Ten-Year Impacts of Individual Development Accounts on Homeownership: Evidence from a Randomized Experiment

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ABSTRACT

This paper presents evidence from a randomized field experiment to evaluate the long-term impact of an incentive for household saving. We examine the effect on homeownership of an Individual Development Account (IDA) program which ran from 1998 to 2003 in Tulsa, Oklahoma. The IDA program provided low-income households with financial education and matching funds for qualified savings withdrawals, including a 2:1 match for housing down payments. About 90 percent of treatment group members opened IDA accounts, and contributions averaged about $1,800. Homeownership rates for both treatment and control groups increased substantially throughout the experiment. Prior work shows that from 1998 to 2003, homeownership rates increased more for treatment group members than for controls. We show in this paper, however, that control group members caught up rapidly with the treatment group after the experiment ended, so that the IDA program had no significant effect on homeownership rates among the full sample in 2009 and had no effect on the duration of homeownership during the study period. The program had a positive impact on homeownership rates among those with above-sample median income ($15,840) at the time they entered the program, but not on other subgroups that we tested.

I. Introduction

How can public policy help low-income people improve their long-term economic prospects? The United States has historically focused on a combination of income maintenance, consumption support, and work incentives to help families maintain a minimum level of subsistence. In recent years, an additional approach has aimed to complement traditional policies by helping low-income households save and accumulate wealth. These programs often provide subsidies to save for a home, get post-secondary education, open or run a business, save for retirement, or save for their children's education.[1]

Individual Development Accounts (IDAs) are a policy tool designed to help low-income people accumulate wealth. As described by Michael Sherraden (1991), IDAs provide people with saving accounts in which withdrawals are matched if they are used for qualified purposes. IDAs were proposed as a universal and progressive system of accounts starting as early as birth. During a demonstration period, they have been implemented as a targeted savings strategy for low-income individuals. From 1999 through 2008, more than 50,000 IDAs were opened at 544 project sites through the federal Assets for Independence (AFI) Program, which provided grants to community-based organizations and local governments (Department of Health and Human Services 2010). Variants of IDAs are now in place or being proposed in numerous other

[1] Beyond the general goal of encouraging wealth accumulation, there are several motivations for encouraging saving by low-income people. First, many public policies already encourage asset accumulation via saving incentives, housing subsidies, and other means. Most benefits, however, accrue to people in the top half of the income distribution (Sherraden 1991; Laurence S. Seidman 2001; Lillian G.Woo, F. William Schweke, and Buchholtz 2004). Second, compared to income-transfer approaches to poverty reduction, asset-development approaches may have greater potential to foster sustainable economic development (Signe-Mary McKernan and Sherraden 2008; Caroline Moser and Anis A. Dani 2008). Third, while the acquisition of major non-financial assets (e.g., a house) can transform a household's standard of living, the up-front financial cost may be out of reach for low-income people (Thomas M. Shapiro 2004). Fourth, the process of accumulating assets may in itself alter people's outlooks and choices, perhaps making them more future-oriented (Sherraden 2001; Daphna Oyserman and Mesmin Destin 2010). Fifth, people need savings to weather temporary setbacks such as a spell of unemployment or an unexpected expense. Sixth, some existing federal policies—such as asset tests for eligibility for particular programs—may discourage wealth accumulation by low-income households. See also Edward N. Wolff (2001), Eric Hurst and James P. Ziliak (2006), Melvin L. Oliver and Shapiro (2006), McKernan, Caroline Ratcliffe, and Yunju Nam (2007), and John K. Scholz and Ananth Seshadri (2009).

countries, as are matched saving accounts for children (Vernon Loke and Sherraden 2009; Rani Deshpande and Jamie M. Zimmerman 2010).

Previous experimental research on IDAs is limited.[3] In *learn$ave*, a randomized IDA experiment in Canada starting in 2001, IDAs had positive impacts on post-secondary education and small-business start-up, two of the qualified uses of contributions in that program (Norm Leckie et al. 2010).

The only randomized experiment with IDAs in the United States took place in Tulsa, Oklahoma from 1998 to 2003 at the Community Action Program of Tulsa County (CAPTC). Eligible applicants—those who were employed and who had prior-year adjusted gross income of below 150 percent of the poverty level—were randomly assigned into a treatment group or a control group. Treatment group members could open an IDA, and contributions of up to $750 per year for three years were matched at 2:1 if withdrawn and used for home purchases or at 1:1 if used for other qualified purposes, which included home repair, investing in a small business, post-secondary education, or saving for retirement. Control group members were restricted from opening an IDA. All project participants were restricted from other homeownership programs at CAPTC. After the four-year experimental program period, IDA eligibility was terminated for the treatment group and members of both the treatment and control groups were released from restrictions on using other CAPTC programs.

The effects of the experiment on homeownership and wealth through 2003 are evaluated

[3] IDAs have also been studied using non-experimental methods. A number of studies (e.g., Gregory Mills et al. 2008b; Ida Rademacher et al. 2010) have compared IDA participants to samples of non-IDA participants. These comparisons are less than ideal because, as we show below, people who signed up for the Tulsa experiment are a non-random sample of low-income households. Other studies examine associations of IDA program and participants characteristics with IDA saving outcomes (Mark Schreiner and Sherraden 2007). These studies are informative but they cannot control for self selection into IDAs nor were they designed with exogenous variation in program design that would enable simple impact tests. Another set of studies (Margaret S. Sherraden et al. 2005; Sherraden and Amanda Moore McBride 2010) report results of in-depth interviews with IDA participants. These analyses illuminate participation patterns in the IDA program and document participants' assessment of results, and do not claim to test impacts.

in three recent studies that report similar results (Michal Grinstein-Weiss et al. 2008; Mills et al. 2008a; Chang-Keun Han, Grinstein-Weiss and Sherraden 2009).[4] The program had a positive and statistically significant impact on homeownership rates over the first five years. Among households who rented at baseline, homeownership rates between 1998 and 2003 rose by 7 to 11 percentage points for treatment group members relative to control group members. Estimated effects on other qualified uses of the withdrawals and on net worth were imprecise and often inconsistent in sign.

These results can be described as short-term impacts. Participants had three years to save in their IDAs, and then they had another six months to use their funds for matched purposes. Longer-term analysis is important for understanding the benefits and costs of IDAs, for at least two reasons.

First, longer-term effects are the ultimate goal of interventions to increase saving, and such effects may take time to develop. For example, saving for a down payment may require more than three years, especially for low-income households. People might initially use the IDA to invest in education, in which case their homeownership rates and financial wealth levels may not be affected until much later. Starting a business may yield higher or lower returns during the start-up period relative to a longer period of time. As a result, long-term performance is an important aspect of possible IDA impact.

Second, there is no experimental study on the long-term effects of IDAs on homeownership and, indeed, very little long-term experimental evidence regarding saving policies in general. Analysis of other (non-saving) policies has shown that long-term effects can

[4] Gary V. Engelhardt et al. (2010) use IDA treatment status as an instrument for homeownership and find no net impact of homeownership on the provision of social capital.

be stronger or weaker than short-term effects.[5] The incentives built into the Tulsa IDA experiment suggest one reason why the long-term effects may be smaller than the short-term effects. Specifically, treatment group members had incentives to purchase homes before the end of 2003 (to receive a 2:1 match) while control group members had incentives to delay home purchases until 2004 (when they would become eligible once again for a variety of CAPTC home-buyer assistance programs). On the other hand, financial education and the impact of the very act of saving and owning wealth (as posited by Sherraden 1991) might spur members of the treatment group to even greater gains after the program ended in 2003.

This paper examines the effects of the Tulsa IDA program on homeownership rates in 2009 and on the duration of homeownership over the 1998-2009 period. The analysis is based on a new survey of treatment and control group members taken about 10 years after the start of the experiment. The hypothesis, formed at the outset of the experiment and tested here, is that IDAs will increase homeownership. To provide some context, we show that between 1998 and 2009, homeownership rates increased dramatically for both the treatment and control groups. This result speaks to the importance, when identifying the effects of an IDA program, of having a control group in order to account for the non-random selection of participants into an IDA and for location-specific influences on homeownership.

Our raw difference-in-difference estimates show a positive (5.5 percentage points) and marginally significant ($p < 0.08$) long-term impact of IDAs on the 2009 homeownership rate. This result, however, is driven by differing homeownership rates for the treatment and control group at baseline. Once we control for this, the difference-in-difference is 1.7 percentage points for owners and 2.7 percentage points for renters, with neither effect statistically significant.

[5] See Douglas Almond and Janet Currie (2010) for a discussion and review of long-term impacts of early childhood interventions and Raj Chetty et al. (2010) for a recent contribution to that literature.

Likewise, in ordinary least squares regressions and propensity score analyses, the 1998–2003 Tulsa IDA experiment has no statistically significant impact on homeownership after 10 years. Combined with earlier results showing positive and significant impacts on homeownership through 2003, our findings are consistent with the incentives embedded in the program, which encouraged treatment group members to buy homes before the end of 2003 and encouraged control group members to postpone home purchase until 2004 or later, when they could take full advantage of IDAs and other homeownership programs at CAPTC. Additionally, because the control group caught up quickly, we find that IDAs had no statistically significant impact on the duration of homeownership during the study period.

We do find some evidence of program impacts on one population subgroup. Over the ten-year period IDAs raised homeownership rates and raised the duration of homeownership for households with above-sample-median incomes relative to those with below-sample-median incomes. IDAs in the Tulsa experiment were targeted to those with low incomes and sample median annual household income was $15,840. However, there were no statistically significant effects for a variety of other subgroups tested.

Besides providing the first evidence on long-term effects of IDAs on homeownership, this is the first study (to our knowledge) to examine the long-term effects of any randomized experiment on saving behavior, this despite a large literature on the effects of billions of dollars of annual public expenditure for subsidies for private saving. The exogenous assignment of treatment status in the current paper creates a rare experiment on the impact on saving subsidies (see also Nava Ashraf, Dean Karlan, and Wesley Yin 2006, Esther Duflo et al. 2006, and Emmanuel Saez 2009 for saving-related experiments). Also, although it is not exclusively a first-time home-buyers program, the Tulsa IDA program provided strong incentives to purchase homes. Engelhardt (1996, 1997) finds strong effects of a Canadian first-time home-buyer's tax subsidy, but there is little evidence from the United States.

The rest of the paper is organized as follows. Section II discusses the experimental design. Section III describes the data and presents descriptive statistics for the analytic sample. Section IV outlines our methods. Sections V and VI present analysis of the effects of the IDA program on homeownership rates and the duration of homeownership over the ten-year period. Section VII discusses issues relating to internal and external validity. Section VIII interprets the results.

