware of and understood the program, the effects appear only among heads of household who received the outreach, and the elasticity of earnings is relatively high and comparable to individuals in other high-knowledge settings. This suggests that policymakers should attend carefully to the implementation details of in-work benefit schemes and other programs that require follow-through to be successful.

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Appendix

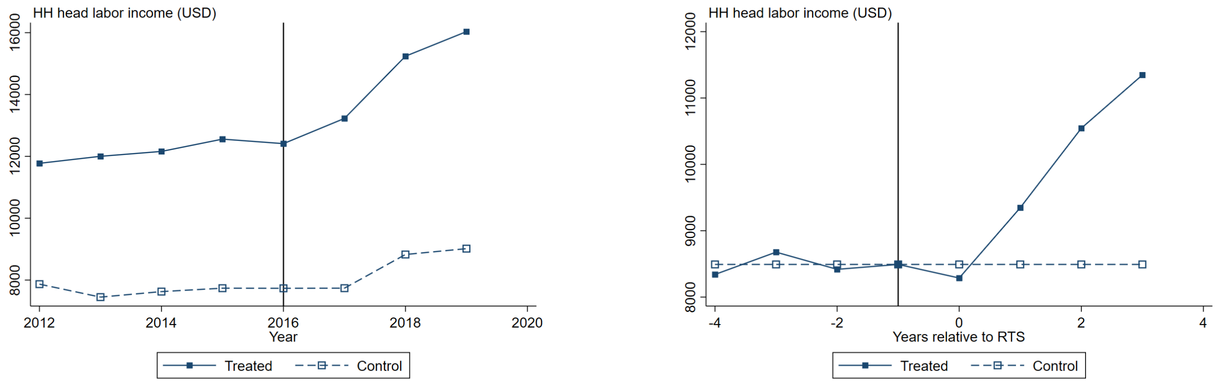
A Additional Tables and Figures

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

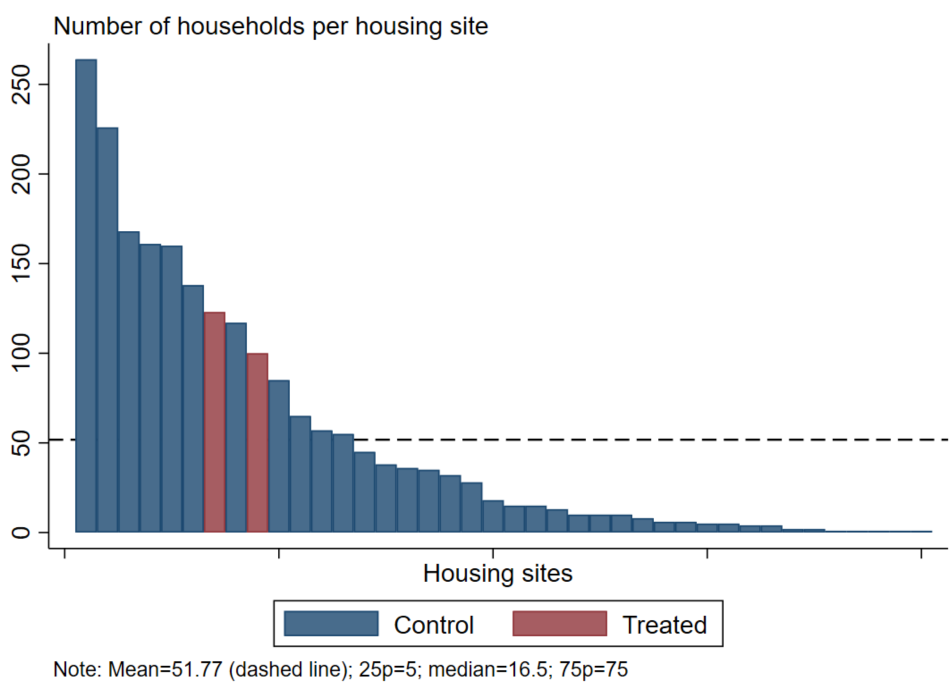


(a) Plotted means

(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 RTS 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

