Effects of Individual Development Accounts on Asset Purchases and Saving Behavior: Evidence from a Controlled Experiment

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Abstract

We evaluate the first controlled field experiment on Individual Development Accounts (IDAs). Including their own contributions and matching funds, treatment group members in the Tulsa, Oklahoma program could accumulate $6,750 for home purchase or $4,500 for other qualified uses. Almost all treatment group members opened accounts, but many withdrew all funds for unqualified purposes. Among renters at the beginning of the experiment, the IDA increased homeownership rates after 4 years by 7-11 percentage points and reduced non-retirement financial assets by $700-$1,000. The IDA had almost no other discernable effect on other subsidized assets, overall wealth, or poverty rates.

I. Introduction

Individual development accounts (IDAs) are saving accounts that provide low-income households with matching payments when the balances are withdrawn and used for special purposes, such as home purchase, business start-up, and investment in education. IDA programs also frequently provide participants with financial education and counseling, as well as reminders and encouragement to make regular contributions. Originally developed by Sherraden (1991), IDAs aim to help low-income households by subsidizing and otherwise encouraging the purchase of qualified assets – defined broadly to include financial instruments, housing, business ownership, and human capital – in contrast to more traditional approaches that emphasize income support.

IDAs have generated substantial attention and bi-partisan political support. By the end of 2006, more than 400 IDA programs were in operation in the United States, with more than 44,000 account holders. Community-based IDA initiatives have received support from foundations, financial institutions, other corporate sponsors, and private donors. Publicly sponsored IDA programs have been enacted in 34 states, the District of Columbia, and Puerto Rico, and through several pieces of federal legislation. Proposals to expand IDAs have been a staple of Clinton Administration and Bush Administration budgets over the past decade. Other countries – notably Canada, Taiwan, and the United Kingdom – have launched similar initiatives.[1]

Despite the growth and popularity of IDAs, however, little is known about their effects on program participants. This paper reports the results of the first controlled

[1] Boshara (2005) provides a concise overview of IDAs. See also Schreiner and Sherraden (2007). Websites developed by the New America Foundation (www.AssetBuilding.org), the Corporation for Enterprise Development (www.cfed.org), and the Center for Social Development at Washington University in St. Louis (http://gwbweb.wustl.edu/csd/) provide comprehensive information on IDAs.

field experiment of the effects of IDAs on household behavior. We evaluate the effects of an IDA program that took place in Tulsa, Oklahoma between 1998 and 2003 as part of the American Dream Demonstration. Eligible applicants – those who were employed, with prior-year family income below 150 percent of the poverty level – were randomly assigned to a treatment group, which was allowed to open an IDA, or to a control group, which was not. Sample group members were interviewed immediately prior to random assignment and approximately 18 and 48 months after assignment.

The program matched IDA withdrawals for new home purchase at the rate of 2:1 and withdrawals for other qualified uses (business start-up or expansion, education, home improvement, and retirement saving) at a 1:1 rate. For each of three years, up to $750 in deposits were subject to match. Therefore, combining accountholders' deposits and matching funds, participants could, during the course of the experiment, accumulate $6,750 for home purchase or $4,500 for the other allowed uses. The potential to accumulate funds for housing, in particular, was significant. The median sale price of existing single-family homes in Tulsa was $89,000 in 1998. The households in the sample had prior-year income well below median levels and so would most naturally be expected to purchase homes below the median price.

Several interesting IDA utilization patterns emerged from the study. A very high percentage (89 percent) of treatment group members opened an IDA, but half of the treatment group either never contributed or withdrew all of their contributions for unmatched purposes. Households who, in the baseline survey, owned their home, already held a bank account, or had more education, contributed larger amounts and were more likely to make matched withdrawals, controlling for other factors.

Both treatment and control group members experienced sharp increases in homeownership over the four years, suggesting that sample members were motivated savers and that the presence of an appropriate control group is critical for evaluating the impact of IDAs. The IDA generated insignificant effects on homeownership rates in the overall sample. Focusing on renters at baseline, the IDA raised homeownership rates by almost 7 percentage points, statistically significant at the 5.8% level. We report evidence, however, of differential attrition across the treatment and control groups for white renters who lived in subsidized housing at baseline (where "subsidized housing" refers to residence in Section 8 housing or a public housing project). Focusing on renters who lived in unsubsidized housing at baseline, the IDA generated an increase of almost 11 percentage points in homeownership rates, statistically significant at the 1.9% level. Given the available data, it is not possible to discern whether the results represent permanent changes in homeownership rates across the groups, or just an acceleration of home purchases that arose due to the time-limited nature of the program.

The IDA reduced financial assets of treatment group members relative to controls in the full renter sample and unsubsidized renter subsample. This is consistent with IDA-holders shifting ordinary financial assets into their IDA, and with home purchasers investing more than just their IDA balance and matching funds in the activities surrounding the purchase of a home. The program had no significant sample-wide or subsample effects on retirement saving, business ownership or equity, or educational courses taken. The program had few treatment effects on self-reported financial outlook or security. Effects on the poverty rate or various measures of income-to-needs ratios were nonexistent.

Although the public discussion of IDAs is often framed in terms of households' acquisition of particular types of subsidized assets, a key policy question is the extent to which increases in holdings of subsidized assets represent net increases in saving and wealth. However, extensive sensitivity tests of the IDA program's net impact on overall household wealth – and thus the extent to which the contributions on the whole represented net additions to saving – proved inconclusive. This was in part because there were few significant effects on subsidized categories of assets. Moreover, the underlying variation in net worth across sample members was enormous relative to the size of IDA contributions, making it difficult to detect significant effects.

Besides providing direct evidence on the effects of the Tulsa IDA program, the analysis in this paper relates to several substantive and methodological issues in public economics. The Tulsa IDA is a form of subsidized saving which shares certain features with Individual Retirement Accounts (IRAs) and 401(k) plans. IRAs allow for qualified withdrawals for first-time home purchase and education, so the results may have implications for the effects of IRAs for lower-income households. Similarly, 401(k) plans feature employer matching contributions that are similar in structure to the IDA matches (Engelhardt and Kumar, forthcoming). An extensive literature examines the effects of IRAs and 401(k) plans on households' accumulation of other financial assets and overall net worth (see Bernheim 2003, Engen et al. 1996, and Poterba et al. 1996 for summaries). A key problem in this literature is the difficulty of isolating variation in household eligibility for such programs that is plausibly independent of households' unmeasured tastes for saving. The random assignment of sample members to treatment and control groups in the Tulsa IDA program thus helps to address a fundamental issue in this literature. Indeed, this paper provides the first experimental evidence on how public policies that subsidize saving affect behavioral measures broader than take-up of the saving incentive or contributions to the saving account.[2]

The analysis also has implications for the effectiveness of first-time homebuyer subsidies. Although it is not literally a first-time homebuyers program, the Tulsa IDA program provided strong incentives for renters to purchase a home. Engelhardt (1996, 1997) finds strong effects of a Canadian tax-subsidized first-time homeownership program on national saving and on the transition to homeownership among renters. There is virtually no prior evidence on the effects of such programs in the United States, however (see Gale et al. 2007).

The paper is organized as follows. Section II describes the experimental design. Section III examines attrition, characteristics of the treatment and control groups at baseline, and IDA utilization patterns. Section IV describes our econometric methods. Section V presents the main results. Section VI discusses interpretations and caveats. Section VII concludes.

II. Experimental Design

The American Dream Demonstration (ADD) is a set of 14 privately funded local IDA programs initiated in the late 1990s.[3] The Tulsa program was the one ADD site to

[2] Duflo et al. (2006) and Saez (2007) provide experimental evidence on how variation in matching rates affects participation in and contributions to Individual Retirement Accounts. We are unable to test the effects of variation in matching rates in the Tulsa IDA program because all treatment group members received the same match. Ashraf et al. (2006) provide experimental evidence on how a commitment savings product offered by a private bank in the Philippines affects household bank account balances.

[3] ADD was organized by the Corporation for Enterprise Development (CFED), with technical guidance and research oversight provided by the Center for Social Development (CSD) of Washington University in St. Louis, evaluation funding from the Ford Foundation and the Charles Stewart Mott Foundation, and operational funding from a broad consortium of foundations. The overall ADD evaluation includes a wide array of other nonexperimental research activities, conducted by (or under the direction of) the Center for Social Development of Washington University in St. Louis. These include an implementation assessment, participant in-depth interviews and case studies, cross-sectional participant survey, community-level assessment, and benefit-cost analysis. For details, see Schreiner et al. (2002) and Sherraden et al. (2005).

adopt an experimental design. The program was administered by the Community Action Project of Tulsa County (CAPTC) – a multi-service community action agency serving low-income residents in the Tulsa metropolitan area – in partnership with the Bank of Oklahoma. This section describes the structure and design of the experiment.[4]

A. Recruitment, Assignment, and Data Collection

Enrollment occurred between October 1998 and December 1999. Information about the IDA Matched Savings Program was distributed through several channels: media outreach; CAPTC's existing social services, tax assistance, and homeownership assistance programs; and mailings to other local social service agencies, current and former CAPTC clients, and people who called to ask about the program. Interested individuals submitted an application and were interviewed to establish eligibility. Sample cases were informed that, if assigned to the control group, they would be unable to enter the IDA program during the four-year study period. Applicants signed a form providing their informed consent regarding random assignment and authorizing the release of financial information. Eligible individuals then participated in a baseline survey that collected information on household income, finances, demographics, and other characteristics.

