Evaluating the Impacts of an Enhanced Family Self-Sufficiency Program

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ABSTRACT

We conduct an impact analysis of the Denver (CO) Housing Authority’s Home Ownership Program (HOP) that relies upon quasi-experimental methodologies (matching, inverse probability weighting) that permit one to draw causal inferences of program impacts with substantial confidence. HOP is an unusual, enhanced variant of the Family Self Sufficiency program that incentivizes and assists participants buy a home. We analyze whether, compared to the control group, HOP participants exhibited significantly earnings growth, economic self-sufficiency, and rates of home buying. We find that HOP participants with a high intensity of participation significantly improved in all four outcomes. These results are robust to model specification and insensitive to omitted variable bias typically found in social sciences. We conclude that a well-conceived and executed public housing authority program aimed at building the financial, human and social assets of low-income households receiving housing assistance can yield substantial benefits to participants.
INTRODUCTION

During the past two decades, public and scholarly discourse around antipoverty policy has shifted from an emphasis on providing income subsistence to one of asset development (Sherraden, 1991). Indeed, asset-building strategies have become important components of a comprehensive approach to providing upward mobility for low-income families (Shapiro & Wolff, 2001; Rohe, Gorham & Quercia, 2005, McKernan & Sherraden, 2008).

Subsidized housing policy has long been employed to enhance income- and asset-building among low-income families (Newman and Schnare, 1988; 1992). The Concerted Services Demonstration, the first programmatic experiment attempting to link public housing with services designed to foster economic independence, occurred in the 1960s. This was followed by two similarly oriented, small-scale demonstrations in the 1980s: Project Self Sufficiency and Operation Bootstrap (Bogdon, 1999; Rohe and Kleit, 1999). Since the mid-1980s, multiple generations of U.S. Department of Housing and Urban Development (HUD) programs have tried to blend housing assistance with a variety of supportive services designed to improve the economic wherewithal of recipients (Bogdon, 1999; Riccio, 1999; Bratt, 2008). In this so-called “housing plus” approach, the provision of basic shelter is augmented by services to support both resident families and the larger community development initiatives in which subsidized housing is located.

Arguably the primary programmatic manifestation of this reoriented subsidized housing policy has been the Family Self-Sufficiency (FSS) program. The National Affordable Housing Act of 1990 authorized FSS, administered by HUD. For the first time, all but the smallest local public housing authorities (PHAs) were required to help participants in their public and assisted housing programs reduce their use of various forms of public aid (Sard, 2001). Subsequently, the aims of FSS were bolstered by the Quality Housing and Work Responsibility Act (QHWRA) of 1998, which gave PHAs flexibility to serve more working poor households and tailor their rent policies to encourage work, and authorized smaller PHAs to develop their own FSS programs (Sard and Lubell, 1999; Cramer and Lubell, 2005). Moreover, QHWRA established new HUD rules for PHA accountability (wherein activities promoting resident self-sufficiency were assessed separately) and a HUD grant program (Resident Opportunities for Self-Sufficiency: ROSS) for supporting PHAs’ employment-promoting activities by subsidizing resident education and training (Sard and Lubell, 2000).

Each generation of self-sufficiency/asset building programs has been evaluated in some fashion: Project Self-Sufficiency in the late 1980s (HUD, 1987, 1988), Operation Bootstrap in the mid-1990s (Blomquist, et al., 1994; Frees, Ellen, and Holm, 1994; Frees, Ellen, and Locke, 1994), and the Jobs-Plus Demonstration (Riccio, 1999) and FSS Program in the late 1990s and early 2000s (Rohe and Kleit, 1999; Ficke and Piesse, 2004). Other evaluations have focused on local initiatives, such as the Lafayette Courts Family Development, a Baltimore Housing Authority pilot program (Shlay and Holupka, 1992; Shlay, 1993); the Gateway Program operated by the Charlotte Public Housing Authority (Rohe and Stegman, 1991; Rohe, 1995; and Rohe and Kleit, 1997); Housing Authority of Portland (Gibson, 2003) and the Rockford Housing Authority (Anthony, 2005). These evaluations uniformly have claimed that employment and earnings of program participants rose substantially, but low percentages of enrollees completed these programs. However, this evidence may be challenged because the aforementioned evaluations typically lack control groups or other statistical techniques that would make it possible to produce reliable estimates of causal program effects.
Our research attempts to respond to this methodological challenge by evaluating the personal impacts of completing an enhanced variant of the FSS program: the Denver Housing Authority’s (DHA) Home Ownership Program (HOP). In assessing impacts on earnings growth, self-sufficiency and homeownership we rely upon quasi-experimental methodologies that permit one to draw causal inferences with substantial confidence.