II. Experimental design

A. The Tulsa Experiment

The Tulsa experiment was part of the American Dream Demonstration (ADD), a set of 14 philanthropically-funded local IDA programs begun in the late 1990s.[6] The IDA program in Tulsa, Oklahoma was administered by CAPTC, and was the only ADD program that was implemented as a random assignment experiment. Recruitment of participants for the experiment took place from October 1998 to December 1999. CAPTC staff recruited participants through contact with people already associated with the organization through the receipt of other CAPTC services, links to other local social-service agencies, and word-of-mouth. Eligibility rules required applicants to be employed with household income below 150 percent of the federal poverty guideline. No other limits were placed on applicants' eligibility.

Participants in the experiment were informed of the nature and goals of the IDA program

[6] The Corporation for Enterprise Development (now known as CFED) proposed and organized ADD. Research on ADD was conceived and initiated by the Center for Social Development (CSD) at Washington University in St. Louis. For the ADD experiment, CSD organized selection of the site and the survey firm, and drafted the initial survey instrument.

and notified that they would not be able to use other matched savings programs at CAPTC nor could they receive any financial assistance for homeownership from CAPTC for the four years of the study period. As a result, during the experimental period through 2003, treatment group members had access to the CAPTC IDA, while both control and treatment group members had available to them a set of other subsidy options at CAPTC that was *less* attractive than those available to the typical low-income household. After 2003, treatments and controls reverted to being eligible for all CAPTC programs. All sample members could use CAPTC services for tax preparation, employment, education, child care, and so on during the experiment period. Control group members could receive homeownership counseling from CAPTC and, if they requested it, they were provided with general financial information and referrals to other agencies in the Tulsa area that provided similar services. At these other agencies, controls were free to seek any service for which they qualified, including financial assistance for homeownership.

Treatment group members had access to financial education, case management, and the Individual Development Account held at the Bank of Oklahoma. The account earned an interest rate of 2–3 percent.[7] Participants could receive matches for up to $750 in deposits each year, with deposits above $750 in a given year eligible to be matched in subsequent years. Participants could make matchable deposits for 36 months after opening the account. Unmatched withdrawals could be made at any time. Matched withdrawals could only be made six or more months after account opening. Withdrawals were matched at 2:1 rate for home purchase and 1:1 for home repair, small business investment, post-secondary education, or retirement saving. A participant who made the maximum matchable deposit in all three years

[7] There were no fees to open or withdraw from the account unless the respondent made more than three withdrawals in one year, which induced a $3 fee. They could also use direct deposit to transfer money automatically into the IDA.

could accumulate $6,750 for a home purchase or $4,500 for other qualified uses. At the end of the program, participants could request to put any remaining IDA balance into a Roth IRA with a 1:1 match.

The financial education component included both general money-management training and asset-specific training.[8] Program staff provided case management including assistance and consultation by phone or in-person, and they sent out monthly deposit reminder postcards. Matches for home purchase were paid to the vendor directly from the bank.

Shortly after completing a baseline survey (wave-1), each of the 1,103 participants was randomly assigned to either the treatment or control group. Because of concerns about differential attrition, the initial assignment ratio was 5:6 for treatment and controls. About halfway through recruitment, the assignment ratio was changed to 1:1. The wave-2 survey was conducted between May 2000 and August 2001, about 18 months after random assignment. An interview with respondents was first attempted by telephone. If telephone attempts were unsuccessful, a field interviewer attempted to arrange an in-person interview at the respondent's residence. The wave-3 survey followed the same process between January and September 2003, about 48 months after random assignment. Interviews were conducted using computer-assisted telephone and personal interviewing methods. Data from these first three surveys were used in the studies cited above.[9]

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[8] Participants were required to attend a minimum of four hours of financial education before they were allowed to open the account, and to accrue 12 hours of general financial education, as well as some asset-specific training, before making a matched withdrawal. The general financial education requirement consisted of six 2-hour courses on topics such as saving strategies, budgeting, credit repair, and financial planning. The asset-specific classes provided information on a particular asset investment. For example, participants who were saving for a home attended classes that addressed how to shop in the real estate market and how to work with real estate agents and loan officers.

[9] These surveys were undertaken by Abt Associates. See Mills et al. (2004) for a detailed description of the data and survey methods.

B. New data

For the current study, we report on a fourth wave of data collection which started in August 2008, about 10 years after random assignment.[10] Because 35 respondents to the baseline survey had died before the wave-4 survey, the potential sample for wave 4 was 1,068 respondents. No differential efforts were used to track down treatment versus control group members, nor were any information sets used if they predominantly identified only treatment or control group members. We imposed these constraints to ensure that we did not collect a sample of study participants that was biased with respect to the treatment. Further, interviews were conducted at an even pace for both the treatment and control groups, which is important given that the recent economic downturn developed and worsened during the period of data collection.

Data collection lasted about 8 months and ended in March 2009. The interviews were primarily in-person for participants living in greater Tulsa; the 17 percent of respondents who lived elsewhere were interviewed by telephone. The primary survey method was changed from telephone interviews in earlier waves to personal interviews in the current survey in order to achieve higher response rates and to collect more complete data, especially for income and wealth (Paul P. Biemer et al. 1991). Wave-4 questions retained the format and content of questions in the earlier surveys. We also added some new questions, addressing respondents' homeownership history and current economic, financial, demographic, community, social, and health status.

As with earlier waves, the wave-4 survey asks participants "snapshot" questions about their current homeownership status at the time of the survey. Unlike other waves, however, the wave-4 survey also asks retrospective questions about their homeownership history.

[10] RTI International provided tracing, data collection, and data management services for Wave-4. The study was approved by the University of North Carolina Institutional Review Board on July 1, 2008.

Specifically, in wave 4, respondents were asked to report on their home ownership history starting in 1998: what their status was at that time; when they bought a house; when they sold it, when they bought another house, when they sold it, etc. Using this information, we construct a homeownership history for each respondent from 1998 to 2009.[11]

III. Preliminary Data Issues

Table 1 reports sample sizes for each of the four survey waves. The wave-4 survey had an overall response rate of 80.1 percent of living baseline sample members, and included interviews with 855 participants, including 407 for the treatment group (representing 78.6 percent of the treatment group), and 448 with the control group (representing 81.5 percent of the control group). This is a slightly higher response rate than at wave 3 (76 percent), despite the fact that the wave-4 survey took place roughly six years later.[12] The relatively high response rate is likely due in part to the change of survey method from telephone to personal interviews. Also, respondents were paid $50 to complete a wave-4 interview, up from $35 in the earlier waves.[13]

Table 2 compares the baseline characteristics of the wave-4 treatment group and the control group members. The differences between groups were tested for significance using two-tailed t-tests and chi-square tests, as appropriate. For the 27 economic and demographic

[11] There are inevitably some conflicts between what people report retrospectively in 2009 about homeownership in earlier years and what people reported in those earlier years as a "snapshot." In the data reported below in the text, we resolve those conflicts by allowing the "snapshot" data to override the retrospective data. We have also performed all of the calculations ignoring the "snapshot" data and the results are virtually identical. Moreover, in both cases, the calculated retrospective homeownership rates in the years when the surveys were taken are very close to those using the "snapshot" data.

[12] Among wave-3 respondents, 131 were not located in wave 4. Conversely, 146 respondents who did not participate in wave 3 were located and participated in wave 4.

[13] Respondents in the last cohort of interviews in the baseline survey were the most difficult to reach and were provided $75 in incentives.

variables shown in Table 2, some of which are described in Appendix 1, there is only one significant difference at ($p < .05$) between the groups. Control group members were 7 percentage points more likely to own total assets worth more than $4,285 (three months of average income). We note also that the homeownership rate was 5 percentage points higher for the control group relative to the treatment group at baseline. This difference is not statistically significant ($p > 0.10$), but it leads to misleading aggregate difference-in-difference results, as discussed in section IV.

The baseline characteristics of the wave-4 sample are similar in all ways except homeownership rate to the baseline characteristics of the wave-3 sample examined in Grinstein-Weiss et al. (2008), and Mills et al. (2008a). The average age is 36 years; median income is $1,320 per month, with more than 50 percent of the sample having at least "some college" experience. About 80 percent of the sample is female, 26 percent is married, 41 percent is black, and 84 percent own a bank account of some kind. As noted in Mills et al. (2008a) and discussed further below, the sample is not representative of low-income households who would have been eligible for the CAPTC IDA. Sample members have more education and are more likely to be single, female, and black than the population of IDA-eligible households.[14]

Table 3 presents data on account utilization for treatment group members who were surveyed at wave 4.[15] About 90 percent of treatment respondents opened an IDA account.

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[14] Although table 2 shows that the wave-4 sample is balanced in terms of almost all baseline characteristics, we also examined attrition patterns from the wave-1 to the wave-4 survey, regressing inclusion in the wave-4 survey on the baseline characteristics listed in table 2, treatment status, and interaction terms between the characteristics and treatment status. Attrition was not significantly related to treatment status, baseline homeownership or their interaction (at $p < 0.05$), but was correlated with a few variables, including one age category, car ownership, an economic strain scale, and interactions between the treatment status indicator and one sample cohort and one liability category. All of these variables are controlled for in the regressions in Table 5 and none raise concerns about biased samples.

[15] The data are taken from the Management Information System for Individual Development Accounts, which is an administrative data set designed by the Center for Social Development at Washington University. Mills et al.

Among those who opened an account, 46 percent reported at enrollment that they intended to save for home purchase. More than 20 percent reported intending to save for home repair, and another 20 percent reported saving for retirement, while smaller shares reported saving for post-secondary education (8 percent) and for starting or running a small business (6 percent). Account holders made average deposits of about $1,855, not including matching funds. Fewer than half of account holders made a matched withdrawal. Including the 10 percent of treatment group members who did not open an account, 58 percent of treatment group members never made a matched withdrawal.[16]

IV. Methodology

We test the effect of being assigned to the treatment group (i.e. being eligible to participate in an IDA program) and thus provide "intent-to-treat" estimates.[17] We use three approaches: difference-in-differences (DiD), ordinary least squares regression, and propensity score analysis. In regression form, the difference-in-difference can be estimated as

$$Y_{4i} - Y_{1i} = \alpha + \beta T_i + \varepsilon_i, \tag{1}$$

(2008a) provide detailed analysis of IDA contribution and withdrawal patterns.

[16] Administrative records reflect account transactions up to March of 2004. It is possible that some respondents may have withdrawn money, with or without a match, after this date.