Within a week after the baseline (Wave 1) interview, applicants were randomly assigned to either the treatment group, which was allowed to participate in the IDA program, or the control group, which was not. The treatment analyzed in this evaluation, therefore, is the *offer* to participate in the IDA program. The assignment ratio was 5:6 for treatment and control groups, respectively, through March 1999. At that

[4] Abt Associates (2004) provides additional details on the structure of the evaluation, as well as information on the financing, implementation, and management of the project.

point, it was determined that the less-than-50-percent chance of entering the treatment group was hindering recruitment efforts, so the ratio was changed to 1:1.[5]

The Wave 2 survey occurred for each case about 18 months after random assignment, between May 2000 and August 2001. An interview 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 occurred about 48 months after random assignment, from January to September 2003, and followed the same process. The average interval between the baseline and Wave 3 interviews was 1,449 days for treatment cases and 1,456 days for controls; the difference is statistically insignificant. Interviews were conducted using computer-assisted telephone and personal interviewing methods.

The survey collected information on earnings; demographic characteristics; assets; debts; assistance received from individuals, organizations, and government; and qualitative measures of financial status. The Management Information System for Individual Development Accounts (MIS IDA), developed and supported by the CSD, provided information on IDA deposits, withdrawals, and on matching funds. For purposes of measuring net worth, MIS IDA provided information on the IDA balances for treatment cases. All other components of net worth were provided by self-reported survey data, with the questions carefully constructed to ensure that respondents did not report their IDA wealth on the survey. The survey did not collect information on the

[5] The 5:6 ratio had been adopted in anticipation of lower survey response rates for control cases than treatments. Such a differential would require a larger number of control cases than treatment cases in the original sample if (as desired) the number of cases with complete interview data was to be approximately balanced between treatment and control groups.

uses of unqualified withdrawals or on respondents' saving goals.[6]

B. Program Rules

To qualify for participation in the Tulsa IDA program, individuals had to be employed and have prior-year family adjusted gross income below 150 percent of the federal poverty guideline. There were no limits on assets.

The Individual Development Account itself was a regular passbook saving account at the Bank of Oklahoma. Interest rates were about 2 to 3 percent during the experiment. Fees to open and maintain accounts were waived, except that a participant who made three withdrawals within a twelve-month period was charged $3 for each additional withdrawal during the period.

Participants could not make a matched withdrawal until six months after opening the account. At that point, withdrawals used for purchase of a primary residence were matched at 2:1. Withdrawals for repair/improvement of a primary residence, post-secondary education,[7] micro-enterprise expansion or startup, or contributions to an IRA were matched 1:1. The match was provided in the form of a check made out to the vendor (e.g., a home mortgage lender).

IDA deposits made within 36 months of the account opening and used for qualified purposes were eligible for the match. The accountholder had up to six additional months to make final matched withdrawals. Remaining balances could be rolled over (at the participant's request) into a Roth IRA with a 1:1 match. For each

[6] Treatment group members identified saving goals when they opened their accounts, but neither treatment nor control group members were asked about such goals in any of the surveys.

[7] The qualifying educational uses include (for the participant or the participant's spouse, child, grandchild, or other dependent): the cost of attending a vocational and technical training institution, community college, four-year college, or university; the cost of obtaining a professional certificate or license; or the fees for obtaining a General Educational Development certificate.

year (measured from the month of account opening), up to $750 in deposits were subject to match. Participants who contributed more than $750 in one year could carry forward the difference as a matchable contribution for the following year. However, individuals who contributed less than $750 in a year were not allowed the following year to make "catch-up" deposits retrospectively.

Treatment group members were required to attend at least four hours of general financial education before opening an account. Prior to a matched withdrawal, participants had to have taken 12 hours of general financial education as well as additional training specific to the type of intended asset purchase. CAPTC program staff also had significant interactions with treatment group members, providing them with regular reminders and encouragement to make contributions and save toward a goal.

During the experiment, control cases were restricted from participating in any other matched savings or homeownership program from CAPTC, including a pre-existing program that provided 1:1 matching funds for down payment and closing costs. Below, we discuss the extent to which this restriction may have affected the generality of the results. Control group members could receive homeownership counseling, and those who requested information about financial assistance for homeownership were referred to other Tulsa-area providers. Control and treatment cases could participate in CAPTC programs that provided loans for micro-enterprise and heating assistance.

IDA balances in this program did not affect eligibility for TANF programs, but could affect eligibility for other public assistance, such as food stamps and Medicaid. The main features of the Tulsa experimental IDA program are broadly similar to those enacted in other IDAs around the country.[8]

III. Data Issues

A. Attrition

The first row of Table 1 reports sample sizes for the first and third survey waves. Of the 1,103 individuals in the baseline survey, 840 – 76 percent – also completed interviews at Wave 3. Retention rates did not vary significantly between the treatment and control group for the full sample. The relatively high retention rate may be due in part to extensive tracking efforts and the incentives provided, equally for both treatment and control cases. Six tracking letters were sent between the various surveys; sample members received $10 for each letter to which they responded. At Waves Two and Three, respondents received $35 for completing the interview.

The next five rows show sample size and attrition rates for various sub-groups in the two surveys. Given the importance of homeownership in the IDA program, we focus on attrition by homeownership status. Likewise, anticipating an empirical finding that is reported below, we report attrition by race and homeownership status. There were no significant differences in attrition rates across baseline treatment and control group members among: all renters, renters who did not live in subsidized housing; or renters

[8] The Annual Report to Congress on the Assets for Independence Act (U.S. Department of Health and Human Services, 2005) includes information for 315 of the 317 IDA grants awarded by the Department of Health and Human Services between 1999 and 2004. The Tulsa experiment was not part of the AFI Program. About 33 percent of AFI grantee organizations are Community Action Agencies, like CAPTC. Most other grantees are community development corporations, human service nonprofits, or faith-based organizations. In AFI programs, the income eligibility level is 200 percent of the poverty line (compared to 150 percent in Tulsa), average financial education requirements are 12.5 hours (compared to 12 in Tulsa), 95 percent offer matching for home ownership, 88 percent for postsecondary education and 85 percent for business start-up, and the most prevalent match rate is 2:1 for each of those three uses (compared to 2:1, 1:1, and 1:1, respectively, for Tulsa).

who lived in subsidized housing. There were, however, significant differences in attrition between treatment and control groups among white renters who lived in subsidized housing at baseline. In our analysis below, we present results for the whole sample as well as for subsamples that did not feature differential attrition.

B. Sample Characteristics at Baseline

The first column of Table 2 presents economic and demographic characteristics of the combined Wave 3 sample (treatment and control cases) as recorded during the baseline interview. Sample members' average age was 36 years, and average monthly household income was less than $1,500. The poverty rate among sample members was 37 percent. Four out of five sample members were female; about one-quarter were married at time of randomization, and 40 percent had never been married. Nearly half of the people in the sample were non-Hispanic Caucasian, and 41 percent were African-American. About 6 percent had no high school diploma or GED, 26 percent had just a high school diploma or GED, and about 69 percent had attended at least some college, including 12 percent of the sample who graduated from a 4-year college. More than 40 percent reported receiving "some" or "a lot of" government assistance during the prior month, and more than 40 percent had no health insurance, despite the requirement that respondents be employed at the time of the eligibility interview.

The first column of Table 3 shows wealth holdings at baseline. About 23 percent of the combined sample already owned their own home; 7 percent had their own business, and 21 percent held retirement saving accounts. About 86 percent had either a checking or saving account, and 84 percent owned at least one motor vehicle. Average wealth holdings were low. Among all sample members, housing equity averaged $4,703, business equity averaged $465, and average retirement account balances equaled $751.

Overall financial assets averaged $2,116, while overall net worth averaged $2,732.

Columns 2-4 of Tables 2 and 3 suggest that randomization was effectively implemented. Significant differences in baseline characteristics of treatment and control group members were about as frequent as would be expected based on chance alone. Relative to controls, treatment group members were more likely to have been married at some point, and were more likely to have a bank account; they also had larger retirement account balances, although the two groups did not have statistically distinguishable levels of overall financial assets.

Columns 5-10 of Tables 2 and 3 show broadly similar results for the sample of baseline renters and for baseline renters who did not live in subsidized housing. In these two sets of comparisons, treatment group members had somewhat higher income and were more likely to have been married at some point. In addition, treatment group members had slightly more children on average and were more likely to have had a bank account prior to randomization.

The households in the Tulsa IDA sample are *not* a representative sample of low-income households. In particular, Tables A1 and A2 in the appendix compare the combined IDA sample (treatments and controls) to households who matched the IDA eligibility requirements – i.e., they were employed and had income below 150 percent of the poverty level – taken from the 1998 Survey of Consumer Finances and from a Tulsa-area subsample of the 2000 Census Public-Use Microdata Sample.[9] The three samples show roughly similar average age, but differ markedly in other respects. IDA sample members have slightly higher average income and are less likely to be married, have

[9] About 1 percent of the IDA sample was older than 65. In contrast, a significantly greater share of the selected SCF and PUMS samples were age older than 65. Therefore, to make valid comparisons across the three samples in Tables A1 and A2, we exclude households older than 65.

health insurance, own a business or home, or have income below the poverty line. They are far more likely to be female and African-American, to receive government assistance, and to have completed at least some college. They also have far lower levels of average wealth than the SCF sample members, but this is largely due to a few outliers in the SCF with extremely high wealth.[10] IDA sample members are more likely to have bank accounts.