Our paper is organized as follows. The first section briefly reviews the prior evaluations of FSS. The second describes the DHA’s HOP initiative being evaluated and program participants. We then present our method for assessing program impacts. The fourth section presents our impact findings. The paper concludes with a discussion of the policy implications derived from our findings.

A Brief Review of the FSS Evaluation Literature

All FSS programs share several elements in common (Sard, 2001; Rohe and Kleit, 1999; Bratt, 2008). PHA staff and representatives from key local service providers form a Program Coordinating Committee that devises an action plan tailored to local realities, which must be approved by HUD. PHA resident households choose voluntarily to participate, and can be screened from participation only on a circumscribed set of characteristics related to motivation (Rohe and Kleit, 1999). Participants sign a five-year contract that delineates their interim and final goals (which must include independence from public assistance) and work or educational responsibilities, and supportive services that will be provided to them. Intensive case management is provided to assist in goal development and achievement, through counseling, information, referral, and advocacy. Finally, FSS provides for an escrow savings account, into which any marginal formulaic increases in rent associated with improvements in participants’ income are deposited. With approval of the PHA, participants can withdraw funds from their escrow account as part of fulfilling terms of their contracts, such as paying tuition for college. Upon successful completing of their contracts and exit from welfare assistance for at least 12 months, FSS participants receive a lump sum payment from their escrow accounts. Conversely, the failure to complete all program and contract requirements results in forfeiture of any funds held in escrow.

A number of PHAs have augmented their FSS Programs by adding elements designed to assist public housing families build financial assets through homeownership and individual development accounts. A 1996 survey of FSS programs by Rohe and Kleit (1999) revealed that 77 percent provided counseling about private rental and homeownership opportunities, 12 percent used homeownership as a benchmark for measuring participant success, and ten percent thought that homeownership assistance was the most important program element in attracting participants to FSS. The Charlotte, NC Housing Authority pioneered the homeownership-focused self-sufficiency strategy through its Gateway Program, which began in 1987 (Rohe, 1995; Rohe and Kleit, 1997). In addition to the FSS escrow accounts, some PHA-sponsored homeownership programs (see Santiago et al. 2010a, b) offer matched savings accounts (IDAs) as part of the PHA bundle of asset building initiatives or in partnership with local organizations. Moreover, ROSS funds provide financial support to public housing residents to build human capital assets through additional schooling and job training.

Lubell (2004a, b) and Sard (2001) underscore the potential of the FSS Program for helping low-income families build assets. Nearly half of FSS participants who had been enrolled in the program for at least 12 months held positive escrow balances averaging $2,400 and with average monthly deposits of $300 (Sard, 2001). Ficke and Plesse (2004) report that the median FSS escrow disbursement was $3,351. Individual
programs report greater asset accumulation and escrow payouts: $3,297 in 22 programs located Oregon and Washington (FSS Annual Report Summary, 2003); $7,000 in Portland (Reid and Lubbell, 2005); and $8,000 in Montgomery County, MD (Reid and Lubbell, 2005). Moreover, FSS participation has been linked to increases in participant earnings, economic self-sufficiency and homeownership (see Ficke and Piesse, 2004, Gibson, 2003. Unfortunately, attrition from FSS programs is quite high: Rohe and Kleit (1999) found that more individuals dropped out of FSS than completed; completion rates were less than 10 percent of all participants.

