

Long-Term Impacts of Individual Development Accounts on Homeownership among Baseline Renters: Follow-Up Evidence from a Randomized Experiment[†]

By MICHAL GRINSTEIN-WEISS, MICHAEL SHERRADEN, WILLIAM G. GALE, WILLIAM M. ROHE, MARK SCHREINER, AND CLINTON KEY*

We examine the long-term effects of a 1998–2003 randomized experiment in Tulsa, Oklahoma with Individual Development Accounts that offered low-income households 2:1 matching funds for housing down payments. Prior work shows that, among households who rented in 1998, homeownership rates increased more through 2003 in the treatment group than for controls. We show that control group renters caught up rapidly with the treatment group after the experiment ended. As of 2009, the program had an economically small and statistically insignificant effect on homeownership rates, the number of years respondents owned homes, home equity, and foreclosure activity among baseline renters. (JEL D14, H75, R21, R31)

How can public policy help low-income people improve their long-term economic prospects? The United States has historically focused on a combination of publicly provided education, 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.¹

*Grinstein-Weiss: Washington University in St. Louis, One Brookings Dr., St. Louis, MO 63130 (e-mail: michalgw@wustl.edu); Sherraden: Washington University in St. Louis, One Brookings Dr., St. Louis, MO 63130 (e-mail: sherrad@wustl.edu); Gale: Brookings Institution, 1775 Massachusetts Ave. NW, Washington, DC 20036 (e-mail: wgale@brookings.edu); Rohe: University of North Carolina at Chapel Hill, 205 East Cameron Ave., Chapel Hill, NC 27599 (e-mail: rohe@email.unc.edu); Schreiner: Washington University in St. Louis, One Brookings Dr., St. Louis, MO 63130 (e-mail: mark@microfinance.com); Key: University of North Carolina at Chapel Hill, 102 Emerson Dr., Chapel Hill, NC 27599 (e-mail: cckey@email.unc.edu). For financial support, we thank the Annie E. Casey Foundation, F. B. Heron Foundation, John D. and Catherine T. MacArthur Foundation, Charles Stewart Mott Foundation, National Poverty Center at the University of Michigan, Rockefeller Foundation, Smith Richardson Foundation, and the University of North Carolina at Chapel Hill. We thank Doug Bernheim, Gary Engelhardt, Ben Harris, Krista Holub, Jeff Kling, Lissa Johnson, Andrea Taylor, Jenna Tucker, and participants at numerous seminars for helpful comments, Steven Dow and Brandy Holleyman at the Community Action Project of Tulsa County for invaluable help throughout the study, and Spencer Smith and Leah Puttkammer for administrative support.

[†]To comment on this article in the online discussion forum, or to view additional materials, visit the article page at <http://dx.doi.org/10.1257/pol.5.1.122>.

¹Beyond the general goal of encouraging wealth accumulation, there are several motivations for encouraging saving by low-income people. First, although many public policies already encourage asset accumulation, the vast preponderance of the benefits accrue to people in the top half of the income distribution (Seidman 2001; Woo, 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 (McKernan and Sherraden 2008; Moser and Dani 2008). Third, while the acquisition of major assets (e.g., a house) can transform

Individual Development Accounts (IDAs) provide people with saving accounts in which withdrawals are matched if they are used for qualified purposes—for example, purchasing a home or furthering post-secondary education—and are designed to help low-income people accumulate wealth (Sherraden 1991). 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 (US Department of Health and Human Services 2010). Variants of IDAs are also in place or under consideration in numerous other countries, as are matched saving accounts for children (Loke and Sherraden 2009; Deshpande and Zimmerman 2010).

Previous experimental research on IDAs is limited. In *learn\$ave*, a randomized IDA experiment in Canada starting in 2001, IDAs had positive impacts on post-secondary education and small-business start-ups, two of the qualified uses of contributions in that program (Leckie et al. 2010). The only randomized experiment with IDAs in the United States took place in Tulsa, OK starting in 1998. Baseline renters who were eligible to participate in that program had, at the end of the program in 2003, a 7 percentage point higher homeownership rate compared to those in the control group. Among renters living in unsubsidized housing at baseline, the impact was 11 percentage points (Grinstein-Weiss et al. 2008; Mills et al. 2008).²

These results can be described as short-term impacts. Participants had three years to save in their IDAs, and another six months to use those savings for qualified purposes. Longer-term analysis is important, however, for at least two reasons. First, longer-term effects are the ultimate goal of policy interventions designed to increase saving. Second, there is no experimental study on the long-term effects of IDAs on homeownership and, indeed, very little long-term experimental evidence regarding the efficacy of saving policies in general. Analysis of other (non-saving) policies has shown that long-term effects can be stronger or weaker than short-term effects.³

For IDAs, the long-term effects could exceed the short-term impacts for several reasons. Saving for a down payment may require more than three years, especially for low-income households. Alternatively, people might initially use the IDA to

a household's standard of living, the up-front financial cost may be out of reach for low-income people (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; Oyserman and 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 Wolff (2001); Hurst and Ziliak (2006); Oliver and Shapiro (2006); McKernan, Ratcliffe, and Nam (2007); and Scholz and Seshadri (2009).

²Estimated homeownership effects for baseline homeowners (and estimated effects for the whole sample on other qualified uses of the withdrawals and on net worth) were imprecise and often inconsistent in sign (Mills et al. 2008; Han, Grinstein-Weiss, and Sherraden 2009). In related experimental work, Engelhardt et al. (2010) use Tulsa IDA treatment status as an instrument for homeownership and find no net impact of homeownership on the provision of social capital. Using data from a Michigan experiment among low-income families with young children, Engelhardt et al. (2011) find significant offset of other educational saving in response to subsidies for college saving accounts. More broadly, Hotz, Imbens, and Klerman (2006) study short-run versus long-run experimental outcomes for alternative welfare-to-work training programs, which aim to promote human capital, a form of saving. Non-experimental analyses of IDAs (see, for example, Sherraden et al. 2005; Schreiner and Sherraden 2007; Mills et al. 2008; Rademacher et al. 2010; Sherraden and McBride 2010) are difficult to interpret because of sample selection issues.

³See Almond and Currie (2011) for a discussion and review of long-term impacts of early childhood interventions and Chetty et al. (2011) for a recent contribution to that literature.

invest in education, in which case their homeownership rates may not be affected positively until that education translates into higher incomes several years after the IDA experiment. Likewise, the cumulative effects of financial education or the impacts of saving and increased wealth (as posited by Sherraden 1991) might spur members of the treatment group to lasting gains relative to the controls after the program ended in 2003.