[17] The intent-to-treat estimates reported in this paper examine the average impact of exposure to the IDA for all members of the treatment group. For some purposes, it is of interest to examine the impact on those who complied with the treatment protocols – an effect called the effect of the treatment on the treated (TOT). The effect is given by TOT = ITT/p, where ITT is the intent-to-treat estimate and p is the probability that a treatment group member complied with the treatment. In the IDA experiment, compliance could be defined in different ways. For example, 90 percent of the treatment group opened an IDA, and 81 percent of the treatment group contributed $100 or more (a measure that Schreiner, Margaret Clancy, and Sherraden (2002) define as a "saver"). TOT estimates are not reported separately below. TOT estimates have the same p-value as ITT estimates.

where i indexes households, Y_4 is an outcome measure in wave 4, Y_1 is an outcome measure in wave 1, T takes the value 1 for treatment group members and 0 for control group members, and ε is an error term. In this specification, α measures the difference in outcomes from wave 1 to wave 4 for control members, and $\alpha + \beta$ represents the difference in outcomes from waves 1 to 4 for the treatment group. This implies that β is the difference-in-differences estimate, the amount by which the outcome changed over time for treatment group members net of any change in the outcome for control group members.

We present OLS regressions of the form:

$$Y_{4i} = \alpha + \beta T_i + \gamma Y_{1i} + \delta X_i + \varepsilon_i, \qquad (2)$$

where X is a vector of household characteristics, observed at baseline. Controlling for X improves the efficiency of the estimates and removes the effects of sample imbalances in the baseline data related to the components of X. Also, unlike equation (1), the specification in (2) allows the effect of the baseline outcome variable to vary from unity.

With a dichotomous outcome variable like homeownership, the assumptions of ordinary least squares regression (OLS) are violated. With a sample size as large as ours, however, OLS estimates converge with probit estimates. Because OLS is simpler than probit to interpret and present, we report OLS results below. Probit produced similar results and so are not reported.

We further test the sensitivity of the results with propensity scoring analysis (PSA), which uses the conditional probability of group membership to rebalance samples on baseline characteristics. We employ two methods: propensity score weighting (Keisuke Hirano and Guido W. Imbens 2001; Shenyang Guo and Mark W. Fraser 2010) and nearest-neighbor propensity score within-caliper matching (Paul R. Rosenbaum 2002). Both approaches begin with the estimation of the propensity score using logistic regression to predict the probability of membership in the treatment group conditional on baseline household characteristics.[18]

The first approach—based on weighting the observations—converts the estimated propensity score into a sampling weight that is applied to the OLS analysis. Consistent with our ITT approach, we estimate weights for the average treatment effect, apply these weights to the OLS model described above, and estimate the treatment effect net of imbalance on observed baseline characteristics.

The second approach—based on matching one treatment and one control group member to each other—creates a new sample within the data where treatment and control groups are finely balanced on observed baseline characteristics. We use nearest-neighbor matching within a caliper, also called greedy matching. This approach relies on there being a large region of common support between treatment and control cases where the odds of finding a close match on the propensity score are high. Fortunately, our data have a broad region of common support, so 83 percent of treatment cases are matchable. For the matching analysis, participants are randomly ordered and for each successive treated case, the closest control case (within 0.25 standard deviations) is identified and the two are matched. We use 1:1 matching with no replacement. A new dataset is constructed consisting only of matched treatment and control cases. Before analysis, the balance of this new sample between treatment and control is checked on relevant covariates.

[18]For the results reported in the text, we use all baseline covariates in the Appendix. The results, however, are insensitive to using subsets of the variables, except for baseline homeownership, as shown in the tables.

V. Effects on Homeownership Rates

A. Difference in Differences

Figures 1–3 and Table 4 illustrate key findings in the difference-in-difference analysis for homeownership rates, using data on all 855 wave-4 respondents, less 3 cases who had missing information on homeownership. There are several important points. First, homeownership rates among both treatment group members and control group members increased considerably over the 10-year period. As shown in Figure 1, for the control group as a whole, the homeownership rate rose from 25.8 percent to 51.6 percent, an increase of 25.8 percentage points, or 100 percent. For the treatment group, the homeownership rate rose from 21.2 percent to 52.5 percent, an increase of 31.3 percentage points, or 148 percent. The strong increase in homeownership among the control group reflects an underlying trend for this population, rather than an IDA effect, suggesting a positive homeownership environment and a highly motivated sample. This again highlights the importance of having a randomized control group in analyzing IDA impacts.

Second, the observed sample-wide difference-in-difference (DiD) estimate is that access to the CAPTC IDA raised homeownership rates by 5.5 percentage points, which is significant at $p < 0.08$.[19] Observed DiD estimates from a random-assignment study are frequently regarded as simple and clear and taken as the main measure of program impact. In this particular case, however, the aggregate DiD measure of impact is misleading. The reason is that DiD assumes that random assignment led to balanced baseline homeownership rates, but, as discussed above, this was not the case, whether due to sampling variation or to some unknown factor. Treatment group members were about 5 percentage points less likely to own a home at baseline than were

[19] All of the p-values for treatment effects in this paper are reported using one-tailed tests. Because there is clear directional hypothesis for homeownership from the outset of ADD, a one-tailed test is appropriate. For comparison, under a two-tailed test, the difference-in-difference estimate reported above would have a p-value of 0.148.

control group members. Because there are more baseline renters and fewer baseline owners in the treatment group than in the control group, and because the homeownership rate rose for baseline renters and fell for baseline owners over time, the aggregate DiD combines a causal effect and a composition effect and leads to an overstatement of the impact of IDAs on homeownership.

The issue can be seen most clearly by comparing the sample-wide results with those for baseline owners and baseline renters, two groups that are mutually exclusive and that exhaustively cover the whole sample. The DiD estimate is 1.7 percentage points for baseline homeowners and 2.7 percentage points for baseline renters, and neither effect is statistically significant. If the baseline homeownership rates were the same for the treatment and control groups, the sample-wide DiD would be a weighted average of the DiD for owners and the DiD for renters, with the weights being the baseline homeownership rate and 1 minus that rate, respectively. However, when the homeownership rates differ in the treatment and control group at baseline—even when the difference is not statistically significant–the sample-wide DiD need not fall between the owner and renter effects, and can be driven instead by the differing sample compositions at baseline.

We provide details on these observations in Appendix 2. The key point is that, in this particular case, the sample-wide DiD estimates are not reliable indicators of the program's impact. Instead, more representative estimates come from the disaggregated DiD and the regression results presented below.

B. OLS and Propensity Scoring

The first row of Table 5 presents OLS regressions.[20] The estimate in the first row and

[20] Due to missing data for some respondents, the sample in the table 5 regressions is reduced to 823 households.

first column of Table 5 estimates (2) with the right-hand side consisting of only a constant, baseline homeownership status, and treatment status. This specification generalizes the DiD estimate by allowing the coefficient on baseline homeownership status to vary from unity. In fact, the coefficient estimate on homeownership status differs greatly from unity. In the full sample, the estimated treatment effects imply that the Tulsa IDA program increased homeownership rates by 1.9 percentage points. Controlling for other covariates, in the second column, raises the estimated impact to 2.9 percentage points. Neither estimate is statistically significant at conventional levels. Appendix Table 1 reports the estimated coefficients for the other covariates.[21] The last four estimates in the first panel of Table 5 report OLS results for baseline owners and baseline renters separately, with and without controls for covariates. The estimated treatment effects range from 1 to 3 percentage points and are not statistically significant at conventional levels.

The second and third panels of Table 5 report treatment effects estimated using the propensity score weighting and matching methods described above.[22] The results are similar to the OLS analysis. For the full sample, propensity scores with weighted regressions yield treatment effect estimates at 2.9 percentage points, and propensity scores using matched regressions yield estimates of less than 1 percentage point. Neither estimate is statistically significant. Adding control variables beyond baseline homeownership has little effect on the impact estimates. The other columns show that treatment effects for baseline homeowners are

[21] The regressions show that, controlling for other factors, respondents were more likely to own a home at Wave-4 if, at baseline, they owned a home, held a bank account, were in the top income bracket, lived in unsubsidized rental housing, held significant amounts of household goods, and were satisfied with their health. They were less likely to own a home if in the age ranges of 25–45 or over 65.

[22] The propensity score greedy matching method reduced the sample from 823 to 650 since, as described above, each treatment group member was matched to at most one control and only matched pairs were included in the sample.

less than 2 percentage points and sometimes negative, while treatment effects for baseline renters are about 3 percentage points in the weighted regressions and less than 1 percentage point in the matching regressions. None of the estimates are significant at conventional levels.

C. Year-by-Year Patterns

The analysis of homeownership described above uses information from "snapshot" questions about respondents' current homeownership status at the time of the surveys. We now turn to the new wave-4 survey questions, described above, about retrospective homeownership patterns. We use these data to explore the year-by-year changes in homeownership, seeking insight about the reasons the treatment effects for 2003 and 2009 differ.

Figure 4 shows year-by-year homeownership rates using the retrospective data. The two middle lines show the homeownership rate for the treatment group and the control group as a whole. The control group starts the period with a higher homeownership rate but in no year is the difference between the treatment group and the control group statistically significant at conventional levels. The two top lines show that baseline homeowners in both groups experienced declines in home ownership over time.

The most interesting results involve baseline renters.[23] By the end of the program period in 2003, the treatment group's increase in homeownership rate is higher than that of the control group by 4.4 percentage points ($p < 0.12$).[24] After the experiment ends, however, the difference declines rapidly. The homeownership rate for baseline renters in the treatment group did not increase from 2003 to 2004, allowing the control group, whose homeownership rate continued to

[23] In each group, about 8 percent of baseline renters reported buying a home in the year of the baseline interview but after the interview date.

[24] By way of comparison, the analogous finding from Mills et al. (2008a), for all renters, is an estimated treatment effect of 6.9 percentage points with a p-value of .058.

rise in 2004, to catch up. This temporal pattern is consistent with the role played by the incentives in the program, whereby the treatment group had incentives to accelerate home purchases to 2003 and earlier, while the control group had incentives to delay such purchases until after 2003.

D. Estimates by subgroup

Table 6 returns to the OLS framework and examines 2009 treatment effects by subgroup, following Mills et al. (2008a). The table presents impact estimates for each subgroup and Chi-square tests on the equality of estimated treatment effects between subgroups. The one statistically significant heterogeneous treatment effect is on subgroups defined by income. Among respondents with income above the sample median ($15,840 per year), the IDA raised the homeownership rate by 10.6 percentage points ($p < .02$) for the treatment group relative to those in the control group, and this result is statistically different from the treatment effect for respondents with income below the median. This suggests that treatment group members with higher baseline incomes may respond differently to the treatment than those whose household income is below the median. These results mirror findings in Mills et al. (2008a) for the period through 2003.