Riccio (2007) has argued that the evidence is weak as to whether FSS has improved participants’ employment, earnings and other self-sufficiency outcomes. He notes that the aforementioned evaluations typically lack control groups or other statistical techniques that would make it possible to deduce causal program effects. This skepticism has been bolstered by the ongoing evaluation of New York City’s Opportunity NYC-Work Rewards FSS program, wherein study participants were randomly assigned to different treatment groups (Verma et al. 2012). Interim results after two and a half years follow-up indicate that neither FSS nor an enhanced variant with incentives for sustained, full-time employment improved labor market outcomes for the full sample. The latter combination did, however, substantially improve employment and earnings outcomes for those who were not employed at the beginning of the study.

Our study aims to contribute to this ongoing debate about the efficacy of FSS. It examines an enhanced FSS program of a type that has previously not been evaluated. Moreover, it employs a variety of quasi-experimental methods that produce findings that can plausibly be considered causal.

THE DENVER HOUSING AUTHORITY’S HOME OWNERSHIP PROGRAM

The low-income family asset-building program we will evaluate is the Home Ownership Program (HOP) developed by the Denver Housing Authority. In the early 1990s, DHA initiated its basic Family Self Sufficiency (FSS) program focusing on improving the educational and employment opportunities of residents and their families. DHA established the HOP as an elective option for qualified FSS participants in late 1994. DHA was awarded a grant in 2000 under the Resident Opportunities for Self Sufficiency (ROSS) Program, which provided supplementary resources for expanding HOP to an additional 450 non-FSS clients by providing funding for their education and training.¹ This mix of funding streams inevitably introduced some heterogeneity into the HOP pool, with some being subject to the regulations and features of FSS, others to those of ROSS, and still others to both. In this analysis we estimate impacts averaged across the pool of 1,717 enrollees who entered the program during the Jan. 1, 2001 through Dec. 31, 2009 period.²

¹ Regrettably, there have been relatively low graduation rates for those who have enrolled in DHA’s FSS and HOP programs since 1995: 25 percent and 20 percent, respectively.
² We excluded 148 enrollees from the sample used to statistically model program impacts because of inability to ascertain the HOP beginning or end date information needed to estimate intensity of treatment. Further, for enrollees who entered HOP in 2008 and 2009 and who did not have a reported end date or a current participant flag in the administrative data, we assumed that they were still active in the program since in any given year approximately 400 residents are in HOP. Below the figures we report for program outcomes apply for this analysis sample of 1,569; for program costs per 100 enrollees we use the full sample of 1,717.
Program Design

In overview, HOP enrollees develop individual training and services plans outlining their human and financial capital asset development goals in collaboration with HOP case management staff. HOP enrollees are eligible for financial assessments, free credit reports, credit repair and money management counseling, classes on a wide variety of topics (e.g., budgeting, debt reduction, saving, purchasing assets such as cars), and Matched Savings Accounts. In addition to the education, counseling, and supportive services provided by the HOP, Denver’s program also includes several financial incentive and assistance programs that enable enrollees to acquire both financial and human capital assets (e.g., grants for tuition and books, child care assistance, transportation costs). FSS-supported HOP enrollees are also eligible for matched escrow accounts. Details follow.

HOP is designed to work in conjunction with its FSS and ROSS programs to overcome five major barriers to asset building among DHA residents: low income, high levels of debt, poor credit rating, lack of employment or job instability, and the lack of savings. To address the barrier of low income, the DHA’s FSS/ROSS programs have developed an employment component emphasizing education and job training. To address debt and credit issues, the HOP program works with DHA residents to develop specific strategies aimed at debt reduction and credit repair. Both programs work with residents to acquire job skills, encourage work and maintain stability of employment. Finally, to address insufficient savings, HOP utilizes the rent escrow account feature of the FSS program as well as matched savings accounts offered by DHA and other community partners to encourage savings for future asset purchases, such as further education and training, microenterprise and homeownership.