On the other hand, the presence of strong intertemporal substitution patterns in response to the timing incentive embedded in the Tulsa IDA program could make the long-term effects 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 their release from the experiment allowed them to be eligible for a variety of home-buyer assistance programs offered by the community organization that implemented the experiment in Tulsa).

This paper examines the long-term effects of the Tulsa IDA program. Using data from a survey of treatment and control group members administered about ten years after the start of the experiment and about six years after the experiment ended, we re-examine the impact of the Tulsa IDA on homeownership rates and related issues, focusing on two groups: all households who rented at baseline (“all renters”) and households who rented at baseline and were living in unsubsidized housing (“unsubsidized renters”). Unsubsidized renters are a subset of all renters. These are the two groups that had the largest treatment effects on homeownership as of 2003, and, as renters at the beginning of the program, they are the sample members who would naturally have been most attracted to a program offering a 2:1 matching rate for down payments.⁴

We present several main findings. First, the Tulsa IDA program had an economically small and not statistically significant effect on the 2009 rate of homeownership. Second, the control group caught up to the treatment group very quickly after the experiment ended in 2003. These results, combined with earlier results showing positive and significant impacts on homeownership through 2003, are consistent with intertemporal substitution on the part of sample members in response to the timing incentives for home purchase embedded in the program. Third, despite the homeownership impact as of 2003, the Tulsa IDA had no economically or statistically significant impact on the number of years in which respondents reported owning a home during the 1998–2009 period. Fourth, the Tulsa IDA had no economically or statistically significant impact as of 2009 on a variety of home-related outcomes, including house value, mortgage debt, the prevalence of fixed-rate versus variable-rate loans, late payments, or foreclosure activity.

Several caveats are appropriate in interpreting these findings. First, the results imply that a three-year Tulsa IDA program had no lasting impact on ten-year homeownership patterns, but do not speak to the effects of a lifelong and permanent IDA program, which was originally proposed by Sherraden (1991). Second, while the sample members were effectively randomized into treatment and control groups,

⁴In Grinstein-Weiss et al. (2011) we show that the IDA had no significant impact on homeownership rates in 2009 or the number of years in which a respondent reported owning a home from 1998–2009 for baseline homeowners.

several issues may affect the generalizability of the results. For example, sample members were not a random cross-section of low-income households. In particular, we show that between 1998 and 2009, homeownership rates increased dramatically for baseline renters in both the treatment and control groups. This result does not measure the impact of the Tulsa IDA program; rather, it speaks to the importance of having a randomized control group to account for the nonrandom selection of participants into the overall IDA experiment and for any location-specific influences on homeownership. Moreover, housing costs, the proportion of sub-prime loans, and both delinquency and foreclosure rates in Tulsa were lower than the respective national figures during the study period.

The analysis and results in this paper bear on several key discussions in economics. First, besides providing the first evidence on long-term effects of a three-year IDA program on homeownership, this is the first study of a randomized experiment (to our knowledge) to examine long-term effects of three-year matching subsidies on saving behavior, despite a large literature on the possible effects of billions of dollars of annual public tax expenditure for subsidies for private saving (Office of Management and Budget 2011). Second, the exogenous assignment of treatment status in the current paper creates a rare experiment on the impact on saving subsidies, free of the biases that arise from non-random selection.⁵

Third, the magnitude of the intertemporal elasticity of substitution in consumption is a key question for a number of issues in economics (see, for example, Hall 1988). While we cannot estimate the overall elasticity because only one component of saving was subsidized in the study, our results nevertheless point to clear patterns of intertemporal substitution, given the timing incentives in the program. Fourth, the paper adds to the literature on the impact of matching contributions on saving behavior (see, for example, Duflo et al. 2006; Engelhardt and Kumar 2007; Saez 2009).

Fifth, although it is not exclusively a first-time home-buyers program, the Tulsa IDA program provided strong incentives for sample members to accumulate down payments. This subsidy created standard income and substitution effects and could be reflected in many different dimensions over which households can adjust behavior in the face of a change in rate of return on saving for housing, for example, the timing of the purchase, the size of the house, and the loan-to-value ratio (Dietz and Haurin 2003). 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. Section IV outlines our methods. Section V presents the empirical results. Section VI discusses issues relating to internal and external validity. Section VII concludes.

⁵In addition to the literature cited in footnote 2, see Ashraf, Karlan, and Yin (2006), Duflo et al. (2006), and Saez (2009) for saving-related experiments and Engen, Gale, and Scholz (1996) and Poterba, Venti, and Wise (1996) for discussion of the problems created when selection is nonrandom.

I. Experimental Design and Data Collection⁶

The IDA program in Tulsa, OK was administered by the Community Action Program of Tulsa County (CAPTC) as part of the American Dream Demonstration (ADD). ADD was a set of 14 philanthropically funded local IDA programs begun in the late 1990s. The Tulsa site was the only ADD program implemented as a random-assignment experiment. Eligibility rules required applicants to be employed with household income below 150 percent of the federal poverty guideline.

Treatment group members had access to financial education, case management, and an IDA 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 in excess of \$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 could accumulate \$6,750 (plus interest) for a home purchase or \$4,500 (plus interest) 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. If they did not, the funds remained in the account and were not matched.⁸

All sample members had to agree not to use other matched savings programs at CAPTC or any other financial homeownership assistance from CAPTC during the four-year 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 were restricted from other CAPTC housing-subsidy programs available to other low-income households. After 2003, treatment and control group members were again 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 also 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 agencies, controls were free to seek any service for which they qualified, including financial assistance for homeownership.

Recruitment of participants for the experiment took place from October 1998 to December 1999. Program applicants were divided into 13 cohorts based on the timing of their applications. After they completed a baseline interview (wave-1), sample members were randomly assigned to either the treatment or the control group.

⁶See Mills et al. (2004) and Grinstein-Weiss et al. (2011) for more information on the data and survey methods.

⁷There were no monthly maintenance fees, nor were there fees to open or withdraw from the account unless the respondent made more than three withdrawals in one year, which induced a \$3 fee. Participants could also use direct deposit to transfer money automatically into the IDA.

