

## **What Do Individual Development Accounts Do?**

### **Evidence from a Controlled Experiment**

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## **Abstract**

This paper evaluates the first controlled field experiment on Individual Development Accounts (IDAs). Including their own contributions and matching funds, treatment group members could accumulate up to \$6,750 for home purchase or \$4,500 for other qualified uses. Almost all treatment group members opened accounts, but many withdrew the balances for unqualified purposes. For black renters at baseline, the IDA raised home ownership rates by almost 10 percentage points over 4 years, but reduced financial assets and business ownership. White renters experienced no home ownership effects, but business equity rose. Home owners used the IDA in different ways than renters.

## **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accounts (IDAs) are saving accounts that provide low-income households with matching payments when the balances are withdrawn and used for special purposes, such as home purchase, business start-up, and investment in education.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. Pioneered by Sherraden (1991), IDAs represent a new approach to helping low-income households that emphasizes both the direct and indirect benefits of accumulating assets. By encouraging saving, IDAs may be more effective than conventional cash and in-kind income-support programs in combating poverty. Even a small amount of saving could be an effective buffer against emergencies or a vehicle for overcoming borrowing constraints and for making investments that have substantial long-term positive effects on life prospects. In addition, the process of saving that IDAs emphasize may in itself promote positive changes in attitudes and behavior.

For several additional reasons, IDAs may also be more effective than conventional tax incentives in encouraging low-income households to save. Contributions to Individual Retirement Accounts (IRAs) and 401(k) plans are rewarded with tax deductions, which are less valuable to low-income households who face low marginal income tax rates than to others. In contrast, IDAs offer generous matching payments that are independent of tax rates and thus do not decline as income falls.<sup>1</sup> The financial education and encouragement to save that many IDA programs provide could also spur

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<sup>1</sup> Duflo et al. (2006) discuss the benefits of matching contributions, as opposed to tax deductions. Gale, Gruber, and Orszag (2006) propose replacing the tax-deductible contributions in 401(k)s and IRAs with a flat-rate government matching contribution.

net saving, independent of any specific subsidy.

In practice, however, the most frequent use of IDAs is to encourage renters to become home owners.<sup>2</sup> The encouragement and assistance through a potentially long and complicated home-buying process, and the emphasis on down payments as a preferred use of funds make IDAs a potentially productive way to boost home ownership. In contrast, more traditional public policies guarantee mortgage loans or subsidize mortgage interest rates, but only if a household has already been able to accumulate a sufficient down payment and navigate the home-buying process on its own.

IDAs have generated substantial attention and bi-partisan political support. By the end of 2003, more than 300 IDA programs were in operation in the United States, with more than 15,000 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.<sup>3</sup>

Despite the growth and popularity of IDAs, however, little is known about their actual effects. This paper reports the results of the first controlled field experiment of the effects of IDAs on household behavior. Indeed, despite an extensive literature on tax

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<sup>2</sup> Throughout the paper, we use the term “renters” to refer to any household head that does not own his or her own home.

<sup>3</sup> Boshara (2005) provides a concise overview of IDAs. Websites developed by the New America Foundation ([www.AssetBuilding.org](http://www.AssetBuilding.org)) and the Center for Social Development at Washington University in St. Louis (<http://gwbweb.wustl.edu/csd/>) provide comprehensive information on IDAs.

incentives for saving, this paper provides the first experimental evidence on how public policies toward saving affect behavioral measures broader than take-up of the saving incentive or contributions to the saving account.<sup>4</sup>

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 family income below 150 percent of the poverty line – 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 about 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, education, home improvement) at a 1:1 rate. For each of three years, up to \$750 in deposits was subject to match. Combining accountholders' deposits and matching funds, participants could thus accumulate sizable amounts, \$6,750 for home purchase or \$4,500 for other allowed uses.

A very high percentage (89 percent) of treatment group members opened IDAs. Given their income levels, participants who made matched withdrawals were able to accumulate significant amounts. Almost half of IDA holders, however, withdrew all of their funds for non-matchable purposes. Households who, in the baseline survey, owned their home or had more education or a bank account contributed more and were more likely to make matched withdrawals, controlling for other factors.