VI. Effects on Duration of Homeownership

Even if the Tulsa IDA program did not affect the long-term homeownership rate for the full sample, it could still have an impact by significantly increasing the amount of time that respondents spend as homeowners. Using the retrospective data discussed above, we estimate the number of years of homeownership during the 10-year period for each respondent. As shown in Figure 5, control group members averaged 4.5 years of homeownership between 1999 and

2009 whereas treatment group members averaged 4.4 years of homeownership. The difference between the two groups is not significant at conventional levels. Moreover, the aggregate comparison is biased by the higher rates of baseline homeownership in the control group. As before, the bias is resolved by examining trends for baseline owners and baseline renters separately and by regression analysis that controls for initial baseline status. Figure 5 shows that, when looking at baseline owners and baseline renters separately, treatment group members experienced slightly longer average durations of homeownership during the sample period. The differences, however, are not statistically significant.

Table 7 presents regression analysis of the effects of the IDA program on the duration of homeownership with the same format and same right-hand side variables as in Table 5. The 18 regressions combine three methods (OLS, propensity score weighting, and propensity score matching), three samples (all respondents, baseline renters, and baseline home owners), and alternatively do and do not control for covariates. The estimated treatment effects are in the range of about 0.1 to 0.4 years, but none of the effects are statistically significantly different from zero.

Table 8 presents the effects of IDAs on the duration of homeownership for the same sub-samples and in the same format as in Table 6. As with the analysis of homeownership at wave 4 presented above, IDA treatment affected high-income respondents relative to low-income respondents. The duration of homeownership for treatment group members earning above the sample median income was 0.87 years longer than for control group members earning above the sample median income, a statistically significant difference ($p < 0.01$).

VII. Discussion

A. Internal Validity

The internal validity of the experiment depends on how well it was implemented. We discuss two countervailing concerns: crossovers and other services. Each issue applies only to the period through 2003 rather than the entire period through 2009.

For the first issue, a formal definition of a *crossover* is a control group member who, during the 1998 to 2003 period, received some part of the treatment—that is, opened an IDA or attended financial-education classes. Crossovers could also be defined more expansively as control group members who, during the experimental period, received access to CAPTC's homebuyer-assistance programs (other than the IDA) or who were able to open an IDA at some other non-CAPTC location.

Larry L. Orr (1999) develops an intent-to-treat estimate adjusted for crossovers, ITT_o, that is calculated as $ITT_o = ITT/(1–c)$ where ITT is the intent-to-treat estimate, c is the proportion of the control group represented by crossovers, and where it is assumed that all treatment group members participate in the treatment.[25] This adjustment alters the magnitude of the estimated treatment effect, but does not alter its statistical significance. We generalize this formula to allow for less than 100 percent participation by members of the treatment group ($p<1$) in IDAs, in which case the resulting adjustment is $ITT_o = ITT*p/(p-c)$.[26]

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[25] In the IDA experiment, crossovers are probably not a representative sample of controls; they are probably more highly motivated to save and so would have done better than the typical control even in the absence of crossover. As a result, dropping crossovers from the sample would undermine the balance between treatments and controls that is the purpose and chief benefit of random assignment.

[26] The adjusted effect, $ITT_o = p(TOT) + (1-p)0 – c(TOT) - (1-c)0$. Collecting terms and noting that $ITT = TOT/p$ yields the equation in the text. The formula in the text collapses to the formula given by Orr when $p=1$. Both formulas are actually upper bounds on the adjustment for crossovers, since they assume that each crossover household received the full treatment. This assumption seems like an overstatement both because even those controls who opened an IDA are unlikely to have received all of the financial education and case management that

The data show 21 control group members who reported participating in an IDA program during the experimental period and an additional 27 who reported participating in CAPTC's down payment assistance program, which was off-limits to both control and treatment group members under the experiment protocol. Even if all 48 members were considered crossovers, c is small (.107 = 48/448), the adjusted impact estimates are only slightly larger than the ITT estimates.[27]

A second issue works in the opposite direction from the crossover effect. As shown in Table 9, treatments were generally more likely than controls to use permitted non-IDA social services at CAPTC—especially tax-preparation services. In addition, although 27 control group members used home buying assistance services for which they were not eligible, 90 treatment group members used such services. It is not clear whether this is an outcome of the IDA program, part of the IDA treatment itself, or merely represents treatment group members misreporting permitted IDA-related home-buyer education as being part of another CAPTC program. The main point, though, is that treatment and control groups received different sets of benefits from CAPTC.

B. External Validity

Efforts to generalize the results estimated above for the Tulsa IDA experiment should account for five considerations.

The first is the condition of housing markets in the United States. The experimental period—1998 through 2003—and up until about 2007, was a time of relatively easy

treatment group members did and because (as discussed in the text below) more than half of those respondents we are counting as crossovers did not open an IDA.

[27] As an example of the magnitude of the effect, a 2 percentage point ITT effect would imply a 2.27 percentage point adjusted effect when c = .107 and p (IDA participation) = .90.

homeownership. During that time, favorable demographics, strong economic conditions, innovations in mortgage markets—particularly sub-prime lending—and public policies and programs supporting homeownership all worked to increase the homeownership rate in aggregate and among low-income households in particular (Raphael W. Bostic and Kwan Ok Lee 2008; Christopher E. Herbert and Eric S. Belsky 2008). The general condition of United States housing markets during this period probably contributed to the large increase in homeownership rates for both the treatment and control groups. In a housing market where obtaining loans is more difficult, IDA program participation may have a stronger impact on home purchase.

A second issue is the housing market in Tulsa. Housing costs in the Tulsa area were substantially below national averages during the experiment, making homeownership even more affordable for low-income people.[28]

A third issue is the availability of other local homeownership assistance. Tulsa seems to have had several affordable-housing programs during the study period, which offered financial assistance. For example, Housing Partners of Tulsa offered down-payment and closing-cost assistance equal to 5 percent of the purchase price upon completion of a home buyer education program (Tulsa Housing Authority 2008). No matched savings were required to receive those funds.[29] IDA programs in areas that do not have other effective and competing homebuyer assistance programs may have stronger impacts.

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[28] The median home price in Tulsa County was $60,300 in 1990, $91,700 in 2001, and $120,000 in 2007 (Owen S. Ard and David Puckett, 2002; American Community Survey 2007). In 2009, the median home price to income ratio for Tulsa County was 2.8, compared to 6.2 for the nation (National Association of Realtors 2009).

[29] Other evidence that may be indicative of the availability of homebuyer assistance programs in Tulsa is the fact that about 90 percent of both treatment and control group members with mortgages held fixed-rate mortgages, during a period of heavy sub-prime lending that tended to feature adjustable rates.

A fourth issue has to do with program design. The Tulsa IDA program was among the first programs in the country when it started in 1998. Based on field experience, many current IDA programs are structured differently in terms of match rates, maximum available matches, duration, qualified uses of the funds, and so on. For example, most of the IDA programs today, funded through the federal AFI program, offer a 5-year saving period (U.S. Department of Health and Human Services 2010). Alternative program designs may result in different program impacts.

Fifth, although the sample in Tulsa may well be a representative subsample of the population most interested in IDAs, it was not a representative sample of all qualified households. Mills et al. (2008a) find substantial differences between Tulsa IDA respondents and IDA-eligible samples drawn from the 1998 Survey of Consumer Finances and from 2000 Census data for the greater Tulsa area. Study participants were more educated, and are more likely to be single, female, and black than the comparison samples of IDA-eligible households. The impact of IDA program participation on a more representative sample of eligible participants may vary from those reported here, although our subgroup analysis suggests that, other than income, there were no statistically significant differences within subgroups.

To provide additional evidence on this, we drew a sample from the 1999 Panel Survey of Income Dynamics (PSID) based on the eligibility rules for the Tulsa IDA. The time elapsed between the 1999 and 2007 waves of the PSID is roughly comparable to the period between the wave-1 and wave-4 surveys described above. Table 10 shows substantial differences in the increase in homeownership between the PSID sample and the Tulsa control group. In the PSID sample, the homeownership rate rose by 14 percentage points, from 30 percent in 1999 to 43 percent in 2007. In contrast, among Tulsa control group members, the homeownership rate rose by 29 percentage points, from 24 percent in 1998-9 to 53 percent by 2009. Among renters in the initial period, the increase in homeownership rates was 19 percentage points higher in the Tulsa control group than in the PSID subsample. All of these differences are highly significant.[30] These results may suggest that controls in the CAPTC experiment either were more motivated to purchase homes or faced more favorable housing market and housing assistance conditions than the general US population with similar observed characteristics. This also demonstrates the importance of using a randomized evaluation to study the effects of IDAs, rather than drawing on a nonrandomized sample of observationally equivalent households that did not self-select into an IDA experiment.

VIII. Conclusion

Based on a longitudinal random-assignment design, this paper presents evidence on the 10-year impacts of an IDA program on homeownership. We find that both treatment and control group members experienced substantial and on-going increases in homeownership rates. For the full sample, however, participation in the Tulsa IDA program did not result in a significantly higher homeownership rates 10 years later. Earlier findings (Grinstein-Weiss et al. 2008; Mills et al. 2008a) show a statistically significant programmatic effect on homeownership rates as of 2003. The longer-term findings show that the IDA program accelerated the onset of homeownership for treatment group households but in the longer run it did not result in a homeownership rate statistically different from the control group. The gap in homeownership

[30] One potential concern with this comparison is that even after selecting for IDA eligibility in 1999, the PSID sample was substantially different from the ADD sample on demographic and financial characteristics. In sensitivity analysis, we reweighted the samples using propensity score radius matching and the basic finding did not change.

increase narrowed rapidly after the program ended in 2003, thus the IDA program did not statistically increase the duration of homeownership during the 10-year period covered by this study.

A plausible explanation for the pattern of results found—a positive effect through 2003 but no significant effect after 10 years—is that is that the specific design of the Tulsa IDA experiment created incentives for treatment group members to accelerate home purchases before 2003 and for control group members to delay purchases. Specifically, treatment group members had incentives to accelerate home purchase given the 2:1 match contribution they could receive for home purchase, which was available only up to 2003. Control group members had incentives to postpone purchases until the experiment ended in 2003, at which point they could take full advantage of the homeownership programs at CAPTC, including financial assistance for down payment and closing costs.