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

TABLE 1—SAMPLE SIZE BY TREATMENT STATUS AND WAVE

	Wave-1		Wave-2		Wave-3		Wave-4	
	<i>n</i>	<i>n</i>	Percent	<i>n</i>	Percent	<i>n</i>	Percent	
All baseline renters								
Control	429	358	83.4	324	75.5	332	77.4	
Treatment	434	363	83.6	318	73.3	320	73.7	
Total	863	721	83.5	642	74.4	652	75.6	
Unsubsidized baseline renters								
Control	287	236	82.2	219	76.3	219	76.3	
Treatment	289	239	82.7	215	74.4	217	75.1	
Total	576	475	82.5	434	75.4	436	75.7	

Note: Percent figures at each wave are calculated as a share of the number of sample members present at baseline.

There were a total of 1,103 baseline sample members. The wave-2 interview was conducted between May 2000 and August 2001. The wave-3 interview was conducted between January and September 2003, about 48 months after random assignment. Interviews were conducted using computer-assisted telephone and personal interviewing methods. Data from the first three interviews were used in the studies cited above.

For the current study, we report on a fourth wave of interviews that took place between August 2008 and March 2009, approximately ten years after random assignment and about six years after the experiment ended. Interviews were conducted at an even pace for both the treatment and control groups, which is relevant given that the recent economic downturn developed and worsened during data collection. The interviews were primarily in-person for participants living in greater Tulsa, while respondents who lived elsewhere (19 percent of baseline renters and 21 percent of baseline unsubsidized renters) were interviewed by telephone. The primary interview method was changed from telephone in earlier waves to personal interviews in the current wave in order to achieve higher response rates and to collect more complete data, especially on income and wealth (Biemer et al. 1991). The wave-4 survey had the same format and content of the earlier surveys along with some new questions, as described in Section V.

II. Preliminary Data Issues

Table 1 reports sample sizes for these groups for each of the four interview waves. The wave-4 interviews included between 73 and 77 percent of wave-1 respondents, defined by rental and treatment status, and included interviews with 652 baseline renters, of whom 436 were unsubsidized. These response rates are about the same as at wave-3, despite the fact that the wave-4 interviews took place roughly six years later. The relatively high response rate is likely due in part to the change of survey method from telephone to personal interviews and intensive tracing efforts. Also, respondents were paid \$50 to complete a wave-4 interview, up from \$35 in the earlier waves.⁹

⁹ Among wave-3 respondents who were renters at baseline, 105 were not located in wave-4; 115 wave-4 respondents who were renters at baseline did not participate in wave-3. Respondents in the last cohort of interviews in the baseline survey were the most difficult to reach and were provided \$75 in incentives.

TABLE 2—BASELINE CHARACTERISTICS OF WAVE-4 TREATMENT AND CONTROL GROUP RESPONDENTS

	All baseline renters			Unsubsidized baseline renters		
	Treatment	Control	Difference	Treatment	Control	Difference
Live in unsubsidized housing	0.68	0.65	0.03	1.0	1.0	0.00
Age (mean)	34.2	34.3	-0.1	34.9	34.5	0.5
Income (mean, monthly)	1,423	1,283	140**	1,526	1,338	188**
Total assets (mean)	5,555	4,891	664	6,385	5,781	604
Total debt (mean)	8,912	8,479	433	9,748	9,818	-70
Education						
High school graduate or less	0.32	0.33	-0.01	0.30	0.30	0.01
Some college	0.41	0.42	-0.01	0.41	0.42	-0.02
College degree or more	0.27	0.24	0.03	0.29	0.28	0.01
Female	0.81	0.84	-0.03	0.76	0.77	-0.01
Race (Caucasian)	0.39	0.43	-0.04	0.47	0.53	-0.06
Married	0.26	0.21	0.06	0.30	0.24	0.06
Presence of children in household	0.82	0.74	0.08**	0.77	0.67	0.11**
Bank account ownership	0.85	0.80	0.05*	0.87	0.84	0.03
Baseline survey cohort (cohort 13)	0.32	0.24	0.08**	0.31	0.21	0.10**
Have health insurance	0.60	0.54	0.06	0.58	0.56	0.02
Own a business	0.04	0.04	0.00	0.06	0.05	0.01
Own other property	0.03	0.02	0.00	0.03	0.02	0.01
Have retirement savings	0.08	0.08	0.00	0.08	0.09	-0.01
Receive welfare payments	0.29	0.30	-0.01	0.21	0.19	0.02
Own car	0.81	0.81	0.00	0.82	0.82	0.00
Satisfied with health	0.86	0.86	0.00	0.85	0.85	0.00
Satisfied with financial situation	0.64	0.60	0.04	0.64	0.65	-0.01
Number of adults in household	0.45	0.43	0.01	0.57	0.50	0.07
Household goods ownership scale	2.32	2.27	0.05	2.43	2.35	0.08
Economic strain scale	0.54	0.55	-0.01	0.56	0.56	0.00
Giving help in the community scale	0.57	0.54	0.02	0.57	0.53	0.04
Getting help in the community scale	0.36	0.37	-0.01	0.36	0.36	-0.01
Community involvement scale	0.39	0.39	-0.01	0.38	0.39	-0.02

Note: $n = 604$ for baseline renters (306 in the control group, 298 in the treatment group) and 403 for unsubsidized renters (199 in the control group, 204 in the treatment group).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 2 summarizes the baseline characteristics of the 604 members of the wave-4 sample for whom information on all covariates was available. The sample is balanced with respect to most of the variables. Relative to controls, treatment group members had slightly higher incomes, and were more likely to own bank accounts and to have children. Treatment group members were also more likely to be from last survey cohort. All of these variables are controlled for using multiple regression analysis.

The baseline characteristics of the wave-4 sample are similar to the baseline characteristics of the wave-3 sample examined in Grinstein-Weiss et al. (2008) and Mills et al. (2008). Among all renters, the average age is 34 years, median income is \$1,352 per month, and more than two-thirds have at least “some college” experience. About 82 percent of the sample is female, 23 percent is married, 41 percent is Caucasian, and 82 percent own a bank account of some kind. As noted in Mills et al. (2008) and discussed further below, the sample is not representative of low-income households who would have been eligible for the Tulsa IDA: sample members have more education and are more likely to be single, female, and African-American than the population of IDA-eligible households.