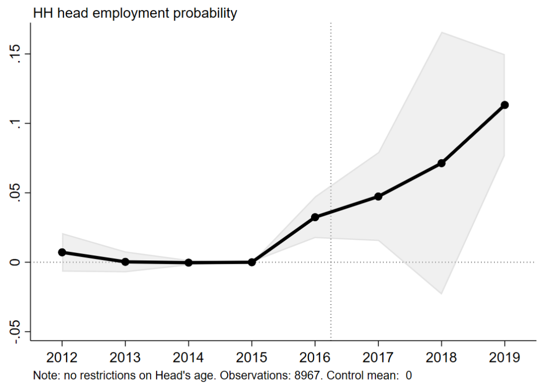
Notes: This table presents difference-in-difference estimates of the average treatment effect of the Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

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

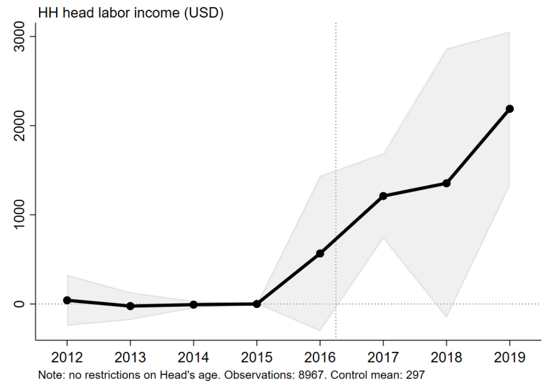
	(1) Earnings	(2) Non-labor income	(3) Total income
$HousingSite_1 \times Post_t$	1580.037***	-836.674***	743.364**
	(432.082)	(151.182)	(353.284)
$HousingSite2 \times Post_t$	1110.989**	-430.913***	680.077*
	(445.342)	(131.235)	(373.608)
Control mean	7975.031	8710.283	16685.314
% Housing Site 1 of control	19.812	-9.606	4.455
% Housing Site 2 of control	13.931	-4.947	4.076
P-value test 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 Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall 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.

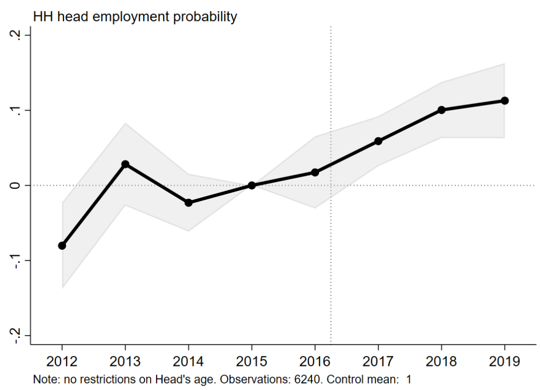
Figure A.6: Earnings and employment changes on the extensive and intensive margin