Our results do show that assignment to the treatment group raised the long-term homeownership rate and duration of homeownership for people with above-sample median income ($15,840 annually) at baseline. This may indicate that while IDA programs are not effective in promoting homeownership among very-low income households, they can be effective for households with higher, although still modest, levels of income. However, in multiple other subgroups, we were unable to detect any impact of IDAs.

Future research should focus on several issues. First, it is important to examine the long-term impact of the Tulsa IDA on other qualified uses of savings—home repair, small business, post-secondary education, or saving for retirement—as well as other outcomes, such as income-to-needs ratios, poverty rates, mortgage choices, loan performance, and net worth. There is some evidence that policy interventions can have longer-term effects through some channels even if the short-term effects through other channels fade out. For example, small class size may have temporary impacts on test scores but longer-term impacts on non-cognitive aspects of behavior and earnings (Chetty et al. 2010). It is important to know whether financial education, the encouragement to save, and the opportunity to have accumulated funds during the IDA program could have longer-term effects, even if controls had caught up six years after the program ended.

Second, because IDAs are made up of a bundle of services, it would be valuable for both policy and research reasons to understand the channels through IDAs may affect behavior and well-being. For example, experimental evidence from the Canadian *learn$ave* program indicates that financial features of the program (contribution level, matching rate, etc.) affected education outcomes, but the addition of financial education services did not (Leckie et al. 2010).

Third, a question that may be of interest is why IDA participants -- treatment and control group members alike -- raised their homeownership rates by more than a random sample of low-income households (as evidenced by the comparison with respondents from the PSID). As noted above, some combination of different motivations for saving, different local housing markets, and different exposure to assistance and education programs could have played important roles. These issues, however, are left for future research.

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Appendix 1: Definitions of variables

Homeownership is measured in wave 1 and wave 4 with a question that asks all respondents “Do you own or rent the home you currently live in?” We assign a 1 to those indicating they own and a zero to those who rent and to the 51 respondents in the wave-4 survey who indicate that they are neither owners nor renters.

All other variables are measured as of the baseline (wave-1) survey only and most are self-explanatory and conventional. Age of the household head is measured in 10-year categories. Total monthly gross household income from all sources is calculated as the sum of income from employment, public assistance, public insurance, informal sources, and other sources such as investment or business income. The variable was categorized to limit the influence of outliers and to emphasize the cumulative effect of income. Indicator variables were created for those respondents who had at least \$1,000 in income, at least \$2,000 in income, and at least \$3,000 in income. Thus, a respondent with \$2,500 in monthly income would have a positive value for the first two indicator variables but not the third. We also include an indicator variable for respondents with missing data for any of the components of income.

Marital status was collapsed into two groups, married and not married, the latter including those who are single, separated, divorced, or widowed. The highest level of education that participants achieved at the time of the baseline survey is categorized into four groups: less than high school, completed high school, attended some college, and graduated from college (the last including respondents who received associate’s degrees). To limit the effect of outliers, we scale total assets and debt by mean monthly income at baseline for the wave-4 respondents and use categories. We also include an indicator variable for any respondent with any missing asset or debt data.

The health measure asks respondents to compare their own health to other people their age on a 5-point scale. The top two categories of relative health are collapsed together into a positive response in the dichotomous measure. The financial satisfaction question asks respondents if they are satisfied on a 4 point scale. The top two categories are combined into the positive response.

Finally, we include a set of scales created from multiple survey items. The economic strain scale is adapted from the family stress model (Rand D. Conger et. al 2002) and includes questions about making ends meet and financial difficulty. A lower score indicates more economic strain. The household goods ownership scale is a count of common "big-ticket" household goods a respondent owns such as refrigerator, washing machine, and dryer. A higher score indicates the ownership of more items.

Three scales probe the connection between respondents and their communities. The "getting help" scale is a count of types of help such as childcare, food support, and emotional support from friends and neighbors. Higher values represent more utilization of support. The "giving help" scale asks about the same set of items but about the respondent providing the types of assistance. Again, higher values represent the provision of more types of help. The community involvement measures the respondent's participation in community activities like fundraisers, politics, and neighborhood organizations. Respondents who report participating more fully in their communities will have a higher score on this scale.

Appendix 2: Difference-in-difference estimates

The difference-in-difference (DiD) estimate for the sample as a whole is given by

(A-1) $DiD = D(T) - D(C)$,

where D(i) represents the difference between the homeownership rate in wave 4 and wave 1 for group i, and i = T, C, representing the treatment and control groups, respectively. D(i), in turn, can be written as:

(A-2) $D(i) = P(Oi)D(Oi) + (1-P(Oi))\, D(Ri)$.

That is, D(i) is a weighted average of the difference in homeownership over time for owners in group i (D(Oi)) and the difference over time for renters in group i (D(Ri)), where the weights are the share of homeowners and renters in group i at baseline (P(Oi) and 1- P(Oi), respectively). This allows the overall DID estimate to be written as:

(A-3) $DiD = P(OT)D(OT)+(1-P(OT))\, D(RT)-\{P(OC)*D(OC)+(1-P(OC))*D(RC)\}$

If the homeownership rate at baseline is the same in the two groups, then P(OT) = P(OC) = P, and equation A-3 collapses to

(A-4) $DiD = P\{D(OT)-D(OC)\} + (1-P)\, \{D(RT)-D(RC)\}$.

That is, under the assumption that the share of homeowners in each group is the same at baseline, the overall DiD is a weighted average of (a) the change in homeownership rates for baseline owners in the treatment group relative to baseline owners in the control group (i.e., the difference in difference among baseline owners) and (b) the change in homeownership rates for baseline renters in the treatment group relative to baseline renters in the control group (i.e., the difference in difference among baseline renters), where the weights are P and 1-P, respectively.

Equation (A-4) accords with common intuition about the mechanics of DID analysis. For example, if 20 percent of baseline owners and renters are homeowners and 80 percent in each group are renters, and the DiD among owners is 2 percentage points and the DiD among renters is 3 percentage points, then (A-4) indicates that the overall DiD would be 2.8 percentage points (i.e., 0.2*2 + 0.8*3 = 2.8).

However, if the baseline homeownership rate differs across the two groups, then even with the same DiD among owners and the same DiD among renters as above, the overall DiD can be less than 2 percentage points or greater than 3 percentage points. The fact that the overall DiD results can be either greater or smaller than each of the respective subgroup effects indicates that the overall DiD estimates are being driven by sample composition issues, not by the effects of any subgroup, and hence are not reliable estimates of program impact.

To illustrate the problem that arises when baseline homeownership rates differ, let P (OT) = P, set (P(OC) = P + x, where x can be positive or negative, and rewrite A-3 as:

$$\text{(A-5)} \qquad DiD = P\{D(OT)–D(OC)\} + (1-P)\ \{D(RT)–D(RC)\} – x\ (D(OC) - D(RC)).$$

This is the same expression as (A-4) except for the last term, involving x. The first two terms represent the effect on owners and renters, which are mutually exclusive and exhaustive subgroups of the overall sample. The third term represents the impact of differing sample composition at baseline. Note, in particular, that whether the difference x is statistically significant does not affect the DiD calculation.

Using the same numerical example above, the sum of the first two terms is .028, as before. The third term can be positive or negative. If x = .05, D(OC) = -.20 and D(RC) = .40, the third term equals .03 and the aggregate DID is .058. However, if x = -.05, the third term is -.03 and the aggregate DID effect is -.002. Clearly, this substantial variation in the DID has nothing to do with the impact of the IDA on owners and renters, it only has to do with sample composition at baseline. As a result, when x is not equal to zero, there is a risk that the aggregate DiD is a misleading indicator of program effects.[31]

In the IDA example in the paper, x = .046, P = . 212, D(OT) = -.209, D(OC) = -.226, D(RT) = .453, D(RC) = .426. As a result, the overall DiD is 5.5 percentage points, but of that total, only about 2.5 percentage points are due to the actual effect on owners and renters whereas almost 3 percentage points – more than half of the total effect – is due to the sample composition issues captured in the last term.

Thus, in the particular data set that we use, the aggregate DiD turns out not to be a reliable indicator of the IDA impacts. Therefore, we focus on the DiD among owners and renters separately and the OLS and propensity score analyses, all of which give remarkably similar estimates of the long-term effects of the IDA.

[31] An even starker example occurs if the difference in difference is 3 percentage points for owners and 3 percentage points for renters. It is very difficult in that case to see how the aggregate effect ought to be represented as anything other than 3 percent. However, with the values of D(OC) and D(RC) used in the example, the aggregate DID is 6 percent if x = .05 and zero if x = -.05.

Figure 1. Homeownership Rates over Time by Treatment and Control

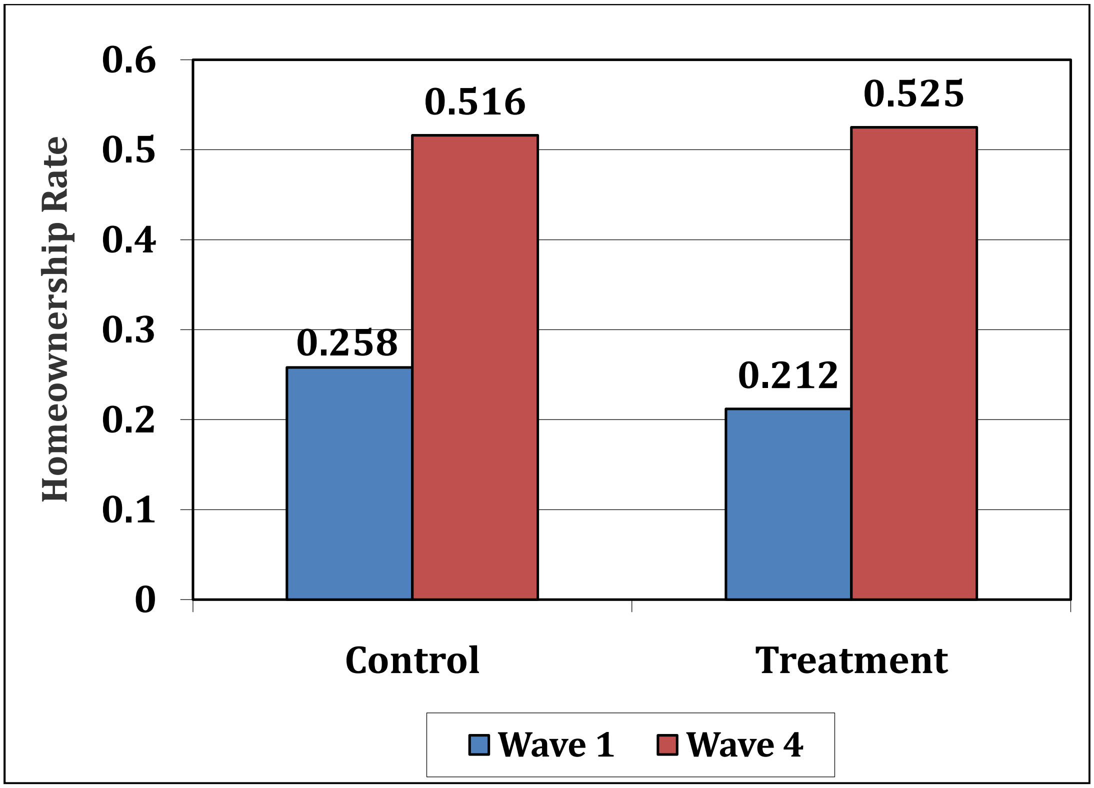


Figure 2. Homeownership Rates over Time by Treatment and Control, Baseline Renters