Our central finding is that the Tulsa IDA program had significant effects on the

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<sup>4</sup> Bernheim (2003), Engen, Gale, and Scholz (1996), and Poterba, Venti, and Wise (1996) provide summaries of the literature on tax incentives for saving. Duflo, et al. (2006) provide experimental evidence on how variations in matching rates affect participation in and contributions to Individual Retirement Accounts.

transition to home ownership among African American households who were renters in the baseline survey. For black renters in the treatment group, home ownership rates after 4 years had risen by 10 percentage points relative to controls, conditioning on observable characteristics. The results differ sharply for white renters, where the IDA generated no economically or statistically significant effect on home ownership. One way to calibrate the magnitude of these effects is to note that the IDA reduced the black-white gap in the overall four-year rate of transition to home ownership by at least 40 percent: the gap was 20 percentage points in the control group compared to 12 percentage points in the treatment group. When we control for household characteristics, the IDA closed more than 70 percent of the gap in transition to home ownership between blacks and whites. These results are consistent with prior evidence that black applicants face discrimination in lending markets and that black families are effectively “discouraged” borrowers, who do not apply for credit because they expect the application to be rejected. The findings suggest that the IDA was effective in reducing one or both of those barriers to home ownership among African Americans.

The impact on home ownership for black renters, however, was accompanied by a decline in financial assets relative to black renters in the control group, perhaps indicating the need to liquidate assets to afford down payments and housing transition costs. Black renters in the treatment group also experienced a decline in business ownership rates relative to controls. In contrast, white renters in the treatment group, who had no change in homeownership relative to controls, experienced a substantial increase in business equity relative to controls. This suggests both that whites and blacks used the program in different ways and that there could be important substitution avenues between business

start-up and first-time home purchase among low-income households.

Black renters who owned bank accounts or had higher educational attainment as of 1998 experienced larger positive treatment effects for home ownership, but also had larger (in absolute value) declines in non-retirement financial assets and business ownership, compared with black renters without bank accounts or a college experience.

Home owners at baseline used the IDA in different ways than renters. For home owners in the treatment group, the likelihood of taking non-degree classes rose sharply relative to controls. Computer purchases also rose dramatically – by 30 percentage points – relative to the control group, even though computer purchase was not a qualified use of the IDA. The IDA had no significant sample-wide effects on other qualified uses, including retirement saving, home improvement, or courses taken as part of a degree program. Nor were there discernible effects on households' poverty status or net worth, though the latter result is difficult to interpret for reasons discussed below.

The paper is organized as follows. Section II describes the experimental design. Section III compares the treatment and control groups at baseline. Section IV examines IDA contribution and withdrawal patterns. Section V describes our econometric methods. Section VI presents the effects on home ownership. Section VII discusses other treatment effects. Section VIII discusses interpretations and caveats. Section IX concludes.

## **II. Experimental Design**

The American Dream Demonstration (ADD) is a set of 14 privately funded local IDA programs initiated in the late 1990s.<sup>5</sup> By design, the Tulsa site was the one ADD

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<sup>5</sup> 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.

site to 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.<sup>6</sup>

#### A. Recruitment, Assignment, and Data Collection

Enrollment occurred between October 1998 and December 1999. Information about the IDA Matched Savings Program (CAPTC 1998) was distributed through several channels: media outreach; CAPTC’s existing social services, tax assistance, and home ownership 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. 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,

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Louis, with evaluation funding from the Ford Foundation and the Charles Stewart Mott Foundation, and with 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 examples, see Schreiner et al. (October 2002) and Sherraden et al (2005).

<sup>6</sup> 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.



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 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.<sup>7</sup>

The Wave Two survey occurred about 18 months after random assignment, between May 2000 and August 2001. For each case, an interview was 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 Three 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 Three 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.

Table 1 reports sample sizes for each of the survey waves. We define the "baseline sample" as the 1,103 randomly assigned individuals and the "analysis sample" as the 840 people who completed the month-48 survey. Retention rates were generally high and did not vary significantly between the treatment and control group. High retention rates may be due in part to extensive tracking efforts and the incentives provided. Six tracking letters were sent between the various surveys; sample members received \$10 for each letter to which they responded. In each survey, respondents received \$35 for completing the interview.