Two stages with different program treatments distinguish HOP. The initial stage is geared towards debt reduction, credit repair, savings accumulation, and employment enhancement. Roughly half of all enrollees have household incomes below $10,000 when they start HOP, and the program tries to build their incomes by 50 to 100 percent during this initial level. Those who are deemed by HOP staff to be within a year of purchasing their own home “graduate” to the advanced stage, the Home Buyers Club, where additional HOP investments are made. Requirements for entry into the Home Buyers Club include being employed with the current employer for at least one year (or have another stable source of mortgage repayments) and having personal savings of at least $500. The Home Buyer’s Club provides intensive real estate and finance training, presentations by housing industry representatives, peer support, and special benefits such as low mortgage interest rates, mortgage fee discounts, downpayment and closing cost assistance, and second mortgage assistance (if necessary). Integrated into the homeownership counseling sessions are discussions about what constitutes a sound home purchase. At this stage of the HOP program, enrollees are expected to attend 9 of the 12 classes offered during the course of the year, complete an intensive, one-day homeownership seminar offered by the Colorado Housing Finance Authority (CHFA), and pass a homeownership exam administered by CHFA.3 As enrollees approach the time to purchase a home, they meet regularly (often weekly or biweekly) with their case manager and other HOP program staff members.

Rent escrow accounts are used in the HOP program for enrollees who also are involved in the Family Self-Sufficiency Program. These HOP/FSS enrollees are able to accumulate escrow funds that are distributed only if they meet all of the goals of their

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3 Home Buyer’s Club participants earn $20 towards downpayment and/or closing costs for every such class they attend
FSS contracts. Escrow funds reflect the additional rent costs that are associated with their increased earnings. Rents are capped at 30% of income at the time of entry into the FSS program. Additional rents in excess of this income cap are placed into an escrow account. For some HOP enrollees, these escrow payouts have exceeded $40,000 at time of FSS Program completion although the average payout was $7,300. HOP program enrollees entering under the ROSS program are eligible to receive $1,000 toward costs associated with education and training. Matched Savings Accounts (MSAs) are available to all HOP enrollees through DHA and are matched at a rate of 1:1 up to a maximum participant contribution of $1,500. MSA account funds plus the DHA match are available only for those who successfully complete HOP and go on to purchase a home through the auspices of the DHA. Penalties for program noncompliance are extensive. In addition to loss of escrow funds and program termination, noncompliant HOP enrollees also lose the DHA match from their matched savings accounts.

The only qualification for participation in HOP is that the individual be (and remain) a DHA resident or Housing Choice Voucher (HCV) subsidy recipient for the duration of their involvement with the program. HOP enrollees complete initial as well as ongoing needs assessments and goal setting. Further, the Denver program offers intensive, on-site case management. Unlike generic FSS contracts, the HOP in Denver does not set time limits for homeownership achievement and asset building counseling. The overarching goal of the program is to enhance self-sufficiency and enable enrollees to leave public housing or voucher subsidies, with homeownership as one of the asset building goals of the program.

Enrollees in HOP

We will refer to all who ever qualified and chose to register for HOP as “enrollees.” In 2009, there were approximately 475 DHA public housing and HCV residents enrolled in the initial level of the HOP program and 20 participants in the Home Buyers Club. According to estimates derived from unpublished DHA data sources, HOP enrollees have numerous characteristics that distinguished them from the typical DHA resident, underscoring the self-selection into the program. Significantly higher fractions of HOP participants are heads of mother-only families with children. HOP enrollees are significantly younger: nearly two-thirds are under the age of 40. Further, they are slightly more likely to speak English and be U.S. citizens. Conversely, they are less likely to be disabled. Significantly higher proportions of HOP enrollees have changed units to obtain employment and training; lower proportions have been evicted for whatever reason. Although during the 2001 through 2009 period there was some overrepresentation of enrollees from two DHA developments—the North Lincoln Campus of Learners and the Curtis Park HOPE VI project—HOP enrollees come in nearly equal numbers from the conventional developments, scattered-site housing units and from HCV residents. As a result, the HOP enrollee pool is drawn from a much broader set of neighborhoods than noted in previous evaluations of programs in Charlotte, Rockford or Seattle.

Given the strong and understandable distinctions between the pool of HOP enrollees and the generic DHA clientele, it would be inappropriate to employ the latter as a control group in measuring program impacts on the former. Instead we employ as controls those enrollees who only participated minimally before dropping out of HOP, as we amplify below.