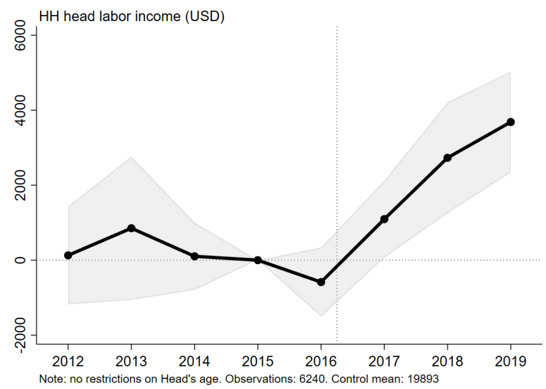
(a) Employment - prior non-workers



(b) Earnings - prior non-workers



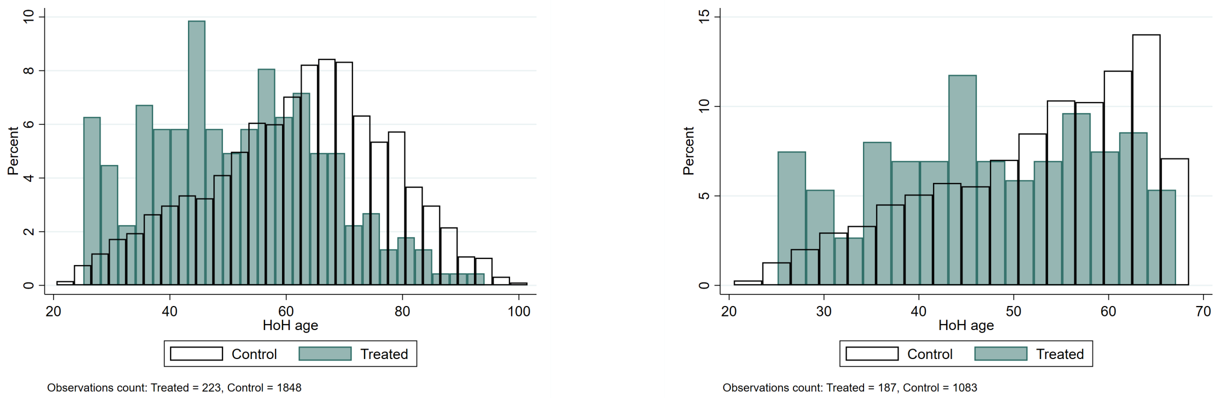
(c) Employment - prior workers



(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%)	

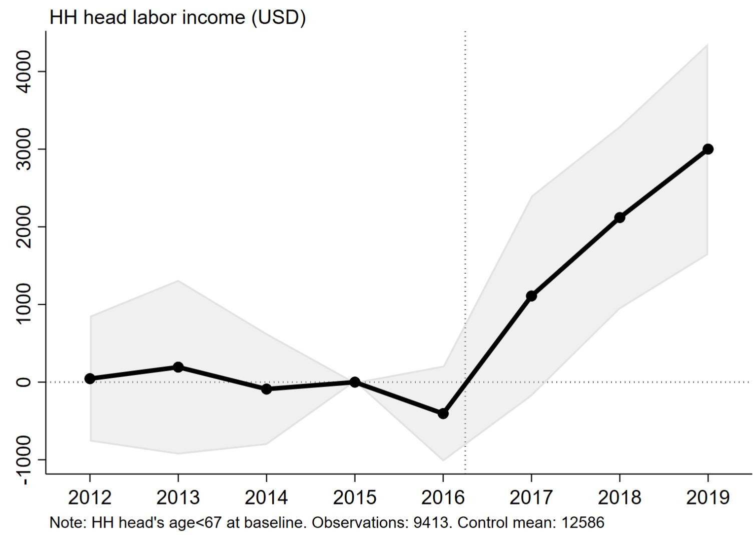
Notes: This table presents descriptive statistics using the CHA administrative data from 2015, one year before the RTS program started.

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

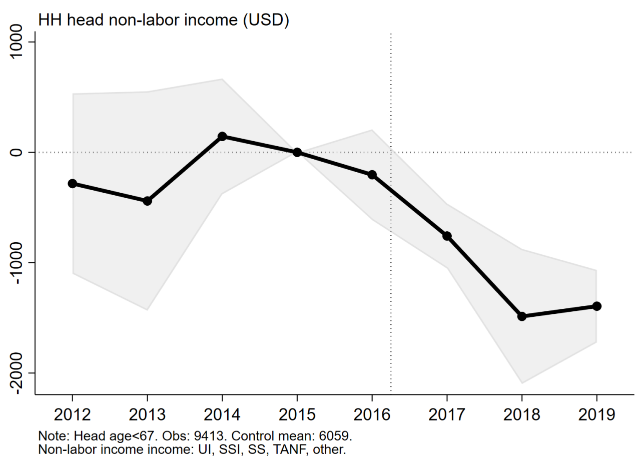
	(1) HH head labor income	(2) HH head non-labor income	(3) HH head total income
$Treat_i \times Post_t$	1392.132**	-814.178***	577.954
	(525.886)	(216.909)	(460.415)
Control mean	12586.032	6058.673	18644.704
% control mean	11.061	-13.438	3.100
Cluster Robust P-values	0.012	0.001	0.217
Ferman-Pinto P-values	0.028	0.001	0.207
Observations	9413	9413	9413