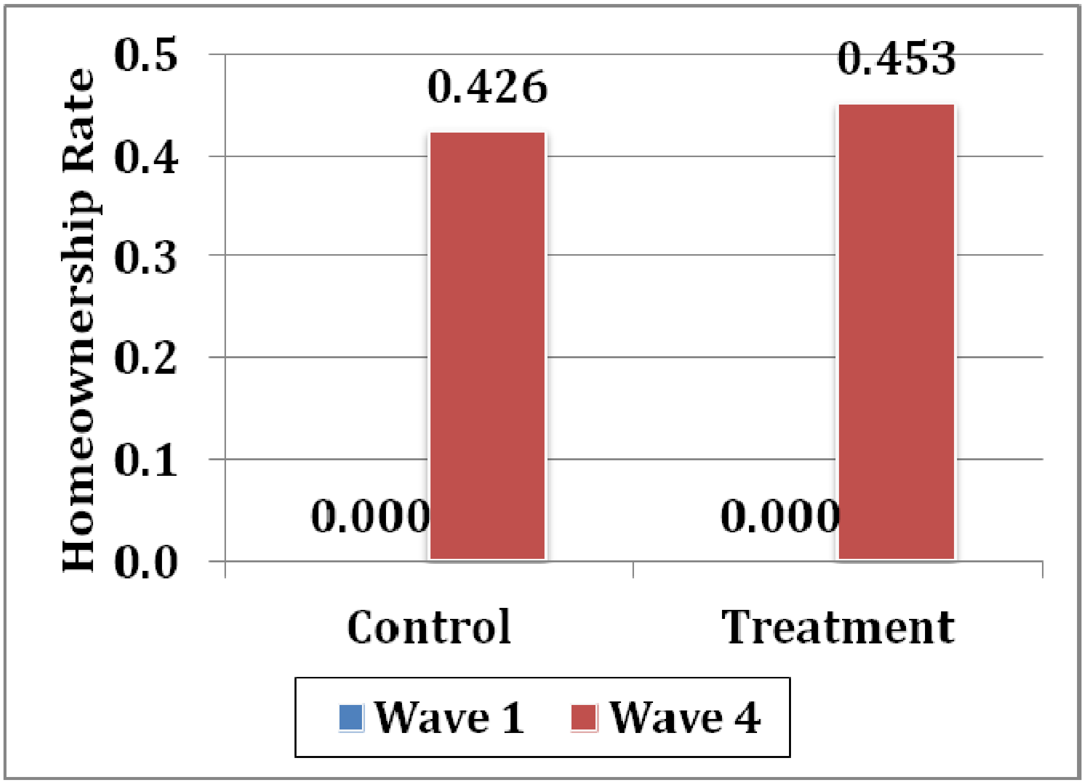


Figure 3. Homeownership Rates over Time by Treatment and Control, Baseline Owners

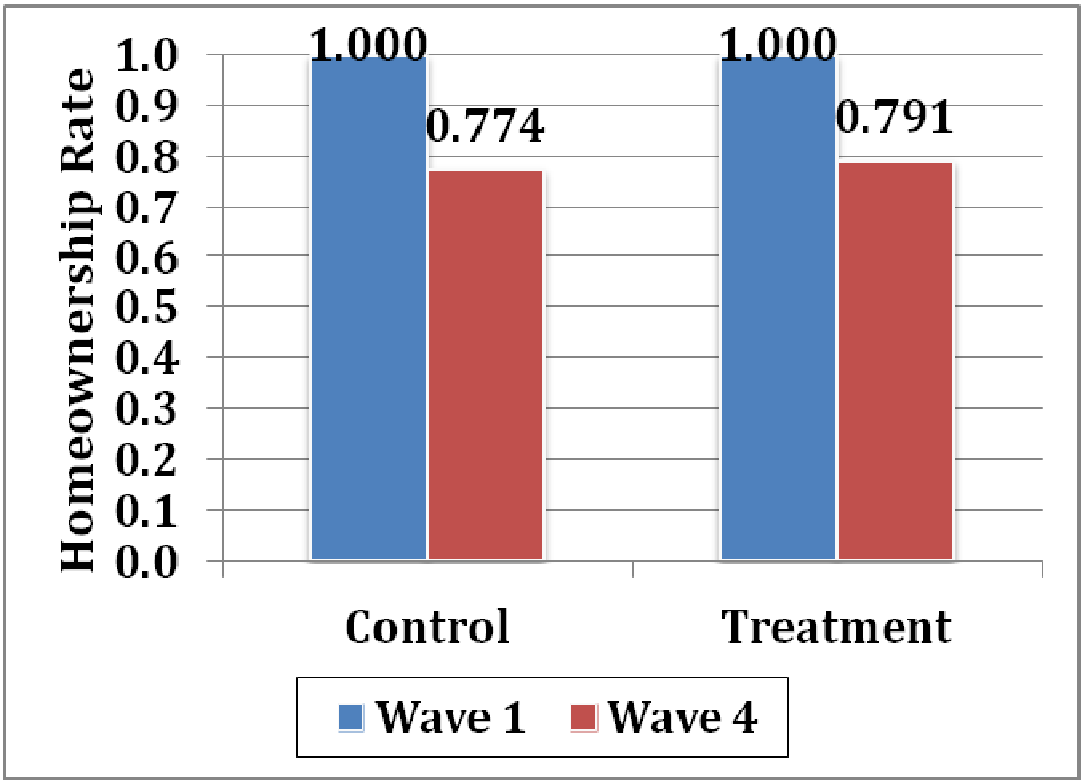
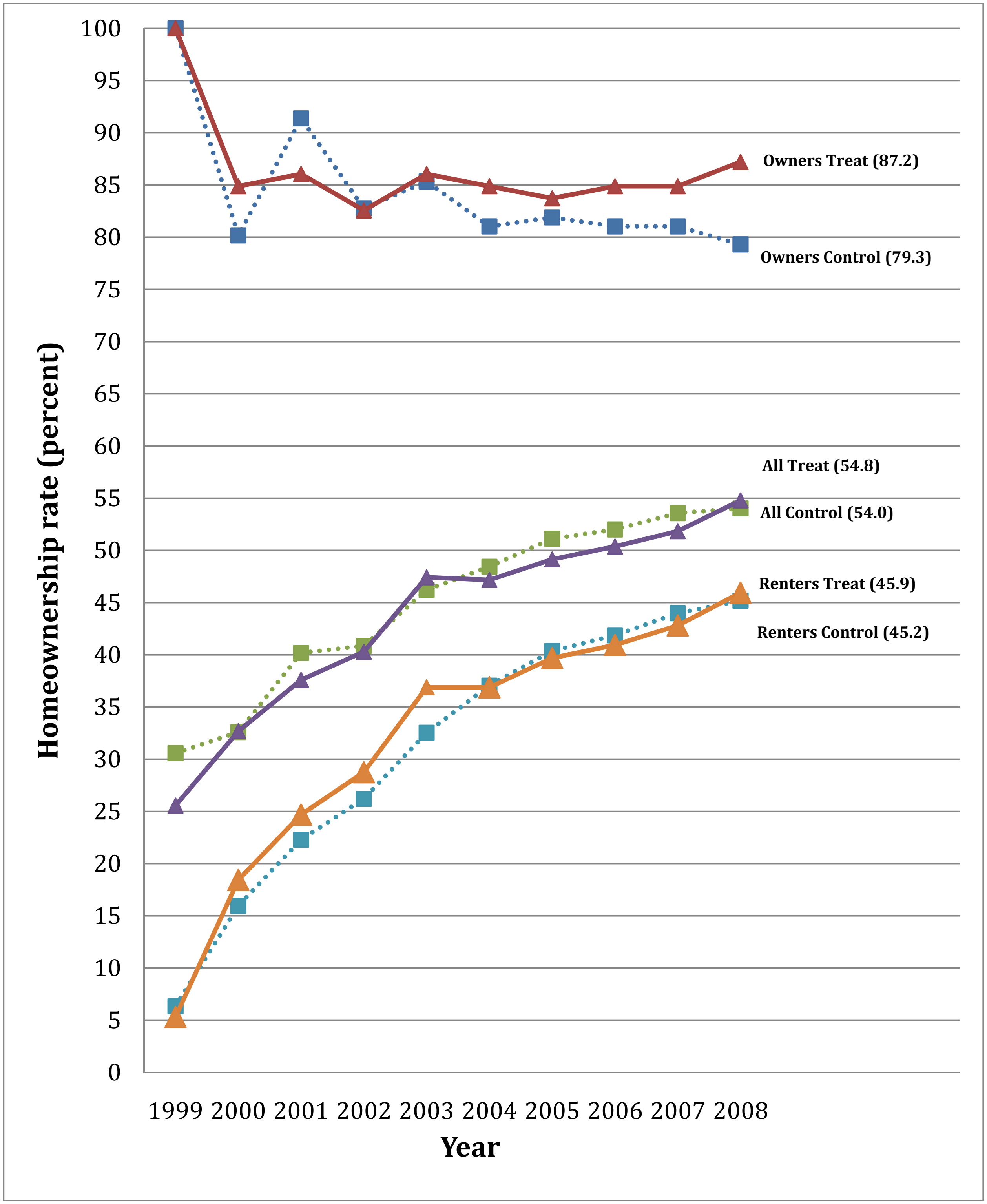
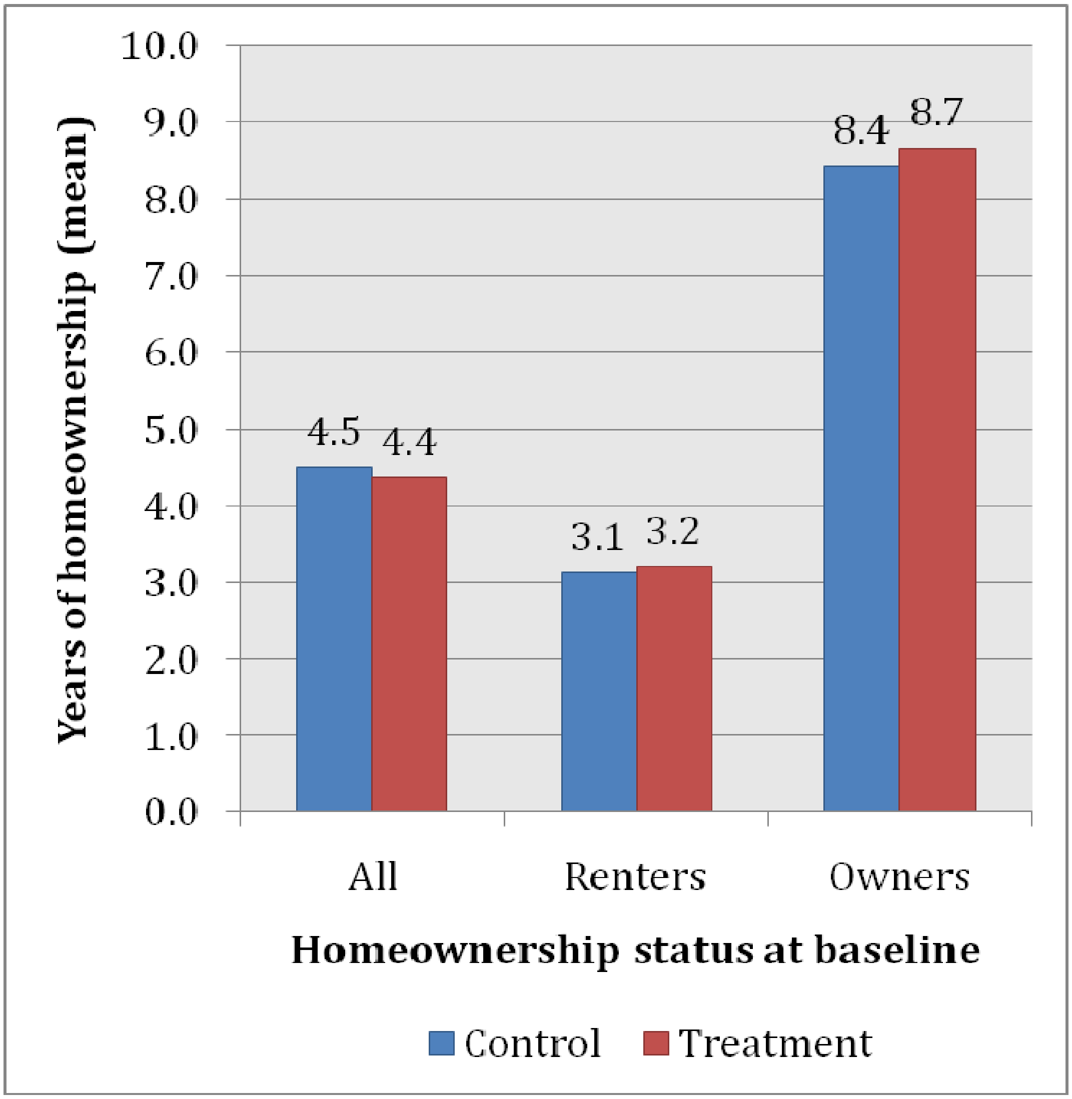