The differences between the Tulsa IDA sample, a random national sample of low-income households, and a random sample of low-income households in the Tulsa area emphasize the importance of having a randomized control group in analyzing IDA behavior. As discussed below, the differences also affect the extent to which the results can be generalized to broader populations.

C. IDA Utilization Patterns

Before turning to analysis of the effects of IDAs, we briefly summarize aggregate IDA patterns and the individual determinants of account utilization. Among treatment group members in the analysis sample, 89 percent opened an IDA. We refer to these individuals as "participants." Almost half of participants opened their IDA in the first three months after random assignment. An account was considered closed when the balance was reduced to zero and there were no subsequent transactions. Participants kept their accounts open for an average of 38 months.

Among treatment group members, cumulative matchable IDA contributions averaged $1,110; 53 percent made the maximum annual contribution of $750 at least

[10] The 99th percentile for net worth is $2.3 million in the SCF sample versus $82,500 in the IDA sample. Similarly, the 99th percentile for financial assets is $406,000 million in the SCF compared with $15,000 in the IDA sample. These figures vastly overstate the differences across most of the wealth distribution. For example, median net worth is $5,450 in the SCF sample and -$165 in the IDA sample. Median financial asset holdings are $750 in the SCF sample and $400 in the IDA sample.

once, and 21 percent contributed the three-year maximum of $2,250. As of October 2003, 40 percent of treatment group members had taken a matched withdrawal, and 77 percent had taken at least one unmatched withdrawal. Unmatched withdrawals accounted for the vast majority – 79 percent – of all withdrawal transactions and a slimmer majority – 54 percent – of all withdrawn funds. (The matching funds themselves are not included in these calculations.) Among treatment group members, 39 percent made contributions, withdrew all of the deposits in unmatched withdrawals, and closed the account. Combined with the fact that 11 percent did not open an account, this implies that half of all treatment group members made no matched withdrawals.

Average matched and unmatched withdrawals (per transaction) were $636 and $194, respectively. Among matched withdrawals, 24 percent of transactions and 31 percent of funds withdrawn were for housing down payments. The average matched withdrawal was $844 for down payments and $576 for other allowed uses. Thus, the average withdrawal including the match was $2,532 and $1,152, respectively.[11]

The timing of IDA activity is also of interest. Contributions peaked in February and March. This is consistent with income tax refunds being a significant source of financing for IDA contributions and with findings from other IDA sites.[12] Matched withdrawals peaked in May, just after the spike in deposits. Unmatched withdrawals were made at a relatively steady rate throughout the year.[13]

Table 4 provides regression analysis of IDA utilization patterns. Participation

[11] As of October 2003, 19 percent of the treatment group still had positive balances in their accounts, with an average balance of $432 among those with positive balances. These balances are included as financial assets in the analysis.

[12] See Sherraden (2002). Smeeding (2002) and Smeeding et al. (2000) discuss and provide evidence on potential interactions between the Earned Income Tax Credit and IDAs.

[13] For further discussion of IDA contribution patterns, see Abt Associates (2004).

rates were quite high across all of the economic and demographic groups and relatively insensitive to traditional drivers of saving behavior such as age, income, or net worth, consistent with results in other IDA projects (Sherraden 2002). Households with heads aged 40-49 did participate and contribute more than other households, but the most noteworthy pattern with respect to age is the much higher likelihood of unmatched withdrawals among households with heads younger than 30 (versus those older than 40). Higher levels of household income tended to raise contributions but have no significant impact on participation or type of withdrawal.

Higher contribution levels and (especially) higher probabilities of making matched withdrawals were associated with having a bank account (perhaps as a proxy for financial knowledge or comfort with financial institutions), owning a home (which also suggests the respondent had participated in the financial system before), and higher educational attainment (which may suggest increased information or sophistication about financial issues). Controlling for other factors, initial net worth, receipt of government assistance, health insurance coverage, and car ownership generally did not influence IDA behavior.

Demographic characteristics also affected utilization. Relative to others, blacks contributed less, made fewer matched withdrawals and more unmatched withdrawals. Divorced household heads were less likely to participate and more likely to make matched withdrawals, while female-headed households were less likely to make matched withdrawals. Heads with children had fewer contributions or matched withdrawals.

Later cohorts of sample members contributed less and were less likely to make matched withdrawals than earlier cohorts. This is consistent with the view that eager savers signed up first and that the difficulty of recruiting motivated sample members rose over time.

IV. Methodology

We estimate the effect of being eligible for an IDA; that is, we provide "intent to treat" (ITT) estimates.[14] For continuous measures of household behavior, we estimate ordinary least squares equations of the form:

(1) $$Y_{3i} = \beta_0 + \beta_1 P_i + \beta_2 Y_{1i} + \beta_3 T_i + \varepsilon_i \,,$$

where the subscript i refers to the individual sample member, Y_{3i} is the value of an outcome variable in the wave 3 survey, Y_{1i} is the value of the corresponding variable in the baseline survey, T_i takes the value of 1 for treatment group members and zero otherwise, the β's are parameters, and ε is the individual-specific error term. The treatment effect is given by β_3. For dichotomous measures, we estimate linear probability models of the same form as (1). To improve the efficiency of the estimated treatment effects and account for any potential correlation between treatment status and observable characteristics at baseline, P_i in our specifications is the propensity score from probit estimation of treatment status, T_i, on a vector of baseline demographic and economic characteristics, X_i (Rosenbaum and Rubin, 1983). This vector includes

[14] The effect of IDA participation – the effect of "treatment on the treated" (TOT) – may also be of interest. If the treatment effect on eligible non-participants is zero and if ITT is the overall impact effect evaluated at the sample mean, the TOT estimate is ITT/p, where p is the IDA take-up rate (Orr 1999). In this experiment, however, this formula should probably be viewed as an upper bound for the TOT effect, because it is not obvious that the effect of the IDA on eligible non-participants is zero. Specifically, the financial education classes and encouragement to save that all treatment group members received could have had a favorable effect on behavior, even among those who did not open an IDA.

indicator baseline variables for: age (30-39, 40-49, 50+, with <30 omitted); having children; annual income (in thousands: 10-20, 20-30, 30+, with <10 omitted); net worth; educational attainment (some college, 4-year degree or more, with high school graduate or less omitted); female; marital status (married, divorced, with single or widowed omitted); receipt of government assistance; health insurance status; race/ethnicity (Black non-Hispanic, other non-Caucasian, with Caucasian non-Hispanic omitted); ownership of a bank account, a car and a home; and the month after the beginning of the experiment in which the sample member enrolled (4-6, 7-9, 10-12, 13-14, with 1-3 omitted).[15] To account for the fact that P_i is estimated, the standard errors for all specifications are calculated based on 499 bootstrap replications of (1), where for each of the bootstrap replications, the propensity score was re-estimated from the baseline-survey sample.

V. Results

A. Effects on Homeownership

The Tulsa IDA program provided its highest matching rate for down payments on primary residences. Table 5 summarizes the key homeownership results. Homeownership rates were roughly equal at baseline in the treatment and control groups – 22.5 percent and 24.3 percent, respectively. They grew rapidly in the first 18 months, to between 34.3 and 35 percent for each group, and then grew further, to 45.6 percent and 42.9 percent, respectively, by the month-48 survey. Sample members were clearly highly motivated to buy homes. Homeownership rose by 23.1 percentage points in the

[15] For each specification, this probit was performed using observations from the baseline survey, including individuals who eventually attrited by the third wave of the study.

treatment group between Waves 1 and 3; even among controls, the rate rose by 18.5 percentage points. The net effect on homeownership given by the difference-in-difference estimate, 4.6 percentage points, is not significantly different from zero.

Not surprisingly, the effects on homeownership are more sharply defined for the overall sample of renters at baseline. Homeownership rates rose by 6.9 percentage points (p = 0.058) among renters in the treatment group relative to controls over the 48-month period, with all of the increase occurring between the second and third surveys.[16] Recall, however, that there was differential attrition among white treatment and control group members who were living in subsidized rental units at baseline. Restricting the sample to unsubsidized renters, the IDA raised homeownership rates by 10.8 percentage points (p = 0.019.)

The last panel of Table 5 reports estimates of the treatment effect from linear probability and probit estimation of the parameters in (1). The estimated treatment effects are generally similar to, but smaller than, the raw difference-in-difference estimates. The treatment effect on homeownership in the overall sample is economically small and statistically insignificant. The OLS point estimate of the treatment effect among renters is somewhat larger, 5.7 percentage points, but is also insignificant (p = 0.112). Among unsubsidized renters, the treatment effect is 9.5 percentage points and highly significant (p = 0.034).