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<sup>7</sup> 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.

The difficulties of obtaining accurate data on components of net worth are well known. Unusually extensive efforts were made to ensure the accuracy of the survey data, especially for financial variables. In conjunction with the Center for Social Development, several criteria were developed to identify and verify responses that might have been misreported or misrecorded. 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. Wave 3 interviewers immediately verified values using range checks incorporated directly into the CATI/CAPI software. For other Wave 3 data values identified for verification (involving a between-wave change or within-wave inconsistency), a Survey Quality Form was administered by telephone during November 2003 or mailed to the respondent.

#### B. IDA Rules

To qualify for participation in the sample, respondents 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,<sup>8</sup> 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 after 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 year (measured from the month of account opening), up to \$750 in deposits was 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.

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 officers also had significant interactions with treatment group members, providing them with reminders and encouragement to make contributions and save toward a goal.

Careful attention was given to ensuring that treatment group members received a

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<sup>8</sup> 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.

uniform, well-described IDA program and that control group members were not allowed access to an IDA. During the experiment, control cases were restricted from participating in any other matched savings or home ownership 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 home ownership 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.

The Management Information System for Individual Development Accounts (MIS IDA), developed and supported by the CSD, provided information on the monthly IDA deposits, withdrawals, interest, and matching funds. 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.

### **III. Sample Characteristics at Baseline**

The first column of Table 2 presents economic and demographic characteristics of the analysis sample at the baseline interview. Sample members' average age was 36 years, and average monthly income was less than \$1,500. Four out of five sample members were female; about one-quarter were married, 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 68 percent had attended at least

some college, including 11 percent of the sample who graduated from a 4-year college. Despite the requirement that respondents be employed at the time of the eligibility interview, 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.

The first column of Table 3 shows wealth holdings at baseline. About 23 percent of the 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 \$4,700, business equity averaged about \$500, and average retirement account balances equaled \$751. Overall financial assets averaged \$2,116, while overall net worth averaged \$2,735.

The second, third, and fourth columns of Tables 2 and 3 show 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.

Comparing the first and fifth columns of Tables 2 and 3 shows that the IDA sample group is *not* a random sample of low-income households. Column 5 reports sample characteristics of households in the 1998 Survey of Consumer Finances (SCF) who matched the same eligibility requirements in the IDA sample – i.e., they were

employed and had income below 150 percent of the poverty line.<sup>9</sup> The two samples show roughly similar average age and income, but differ markedly in other respects. IDA sample members are more likely to be female, African-American, divorced or separated, and receiving government assistance. They are less likely to have health insurance, own a business or home, and have far lower levels of wealth than the SCF sample members. However, IDA sample members also have significantly more educational attainment and are more likely to own bank accounts than their SCF counterparts. These differences could arise from differences between the Tulsa population and the national population of low-income households or from differences between households who are interested in IDAs and those who are not. In either case, however, the differences emphasize the importance of having an explicit, randomized control group in analyzing IDA behavior and affect the extent to which the results can be generalized to broader populations.

#### **IV. IDA Activity**

Before turning to analysis of the effects of IDAs, we briefly summarize aggregate IDA patterns and the individual determinants of account utilization. Appendix Table 1 provides background information on participation, contributions, and withdrawals. 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 in which they were eligible. An account was considered closed when the balance was reduced to zero and there were no subsequent transactions. As described later, some closures represent dropouts; others represent successful program

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<sup>9</sup> In addition, because there were only 9 households in the IDA experiment of age 65 or older, we restricted the SCF sample to household heads aged less than 65.

completion. 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 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 slim majority – 54 percent – of all withdrawn funds. Among the treatment group, 37 percent made contributions but 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 48 percent of treatment group members made no matched withdrawals.

Average matched and unmatched withdrawals 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.<sup>10</sup>

The timing of IDA activity is also of interest. Contributions peaked sharply 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.<sup>11</sup> Matched withdrawals peaked in May, just after the spike in deposits. Unmatched

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<sup>10</sup> 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.