Figure 4. Year-to-Year homeownership rate



Source: Authors' calculations

Figure 5. Duration of homeownership by treatment status and baseline homeownership



Source: Authors' calculations

Table 1

Sample Size by Treatment Status and Survey Wave

	n	percent	n	percent	n	percent	n	percent
Full Sample	1103	100	933	84.6	840	76.2	855	80.1
Controls	566	100	472	83.4	428	75.6	448	81.5
Treatments	537	100	461	85.8	412	76.7	407	78.6

Source: Authors' calculations.

Note: The percent figures are calculated as a share of the 1,103 baseline sample members for waves 1, 2, and 3, and as a share of the 1,068 baseline sample members who were still alive at the time of the wave-4 survey for wave 4.

Table 2
Baseline Characteristics of Wave-4 Treatment and Control Group Respondents

	N	Treatment	Control	Difference	SE	P
Homeownership	854	0.21	0.26	-0.05	0.03	0.106
Age						
under 25	853	0.15	0.13	0.03	0.02	0.289
25-35	853	0.34	0.37	-0.02	0.03	0.455
35-45	853	0.32	0.29	0.02	0.03	0.481
45-55	853	0.14	0.16	-0.02	0.02	0.451
55-65	853	0.04	0.05	-0.01	0.01	0.38
65+	853	0.01	0.00	0.01	0.01	0.205
Income						
Income at least $1,000/month	855	0.71	0.72	0.00	0.03	0.897
Income at least $2,000/month	855	0.18	0.17	0.01	0.03	0.583
Income at least $3,000/month	855	0.05	0.03	0.02	0.01	0.12
Income is missing	855	0.03	0.04	-0.01	0.01	0.468
Female	855	0.79	0.81	-0.01	0.03	0.656
Education						
Less than high school	854	0.07	0.07	0.00	0.02	0.812
High school graduate	854	0.26	0.26	0.00	0.03	0.981
Some college	854	0.41	0.42	-0.01	0.03	0.756
College degree or more	854	0.26	0.26	0.01	0.03	0.851
Bank account ownership	840	0.86	0.83	0.04	0.02	0.139
Race						
White	855	0.44	0.47	-0.03	0.03	0.364
Black	855	0.43	0.39	0.04	0.03	0.243
Other	855	0.13	0.14	-0.01	0.02	0.717
Married	855	0.27	0.26	0.00	0.03	0.884
Baseline survey cohort						
Cohort 1-3	855	0.15	0.18	-0.03	0.03	0.252
Cohort 4-6	855	0.21	0.22	-0.01	0.03	0.673
Cohort 7-9	855	0.17	0.16	0.01	0.03	0.796
Cohort 10-12	855	0.28	0.26	0.01	0.03	0.698
Cohort 13	855	0.20	0.17	0.02	0.03	0.398
Total Assets						
Total Assets under $1428	855	0.22	0.22	0.00	0.03	0.996
Total Assets $1429-$2856	855	0.12	0.10	0.02	0.02	0.298
Total Assets $2857-$4284	855	0.09	0.08	0.00	0.02	0.850
Total Assets $4285 and up	855	0.41	0.48	-0.07	0.03	0.041
Total Assets missing	855	0.16	0.12	0.04	0.02	0.064
Total Debt						
Total Debt under $1428	855	0.20	0.21	-0.01	0.03	0.756
Total Debt $1429-$2856	855	0.07	0.07	0.00	0.02	0.998
Total Debt $2857-$4284	855	0.06	0.06	0.00	0.02	0.834
Total Debt $4285 and up	855	0.47	0.48	-0.01	0.03	0.816
Total Debt missing	855	0.19	0.18	0.01	0.03	0.623
Live in unsubsidized housing	848	0.75	0.75	0.00	0.03	0.959
Have health insurance	853	0.59	0.58	0.02	0.03	0.587
Own a business	854	0.08	0.07	0.01	0.02	0.608
Own other property	855	0.05	0.03	0.02	0.01	0.248
Have retirement savings	853	0.09	0.08	0.02	0.02	0.358
Receive welfare payments	855	0.25	0.27	-0.02	0.03	0.523
Own car	855	0.84	0.85	-0.01	0.02	0.755
Satisfied with health	855	0.86	0.86	0.00	0.02	0.973
Satisfied with financial situation	855	0.63	0.60	0.03	0.03	0.315
Number of adults in the household	855	0.47	0.52	-0.05	0.05	0.308
Number of children in the household	855	1.72	1.62	0.11	0.09	0.250
Household goods ownership scale	855	2.70	2.70	0.00	0.16	0.992
Economic strain scale	855	0.56	0.57	-0.01	0.02	0.516
Giving help in the community scale	855	0.56	0.54	0.02	0.01	0.172
Getting help in the community scale	855	0.36	0.36	0.00	0.01	0.955
Community involvement scale	855	0.39	0.40	-0.01	0.02	0.546

Source: Authors' calculations. Variables are defined in Appendix 1. Reported p-values are for 2-tailed tests.

Table 3

IDA Utilization by Wave-4 Account Holders

Reason for Saving	Share of Treatment Group	Average Contribution $	Probability of Making a Matched Withdrawal
Any	1.000	1855	0.467
Home purchase	0.462	1402	0.177
Home Repair	0.209	2278	0.792
Small Business	0.057	1526	0.714
Education	0.076	2330	0.708
Retirement Saving	0.196	2384	0.476

Source: MIS IDA. IDA participants could make more than on matched withdrawal and there is no requirement that the matched withdrawal was made for the originally reported motive for saving.

Table 4

IDA Treatment Effects on Homeownership at Wave 4: Difference-in-Difference Estimates

Homeownership rate	Treatment	Control	Diff	SE	P
	Full Sample (N=852)				
Baseline	0.212	0.258	-0.046	0.029	0.943
Wave-4	0.525	0.516	0.009	0.034	0.397
Wave-4 - baseline	0.313	0.258	0.055	0.038	0.074
	Baseline owners (N=201)				
Baseline	1.000	1.000	0.000	0.000	.
Wave-4	0.791	0.774	0.017	0.059	0.389
Wave-4 - baseline	-0.209	-0.226	0.017	0.059	0.389
	Baseline renters (N=651)				
Baseline	0.000	0.000	0.000	0.000	.
Wave-4	0.453	0.426	0.027	0.039	0.243
Wave-4 - baseline	0.453	0.426	0.027	0.039	0.243

Source: Authors' calculations. Reported p-values are for 1-tailed tests.