Table 6 examines heterogeneous treatment effects for homeownership. In light of well-known differences in homeownership rates across racial groups and possible

[16] Among homeowners at baseline, homeownership rates actually fell somewhat among treatment group members relative to controls over time, but the difference was not statistically significant.

discrimination in housing markets, we first decompose the results for renters by race.[17] Among all renters, the treatment effect is 9.8 percentage points among blacks and 1.8 percentage points among whites. Although this difference in point estimates might, at first glance, suggest an important role for IDAs in helping borrowers overcome racial discrimination, the two impact effects are not statistically different from each other. Moreover, as shown in the last two columns, the difference in point estimates shrinks dramatically and remains insignificant after controlling for attrition by removing renters who lived in subsidized housing. Essentially, there is no important difference across races in the treatment effects of the IDA.[18] The only statistically significant heterogeneous treatment effect in the full renter sample is among high- and low-income individuals, where the treatment effect for high-income individuals is quite substantial and significant.[19]

B. Other Subsidized Assets

Table 7 shows the effects of the IDA on other subsidized assets. In general, the IDA had very few effects on holdings of other subsidized assets. Among unsubsidized renters, for whom the homeownership effect was largest, non-retirement financial assets fell by $1,119 ($p$ = 0.044) for the treatment group relative to controls. Among all renters, where the homeownership effect was smaller, but still marginally significant, the reduction in financial assets is likewise smaller in absolute value, about $700, and

[17] Gaps among black and white households in homeownership rates and in the transition to homeowner status have proven large, persistent, and difficult to explain fully with observable characteristics. See Abt Associates (2005), Charles and Hurst (2002), Collins and Margo (2001), Gabriel and Rosenthal (2005), and the citations therein.

[18] This stands in contrast to conclusions developed in earlier versions of this work (Mills, Gale, and Patterson 2006), which were estimated on the full sample of renters.

[19] Grinstein-Weiss et al. (2007) provide further discussion of the home ownership effects.

marginally significant (p = 0.092). These results are consistent with the possibility that IDA holders used either their pre-existing stock of financial assets or current-period saving that they would have used anyway to fund their IDAs. The findings are also consistent with the view that treatment group members who bought homes spent more – on the down payment, closing costs, moving expenses, and home improvements – than just their IDA contribution plus the matching funds.[20] Indeed, the other significant treatment effect in the table is for the likelihood of undertaking home improvement among unsubsidized renters.[21] Despite the increase in homeownership, there were no significant treatment effects on home equity levels. As shown in the table, the IDA had no significant treatment effects for any of the groups on the other qualified uses, including business start-up or expansion, retirement saving, or education. (For homeowners at baseline, there was a marginally significant treatment effect on non-degree educational courses.)

C. Net Worth

While the IDA is intended to help low-income households by subsidizing the accumulation of specific types of assets, a broader and critical economic question is the extent to which subsidizing particular types of assets results in an increase in overall levels of saving or wealth. Before turning to this issue, it is worth noting that the effects of IDAs on net worth may not be as meaningful as, for example, the effects of eligibility for 401(k) plans on net worth. In evaluating traditional tax-based saving incentives, the

[20] This is consistent with the findings in Engelhardt and Mayer (1998) and Engelhardt (2003) for first-time homebuyers.

[21] Note that it would be irrational for home purchasers to forgo a 2:1 match on down payment in order to obtain a 1:1 match on home improvement. This suggests that the home improvement treatment effect shown in the table is likely to be derived from the home ownership effect, rather than a direct effect of the IDA. For example, there was no significant treatment effect for home improvement among the sample of baseline owners.

effect on household net worth is a critical determinant of the overall impact of the program (Engen et al. 1996, Poterba et al. 1996). In contrast, the effect on net worth is not a sufficient statistic for evaluating the impact of an IDA program, at least in the short-run. Purchasing a home, for example, often generates costs associated with settlement, moving, and new appliances or furniture, each of which serves to reduce measured net worth. Enrolling in classes raises human capital, but the tuition, other expenses, and foregone short-term earnings may reduce measured net worth, too.

Table 8 reports a series of net worth regressions. The first column in the top panel of the table reports OLS estimates of treatment effects on net worth and finds positive but insignificant impacts for the full sample, the sample of unsubsidized renters, and the full sample less subsidized renters. (We do not report separate estimates of net worth effects for owners, given the absence of any significant impact for that group.)

Two inter-related concerns in the analysis of net worth data are the role of outliers and the possibility of measurement error. The difficulties of obtaining accurate data on components of net worth are well known. After some potentially suspect values in some Wave 1 and 2 net worth records were observed by the Center for Social Development (CSD), several criteria were developed in conjunction with CSD to identify and verify responses that might have been misreported or incorrectly recorded. Responses were verified if: they fell outside a specified range for each question; the change in the recorded value between one wave and the next fell outside a specified range; or the value was inconsistent with another response in the same wave. For items identified for verification in Waves 1 or 2, respondents were asked to correct or confirm the previously recorded value by responding to an individualized Survey Quality Form, which was mailed with the Month 45 tracking letter. For those not responding, the form was administered in the Wave 3 survey. By Wave 3, interviewers were fully aware of the issues, and immediately verified values using range checks incorporated directly into the CATI/CAPI software. For other Wave 3 data values identified for verification (involving a large between-wave change or within-wave inconsistency), a Survey Quality Form was administered by telephone during November 2003 or mailed to the respondent, with the answers incorporated into the final data set used here.

We perform a wide variety of sensitivity tests. The upper part of Table 8 reports outlier robust regressions, which down-weight the role of outliers, and median regressions, which eliminate the role of outliers to the extent that the true value of net worth remains on the same side of the median as the reported value. These estimates provide no evidence that IDAs raise net worth. The lower part of Table 8 reports estimates based on trimming the sample at 5 different points, based on three different measures (Wave 1 net worth, Wave 3 net worth, and the change in net worth between Waves 1 and 3). None of the resulting 45 estimates are positive and significantly different from zero, and one quarter of the point estimates are negative. As an additional metric, Figure 1 plots quantile treatment effects for net worth for the three samples.[22] In the middle of the quantile range, the point estimate hovers very close to zero. At higher and lower quantiles, point estimates rise but are never significantly different from zero..

Thus, we conclude that, in practice, the substantial underlying variability of net worth, combined with the relatively small sample size and the relatively small potential "stimulus" to net worth provided by the IDA contributions (relative to the underlying

[22] The figure also shows the boundaries of the bootstrapped 90% confidence intervals for the treatment effects as dotted lines and mean effects as horizontal dashed lines. These confidence intervals were based on 499 bootstrap replications for each quantile estimation of (1). For each of the bootstrap replications, the propensity score was re-estimated from the baseline-survey sample.

variation in net worth), make it impossible to distinguish between the views that all or none of the contributions are net additions to a participant's net worth.

D. Other Measures of Financial Status

By providing financial incentives to accumulate wealth, financial education and significant case-management expertise, IDAs can in principle influence a wide variety of financial activities. Table 9 reports estimates of IDA treatment effects on households' Wave 3 financial situation and poverty status. Previous research has shown that the prevalence of public and private transfers depends critically on the economic status of the recipient (McGarry and Schoeni, 1995). After 4 years, however, treatment group members were no less likely to receive help from government, individuals, or organizations than were control group members. Likewise, relative to control group members, treatment group members were not more likely to think their financial situation had improved over the last 18 months, and they did not feel it was easier to make ends meet. There is some evidence that the treatment group members felt more hopeful about their financial situation in the future, but among unsubsidized renters – where the homeownership effects were largest – treatment group members were less likely to be satisfied with their current financial situation than were control group members.

There has also been a substantial amount of discussion about the role of IDAs as a poverty-reducing device (Sherraden 1991, Boshara 2005). The Tulsa IDA, however, had no significant impact on the share of treatment group members who, in Wave 3, exceeded 50 percent, 100 percent or 150 percent of the poverty threshold or on income-to-needs ratios generally.

VI. Discussion

Several aspects of the design and implementation of the experiment raise issues of interpretation. Treatment group members had incentives to accelerate home purchases into the sample period, and control group members had incentives to delay purchases until the sample period ended. For the treatment group, the incentive to accelerate arose because the program matched contributions that were made during the four-year period and used for a down payment within that time at a 2:1 rate. Down payments made in future years were effectively matched at a 1:1 rate (if the IDA funds were rolled over into a Roth IRA and then used for home purchase sometime in the future). A treatment group renter who was planning to buy a home at some point in the future therefore may have accelerated the buying decision due to the program. For the control group, the incentive to delay home purchase stemmed from the program requirement that control group members not participate in other homeownership programs at CAPTC during the evaluation. This implies that the homeownership subsidy options for control group members were less attractive during the experiment than the options faced by typical low-income households, and that the options would improve once the experiment ended.

The incentives for control group members to delay would be muted to the extent that they had access to other IDAs or social service programs during the time period. However, the survey asked about uses of other social services at CAPTC. A comparison of means reported in Appendix Table 3 show that, if anything, treatment group members used *more* additional services at CAPTC than controls, especially with regard to small business training, Head Start, and tax preparation. It is difficult to discern whether the increased usage of other services should best be considered an effect of the IDA program or as a component of “broader IDA” intervention. In either case, however, the result suggests that the control group was not capturing additional net services, outside of the IDA, relative to the treatment group.[23]

To the extent that either incentive influenced the timing of home purchases, the results above would overstate the effect, during the first four years, of a broadly adopted IDA program that was perceived to be permanent and existed in conjunction with other already-established programs. (The long-term effect of such an IDA could be larger or smaller than the estimated effects above.) It is certainly plausible that some of the purchases represent accelerations of home buying that would have occurred in the future even in the absence of the program. Moreover, the fact that almost all of the effects on homeownership took place between Wave 2 and Wave 3 (see Table 5) is consistent with an “acceleration” explanation. There are other reasonable explanations, however, of the fact that the homeownership effect occurred after 18 months. For example, in order to obtain 3 years’ worth of matching contributions, renters would have needed to hold their accounts open at least 25 months.