<sup>11</sup> See Sherraden (2002). Smeeding (2002) and Smeeding, Phillips, and O’Conner (2004) discuss and provide evidence on potential interactions between the Earned Income Tax Credit and IDAs.

withdrawals were made at a relatively steady rate throughout the year.

Table 4 provides regression analysis of IDA utilization patterns. Participation 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 significant effect of age is the much higher likelihood of unmatched withdrawals among households with heads younger than 30. Higher levels of household income tend to raise contributions but have no significant impact on participation or type of withdrawal.

More effective use of the IDA – including higher participation rates, higher contribution levels, and (especially) higher probabilities of making matched withdrawals – was 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 levels, receipt of government assistance, health insurance coverage, and car ownership did not influence IDA behavior.

Demographic characteristics also affected utilization. Relative to other groups, blacks participated and 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 made fewer matched withdrawals than earlier cohorts. This is consistent with the view that eager savers signed up first and that the difficulty of recruiting sample members rose over time.

## V. Methodology

We estimate the effect of being eligible for an IDA; that is, we provide “intent to treat” (ITT) estimates.<sup>12</sup> For continuous measures of household behavior, we estimate ordinary least squares equations of the form:

$$(1) \quad Y_{3i} = \beta_0 + \beta_1 X_i + \beta_2 Y_{1i} + \beta_3 Z_i + \beta_4 T_i + \varepsilon_i,$$

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,  $X_i$  is a vector of baseline demographic and economic characteristics,  $Z_i$  is a vector of baseline covariates discussed further below,  $T_i$  takes the value of 1 for treatment group members and zero otherwise, the  $\beta$ 's are parameters, and  $\varepsilon$  is the individual-specific error term.<sup>13</sup> The estimated treatment effect is given by  $\beta_4$ .

For dichotomous outcomes (e.g., homeownership), we estimate probit models:

$$(2) \quad \Pr(Y_{3i} = 1) = \Phi(\beta_0 + \beta_1 X_i + \beta_2 Y_{1i} + \beta_3 Z_i + \beta_4 T_i),$$

where  $\Phi$  is the cumulative standard normal distribution.

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<sup>12</sup> 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, this formula should probably be viewed as an upper bound for the TOT effect, since the financial education classes and encouragement to save that all treatment group members received could have had a favorable effect on behavior, even among people who did not open an IDA during the evaluation.

<sup>13</sup> As noted above, the ratio of treatment group members to control group members was 5:6 before March 1999 and 1:1 afterwards. The regression results weigh the observations so that the weighted populations have a 1:1 ratio of treatment to control group members in each month of random assignment.

We condition on a variety of covariates in order to improve the precision of the estimated treatment effects. The covariates in  $X$  include indicator 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); 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); race/ethnicity (Black non-Hispanic, other non-Caucasian, with Caucasian non-Hispanic omitted); ownership of a bank account and a home at baseline; 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).<sup>14</sup>

Although the treatment and control groups are comparable at baseline (Tables 2 and 3), random assignment and differential attrition may nevertheless have affected the composition of the analysis sample. Accordingly, we estimate probits testing whether any of an extensive list of baseline characteristics were correlated with either treatment group status or attrition by the Wave-3 interview. To control for any effects of these imbalances on the estimation of program impacts, we include in the vector  $Z$  in (1) and (2) all of the variables not already included in  $X$  that had a statistically significant coefficient in the probits.<sup>15</sup> To be sure, some sample imbalances are present to some degree in any randomized experiment and do not in themselves indicate a failure of

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<sup>14</sup> Relative to the variables listed in Table 4, we omit controls for net worth, government assistance, health insurance, and car ownership, since these did not generally influence IDA contributions. In specifications that include only one racial group, all of the race-related variables are dropped. Likewise, in specifications that include only owners or only renters, we drop the indicator variable for home ownership in wave 1.

<sup>15</sup> The vector  $Z$  includes controls for property ownership, number of adults in household, “success in carrying out plans,” “hard to make ends meet,” “thought about getting additional education,” “gave food or loaned a tool,” “can afford leisure activities,” “last month was a typical month for income,” “financial situation has gotten worse,” any income from child support, any income from alimony, any overdue rent, any educational debt, any business debt, retirement savings, and liquid assets.

random assignment or a problem due to differential attrition.