TABLE 3—INTENDED IDA UTILIZATION BY WAVE-4 ACCOUNT HOLDERS

Reason for saving	Percentage of account holders	Average contribution (\$)	Percentage with any matched withdrawal
All baseline renters			
Any	100.0	1,695	37.8
Home purchase	57.9	1,417	16.2
Home repair	13.2	2,158	88.6
Education	6.4	1,848	52.9
Retirement	17.3	2,233	65.2
Business	5.3	1,654	42.9
Unsubsidized baseline renters			
Any	100.0	1,960	43.2
Home purchase	55.7	1,713	21.4
Home repair	16.8	2,111	87.1
Education	5.4	1,894	60.0
Retirement	17.3	2,572	62.5
Business	4.9	2,159	55.6

Although Table 2 shows that the wave-4 sample is balanced in terms of most baseline characteristics, we also examined attrition patterns from the wave-1 to the wave-4 interviews, regressing inclusion in the wave-4 interviews on the baseline characteristics listed in Table 2 and treatment status. Attrition was not significantly related to treatment status, but it was correlated with a few variables, including car ownership, a scale of household goods ownership, and receipt of help from the community. All of these variables are controlled for in the subsequent analysis and none raise concerns about systematically biased samples.

Table 3 presents data on intended IDA use by treatment group members who were interviewed at wave-4. About 89 percent of all renters in the treatment group opened an IDA. Among all renters who opened an IDA, 58 percent reported an intention to save for home purchase, 13 percent for home repair, and 17 percent for retirement. Average deposits were \$1,695, not including matching funds. Fewer than half of all IDA holders made a matched withdrawal. Including the 11 percent of treatment group members who did not open an account, 66 percent of treatment group members never made a matched withdrawal.¹⁰ Similar results hold for unsubsidized renters.

III. Methodology

We test the effect of being assigned to the treatment group (that is, being an eligible applicant who is allowed to participate in the Tulsa IDA program) and thus provide “intent-to-treat” estimates.¹¹ As described below, we use four approaches: unadjusted

¹⁰As discussed in Mills et al. (2008) in an examination of the full sample (including baseline homeowners), the likelihood of making a matched withdrawal was positively associated with having a bank account, having higher educational attainment, or being a homeowner at baseline and negatively associated with being African-American or female or having children.

¹¹The intent-to-treat estimates reported in this paper examine the average impact of exposure to the IDA. For some purposes, it is of interest to examine the impact on those who complied with the treatment protocols; this is called the effect of the treatment on the treated (TOT). It 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, about 90 percent of the all-renter or unsubsidized-renter treatment groups opened an IDA, and 84 percent of those groups contributed \$100 or more (a measure that Schreiner, Clancy, and Sherraden (2002) define as a “saver”). TOT estimates are not reported separately below.

difference-in-differences estimates, ordinary least squares (regression-adjusted difference-in-difference estimates), and two forms of propensity-score analysis. The unadjusted difference-in-difference is given by

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

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, $\alpha + \beta$ represents the difference in outcomes from waves 1 to 4 for the treatment group, and so β is the difference-in-differences estimate, that is, the amount by which the outcome changed over time for treatment group members net of any change in the outcome for control group members. Note also that because we examine the impact on homeownership using a sample of baseline renters, Y_{1i} is zero for all sample members.

Ordinary least squares estimates adjust the difference-in-differences for some observed household characteristics:

$$(2) \quad Y_{4i} - Y_{1i} = \alpha + \beta T_i + \gamma \mathbf{X}_i + \varepsilon_i,$$

where \mathbf{X} is a vector of household characteristics observed at baseline. Controlling for \mathbf{X} improves the efficiency of the estimates. (Probit analysis produced similar results and is not reported.) To examine how impacts differ across subgroups in the sample, we also report treatment effects interacted with a subset \mathbf{Z} of the \mathbf{X} variables, in regressions of the form

$$(3) \quad Y_{4i} - Y_{1i} = \alpha + \beta T_i + \gamma \mathbf{X}_i + \delta \mathbf{Z}_i T_i + \varepsilon_i.$$

We further test the sensitivity of the results with propensity-score analysis, which uses the estimated conditional probability of group membership to rebalance samples on baseline characteristics. We employ propensity-score weighting (Hirano and Imbens 2001; Guo and Fraser 2010) and nearest-neighbor propensity score within-caliper matching (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.¹²

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 any 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 so that equation (2) is estimated on treatment and control groups that are finely balanced on observed

¹²For 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 as shown in the tables.

TABLE 4—IDA TREATMENT EFFECTS ON HOMEOWNERSHIP AT WAVE-4

	All baseline renters			Unsubsidized baseline renters		
Wave-4 homeownership rates						
Treatment group			0.440			0.485
Control group			0.431			0.482
	Coefficient	Standard error	<i>p</i> -value	Coefficient	Standard error	<i>p</i> -value
Treatment effects						
Difference-in-differences	0.008	0.040	0.838	0.003	0.050	0.954
OLS (Regression-adjusted difference-in-differences)	0.006	0.040	0.872	0.013	0.052	0.797
Propensity score weighted	0.004	0.040	0.921	0.002	0.051	0.976
Propensity score matched sample	0.026	0.044	0.551	0.009	0.058	0.875

Note: $n = 604$ for all baseline renters and $n = 403$ for unsubsidized baseline renters for all specifications except the matched regressions, where $n = 476$ and $n = 276$, respectively.

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, this condition is met in our data, so 79 percent of treatment cases are matchable among all renters and 68 percent among unsubsidized renters. For the matching analysis, participants are randomly ordered and for each successive treated case, the closest control case (within 0.25 standard deviations of the estimated propensity score) 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. Balance is evaluated using chi-square tests and *t*-tests as appropriate, verifying that, after matching, the treatment and control groups do not differ significantly on these variables.

IV. Results

A. 2009 Homeownership Rates

Table 4 presents the key findings for 2009 homeownership rates. As shown in the first panel, among all wave-1 renters, the wave-4 homeownership rate was 44.0 percent for treatment group members and 43.1 percent for control group members. For unsubsidized renters, the 2009 homeownership rates are higher, 48.5 percent for the treatment group and 48.2 percent for the control group. The strong increase in homeownership over the 1998–2009 period among the control group reflects an underlying trend for this population, rather than an IDA effect, suggesting a sample highly motivated to save for a home and/or a positive local homeownership environment.