Two concerns with the external validity of the results may also arise. The experiment took place in a city with low housing prices during a period when the underlying national homeownership rate was rising. Down payment subsidies may be more effective in places and times where down payment constraints are more binding, in

[23] An additional issue is that attendance and other records indicate that up to 31 (7.2 percent of) control group members may have received access to some (not all) of the educational services and financial assistance with housing that was intended for the treatment group only. None of the 31 individuals, however, was allowed to open an IDA. If all of the crossovers received the entire treatment, the appropriate adjustment would multiply the estimated treatment effects by 1/(1-r), where r is the rate of crossover (Bloom 1984). This would raise the estimated treatment effects by 8 percent (but would not affect statistical significance). A correction of this magnitude, however, is almost certainly too large, since it assumes that all 31 cases received all of the services intended for treatments, including the option of opening an IDA.

which case the Tulsa results would understate the effects of a broader IDA program.

Also, while there is no reason to think the sample members are unrepresentative of the type of household that would apply for an IDA if a broader program existed, it is nevertheless clear that the analysis sample is not a random draw of all low-income households – both in demographic and wealth characteristics and in motivation to buy homes. This suggests that the results apply to households who would want to apply for an IDA, but not necessarily to the whole low-income population. This does not seem inappropriate, however. Because of the intensive interaction between treatment group members and program staff required to implement the Tulsa IDA program, it seems unlikely that an IDA with the same features could be taken to a full national scale. Thus, the effects of the IDA on the population that would be likely to sign up for IDAs that are limited in sample size seem relevant.

VII. Conclusion

This paper presents the first experimental evidence on Individual Development Accounts and on how public policies that subsidize saving can affect outcomes broader than contributions to the subsidized accounts. Despite strong incentives, regular interaction between program staff and treatment group participants, and the presence of a strongly motivated group of savers, we find generally weak sample-wide effects of the Tulsa IDA program on household behavior. There are *no* sample-wide impacts on holdings of subsidized assets. The strongest subgroup effect occurs for homeownership among renters. At 7-11 percentage points, this effect is economically and statistically significant, but it is offset to some extent by a reduction in non-retirement financial assets and could be upwardly biased due to short-term time-shifting of home purchases.

More generally, the analysis highlights the importance of having a control group determined by random assignment. Members of the treatment group did experience substantial improvement in their financial and housing situation over the four-year period, but for most outcomes and sub-groups these improvements were not significantly different from those in the control group.

Evaluation of the program's overall success or failure, however, is beyond the scope of this paper and hinges on numerous issues that cannot be addressed here. For example, the large share of treatment group members who made unmatched withdrawals could be considered evidence of program ineffectiveness, since the funds were not used for targeted purposes, or program success, to the extent that the IDA made it possible for households to set aside funds that could be used for any of a variety of purposes. Likewise the increase in home ownership could be considered a success, in that it provides new opportunities to households. Alternatively, it could represent an undue encouragement of such families to buy homes, which could create financial risks and costs they are not equipped to handle.

Besides trying to disentangle these questions, future research could usefully focus on several issues. First, through what mechanisms do IDAs affect behavior? IDAs bundle together a significant number of formal incentives (match rates, contribution limits, allowable uses), informal or less formal assistance (financial education, encouragement and advice from program staff), and even some disincentives (possibly reduced eligibility for government programs). We have no way of sorting out the relative impacts of particular factors or components of the Tulsa IDA program. Future experiments should aim to clarify the effects of different features of IDAs in particular and subsidies for saving in general on the various populations of interest. A potentially important distinction in this regard is the relative role of “hard” incentives, such as matching rates for contributions, versus “soft” incentives or program features, such as information, encouragement, and attention from program staff. Recent analysis has shown that, controlling for underlying economic incentives, the provision of information and the presentation of choices can significantly influence household saving behavior.[24]

Second, what are the longer-term effects of IDAs? IDAs are intended to be more than simply saving accounts; they are intended to induce behavioral changes – acquiring education, buying a home, or starting a business – that fundamentally alter households' lifetime prospects. Such gains may take time to develop. For example, treatment group members who used the IDA to acquire education may benefit in the future from enhanced employment and earnings prospects. In contrast, the gains in homeownership in the four years of the Tulsa IDA experiment may persist, grow, or shrink over time.

Third, how do the costs and benefits of IDAs stack up against other policy options? There is little evidence on these questions. Schreiner (2006) estimates the costs of running the Tulsa IDA program for 36 months at $922,472, excluding the matching funds. He calculates the administrative costs to be about $1,949 per participant, or $61 per participant-month or $3.06 per dollar saved by IDA participants. Whether the costs of IDAs are large or small—given the estimated effects on economic behavior, the social valuation of those effects, and the costs and benefits of alternatives—remains an open and important question.

[24] See, for example, Bertrand et al. (2005), Burman et al. (2007), Duflo et al. (2006), Madrian and Shea (2001), Thaler and Benartzi (2004), and Saez (2007).

Appendix: Data Construction

For financial assets or account balances, respondents are simply asked to report the current value, e.g., "How much do you have in retirement accounts like IRAs?" Debts are likewise obtained through straightforward questions of the type: "How much do you owe on personal loans from banks or credit unions?" For real assets, respondents are asked to approximate the current market value. Home value, for instance, is obtained through the question "How much do you think your home would sell for now?" Other variables are constructed as follows:

- Home Equity = Home value - mortgage debt.

- Business Equity = Business assets - business debt. Business assets are the response to "How much do you think your business assets are worth? By business assets, I mean things like buildings, vehicles, equipment, inventory, materials, supplies, bank accounts, etc." Business debt includes (as separate survey items) business loans from banks or credit unions and business loans from friends or relatives.

- Retirement Saving Balance includes IRA balances and balances in 401(k)s and similar workplace accounts.

- Non-Retirement Financial Assets = Liquid Assets + Other Non-Retirement Financial Assets. Liquid Assets consist of checking account balances, savings account balances, money market accounts, and CDs. Other Non-Retirement Financial Assets consist of US savings bonds, educational accounts, stock, bonds, or mutual funds, money stored with family and friends, money stored at home, and balances in Christmas Club or vacation accounts.

- Net Worth = Non-Retirement Financial Assets + Retirement Saving Balance + Real Assets – Liabilities. Real Assets include vehicles, rental property and other real estate, owner-occupied housing, and business assets. Liabilities include home (mortgage) loans, car (or other vehicle) loans, home improvement or equity loans, business loans from banks or credit unions, business loans from friends or relatives, credit cards or charge accounts, installment loans for major purchases like furniture or appliances, educational or school loans, debt consolidation loans or bills owed to collection agencies, loans for property besides your home, personal loans from banks or credit unions, personal loans from friends or relatives, medical bills, past-due rent payments, past-due phone bills, past-due utility bills, past-due bills for record and book clubs, or other bills owned for more than one month.

Several outcomes are dichotomous variables, some of which combine a series of four or five responses. These variables, the corresponding survey questions, and the responses coded as affirmative (=1) are as follows:

- Respondent indicates “No help at all” to “When it comes to making ends meet, how much help do you get from family and friends? Do you get...”

- Respondent indicates “No help at all” to “When it comes to making ends meet, how much help do you get from food pantries, churches, family services, and other organizations? Do you get...”

- Respondent indicates “No help at all” to “When it comes to making ends meet, public assistance programs, such as TANF (Temporary Assistance for Needy Families), SSI, food stamps, Medicaid, and housing assistance? Do you get...”

- Respondent indicates “Gotten Better” to “During the past two years, has your financial situation gotten better, gotten worse, or stayed the same?”

- Respondent indicates “Very satisfied” or “Somewhat satisfied” to “As far as you and our family are concerned, how satisfied are you with your current financial situation? Would you say you are…?”

- Respondent indicates “Very hopeful” or “Somewhat hopefully” to “How hopeful would you say your financial situation looks? Would you say…?”

- Respondent indicates “Very easy” or “Easy” to “Overall, how hard or easy is it to make ends meet? Would you say it is….?”

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Table 1
Sample Size and Completion Rates Across Sub-Samples by Wave 3

	Overall Sample			Treatment Group			Control Group			Difference[b]
Sample Restrictions[a]	**Baseline**	**Wave 3**	**Completion**	**Baseline**	**Wave 3**	**Completion**	**Baseline**	**Wave 3**	**Completion**	**T - C**
Full Sample	1103	840	76.2%	537	412	76.7%	566	428	75.6%	1.1%
Renters	864	643	74.4%	435	319	73.3%	429	324	75.5%	-2.2%
Unsubsidized Renters	583	439	75.3%	294	218	74.1%	289	221	76.5%	-2.4%
Subsidized Renters	281	204	72.6%	141	101	71.6%	140	103	73.6%	-2.0%
Non-White Subsidized	211	151	71.6%	104	78	75.0%	107	73	68.2%	6.8%
White Subsidized Renters	70	53	75.7%	37	23	62.2%	33	30	90.9%	-28.7% ***

[a.] Defined by status in the baseline survey.

[b.] Statistical significance is indicated as follows: *** = p<.0.01; ** = p<0.05; * = p<0.10.