## **VI. Effects on Home Ownership**

We focus first on the effect of the IDA on encouraging home ownership. Helping low-income individuals transition into home ownership is a central goal of most IDA programs, and the Tulsa IDA program provided its highest matching rate for down payments on primary residences. In addition, analysis of binary outcomes helps sidestep some measurement and sampling variation issues that arise with continuous and broader measures of wealth, a relevant consideration given that the analysis sample consists of only 840 households.

Table 5 summarizes the key home ownership results. Home ownership 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 and 35 percent for each group, and then grew further, to 46 percent and 43 percent, respectively, by the month-48 survey. Sample members were clearly highly motivated to buy homes. Home ownership rose by 23 percentage points in the treatment group between Waves 1 and 3; even among controls, the rate rose by more than 18 percentage points. However, the net effect on home ownership, given by the difference-in-difference estimate, is just 4.6 percentage points and insignificantly different from zero.

Not surprisingly, the effects on home ownership are more sharply defined for the overall sample of renters at baseline. Home ownership rates rose by a statistically significant 7 percentage points among renters in the treatment group relative to controls over the 48-month evaluation period, with all of the increase occurring between the

second and third surveys.<sup>16</sup>

In light of well-known differences in home ownership rates across racial groups and possible discrimination in housing markets, we decompose the results for renters by race. The IDA program had a substantial effect on black renters but not on white renters. Home ownership rates rose by more than 12 percentage points ( $p < .05$ ) for black renters in the treatment group relative to controls. In contrast, for white renters, the difference between the treatment and control groups was small and insignificant.

The last panel of Table 5 reports probit estimates that condition on the X and Z vectors. The estimated treatment effects are generally similar to, but smaller than, the raw difference-in-difference estimates.<sup>17</sup> The treatment effect on homeownership in the overall sample is economically small and statistically insignificant. The point estimate of the treatment effect among renters is somewhat larger, almost 6 percentage points, but is also not statistically significant. Among black renters, however, the IDA raises home ownership rates by almost 10 percentage points ( $p < .06$ ), controlling for other factors. In contrast, among white renters, the effect is tiny and statistically insignificant.

The difference across races in IDA treatment effects on the transition to home ownership is both striking and significant. Gaps in home ownership rates between black and white households in the United States have proven large, persistent, and difficult to explain fully with observable characteristics.<sup>18</sup> The unexplained gap is consistent with the possibility of racial discrimination by lenders and with the presence of overly

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<sup>16</sup> Among home owners at baseline, home ownership rates actually fell somewhat among treatment group members relative to controls over time, but the difference was not statistically significant.

<sup>17</sup> Estimates of the treatment effects in linear probability models are very close to the probit estimates.

<sup>18</sup> See Abt Associates (2005), Charles and Hurst (2002) Collins and Margo (2001), Gabriel and Rosenthal (2005), and the cites therein.

“discouraged” black borrowers, who do not apply for mortgages because they think they will be rejected. In principle, the existence of IDA balances and matching funds could prove sufficient to overcome prejudice on the part of lenders or to give black households increased confidence to apply for a loan.

In practice, the Tulsa IDA appears to have helped close the gap in rates of transition to home ownership among sample members of different racial groups. Among the control group, roughly 37 percent of white renters and 17 percent of black renters transitioned to home ownership over the four-year period (Table 5).<sup>19</sup> In contrast, in the treatment group, 41 percent of white renters transitioned to homeownership compared to 29 percent of black renters. Thus, based on the summary data, the Tulsa IDA program reduced the gap in transition to home ownership between blacks and whites from 20.6 percentage points in the control group to 11.8 percentage points in the treatment group, a reduction of 43 percent.