As shown in the second panel, the observed difference-in-difference estimates, reflecting the impact of the Tulsa IDA program, are economically small—0.8 percentage points for all renters and 0.3 percentage points for unsubsidized renters—and

TABLE 5—IDA TREATMENT EFFECTS ON HOMEOWNERSHIP AT WAVE-4:
OLS REGRESSIONS WITH INTERACTION EFFECTS

	All baseline renters			Unsubsidized baseline renters		
	Coefficient	Standard error	<i>p</i> -value	Coefficient	Standard error	<i>p</i> -value
Treatment effect	-0.106	0.186	0.568	-0.156	0.234	0.505
Treatment × higher income	0.167	0.084	0.046	0.200	0.107	0.063
Treatment × female	0.062	0.118	0.597	0.163	0.134	0.227
Treatment × Caucasian	-0.101	0.086	0.244	-0.088	0.109	0.419
Treatment × married	0.112	0.108	0.299	0.087	0.134	0.517
Treatment × some college	-0.014	0.096	0.887	-0.082	0.126	0.516
Treatment × college grad	0.120	0.106	0.256	-0.063	0.134	0.636
Treatment × children	-0.050	0.102	0.624	-0.147	0.124	0.235
Treatment × cohort	0.068	0.092	0.462	-0.045	0.123	0.713
Treatment × banked	-0.041	0.105	0.697	0.055	0.142	0.697
Treatment × welfare	0.045	0.089	0.615	-0.013	0.128	0.919
Treatment × car	0.009	0.105	0.933	0.082	0.137	0.548
Treatment × insurance	0.007	0.082	0.931	0.042	0.107	0.692

Note: $n = 604$ for all baseline renters and $n = 403$ for unsubsidized baseline renters.

not significantly different from zero.¹³ In contrast, the 2003 estimates were 7 percentage points ($p < 0.06$) and 11 percentage points ($p < 0.02$) for the two groups, respectively (Mills et al. 2008), implying that the significant impacts observed in 2003 were no longer present by 2009.

The next three panels report OLS and propensity-score weighting and matching methods. All of the estimated treatment effects are economically small: about 1 percentage point in the OLS analysis, less than 1 percentage point for propensity-score weighting, and about 0.9–2.6 percentage points for propensity-score matching analysis, and the estimates are not statistically significant.

Table 5 presents OLS estimates of the 2009 homeownership effects allowing the treatment effect to differ by subgroup of the sample (as in equation (3)). In both samples, estimated treatment effects are about 16–20 percentage points higher ($p < 0.05$ for all renters, $p < 0.07$ for unsubsidized renters) for households with incomes above the median than for those with incomes below the median. All other interaction effects are not significantly different from zero. These results mirror findings in Mills et al. (2008) for the period through 2003.

B. Year-by-Year Patterns

The analysis above uses “snapshot” questions that ask respondents about their current homeownership status at the time of the survey. Unlike other waves, however, the wave-4 interview also asks retrospective questions about homeownership. Specifically, in wave-4, respondents were asked to report on their homeownership history starting in 1998: what their status was at that time; when they bought a home; when they sold it; when they bought another home; when they sold it; and so on. Using this information, we construct a homeownership history for each respondent

¹³ All *p*-values and references to statistical significance in this paper are based on two-tailed tests.

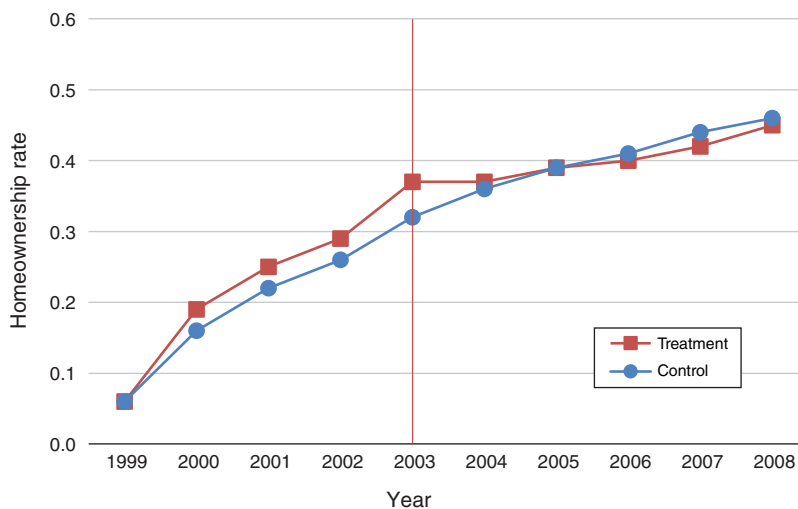


FIGURE 1. YEARLY HOMEOWNERSHIP RATES: ALL BASELINE RENTERS

Notes: The graph shows the likelihood that a household owned a home at any point during the year. The data represent answers to retrospective questions. Differences between answers to retrospective questions and answers to snapshot questions in prior surveys are resolved in this graph in favor of the snapshot questions. Resolving the differences in favor of retrospective answers does not change the basic trends shown. For purposes of this graph, 1998 and 1999 are collapsed into a single year, as are 2008 and 2009. The sample size for each year is 604.

from 1998 to 2009, using these data to explore year-by-year changes in homeownership, seeking insight about why the treatment effects for 2003 and 2009 differ.¹⁴

Figure 1 shows year-by-year homeownership rates for all baseline renters using the retrospective data.¹⁵ By the end of the program period in 2003, the treatment group's increase in the homeownership rate is higher than that of the control group by 5.0 percentage points ($p < 0.21$).¹⁶ After the experiment ends, however, the difference declines rapidly. In the first year after the experiment ended, from 2003 to 2004, the homeownership rate did not change for all renters in the treatment group, but it rose by 5.0 percentage points for all renters in the control group. By 2005, the homeownership rates were identical, and they then remained very close for the two groups from 2006 until the end of the sample period.¹⁷

Similar results occur for unsubsidized renters, as shown in Figure 2. The treatment and control groups had differences in homeownership rates of 7.0 percentage

¹⁴ When there are conflicts between what people report retrospectively in 2009 about homeownership in earlier years and what people reported in those earlier years as a "snapshot," we use the "snapshot" data. We have also performed the calculations ignoring the "snapshot" data and the main finding—that the impact on homeownership rates disappears after 2003—is similar.

¹⁵ In each group, about 6 percent of baseline renters reported buying a home in the year of the baseline interview but after the interview date.

¹⁶ By way of comparison, the analogous finding from Mills et al. (2008), for all renters, is an estimated treatment effect of 6.9 percentage points with a p -value of 0.06.