MEASURING IMPACTS FROM HOP PARTICIPATION
Analytical Strategy

In order to estimate the impact of a policy intervention, one must meet several assumptions under the potential outcomes framework (Neyman, 1990; Rubin, 1974). In a randomized policy experiment with both a random sample of subjects and random assignment to treatment (T), the average treatment effect (ATE) of outcome Y is defined under a potential outcomes framework as the difference in means between the treatment (Y\textsubscript{i1}) and control group (Y\textsubscript{i0}):

\[
\text{ATE} = E(Y_{i1} \mid T_i=1) - E(Y_{i0} \mid T_i=0)
\]  

[1]

However, in the case of the HOP, treatment assignment is not randomized, but based on the aforementioned eligibility criteria. Suppose that for each individual observation i during time t (before the program treatment is applied) the outcome (Y) under investigation is a function of a set of measured personal characteristics ([X]) and unmeasured, fixed (i.e., time invariant) personal characteristics ([U]), plus an error term:

\[
Y_{it} = \alpha_{t} + [X_{it}] [\beta] + [U] [\delta] + \varepsilon
\]  

[2]

Similarly, in a later period t+1 after the treatment T has been applied (to some i) the outcome can be described as a function of the current set of measured characteristics and the same set of unmeasured characteristics:

\[
Y_{it+1} = \alpha_{t+1} + [X_{it+1}] [\beta] + \sigma T_{it+1} + [U] [\delta] + \gamma
\]  

[3]

where T=1 if individual treated; zero otherwise. The main challenge in accurately assessing \(\sigma\), the average treatment effect, is that [U] may be correlated both with \(\gamma\) and T due to selection into the treatment group. Furthermore, due to functional form assumptions of regression, a misspecified [X] may bias \(\sigma\) in either direction (Ho et al., 2007).

In this study, although we do not observe HOP participants’ outcome without HOP, or control participants’ outcome with HOP, we can still make progress and estimating the treatment effect on the treated (ATET) provided we meet the conditional independence assumption, also called ignorability (i.e., formally Y\textsubscript{it}, Y\textsubscript{i0} \perp T\textsubscript{it+1} \mid X\textsubscript{it+1}):

\[
\text{ATET} = E(E(Y_{it+1} \mid X_{it+1}, T_{it+1}=1) - E(Y_{it+1} \mid X_{it+1}, T_{it+1}=0) \mid T_{it+1} = 1)
\]  

[4]

We employ two strategies for pre-processing the data to meet the conditional independence assumption in order to estimate ATET. We make the argument that [U] should be correlated with [X] based on face validity, and claim conditional independence. For three of our HOP outcomes, we employ nearest neighbor matching using Mahalanobis distance (Rubin, 1980). In the case of earnings outcome after HOP, a difference-in-differences regression analysis combined with inverse probability weights (Wooldridge, 2002), as explained below.

Matching Samples

Statistical matching of samples based on observed characteristics of treatment and control groups as a way of reducing bias from selection has been employed frequently in impact analyses (Heckman, Ichimura and Todd 1997, 1998; Heckman and
Navarro-Lozano, 2004). Although such matching procedures assume that unobservables are highly correlated with observed characteristics, recent work suggests that this may not be implausible given that matching methods have been shown to approximate experimental results when a wide array of covariates is included (Shadish, Clark and Steiner, 2008, Cook and Steiner, 2010). As suggested by Ho et al. (2007), Angrist and Pischke (2009) and Crump et al. (2009) we use matching before estimating ATET [4]. Specifically, we use the Mahalanobis distance metric and one to one matching with replacement. Because the matching pre-processing is orthogonal to the estimate of the treatment effect, this provides an added advantage: no additional adjustments for multiple comparisons are required to the standard errors (Angrist and Pischke, 2009).

Our matching employs individual characteristics related to treatment assignment including gender, ethnicity, in FSS, in ROSS, DHA housing type (conventional public housing or HCV), and starting year in the program. One limitation is a known source of missing data: baseline eligibility data (e.g., baseline savings, baseline employment) are not available for the entire sample. To ensure that the results are robust to different matching specifications (e.g., one to many matching and matching with a propensity score), we assessed covariate balance using standardized differences and variance ratios (Austin, 2009). Finally, we would note that all in our sample reside in the same metropolitan area, thereby implicitly controlling for one element that has previously been identified as a crucial source of bias (Heckman, Ichimura and Todd, 1997). In order to assess the potential omitted variable bias, we use sensitivity analysis by calculating Rosenbaum’s bounds (Rosenbaum, 2002). This analysis tells us how large the omitted variable bias would have to bias treatment assignment in odds ratios to in turn bias the estimate of the ATET\(^4\).