Table 6 reports regressions of home ownership at month 48 on race and other household characteristics. The first two columns of Table 6 control only for race and a constant and thus mirror the raw data almost exactly. In the control group, black renters at baseline are 19 percentage points less likely to own a home at month 48; in the treatment group, the figure falls to 11 percentage points. The second pair of columns shows even stronger effects of the IDA in regressions that control for the full battery of characteristics in X and Z. Controlling for household characteristics has a very small effect on the racial gap in transition to home ownership in the control group, reducing the

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<sup>19</sup> These results are consistent with Charles and Hurst (2002), who use data from the Panel Survey of Income Dynamics from 1991 to 1996 and find that 30 percent of white households and 12 percent of black households transitioned into homeownership. The transition rates in the Tulsa IDA were somewhat higher, since the IDA naturally attracted motivated home buyers.

gap from 19.3 percentage points to 17.5 percentage points. In contrast, controlling for household characteristics has a big effect on the gap in the treatment group. The point estimate for the unexplained gap falls to less than 5 percentage points (and is not significantly different from zero), suggesting that the treatment closes the gap in home ownership transition by more than 70 percent.

## **VII. Other Treatment Effects**

Table 7 reports a variety of additional treatment effects that help to clarify and qualify the differing effects of the IDA program on black and white renters. Not surprisingly, black renters in the treatment group showed economically and statistically significant increases in home equity relative to the control group. The increase of more than \$4,000 is substantial. Relative to controls, however, black renters in the treatment group also experienced *declines* in liquid financial assets and in business ownership. At \$1,348, the decline in liquid assets is almost one third of the increase in home equity. The decline in business ownership – 3.5 percentage points – is 35 percent as large as the increase in home ownership rates. These declines are consistent with scenarios in which purchasers of homes use some of their existing assets (in addition to the IDA balance and the matching funds) to make a down payment and to finance the transition to a new home (moving costs, furniture purchases, etc.), and in which households, given the 2:1 match for down payments compared to the 1:1 match for business start-up, choose to redirect toward housing some of the resources that otherwise would have gone to business creation. Thus, the favorable effect of the IDA on home ownership among black renters

is not an unqualified success. It appears to have left the new home owners with fewer liquid assets and with fewer businesses than they otherwise would have owned.

The pattern of treatment effects for white renters, for whom the IDA had no effect on home ownership, was very different. White renters in the treatment group had no increase in home equity and no (significant) decline in financial assets, but they did experience an increase in business equity of \$1,747 ( $p=.04$ ), relative to controls. This effect is substantial relative to the average IDA contributions noted above. This suggests successful use of the IDA, and again suggests a trade-off between business start-up and home purchase among IDA participants.

Given the primary focus of encouraging home ownership in the Tulsa IDA program, our analysis focuses primarily on how renters responded. As shown in Table 7, however, treatment effects for home owners at baseline were different from either renter group. Homeowners in the treatment group did not have significant increases in home equity or liquid assets, but they did experience a very large – 17 percentage point – increase in the likelihood of taking a non-degree course, which was a matchable use of IDA withdrawals. Interestingly, home owners in the treatment group also experienced a massive increase in computer ownership rates – 30 percentage points – relative to controls, even though computer purchase was *not* a matchable use of IDA funds.

As shown in the table, the IDA had no significant treatment effects for any of the groups on several other subsidized uses, including the likelihood of having a retirement saving account, undertaking home improvements, or enrolling in degree-related courses. Nor did the IDA have any effect on the share of the households in poverty (not shown).

For completeness, we also show that treatment effects for net worth are positive but not statistically significant. The information value of this result should be kept in perspective. First, even an estimated negative effect of IDAs on net worth would not be a clear-cut indicator that the program failed, since some successful uses of the IDA could reduce measured net worth in the short run.<sup>20</sup> Purchasing a home, for example, often generates costs associated with settlement, moving, and new appliances or furniture, all of which serves to reduce measured net worth. Enrolling in classes raises human capital, but the tuition and other expenses reduce measured net worth, too. Second, 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, make it impossible to distinguish between the views that all or none of the contributions are net additions to net worth.<sup>21</sup> As a result, although it is interesting in principle to examine the effects of the IDA on net worth, it turns out not to be very informative in practice, at least for the sample and the IDA program in question.