¹⁷ The end of the sample period is 2008 for some households, 2009 for others. We combine the last observation for each household into the 2008 figure.

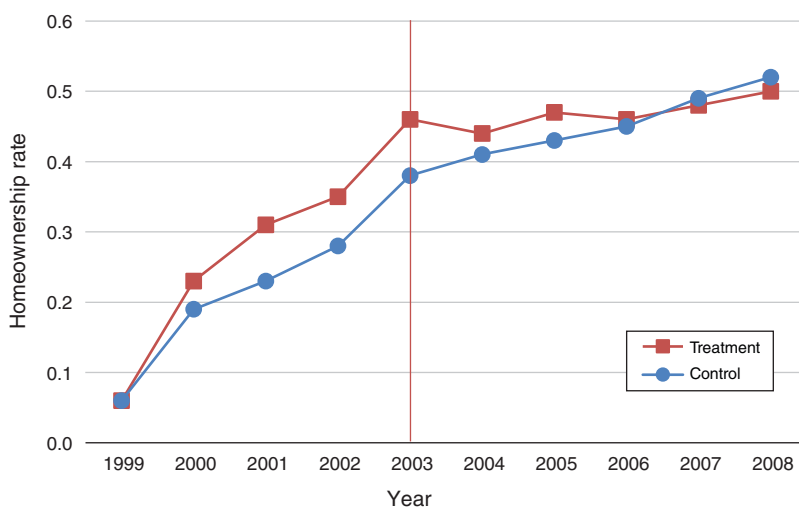


FIGURE 2. YEARLY HOMEOWNERSHIP RATES: UNSUBSIDIZED BASELINE RENTERS

Notes: The graph shows the likelihood that a household owned a home at any point during the year. The data represent answers to retrospective questions. Differences between answers to retrospective questions and answers to snapshot questions in prior surveys are resolved in this graph in favor of the snapshot questions. Resolving the differences in favor of retrospective answers does not change the basic trends shown. For purposes of this graph, 1998 and 1999 are collapsed into a single year, as are 2008 and 2009. The sample size for each year is 403.

points ($p < 0.10$) in 2001, and 7.0 percentage points ($p < 0.14$) in 2003.¹⁸ After the program ended, however, between 2003 and 2006, the homeownership rate was constant for the treatment group, while it rose by 7.0 percentage points in the control group.

These temporal patterns are consistent with intertemporal substitution by households in response to the incentives for the treatment group to accelerate home purchases to 2003 or earlier, and the incentives for the control group to delay home purchases until after 2003.

C. Number of Years of Homeownership

Even if the Tulsa IDA program did not affect the long-term homeownership rate, it could still have an impact by increasing the time that respondents spent as homeowners. Using the retrospective data discussed above, we estimate the number of different years in which a respondent indicated that they owned a home during the ten-year period.¹⁹

As shown in the top row of Table 6, among all renters, the average number of years in which respondents owned a home was 3.17 for treatment group members

¹⁸The analogous finding from Mills et al. (2008), for all renters, is an estimated treatment effect of 10.8 percentage points in 2003 with a p -value of 0.019.

¹⁹This is not a strict measure of the duration of time that a respondent owned a home; it is, rather, a count of the number of years in which a respondent indicated that they owned a home for at least some time because the wave-4 survey only asked for the year of home purchase.

TABLE 6—IDA TREATMENT EFFECTS ON THE NUMBER OF YEARS IN WHICH RESPONDENTS OWNED A HOME, 1998–2009

	All baseline renters				Unsubsidized baseline renters			
	Wave-4 mean duration of homeownership (years)							
Treatment group	3.174				3.745			
Control group	3.092				3.452			
	Treatment effects							
	OLS		Tobit		OLS		Tobit	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Unweighted	0.076	0.278	0.100	0.504	0.313	0.367	0.401	0.581
Propensity score weighted	0.049	0.278	0.021	0.513	0.243	0.361	0.247	0.587
Propensity score matched sample	0.267	0.309	0.462	0.543	0.229	0.426	0.262	0.651

Note: $n = 604$ for all baseline renters and $n = 403$ for unsubsidized baseline renters for all specifications except the matched regressions, where $n = 476$ and $n = 276$, respectively.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

and 3.09 for control group members, a difference that is both economically small and not significantly different from zero. Regression estimates from OLS and the two propensity scoring methods yield similar non-significant differences, with treatment effect estimates between 0 and 0.3 years. Because the number of years of homeownership is bunched at zero—that is, for people who were renters throughout the entire 1998–2009 period—we also estimate these effects using a Tobit specification. These point estimates are slightly higher but still economically small and statistically non-significant.²⁰ Similar results apply to the sample of unsubsidized renters at baseline, also shown in the table. In summary, the Tulsa IDA had economically small and not statistically significant impact on the number of years in which a household reported owning a home through 2009.

Table 7 presents OLS and Tobit regressions for the same outcome as Table 6, but for the same subsamples and in the same format as in Table 5. The treatment effect is about one year longer for households above median income, and it is marginally significant for all renters ($p = 0.06$) but not for unsubsidized renters in the Tobit regression. No other statistically significant effects are observed.

D. Other Dimensions of Homeownership

The Tulsa IDA program subsidized down payments on home purchases. As noted above and discussed in Dietz and Haurin (2003), these subsidies generate standard income and substitution effects that could have effects on other dimensions of home buying beyond the overall homeownership rate. In Table 8, we present treatment effects on home value, mortgage value, home equity, mortgage characteristics,

²⁰We also assessed the effect of treatment assignment on the timing of entry into homeownership. Because the time to homeownership is measured in years (integers), we estimated the treatment effect with a discrete time logit hazard model. These estimates showed no significant effect of treatment for either group of renters.