\textit{Difference-in-Differences Regression Model Using Inverse Probability Weighting}

In cases where covariate balance is not achieved between treatment and control groups, it is possible to use regression to adjust estimates of ATET and reduce bias. This is particularly well suited for interventions with attrition (Wooldridge, 2007). Under the similar set up, we could also specify a model that combines observations at t and t+1 for both treatment and non-treatment (control) groups and distinguishes pre- and post-treatment periods of observation (P=1 if post-treatment period t+1, 0 otherwise):

\[ Y_i = \alpha_i + [X_i] [\beta] + \psi T_i + \lambda P_i + \sigma T_i P_i + [U_i] [\delta] + \varepsilon \]  \hspace{1cm} [5]

The parameter \( \psi \) gives us the initial, pre-treatment “difference” between treatment and control groups’ \( Y \); \( \lambda \) gives the difference in outcomes for both groups that may be only due to temporal shifts in unmeasured factors affecting \( Y \) between t and t+1; again \( \sigma \) is the measure of the treatment effect, the “differences in differences” (D-in-D) post-treatment. This model assumes that T controls for [U], insofar as any systematic differences across the groups in [U] should be measured by the pre-treatment “difference” between treatment and control groups’ \( Y \). Note that the basic D-in-D models omit the [X] term, assuming that any differences will be picked up in the initial, pre-treatment “difference” between treatment and control groups. This is not as strong as trying to control for them explicitly, making it more plausible that T controls for [U], not the combination of [X] and [U]. Thus we employ inverse probability weighting to condition

\(^4\) Estimate of ATET is conducted in Stata 14 using tefects nnmatch with robust Abadie and Imbens (2011) standard errors. Covariate balance tables available upon request. We use the rbounds package in Stata 14 to conduct sensitivity analysis.
on [X] before applying regression regression adjustment [3], thus providing a doubly robust estimate of the ATET (Wooldridge, 2007).\(^5\) This estimator is consistent if the D-in-D regression is correctly specified or the treatment assignment model is correctly specified. Of course, both inverse probability weighting and D-in-D models can fall prey to time-varying unobservables differentially and systematically impinging on treatment and control groups that might be strongly correlated with Y (as do all experimental designs). Accordingly, we conduct sensitivity analysis for this model as well.

**Measuring Treatment and Control Groups**

For purposes of this analysis we have simplified the operationalization of “treatment” under the HOP because the treatments as described above were often bundled and administered in idiosyncratic ways in different temporal patterns. Here we define those who have been “treated” by HOP as “participants.” These individuals are enrollees who have remained in the program at least 12 months since enrollment and participated in an appropriate number of HOP activities; within the participants we distinguish those who received Home Buyers Club training. Participants have received somewhat varied types and intensities of treatments, experienced different tenures within HOP, and differed on whether they ultimately bought a home through the auspices of HOP. Thus, we will focus on estimating “average treatment effects on the treated” across this treatment bundle, distinguishing only two varieties of treatment: “moderate intensity” (greater than or equal to 12 months in HOP; did not enter HBC: 59.2 percent of enrollees) and “high intensity” (participated in HBC regardless of length of duration or completion: 18.4 percent of enrollees). The breakdown of the participant group by broad categories of intensity of treatment and home tenure outcome is presented in Table 1.

![Table 1 about here]

Our “control” group receiving only a low-intensity treatment consists of enrollees who initially qualified for and voluntarily enrolled in HOP but either never attended HOP activities or dropped out of the program within a year; this group constitutes 22.5 percent of our 2001-2009 HOP cohort. Face validity suggests this is a sensible control group, as they met the same eligibility criteria and self-selected to enroll like the treatment group. For our impact analysis we will create synthetically even closer comparability through matching and inverse probability weighting, as explained below.