Notes: This table presents difference-in-difference estimates of the average treatment effect of the Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

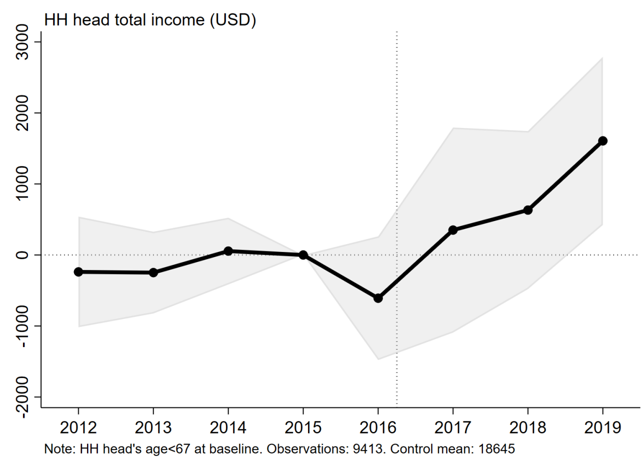
Figure A.10: Effect of the RTS program on younger head of household's income (age<67)



(a) Earnings



(b) Non-labor income



(c) Overall 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 RTS 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

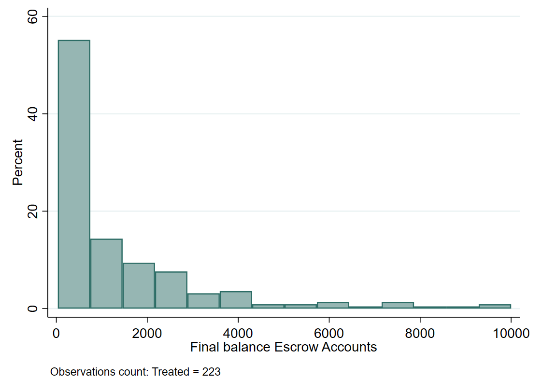
Notes: This table presents difference-in-difference estimates of the average treatment effect of the Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall income (column 3). All regressions include household head fixed effects and year fixed effects. Standard errors clustered 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

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 head 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 Rent-to-Save program on earnings (column 1), non-labor income (column 2), and overall income (column 3). Panel A shows estimates for female head of households 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, which adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in housing site size.

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 percent of treated heads of household (108 of 223) and 17 percent 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 and comments on the program, can be found in Section 4.2.5. 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
Awareness of other CHA programs			
Accomodation policy	0.732 (0.444)	0.689 (0.465)	0.404
Hardship policy	0.523 (0.500)	0.624 (0.487)	0.078
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.