Table 5

IDA Treatment Effects on Homeownership at Wave 4: OLS and Propensity Score Estimates

	Full Sample		Baseline Owners		Baseline Renters	
	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]
Control for Covariates	No	Yes	No	Yes	No	Yes
OLS regressions						
Treatment Status	0.019	0.029	0.016	-0.012	0.02	0.030
	(0.033)	(0.033)	(0.060)	(0.066)	(0.040)	(0.039)
	[0.283]	[0.193]	[0.397]	[0.571]	[0.304]	[0.22]
Homeownership	0.340	0.240	---	---	---	---
	(0.039)	(0.049)				
	[0.000]	[0.000]				
N	823		197		626	
Propensity score -- weighted regressions						
Treatment Status	0.029	0.029	0.016	-0.016	0.033	0.026
	(0.034)	(0.033)	(0.060)	(0.069)	(0.040)	(0.039)
	[0.197]	[0.19]	[0.395]	[0.591]	[0.206]	[0.254]
Homeownership	0.349	0.259	---	---	---	---
	(0.036)	(0.047)				
	[0.000]	[0.000]				
N	823		197		626	
Propensity score -- matching regressions						
Treatment Status	0.009	0.004	0.018	-0.035	0.007	0.005
	(0.038)	(0.036)	(0.071)	(0.081)	(0.044)	(0.043)
	[0.404]	[0.455]	[0.401]	[0.668]	[0.440]	[0.456]
Homeownership	0.328	0.225	---	---	---	---
	(0.045)	(0.056)				
	[0.000]	[0.000]				
N	650		145		505	

Source: Authors' calculations. Reported p-values represent 1-tailed tests for treatment status, 2-tailed tests for baseline home ownership status.

Table 6

IDA Treatment Effects on Home Ownership at Wave 4: OLS Estimates for Subsamples

	b	p	b	p
Race				
	White		Non-white	
Treatment effect	-0.003	0.945	-0.003	0.942
Difference in treatment effect	0.000	0.999		
Age				
	35 and over		Under 35	
Treatment effect	0.012	0.777	0.039	0.383
Difference in treatment effect	0.184	0.668		
Income				
	Median income and above		Below median income	
Treatment effect	0.106	0.018	-0.045	0.313
Difference in treatment effect	5.760	0.016		
Education				
	More than HS		HS or less	
Treatment effect	0.018	0.640	0.040	0.467
Difference in treatment effect	0.105	0.745		
Children in the Household				
	Has children		No children	
Treatment effect	0.027	0.447	-0.045	0.503
Difference in treatment effect	0.899	0.343		
Survey Cohort				
	Cohort 12 or 13		Earlier cohorts	
Treatment effect	0.007	0.913	0.015	0.684
Difference in treatment effect	0.012	0.912		
Single motherhood				
	Single mother		Not single mother	
Treatment effect	-0.020	0.679	0.038	0.368
Difference in treatment effect	0.823	0.364		
Banked				
	Banked		Unbanked	
Treatment effect	0.018	0.606	0.021	0.762
Difference in treatment effect	0.002	0.968		
Welfare recipient				
	Welfare recipient		Non-recipient	
Treatment effect	0.010	0.864	0.012	0.753
Difference in treatment effect	0.001	0.979		
Car ownership				
	Owns car		No car	
Treatment effect	0.029	0.395	0.042	0.629
Difference in treatment effect	0.019	0.890		
Health insurance				
	Insured		Uninsured	
Treatment effect	0.015	0.718	0.016	0.751
Difference in treatment effect	0.000	0.989		
Marital status				
	Married		Not Married	
Treatment effect	0.073	0.235	0.000	0.997
Difference in treatment effect	1.035	0.309		

Source: Authors' calculations. Reported p-values are for 1-tailed tests for treatment effects, 2-tailed tests for differences in treatment effects.

Table 7
IDA Treatment Effects on Duration of Homeownership: OLS and Propensity Score Estimates

	Full Sample		Baseline Owners		Baseline Renters	
	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]	b/(se)/[p]
Control for Covariates	No	Yes	No	Yes	No	Yes
OLS regressions						
Treatment Status	0.122	0.189	0.174	0.352	0.106	0.201
	(0.236)	(0.230)	(0.383)	(0.424)	(0.286)	(0.278)
	[0.303]	[0.206]	[0.326]	[0.204]	[0.356]	[0.235]
Homeownership	5.436	4.551	---	---	---	---
	(0.277)	(0.344)				
	[0.000]	[0.000]				
N	823		197		626	
Propensity score –- weighted regressions						
Treatment Status	0.185	0.18	0.144	0.324	0.199	0.171
	(0.240)	(0.227)	(0.382)	(0.435)	(0.292)	(0.273)
	[0.220]	[0.213]	[0.354]	[0.229]	[0.248]	[0.267]
Homeownership	5.470	4.603	---	---	---	---
	(0.240)	(0.342)				
	[0.000]	[0.000]				
N	823		197		626	
Propensity score – matching regressions						
Treatment Status	0.075	0.077	0.259	0.476	0.022	0.075
	(0.266)	(0.253)	(0.435)	(0.500)	(0.319)	(0.300)
	[0.390]	[0.380]	[0.277]	[0.172]	[0.473]	[0.402]
Homeownership	5.441	4.542	---	---	---	---
	(0.320)	(0.390)				
	[0.000]	[0.000]				
N	650		145		505	

Source: Authors' calculations. Reported p-values represent 1-tailed tests for treatment status, 2-tailed tests for baseline home ownership status.

Table 8

IDA Treatment Effects on Duration of Home Ownership: OLS Estimates for Subsamples

	b	p	b	p
Race				
	White		Non-White	
Treatment Effect	0.02	0.4775	-0.111	0.628
Difference in treatment effect	0.131	0.788		
Age				
	35 and over		Under 35	
Treatment Effect	-0.102	0.6115	0.091	0.398
Difference in treatment effect	-0.193	0.701		
Income				
	Median income and above		Below median income	
Treatment Effect	0.87	0.007	-0.299	0.806
Difference in treatment effect	1.169	0.018		
Education				
	More than HS		HS or less	
Treatment Effect	0.16	0.2945	0.152	0.36
Difference in treatment effect	0.008	0.988		
Children in the Household				
	Has children		No children	
Treatment Effect	0.052	0.4255	-0.038	0.528
Difference in treatment effect	0.09	0.881		
Survey Cohort				
	Cohort 12 or 13		Earlier Cohorts	
Treatment Effect	0.111	0.401	0.052	0.43
Difference in treatment effect	0.059	0.911		
Single motherhood				
	Single mother		Not single mother	
Treatment Effect	-0.238	0.744	0.207	0.272
Difference in treatment effect	-0.445	0.371		
Banked				
	Banked		Unbanked	
Treatment Effect	0.21	0.221	0.14	0.4
Difference in treatment effect	0.07	0.91		
Welfare recipient				
	Welfare recipient		Non-recipient	
Treatment Effect	-0.144	0.629	0.103	0.365
Difference in treatment effect	-0.247	0.641		
Car ownership				
	Owns car		No car	
Treatment Effect	0.225	0.2005	-0.561	0.842
Difference in treatment effect	0.786	0.205		
Health Insurance				
	Insured		Uninsured	
Treatment Effect	0.473	0.0765	-0.452	0.884
Difference in treatment effect	0.925	0.066		
Marital status				
	Married		Not married	
Treatment Effect	0.03	0.4765	-0.058	0.58
Difference in treatment effect	0.088	0.881		

Table 9

Utilization of CAPTC Services During the Experimental Period

	N	Treatment	Control	Difference	P
Social programs	807	0.121	0.086	0.035	0.095
Workforce programs	807	0.031	0.021	0.010	0.393
Medical services	806	0.121	0.126	-0.005	0.828
Youth programs	806	0.124	0.086	0.038	0.077
Small business programs	807	0.067	0.012	0.055	0.000
Home buying programs	806	0.233	0.067	0.166	0.000
Education services	807	0.032	0.026	0.006	0.681
Tax preparation services	807	0.463	0.379	0.084	0.016

Source: Authors' calculations.

The sample for this table includes wave-4 respondents who were also in either wave 2 or wave 3.

Table 10

Change in Homeownership Rates:
IDA control group sample versus IDA-Eligible PSID Sample

	Tulsa IDA Control Group	IDA-Eligible PSID Sample	Difference	P
Whole Sample				
Homeownership in wave 1/1999	0.24	0.30	-0.06	.037
Homeownership in wave 4/2007	0.53	0.43	0.10	.001
Difference	0.29	0.14	0.16	.000
Owners in wave-1/1999				
Homeownership in wave 1/1999	1.00	1.00	0.00	-
Homeownership in wave 4/2007	0.79	0.84	-0.05	0.277
Difference	-0.21	-0.16	-0.05	0.277
Renters in wave-1/ 1999	0.00	0.00	0.00	-
Homeownership in wave 1/1999	0.45	0.26	0.19	0.00
Homeownership in wave 4/2007	0.45	0.26	0.19	0.00
Difference				

Source: Authors' calculations.

Appendix Table 1
Coefficients for OLS regression (N=823)

	b	P
Treatment Status	0.028	0.193
Homeownership	0.24	0.000***
Age		
25-35	-0.099	0.073
35-45	-0.135	0.018*
45-55	-0.168	0.010**
55-65	-0.116	0.22
65+	-0.586	0.002**
Income		
Income at least $1,000/month	0.037	0.364
Income at least $2,000/month	0.023	0.655
Income at least $3,000/month	-0.197	0.045*
Income is missing	0.185	0.076
Female	-0.029	0.546
Education		
High school graduate	0.01	0.883
Some college	0.01	0.883
College degree or more	0.078	0.305
Bank account ownership	0.145	0.003**
Race		
Black	-0.022	0.576
Other	0.066	0.206
Married	0.023	0.63
Baseline Survey Cohort		
Cohort 4-6	-0.061	0.252
Cohort 7-9	-0.029	0.614
Cohort 10-12	-0.078	0.13
Cohort 13	-0.083	0.146
Total Assets		
Total Assets $1429-$2856	0.097	0.137
Total Assets $2857-$4284	0.016	0.817
Total Assets $4285 and up	0.061	0.283
Total Assets missing	-0.025	0.707
Total Debt		
Total Debt $1429-$2856	0.001	0.989
Total Debt $2857-$4284	-0.005	0.945
Total Debt $4285 and up	-0.028	0.563
Total Debt missing	-0.058	0.301
Housing unsubsidized	0.102	0.020*
Have health insurance	0.001	0.981
Own a business	-0.012	0.856
Own other property	0.026	0.758
Have retirement savings	0.041	0.496
Receive welfare payments	0.026	0.505
Own car	-0.02	0.712
Satisfied with health	0.166	0.001***
Satisfied with financial situation	-0.035	0.365
Number of adults in the household	-0.01	0.715
Number of children in the household	-0.013	0.403
Household goods ownership scale	0.032	0.000***
Economic strain scale	0.062	0.405
Giving help in the community scale	-0.145	0.147
Getting help in the community scale	0.007	0.944
Community involvement scale	0.149	0.075
Intercept	0.171	0.223

Source: Authors' calculations. P-values represent 1-tailed tests for treatment status, 2-tailed tests for all other variables.