TABLE 7—IDA TREATMENT EFFECTS ON THE NUMBER OF YEARS IN WHICH RESPONDENTS OWNED A HOME, 1998–2009

	OLS and Tobit regressions with interaction effects							
	OLS				Tobit			
	All baseline renters		Unsubsidized baseline renters		All baseline renters		Unsubsidized baseline renters	
	Coefficient	SE	Coefficient	SE	Coefficient	SE	Coefficient	SE
Treatment effect	-2.533*	1.427	-2.541	1.809	-4.446*	2.598	-4.716	2.983
Treatment × higher income	0.880	0.608	1.038	0.785	2.014*	1.067	1.611	1.220
Treatment × female	0.501	0.890	1.540	1.010	0.488	1.496	2.372	1.571
Treatment × Caucasian	-0.567	0.626	-0.418	0.788	-1.159	1.084	-0.656	1.223
Treatment × married	1.099	0.786	1.352	0.974	1.688	1.368	2.100	1.524
Treatment × some college	0.508	0.672	0.014	0.883	1.496	1.245	0.322	1.422
Treatment × college grad	0.644	0.772	-1.090	1.006	1.688	1.319	-1.630	1.559
Treatment × children	-0.151	0.749	-0.704	0.909	0.138	1.384	-0.923	1.480
Treatment × cohort	-0.047	0.650	-0.876	0.901	-0.064	1.168	-1.361	1.421
Treatment × banked	0.566	0.739	0.885	1.026	1.094	1.538	2.034	1.876
Treatment × welfare	0.370	0.624	0.413	0.921	0.856	1.140	0.916	1.453
Treatment × car	0.588	0.718	0.838	0.980	0.503	1.332	1.361	1.583
Treatment × insurance	0.767	0.587	0.845	0.772	0.777	1.038	0.823	1.195

Note: $n = 604$ for all renters and $n = 403$ for unsubsidized renters.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

TABLE 8—IDA TREATMENT EFFECTS ON ADDITIONAL HOMEOWNERSHIP AND MORTGAGE CHARACTERISTICS

Subsample	Outcome	n	Coefficient	SE	p -value	Control group mean	
All baseline renters	Wave-4 homeowners	Home value at Wave-4	247	4,279	10,905	0.695	110,169
		Home debt at Wave-4	246	5,958	9,797	0.544	76,094
		Home equity at Wave-4	235	715	5,813	0.902	32,758
	Wave-4 mortgage holders	Have fixed interest rate?	224	-0.014	0.038	0.711	0.95
		Rate on primary mortgage	223	-0.014	0.387	0.971	6.49
	Anyone who owned since baseline	Ever 30 days late?	232	-0.022	0.063	0.729	0.34
		Ever 90 days late?	230	0.058	0.046	0.207	0.08
		Ever foreclosed on?	297	0.025	0.024	0.297	0.03
	Unsubsidized baseline renters	Wave-4 homeowners	Home value at Wave-4	185	-2,141	15,514	0.890
Home debt at Wave-4			183	197	12,597	0.988	83,661
Home equity at Wave-4			177	-2,112	8,265	0.799	35,453
Wave-4 mortgage holders		Have fixed interest rate?	167	-0.025	0.046	0.587	0.95
		Rate on primary mortgage	146	-0.090	0.374	0.811	6.36
Anyone who owned since baseline		Ever 30 days late?	174	0.007	0.070	0.918	0.33
		Ever 90 days late?	172	0.079	0.056	0.160	0.08
		Ever foreclosed on?	221	0.033	0.028	0.229	0.02

and loan repayment performance, using OLS estimates of the form in (2) for the sample of all renters. Similar results occurred for the sample of baseline renters. Online Appendix 1 describes the measurement of the outcome variables. There are

no substantive or statistically significant impacts of treatment on these outcomes as of wave-4. Treatment effects on home value, mortgage value, and home equity are of opposite sign for the two groups. Overall, homebuyers held an average of about \$32,319 in home equity, with substantive and statistically similar amounts for those in the treatment and control groups.

Likewise, treatment group members did not have different types of mortgages (fixed versus adjustable rates) or face different interest rates than control group members. About 95 percent of primary mortgages had fixed-rate terms, and the average loan carried an interest rate of about 6.5 percent. There are no statistically significant effects of the Tulsa IDA program on the likelihood that a current homeowner has ever been late on a mortgage payment, or has had a home go into foreclosure.

V. Discussion

A. Internal Validity

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

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 at CAPTC. 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.

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.²¹ We generalize this formula to allow for less than 100 percent participation by members of the treatment group ($p < 1$) in Tulsa IDAs, in which case the resulting adjustment is $ITT_o = ITT \times p / (p - c)$.²² Ten control group members reported participating in an IDA program during the experimental period and an additional 22 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

²¹In 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.

²²The adjusted effect, $ITT_o = p \times (TOT) + (1 - p) \times 0 - c \times (TOT) - (1 - c) \times 0$. 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 (1999) 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 treatment group members did and because (as discussed in the text below) more than half of the respondents who we are counting as crossovers did not open an IDA.

TABLE 9—UTILIZATION OF CAPTC SERVICES, 1998–2003

	<i>n</i>	Treatment	Control	Difference	<i>p</i> -value
All baseline renters					
Social programs	571	0.128	0.100	0.028	0.290
Workforce programs	571	0.028	0.031	−0.003	0.857
Medical services	570	0.117	0.135	−0.018	0.529
Youth programs	570	0.142	0.090	0.052	0.051
Small business programs	571	0.068	0.014	0.054	0.001
Home buying programs	570	0.263	0.076	0.187	0.000
Education services	571	0.032	0.038	−0.006	0.701
Tax preparation services	571	0.459	0.372	0.087	0.036
Unsubsidized baseline renters					
Social programs	380	0.094	0.074	0.020	0.479
Workforce programs	380	0.016	0.016	0.000	0.990
Medical services	379	0.126	0.101	0.025	0.450
Youth programs	379	0.105	0.059	0.046	0.101
Small business programs	380	0.073	0.005	0.068	0.001
Home buying programs	379	0.283	0.080	0.203	0.000
Education services	380	0.042	0.016	0.026	0.130
Tax preparation services	380	0.455	0.386	0.069	0.172

all 32 members were considered crossovers, however, c is small ($0.105 = 32/306$), and the adjusted impact estimates are only slightly larger than the ITT estimates.²³

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 and financial services at CAPTC—especially tax-preparation services. In addition, although 7.6 percent of control group members used home-buying assistance services for which they were not eligible, 26.3 percent of treatment group members used such services. It is not clear whether these differences should be considered an outcome of the Tulsa IDA program, part of the IDA treatment itself, or merely 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 and that controls did not offset the difference in IDA eligibility by disproportionately receiving other CAPTC services.

B. External Validity

Efforts to generalize the results estimated above for the Tulsa IDA experiment should account for several considerations. The first is the condition of housing markets in the United States. The experimental period—1998 through 2003—and up until about 2007, featured favorable demographics, strong economic conditions, and innovations in mortgage markets—particularly sub-prime lending—that plausibly worked to increase the homeownership rate among low-income households (Bostic and Lee 2008; Herbert and Belsky 2008).