Table 2

Baseline Demographic and Economic Characteristics

Sample Characteristics	Full Sample Combined Sample (n=840)	Full Sample Treatment Group (n=412)	Full Sample Control Group (n=428)	Full Sample Difference T - C [a]	Baseline Renters Treatment Group (n=319)	Baseline Renters Control Group (n=324)	Baseline Renters Difference T - C [a]	Baseline Unsubsidized Renters Treatment Group (n=218)	Baseline Unsubsidized Renters Control Group (n=221)	Baseline Unsubsidized Renters Difference T - C [a]
Age	36.3	36.3	36.4	-0.1	34.8	35.0	-0.2	35.3	34.7	0.6
Monthly Household Income	$1,453	$1,490	$1,416	$74	$1,489	$1,343	$146 **	$1,606	$1,384	$221 **
Female (%)	79.9%	78.9%	80.8%	-2.0%	80.9%	83.0%	-2.1%	75.7%	76.9%	-1.2%
# of Children in Household	1.7	1.7	1.6	0.1	1.8	1.6	0.2	1.7	1.4	0.3 **
Marital Status (%)										
Never Married	40.1%	35.9%	44.2%	-8.2% **	40.4%	51.2%	-10.8% ***	35.3%	48.9%	-13.5% ***
Married	26.2%	28.2%	24.3%	3.9%	25.4%	20.1%	5.3%	29.4%	23.5%	5.8%
Divorced or Separated	31.0%	33.3%	28.7%	4.5%	32.6%	26.9%	5.8%	33.5%	25.8%	7.7% *
Widowed	2.7%	2.7%	2.8%	-0.1%	1.6%	1.9%	-0.3%	1.8%	1.8%	0.0%
Race/Ethnicity (%)										
Caucasian, Non-Hispanic	47.0%	44.9%	49.1%	-4.2%	39.5%	45.4%	-5.9%	47.2%	52.9%	-5.7%
African-American, Non-	41.0%	43.0%	39.0%	3.9%	48.0%	42.9%	5.1%	40.8%	34.4%	6.4%
Other	12.0%	12.1%	11.9%	0.2%	12.5%	11.7%	0.8%	11.9%	12.7%	-0.7%
Educational Attainment (%)										
Less than High School	5.5%	6.3%	4.7%	1.6%	4.1%	4.3%	-0.2%	3.2%	3.6%	-0.4%
High School Diploma or GED	25.7%	25.0%	26.4%	-1.4%	24.8%	28.1%	-3.3%	23.9%	29.4%	-5.6%
Less than BA	57.1%	56.6%	57.7%	-1.2%	59.6%	56.8%	2.8%	57.8%	54.3%	3.5%
BA or more	11.5%	12.1%	11.0%	1.2%	11.6%	10.8%	0.8%	15.1%	12.7%	2.5%
Receive Gov't Assistance (%)	42.4%	42.7%	42.2%	0.6%	49.2%	48.0%	1.2%	32.1%	33.6%	-1.5%
With Health Insurance (%)	58.3%	59.0%	57.6%	1.4%	59.6%	54.2%	5.4%	60.1%	55.5%	4.6%
Poverty Rate (%)	36.9%	37.1%	36.7%	0.5%	37.9%	40.1%	-2.2%	30.7%	32.1%	-1.4%

[a.] Statistical significance is indicated as follows: *** = p<.0.01; ** = p<0.05; * = p<0.10.

Table 3

Baseline Financial Characteristics

Sample Characteristics	Full Sample: Combined Sample (n=840)	Full Sample: Treatment Group (n=412)	Full Sample: Control Group (n=428)	Full Sample: Difference T - C [a]	Baseline Renters: Treatment Group (n=319)	Baseline Renters: Control Group (n=324)	Baseline Renters: Difference T - C [a]	Baseline Unsubsidized Renters: Treatment Group (n=218)	Baseline Unsubsidized Renters: Control Group (n=221)	Baseline Unsubsidized Renters: Difference T - C [a]
Ownership Probabilities (%)										
Own Home	23.5%	22.6%	24.3%	-1.7%	0.0%	0.0%	.	0.0%	0.0%	.
Own Business	6.8%	7.8%	5.8%	1.9%	4.1%	4.9%	-0.9%	5.5%	5.4%	0.1%
Have Retirement Savings	21.1%	23.1%	19.2%	3.9%	21.6%	17.3%	4.3%	22.0%	19.9%	2.1%
Have a Bank Account	85.6%	88.6%	82.7%	5.9% **	88.7%	79.3%	9.4% ***	89.9%	81.4%	8.5% **
Own Car	84.2%	84.2%	84.1%	0.1%	80.6%	80.6%	0.0%	82.6%	81.9%	0.7%
Average Holdings[b]										
Home Equity	$4,703	$4,228	$5,161	-$931	$0	$0	.	$0	$0	.
Business Equity	$465	$391	$536	-$145	-$33	$347	-$380	-$26	$549	-$574
Non-Retirement Fin. Assets	$1,365	$1,245	$1,481	-$236	$1,175	$989	$186	$1,425	$979	$446
Retirement Account Balances	$751	$939	$569	$370 *	$647	$398	$249	$740	$514	$225
Financial Assets	$2,116	$2,184	$2,050	$135	$1,821	$1,386	$435	$2,165	$1,493	$672
Net Worth	$2,732	$2,107	$3,334	-$1,227	-$3,755	-$3,952	$197	-$4,245	-$3,654	-$590

[a.] Statistical significance is indicated as follows: *** = p<.0.01; ** = p<0.05; * = p<0.10.

[b.] Including Non-owners

Table 4
Determinants of IDA Participation, Contributions, and Withdrawals

Baseline Characteristic	Prob. Contribute if in Treatment (Probit) (n = 412)		Cumulative Matchable Contributions if in Treatment (Tobit) (n = 412)		Prob. Matched Withdrawal if Contributed (Probit) (n = 366)		Prob. Unmatched Withdrawal if Contributed (Probit) (n = 366)	
	dF/dx[a]	P-value	Coef.	P-value	dF/dx[a]	P-value	dF/dx[a]	P-value
Age 30-39	-0.010	0.749	-149	0.264	-0.107	0.161	-0.080	0.125
Age 40-49	0.082	0.016	275	0.073	0.002	0.984	-0.107	0.074
Age 50 +	-0.030	0.619	-112	0.610	0.114	0.354	-0.230	0.015
Annual Income: 10k-20k	0.031	0.370	228	0.121	0.040	0.638	-0.048	0.314
Annual Income: 20k-30k	0.041	0.267	373	0.034	0.052	0.600	-0.033	0.582
Annual Income: 30k +	0.047	0.287	436	0.043	0.079	0.505	-0.077	0.320
Some college	0.074	0.013	337	0.004	0.075	0.274	-0.022	0.533
4-year degree or more	0.046	0.213	835	0.000	0.246	0.016	0.012	0.829
Have bank account	-0.001	0.989	330	0.052	0.239	0.015	-0.026	0.628
Own home	0.054	0.140	517	0.001	0.270	0.001	0.007	0.873
Net worth	0.000	0.816	0	0.178	0.000	0.231	0.000	0.061
On gov't assistance	-0.019	0.496	-139	0.218	0.006	0.931	0.013	0.720
Have health insurance	-0.025	0.350	-175	0.107	-0.084	0.168	0.024	0.468
Own car	0.004	0.904	146	0.313	0.121	0.161	0.005	0.925
Married	-0.045	0.273	-17	0.911	0.051	0.556	0.019	0.690
Divorced	-0.061	0.100	-42	0.757	0.214	0.005	0.001	0.976
Have children	-0.039	0.328	-476	0.003	-0.149	0.072	0.052	0.277
Female	-0.021	0.548	-12	0.937	-0.169	0.041	0.047	0.311
Black	-0.056	0.078	-328	0.006	-0.150	0.022	0.062	0.084
Other non-white	-0.063	0.193	-25	0.884	-0.062	0.510	0.031	0.531
Cohort 4-6	-0.104	0.044	-779	0.000	-0.236	0.009	-0.055	0.426
Cohort 7-9	0.000	0.996	-358	0.048	-0.124	0.190	-0.170	0.034
Cohort 10-12	0.004	0.919	-448	0.007	-0.163	0.068	-0.119	0.091
Cohort 13 +	-0.068	0.196	-594	0.001	-0.216	0.023	-0.182	0.032

[a.] dF/dx is for discrete change of dummy variable from 0 to 1

Table 5
Effects on the Transition to Home Ownership

Sample	**Baseline Renters (n = 643)**		**Baseline Unsubsidized Renters (n = 438)**		**Full Sample (n = 840)**	
Homeownership Rates	**Mean**	**P-value**	**Mean**	**P-value**	**Mean**	**P-value**
Wave 1						
Treatment	.	.	.	.	0.225	0.001
Control	.	.	.	.	0.243	0.001
T-C Difference	.	.	.	.	-0.017	0.556
Wave 2						
Treatment	0.164	0.001	0.215	0.001	0.343	0.001
Control	0.167	0.001	0.201	0.001	0.35	0.001
T-C Difference	-0.003	0.925	0.014	0.726	-0.007	0.829
Wave 3						
Treatment	0.348	0.001	0.431	0.001	0.456	0.001
Control	0.279	0.001	0.323	0.001	0.429	0.001
T-C Difference	0.069	0.058	0.108	0.019	0.028	0.419
Wave 3 - Wave 1						
Treatment	0.348	0.001	0.431	0.001	0.231	0.001
Control	0.279	0.001	0.323	0.001	0.185	0.001
T-C Difference	**0.069**	**0.058**	**0.108**	**0.019**	**0.046**	**0.168**
Estimated Treatment Effects Controls =Y_1, P	**dF/dX[a]**	**P-value[b]**	**dF/dX**	**P-value**	**dF/dX**	**P-value**
OLS	**0.057**	**0.112**	**0.095**	**0.034**	**0.031**	**0.254**
Probit	**0.057**	**0.118**	**0.095**	**0.034**	**0.034**	**0.316**

[a.] dF/dx is for discrete change of dummy variable from 0 to 1.
[b.] P-values are bootstrapped from 499 replications.