For this paper we conducted four quasi-experimental impact evaluations of HOP for the outcomes of: a) earnings growth during HOP, b) earnings growth after HOP, c) self-sufficiency and d) homeownership.\(^6\) The impact was estimated by assessing differences across matched samples. This approach has been shown to yield a plausible estimate of causal impact (Heckman, Ichimura and Todd 1997, 1998; Heckman

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\(^5\) We use the tefects ipwra command in Stata 14. This model allows estimate of either ATE, as specified in equation [5], but for consistent presentation we estimate ATET. The outcome model contains a subset of the treatment model: gender, DHA housing type, in FSS, and HOP start year. The start year was converted to a binary variable at the midpoint of the study period in order to avoid multicollinearity and allow the maximum likelihood function to converge.

\(^6\) By “after HOP” we mean the period between HOP exit and time of personal survey conducted as part of our study, results are annualized to make estimates comparable. By “self-sufficiency” we mean that the participant either: (1) moved out of DHA to accept employment in another locale; (2) evinced income gains so substantial that they disqualified the family from housing assistance and/or (3) chose to move into private rental or owner-occupied accommodation because of their improved economic circumstances.
and Navarro-Lozano, 2004; Shadish, Clark and Steiner, 2008, Cook and Steiner, 2010) For our earnings impact growth after HOP estimates, we employed a difference-in-differences regression adjustment combined with inverse probability weighting (Wooldridge, 2002) to gain a doubly robust impact estimate.

Results

We found that the HOP moderate-intensity treatment group did not evince any statistically significant differences in outcomes compared to the matched low-intensity treatment group. HOP participants who were intensely treated (i.e., participated in Home Buyers Club activities) gained considerably, however, in all four realms compared to both matched low-intensity and moderate-intensity groups. The impact parameter estimates and their standard errors are presented in Table 2. The HOP high-intensity treatment increased annual earnings during HOP by $3,294, increased annual earnings after HOP by $3,460, increased the probability of becoming self-sufficient by 0.40, and increased the probability of becoming a homeowner within five years of program enrollment by 0.27. All these impact parameters proved statistically greater than zero at the .01 significance level.

Because estimation of treatment effects can be biased by an unobserved variation in the treatment assignment process, we conducted sensitivity analysis to assess the effect of possible bias on our results. The ATET of HOP on increased annual earnings during HOP is insensitive to an omitted variable with an odds ratio as great as 2.0, the ATET of HOP on increased annual earnings after HOP is insensitive to an omitted variable with an odds ratio as great as 4.0, the ATET of HOP on becoming self-sufficient is insensitive to an omitted variable with an odds ratio as great as 4.0, and the ATET of HOP on becoming a homeowner within five years of program enrollment is insensitive to an omitted variable with an odds ratio as great as 2.0. It is unlikely that some combination of unobserved covariates would have an odds ratio as high as 2.0 for this social intervention after conditioning on gender, ethnicity, in FSS, in ROSS, DHA housing type, and starting year in the program. We conclude that the outcomes are the result of having high intensity in the program and not due to the selection process or attrition. For point estimates of the upper and lower bounds of ATET and confidence intervals on the ATET in the presence of different odds ratios, see Table 3. As we amplify below, all these outcomes generate a variety of benefits for not only the participants but also for their children and the communities in which they purchased homes.

CONCLUSION

The last two decades have witnessed an unmistakable evolution of assisted housing policy in the United States. As epitomized by the Family Self-Sufficiency Program, decent, affordable housing increasingly has been viewed not merely as an end in itself but as a means for family asset-building and reductions in their long-term needs for public assistance. We have conducted an impact analysis of an enhanced FSS

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7 For simplicity we present the impact estimates derived from the test of the high-intensity group compared to a matched sample of the combined low- and moderate-intensity group.
program implemented by the Denver Housing Authority, employing quasi-experimental approaches from which plausible causal inferences can be drawn.