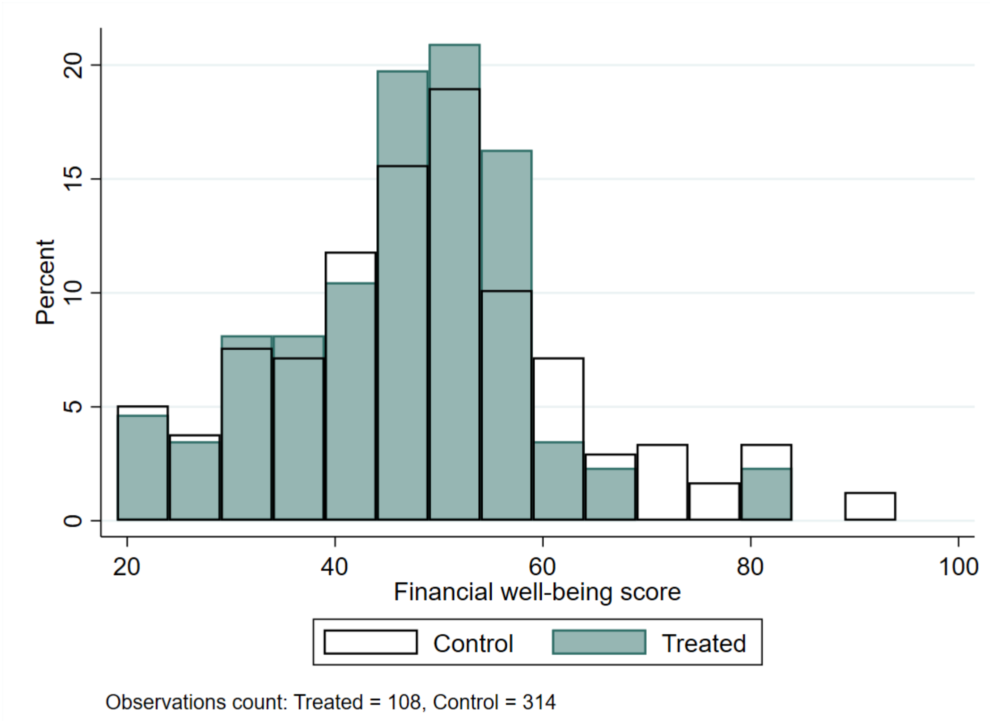
Given the low response rate of the 2017 survey—48 percent of treated heads of household and only 17 percent of control heads—the summary statistics in Table B.1 should be interpreted cautiously. Among those who did respond, treated participants are on average younger than controls, while the share of women is similar in both groups. Awareness of other Cambridge Housing Authority supports is broadly comparable: 69 percent of treated and 73 percent of control respondents know about the accommodation policy, and 62 percent vs 52 percent are familiar with the hardship policy. Self-reported financial behavior differs only on one dimension: investment activity is higher in the treated group (28 percent) than the control group (16 percent), whereas the proportions who saved (23 percent) or reduced debt (40 percent) 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. Visually, the shapes of the treated and control histograms are similar—the treated sample shows a slightly thicker bar in the 50–55 bin, while the control sample has a few more observations above 70—but overall the two distributions overlap substantially. 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.[13]

[13]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

POLICY + TECHNOLOGY LAB EAST/WEST

COMPASS
WORKING CAPITAL
WHERE FAMILIES ASPIRE. PLAN. INVEST

Rent-to-Save at Corcoran
Account Statement

Enclosed is a copy of your Rent-to-Save account statement:

On **March 1, 2016** your Rent-to-Save Account was automatically created. This account 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 into your account.

If your rent goes up because you're making more money at work, then
50%
of that rent increase is automatically placed into your account.

For example, if your monthly rent payment is $400, in three years you will have:

$144
if you do nothing and your rent stays the same.

$1,878
if you get a raise at work and your rent goes up to $500 a month.

To learn more about your account:
Call Lucia at 617-790- or
visit www.compassworkingcapital.com/Corcoran

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

Figure C.2: Letter

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	OPENING BALANCE
$400.00	$500.00	$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

POLICY + TECHNOLOGY LAB EAST / WEST

COMPASS
WORKING CAPITAL
WHERE FAMILIES ASPIRE, PLAN, INVEST

Have questions or concerns about your Rent-to-Save Account?

Drop by the Jefferson Park Community Room to discuss your account individually:

4:00pm-7:00pm
Wednesday, August 17th

You can also find out how the Rent-to-Save program can help you pay off debt, build credit, and prepare for homeownership or retirement.

Financial coaching is provided by the non-profit Compass Working Capital and any financial information you discuss is strictly confidential.

For more information about Compass visit www.compassworkingcapital.org or call Lucia at 857-317-

www.Rent-to-Save.org

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