Table 6

Heterogeneous Homeownership Treatment Effects[a]

Sample	Baseline Renters (n = 643)		Baseline Unsubsidized Renters (n = 438)	
Baseline Characteristic	**TE[b]**	**P-value[c]**	**TE**	**P-value**
A. White	0.018	0.669	0.077	0.212
Black	0.098	0.064	0.101	0.100
	{0.429}		{0.778}	
B. Greater than 34 years old	0.033	0.525	0.046	0.449
Less than or equal to 34 years old	0.079	0.116	0.138	0.036
	{0.517}		{0.417}	
C. Household income above median	0.140	0.004	0.134	0.032
Household income below median	-0.030	0.609	0.050	0.437
	{0.024}		{0.361}	
D. High school or less education	0.016	0.762	0.179	0.040
Greater than HS education	0.075	0.080	0.058	0.289
	{0.489}		{0.232}	
E. Had a bank account	0.065	0.128	0.101	0.056
Did not have a bank account	-0.007	0.970	0.032	0.818
	{0.449}		{0.609}	
F. Received Gov't Assistance	0.028	0.573	0.156	0.060
Did not Receive Gov't Assistance	0.086	0.136	0.065	0.204
	{0.437}		{0.365}	
G. Had a Car	0.058	0.148	0.087	0.076
Did not have a Car	0.044	0.485	0.115	0.301
	{0.998}		{0.778}	
H. Had health insurance	0.080	0.064	0.089	0.108
Did not have health insurance	0.024	0.709	0.100	0.156
	{0.417}		{0.946}	
I. Married	0.037	0.669	0.087	0.365
Not Married	0.057	0.136	0.092	0.080
	{0.810}		{0.946}	
J. Had children in household	0.057	0.196	0.092	0.080
Did not have children in household	0.059	0.381	0.103	0.253
	{0.910}		{0.994}	
K. Cohorts 1 - 6	0.085	0.116	0.171	0.024
Other Cohorts	0.037	0.373	0.041	0.485
	{0.557}		{0.196}	

[a.] OLS Regressions that condition on baseline propensity score as discussed in the text.

[b.] P-values in brackets represent an F-test of the significance of differences in treatment effects.

[c.] All P-values are calculated from 499 bootstrap replications

Table 7
Other Treatment Effects[a]

Other Assets	Full Sample (n = 840)		Renters[b] (n = 643)		Unsubsid. Renters[b] (n = 438)		Full Less Unsub. Renters[b] (n = 635)		Owners[b] (n = 197)	
	TE	**P-value[c]**	**TE**	**P-value[c]**	**TE**	**P-value[c]**	**TE**	**P-value[c]**	**TE**	**P-value[c]**
Non-retirement financial assets	-1,925	0.048	-725	0.092	-1,119	0.044	-2,595	0.036	-5,839	0.513
Home improvement (0,1)	0.043	0.152	0.053	0.096	0.074	0.084	0.053	0.144	-0.055	0.473
Home equity	1,493	0.309	1,147	0.369	71	0.918	828	0.693	1537	0.770
Own Business (0,1)	-0.002	0.882	-0.006	0.770	-0.014	0.637	-0.010	0.697	-0.010	0.878
Business equity	937	0.633	-545	0.553	788	0.168	2457	0.248	999	0.778
Non-degree course (0,1)	0.040	0.305	0.031	0.465	-0.016	0.733	0.006	0.918	0.119	0.108
Degree course (0,1)	0.002	0.966	0.027	0.457	0.042	0.377	0.007	0.862	-0.025	0.669
Have Retirement Saving (0,1)	-0.006	0.826	0.002	0.978	0.006	0.890	-0.010	0.741	-0.078	0.212
Retirement Saving Balance	393	0.244	113	0.585	49	0.717	430	0.377	906	0.597

[a.] OLS Regressions that condition on Y_1 and propensity score as discussed in the text.
[b.] Defined by status in the baseline survey.
[c.] P-values are calculated from 499 bootstrap replications.

Table 8

Outlier Robust Treatment Effects for Net Worth at Wave 3[a]

Net Worth at Wave 3	OLS		Outlier Robust OLS		Median Treatment Effects	
	TE	P-value[c]	TE	P-value	TE	P-value
Full Sample	1,397	0.733	98	0.926	518	0.894
Unsubsidized Renters[b]	273	0.942	80	0.994	-189	0.749
Full Sample less Subsidized Renters[b]	1,339	0.854	1,041	0.589	185	0.986

Amount Trimmed Off Each Tail	0.5%		1.0%		1.5%		2.0%		2.5%	
	TE	P-value	TE	P-value	TE	P-value	TE	P-value	TE	P-value
A. Full Sample										
Trim Wave 1 Net Worth	$1,238	0.834	-$877	0.854	-$866	0.754	-$1,107	0.733	-$1,008	0.721
Trim Wave 3 Net Worth	$311	0.850	$1,127	0.581	$1,063	0.473	$1,508	0.357	$1,763	0.309
Trim Change in Net Worth	$498	0.733	$1,390	0.401	$2,252	0.289	$2,199	0.204	$1,888	0.212
B. Unsubsidized Renters[b]										
Trim Wave 1 Net Worth	-$84	0.994	$246	0.978	$457	0.914	$550	0.886	$544	0.866
Trim Wave 3 Net Worth	$210	0.870	$542	0.729	$959	0.673	$2,160	0.529	$2,024	0.489
Trim Change in Net Worth	-$296	0.890	$1,152	0.697	$1,717	0.469	$2,156	0.373	$1,568	0.357
C. Full Sample less Subsidized Renters[b]										
Trim Wave 1 Net Worth	$1,152	0.914	-$2,016	0.810	-$2,201	0.613	-$1,799	0.573	-$2,274	0.557
Trim Wave 3 Net Worth	-$1,216	0.657	$62	0.910	-$142	0.946	$132	0.729	$1,906	0.597
Trim Change in Net Worth	-$895	0.762	$362	0.818	$1,545	0.549	$2,789	0.373	$2,342	0.329

a. Each regression conditions on baseline networth and propensity score as discussed in the text.
b. Defined by status in the baseline survey.
c. P-values calculated from 499 bootstrapped replications.

Table 9
Wave 3 Financial Situation and Poverty[a]

Sample	Full Sample (n = 840)			Unsubsidized Renters[b] (n=439)			Full Sample Less Subsidized Renters[b] (n=635)		
	TE	P-Value[c]	Control[d]	TE	P-Value	Control[d]	TE	P-Value	Control[d]
A. Receive No Help from Friends or Family to Make Ends Meet (0,1)	-0.006	0.878	0.518	0.015	0.697	0.523	0.016	0.653	0.534
B. Receive No Help from Organizations to Make Ends Meet (0,1)	-0.025	0.405	0.754	-0.046	0.261	0.768	-0.013	0.669	0.793
C. Receive No Help from the Government to Make Ends Meet (0,1)	-0.013	0.693	0.637	-0.033	0.429	0.691	-0.011	0.754	0.719
D. During last 18 months Financial Situation has Improved (0,1)	0.037	0.289	0.438	-0.026	0.589	0.505	0.008	0.862	0.466
E. Currently Satisfied with Financial Situation (0,1)	-0.009	0.758	0.362	-0.074	0.024	0.376	-0.019	0.621	0.375
F. Hopeful about Financial Situation (0,1)	0.023	0.144	0.693	-0.010	0.593	0.711	0.014	0.477	0.703
G. Feels it is Easy to Make Ends Meet (0,1)	-0.023	0.148	0.110	-0.027	0.325	0.119	-0.024	0.212	0.120
H. Poverty Status									
Exceeds 50% of Pov. Threshold	0.004	0.798	0.945	0.026	0.144	0.937	0.014	0.333	0.94
Exceeds 100% of Pov. Threshold	-0.014	0.549	0.794	0.015	0.653	0.784	0.000	0.974	0.81
Exceeds 150% of Pov. Threshold	-0.018	0.509	0.619	-0.028	0.461	0.655	-0.026	0.381	0.65
Income-to-Threshold Ratio	-0.039	0.754	1.780	-0.048	0.782	1.921	-0.051	0.697	1.92

a. OLS Regressions that condition on Y_1 and propensity score as discussed in the text.

b. Defined by status in the baseline survey.