²³ As an example of the magnitude of the adjustment, $c = 0.105$ and p (IDA participation) = 0.90 implies that TOT is 13 percent larger than ITT, so that if the ITT were 1 percentage point, the point estimate of the TOT would be 1.13 percentage points.

A second consideration is the housing market in Tulsa. Housing costs in the Tulsa area were substantially below the national average during the experiment, perhaps making homeownership relatively affordable for low-income households in the area.²⁴ Moreover, while subprime lending became more prevalent in the United States during the study period, the Tulsa area had a lower share of subprime loans, and lower rates of delinquency and foreclosure compared to the national average (National Association of Realtors 2009).

A third consideration is the availability of other local homeownership assistance. Tulsa may have had more affordable-housing programs during the study period than other locations. 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.

A fourth consideration has to do with program design. The Tulsa IDA program was among the first IDA programs in the country when it started in 1998. Based on field experience and other factors, 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 IDA programs today funded through the federal AFI program offer a five-year saving period (US Department of Health and Human Services 2010).

Fifth, although the sample in Tulsa may well be representative of the population most interested in IDAs, it was not a representative sample of all IDA-eligible households. Mills et al. (2008) 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 they were more likely to be single, female, and African-American than 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 renters who matched 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 that the homeownership rate increased to 39 percent in the PSID sample in 2007, compared to 43 percent among Tulsa control group members in 2009. This difference is not statistically significant, but 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. Reweighting the samples using propensity scores, the homeownership rate for the Tulsa control group in 2009 is more than 10 percentage points ($p < 0.05$) higher than for the PSID sample in 2007. Even larger differences

²⁴The median home price (in current dollars) in Tulsa County was \$60,300 in 1990, \$91,700 in 2001, and \$120,000 in 2007 (Ard and 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).

TABLE 10—CHANGE IN HOMEOWNERSHIP RATES: TULSA CONTROL GROUP
COMPARED TO PSID IDA-ELIGIBLE SAMPLE

	All baseline renters				Unsubsidized baseline renters			
	Tulsa control group	PSID	Difference	<i>p</i> -value	Tulsa control group	PSID	Difference	<i>p</i> -value
Unadjusted	0.430	0.390	0.040	0.243	0.480	0.403	0.077	0.079
With propensity score weighting	0.457	0.350	0.107	0.044	0.497	0.349	0.148	0.020

Note: $n = 307$ for all baseline renters in the Tulsa control group, $n = 623$ for all baseline renters in the PSID sample, $n = 200$ for unsubsidized baseline renters in the Tulsa control group, and $n = 362$ for unsubsidized baseline renters in the PSID sample.

occur for the two samples of unsubsidized renters, where the Tulsa control group's homeownership rate is almost 15 percentage points higher ($p < 0.02$).

These results suggest that controls in the Tulsa 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.

VI. 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 the Tulsa IDA program had an economically small and not statistically significant impact on homeownership as of 2009. Earlier findings (Grinstein-Weiss et al. 2008; Mills et al. 2008) show a statistically significant programmatic effect on homeownership rates as of 2003. However, we show that the estimated program impact as of 2003 disappears rapidly after the program ended. Homeownership rates in the treatment group stayed flat until homeownership rates in the control group caught up. Finally, we show that the program had an economically small and not statistically significant effect on the number of years in which respondents reported owning a home during the 1998–2009 period and on other measures of home buying activity, such as home equity, the mortgage interest rate, or default and foreclosure activity.

These results provide new evidence that relates to several key issues in economics, including the effects of incentives and matching contributions for saving, the effects of homebuyer subsidy programs, and the extent of intertemporal substitution in consumption. In particular, a plausible explanation for the pattern of results found—a positive effect through 2003 but no effect after ten years—is that households substituted home purchases intertemporally in response to the incentives in the Tulsa IDA experiment. Treatment group members had incentives to accelerate home purchases before 2003 in order to claim a 2:1 match for down payments. 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.

It is worth emphasizing that the analysis addresses the impact of a three-year IDA program, rather than a permanent IDA, as proposed originally by Sherraden (1991). Future research should focus on several issues. First, while our analysis focuses on baseline renters and the impact on homeownership, the long-term effects of the Tulsa IDA for all sample members and on other qualified uses of savings—home repair, small business, post-secondary education, or saving for retirement—as well as other outcomes, such as net worth, are of interest.

Second, along with the potential benefits of IDAs, additional information on the costs of IDAs in various contexts should be pursued. Schreiner (2006), for example, calculates the administrative costs of the Tulsa IDA to be about \$1,949 per participant.

Third, 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 the positive education impacts, but that the addition of financial education services did not (Leckie et al. 2010).

Fourth, a question that may be of interest is why participants in the Tulsa IDA experiment—treatment and control group members alike—increased 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.

APPENDIX: 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 0 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 with an indicator variable for age equal to or above the sample median of 35 years. 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 dichotomized to limit the influence of outliers, and included in models as an indicator variable for equal to or above the sample median income (about \$15,384 annually among all renters); the sample median was recalculated for models with different samples such as the analyses of baseline unsubsidized renters.

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 three groups: less than high school or 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 (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 child care, 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.

Home value and home debt were self reported at wave-4 by respondents who owned a home at that wave. For home value, respondents were asked how much they thought their home would sell for on the day of the interview. For respondents who report owning multiple homes, home value represents the sum of all estimated home values. Home debt is the sum of the amount the respondent reports as owed on outstanding home mortgage loans. Respondents reported debt associated with each loan individually. Home equity is the simple difference between home value and home debt.

The mortgage characteristics reported in Table 8 come from self-reports by those identified as mortgage holders at wave-4. Respondents who held multiple mortgages were asked to identify their largest mortgage, which is analyzed in Table 10. Respondents were asked to report whether that mortgage had a fixed rate and the current interest rate on the loan.

Questions regarding the loan performance outcomes in Table 8 were asked of all respondents who reported owning a home at any point between their baseline interview and their wave-4 interview. Respondents self-reported whether they had ever been 30 or 90 days late on mortgage payments. Among those who had ever owned since baseline and who reported leaving an owned home, respondents were asked the circumstances of their leaving each home, including whether they sold the home or a bank or mortgage company foreclosed on the home. Any respondent who reported ever having been foreclosed upon was included in the foreclosed category.

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