The foremost policy implication from our analysis is clear. A well-conceived and executed public housing authority program aimed at building the financial, human and social assets of low-income households receiving housing assistance can yield substantial benefits to participants. Of course, we acknowledge that the experience with HOP cannot necessarily be generalized to all FSS programs run by other housing authorities. Indeed, we recognize that DHA is an exceptionally well-run and innovative authority, having been awarded HUD’s “high performer” designation for many consecutive years. For a wider-ranging, more general investigation of the impacts of FSS, analysts must wait for the currently ongoing evaluation being conducted for HUD by MDRC (Verma, Tessler, Miller, Riccio, Rucks, & Yang, 2012).

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**Table 1. Distribution of HOP enrollees by intensity of treatment and mean outcomes (2001-2009 cohort)**

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Low Intensity</th>
<th>Moderate Intensity</th>
<th>High Intensity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share of Enrollees</td>
<td>22.5%</td>
<td>59.2%</td>
<td>18.4%</td>
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<tr>
<td>Mean Annual Earnings Change during HOP</td>
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<td>1353</td>
<td>4411</td>
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<tr>
<td>Mean Annual Earnings Change after HOP**</td>
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<td>468</td>
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<tr>
<td>Self-Sufficiency Exit*</td>
<td>14.0%</td>
<td>12.6%</td>
<td>54.8%</td>
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<tr>
<td>Purchased Home Within 5 Years</td>
<td>4.5%</td>
<td>13.5%</td>
<td>52.5%</td>
</tr>
</tbody>
</table>

* Moved into private rental voluntarily or income-ineligible for DHA; purchased home; moved for higher education
** Low- and Moderate-Intensity groups combined due to small N
Note: mean values for outcomes are not adjusted
Table 2. Estimated impacts of high-intensity HOP treatment

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Low/Mod Intensity Mean</th>
<th>High Intensity Mean</th>
<th>Difference (ATET)</th>
<th>AI Robust Std. Err.</th>
<th>z</th>
<th>Treated Obs</th>
<th>Control Obs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean Annual Earnings Change During HOP</td>
<td>$1,116</td>
<td>$4,411</td>
<td>$3,294</td>
<td>$628</td>
<td>5.25</td>
<td>234</td>
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<td>Mean Annual Earnings Change After HOP</td>
<td>$789</td>
<td>$4,249</td>
<td>$3,460</td>
<td>$1,077</td>
<td>3.21</td>
<td>77</td>
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<tr>
<td>Purchased Home Within 5 Years of HOP Start</td>
<td>.02</td>
<td>.30</td>
<td>.27</td>
<td>.03</td>
<td>8.78</td>
<td>241</td>
<td>241</td>
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<tr>
<td>Self-Sufficiency Exit*</td>
<td>.14</td>
<td>.55</td>
<td>.40</td>
<td>.04</td>
<td>10.45</td>
<td>241</td>
<td>241</td>
</tr>
</tbody>
</table>

Note: All differences significant at $p < .01$; impacts based on matched or weighted sample comparisons (see text for details).

* Moved into private rental voluntarily or income-ineligible for DHA; purchased home; moved for higher education.
Table 3. Rosenbaum’s Sensitivity Analysis of Treatment Effect Estimates

<table>
<thead>
<tr>
<th></th>
<th>Gamma</th>
<th>sig+</th>
<th>sig-</th>
<th>t-hat+</th>
<th>t-hat-</th>
<th>CI+</th>
<th>CI-</th>
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<td>Mean Annual Earnings</td>
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<td>2164</td>
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<td>.060</td>
<td>.000</td>
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<td>.000</td>
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<td>$3,798</td>
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<td>.000</td>
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<td>$1,224</td>
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<td>.000</td>
<td>$1,224</td>
<td>$4,621</td>
<td>$522</td>
<td>$7,074</td>
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<tr>
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<td>.000</td>
<td>$1,224</td>
<td>$4,621</td>
<td>$522</td>
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<tr>
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<td>Self-Sufficiency Exit*</td>
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</table>

Note: gamma = odds of differential assignment due to unobserved factors; sig+ = upper bound significance level; sig- = lower bound significance level; t-hat+ = upper bound Hodges-Lehmann point estimate; t-hat- = lower bound Hodges-Lehmann point estimate; CI- = lower bound confidence interval (a=.95); CI+ = upper bound confidence interval (a=.95) * Moved into private rental voluntarily or income-ineligible for DHA; purchased home; moved for higher education.