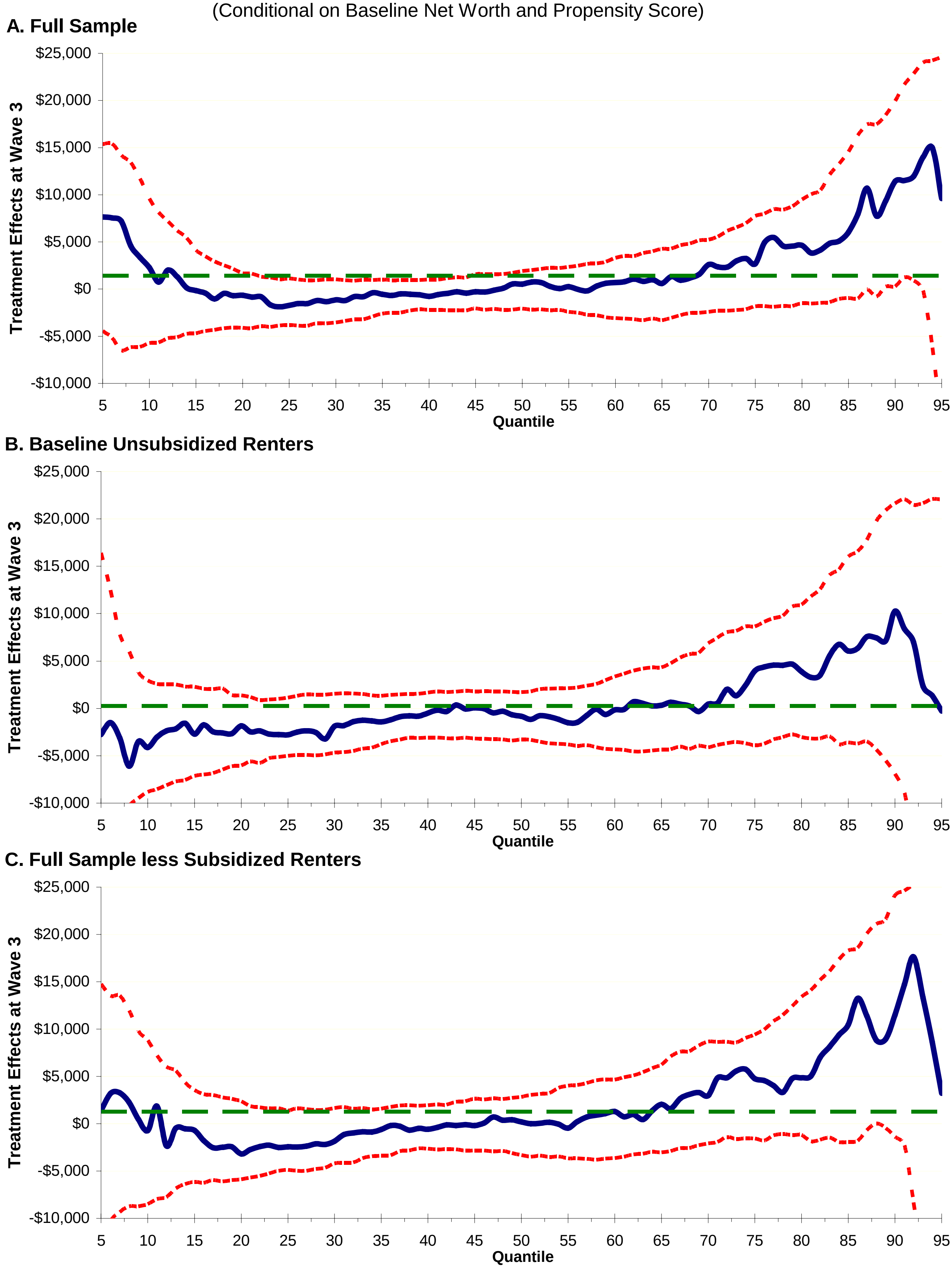
c. P-values are calculated from 499 bootstrapped replications.

d. Measured as the percentage of control participants with a value equal to 1 at Wave 3

Figure 1

Quantile and Mean Treatment Effects for Net Worth at Wave 3[a]

(Conditional on Baseline Net Worth and Propensity Score)



[a] Horizontal dashed line is mean treatment effect and the dotted line represents the 90% CI for quantile treatment effects.

Table A-1

Baseline Demographic and Economic Characteristics: Analysis Sample, the 1998 Survey of Consumer Finances, and the 2000 Decennial Census (IPUMS) for the Tulsa MSA

Sample Characteristics	Analysis Sample (n=840)	1998 SCF[b] (n=1,927)	Difference[a] Analysis - SCF	Tulsa MSA[c] (n=24,954)	Difference[a] Analysis - Tulsa
Age	36.3	35.5	0.8 *	35.2	1.1 ***
Monthly Household Income	$1,453	$1,120	$333 ***	$1,310	$143 ***
Female (%)	79.9%	38.9%	41.0% ***	47.1%	32.8% ***
# of Children in Household	1.7	1.4	0.3 ***	1.3	0.4 ***
Marital Status (%)					
Never Married	40.1%	40.3%	-0.2%	35.0%	5.1% ***
Married	26.2%	38.4%	-12.2% ***	31.9%	-5.7% ***
Divorced or Separated	31.0%	18.0%	13.0% ***	30.5%	0.5%
Widowed	2.7%	3.3%	-0.6%	2.6%	0.1%
Race/Ethnicity (%)					
Caucasian, Non-Hispanic	47.0%	55.3%	-8.3% ***	59.7%	-12.7% ***
African-American, Non-Hispanic	41.0%	19.9%	21.1% ***	18.6%	22.4% ***
Other	12.0%	24.9%	-12.9% ***	21.7%	-9.7% ***
Educational Attainment (%)					
Less than High School	5.5%	23.5%	-18.0% ***	16.6%	-11.1% ***
High School Diploma or GED	25.7%	39.7%	-14.0% ***	38.4%	-12.7% ***
Less than BA	57.1%	22.1%	35.0% ***	33.4%	23.7% ***
BA or more	11.5%	14.7%	-3.2% **	11.6%	-0.1%
Receive Gov't Assistance[d] (%)	42.4%	18.3%	24.1% ***	7.2%	35.2% ***
With Health Insurance (%)	58.3%	71.0%	-12.7% ***	.	.
Poverty Rate (%)	36.9%	52.4%	-15.4% ***	46.0%	-9.1% ***

[a.] Statistical significance is indicated as follows: *** = p<.0.01; ** = p<0.05; * = p<0.10.

[b.] The SCF sample is limited to those employed, 65 or younger, and with income below 150% of the poverty line.

[c.] The IPUMS sample is limited to head-of-households who reside in the Tulsa MSA, are currently employed, 65 or younger, and with income below 150% of the poverty line.

[d.] The IPUMS only indicates if an individual receives TANF, Social Security, or Supplementary Security Income.

Table A-2
Baseline Financial Characteristics:
Analysis Sample and 1998 Survey of Consumer Finances

Sample Characteristics	Analysis Sample (n=840)	1998 SCF[b] (n=1,927)	Difference[a] Analysis - SCF
Ownership Probabilities (%)			
Own Home	23.5%	34.7%	-11.2% ***
Own Business	6.8%	8.9%	-2.1% ***
Have Retirement Saving	21.1%	18.5%	2.5% ***
Have a Bank Account	85.6%	74.8%	10.8% ***
Own Car	84.2%	79.6%	4.5% ***
Average Holdings[c]			
Home Equity	$4,703	$15,447	-$10,751 ***
Business Equity	$465	$10,654	-$10,187 ***
Non-Retirement Financial Assets	$1,365	$8,659	-$7,293 ***
Retirement Account Balances	$751	$1,958	-$1,208 ***
Financial Assets	$2,116	$10,617	-$8,501 ***
Net Worth	$2,732	$43,456	-$40,721 ***

a. Statistical significance is indicated as follows: *** = p<.0.01; ** = p<0.05; * = p<0.10

b. The 1998 SCF sample is limited to those employed, 65 or younger, and with income below 150% of the poverty line.

c. Including Non-owners

Table A-3

Participant Utilization of Other CAPTC Services Between Baseline and Wave 3[a]

Sample	All (n = 840)	Renters[b] (n = 643)	Unsubsidized Renters[b] (n = 438)	Owners[b] (n = 197)
<u>During the Past 48 months, Have You Received from CAPTC…</u>				
A. Help with transportation, getting food, obtaining ID cards, or dealing with medical emergencies?				
Treated	0.114	0.119	0.092	0.097
Control	0.096	0.108	0.081	0.058
	{0.388}	{0.658}	{0.702}	{0.304}
B. Welfare-to-Work or Work First program services, including job readiness, search, or retention services?				
Treated	0.034	0.038	0.023	0.022
Control	0.028	0.037	0.023	0.000
	{0.620}	{0.969}	{0.983}	{0.134}
C. Medical services, including the medical clinic, eyeglass clinic, or help with health insurance?				
Treated	0.133	0.132	0.133	0.140
Control	0.124	0.130	0.090	0.106
	{0.676}	{0.939}	{0.158}	{0.469}
D. Child development program services, including FirstStart, HeadStart, or the School Age Program?				
Treated	0.126	0.144	0.106	0.065
Control	0.089	0.093	0.068	0.077
	{0.080}	{0.043}	{0.162}	{0.737}
E. Community Enterprise Opportunities (or CEO), including small business training and support?				
Treated	0.070	0.072	0.078	0.065
Control	0.014	0.015	0.005	0.010
	{0.001}	{0.001}	{0.001}	{0.038}
F. First-time Homebuyer's Program, including help with a downpayment and closing costs?				
Treated	0.245	0.292	0.317	0.086
Control	0.070	0.080	0.081	0.038
	{0.001}	{0.001}	{0.001}	{0.165}
G. Learning Lab, including GED, literacy, life skills, and English-as-a-second-language classes?				
Treated	0.029	0.028	0.037	0.032
Control	0.023	0.031	0.018	0.000
	{0.602}	{0.843}	{0.233}	{0.065}
H. Free tax preparation?				
Treated	0.485	0.492	0.472	0.462
Control	0.402	0.401	0.403	0.404
	{0.015}	{0.020}	{0.141}	{0.410}

[a.] P-value for significance of difference in utilization of other CAPTC services in brackets.

[b.] Defined by status in the baseline survey.