To provide information on which types of households are most likely to benefit from an IDA and to examine the robustness of the findings in table 7, table 8 provides more finely-grained estimates of treatment effects among black renters. Earlier results (table 4) show that households with bank accounts and with more education were more likely to use IDAs effectively. Table 8 shows that black renters with bank accounts at baseline account for all of the treatment effect on home ownership. The IDA raised their

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<sup>20</sup> In contrast, in evaluating traditional tax-based saving incentives, the effect on household net worth is a critical determinant of the overall impact of the program (Engen, Gale, and Scholz 1996, Poterba, Venti, and Wise 1996).

<sup>21</sup> Similar problems arose in quantile and robust regressions and under different methods of trimming the data set to deal with outliers.



home ownership rate by 14.6 percentage points (in probit analysis, 12.3 percentage points in a linear probability model). These households also had larger increases in home equity and larger declines in non-retirement financial assets and business ownership than the overall group of black renters shown in Table 5. Black renters without bank accounts at baseline had starkly different patterns: no positive treatment effects for home ownership, a decline in home equity, and an increase in non-retirement financial assets. Differences across education groups are broadly similar but not quite as stark. Black renters with higher educational attainment experienced treatment effects that were very similar to those with bank accounts. Black renters with less educational attainment did not show statistically significant effects on home ownership or home equity and had an increase in business ownership rates.

The results in Table 8 provide support for the interpretation of the basic findings given above by showing that the subset of black renters who increased home ownership were also the ones who reduced their financial assets and their business ownership rates; and that the groups who did not raise home ownership had different patterns for home equity, financial assets and business ownership. The results also highlight the potential role that sophistication or comfort with financial institutions or education more generally may play in facilitating respondents' use, and the policy effectiveness, of IDAs.

## **VIII. 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 arises because the Tulsa IDA matched down payments made during a four-year period 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 at the end of the program 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 stems from the program requirement that control group members not participate in other home ownership programs at CAPTC during the evaluation. This rule implies that the home ownership 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.

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.) While 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, several factors suggest that the timing incentives did not play a dominant role. First, it is clear that the timing incentives did not affect home purchases of white renters since there was no treatment effect on home ownership for that group to begin with. Second, if white renters in the control group were saving money to make a down payment after the experiment ended, at which time they would be eligible

for other home ownership subsidies, there should have been a decline in financial assets for white renters in the treatment group relative to controls. Table 7 shows that no significant decline in financial assets occurred for this group. It is possible, of course, that timing incentives could affect black renters even though they did not affect white renters; we do not have a way of assessing that issue.

Third, if the effects on home ownership were accelerations of future purchases, rather than purchases that otherwise would never have occurred, it would be plausible that the effects would occur most strongly for younger households. In 2003, nation-wide home ownership rates for black households were 12 percent for households with heads between the ages of 20 and 24, rose to 52 percent for households aged 40-49, and then peaked at 66 percent among households older than 65.<sup>22</sup> If the IDA program simply accelerated the age at which individuals become home owners, we would expect intuitively to see the biggest effects in younger groups where home ownership rates rise rapidly with age. That is, if home ownership rates are relatively flat among older groups, there are fewer future first-time home purchases for older renters to accelerate to the present. Instead, the data show the largest treatment effects in the older age groups.<sup>23</sup> Any lingering questions about the importance of the timing incentives could best be addressed by a longer-term follow-up survey of the treatment and control groups.

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

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<sup>22</sup> Authors' calculations from the American Housing Survey, 2003. See also Gale, Gruber, Orszag, and Stephens-Davidowitz (2006).

<sup>23</sup> Specifically, in a linear probability model using the sample of black renters age 40 and older, the treatment effect for home ownership was 18 percentage points ( $p = .025$ ). Using the sample of black renters under the age of 40, the treatment effect was 6.7 percentage points and not statistically different from zero ( $p = .245$ ). Probits could not be used because certain variables predicted failure perfectly.

underlying home ownership rate was rising. Down payment subsidies may be most effective in places and times where down payment constraints are more binding, in 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 implies that the results apply to the sample of households who are likely to want to apply for an IDA, but certainly not to the whole low-income population. Thus, for example, IDAs might be expected to reduce the gap in transition to home ownership by race by the amounts indicated above among households who are interested in applying for an IDA. But the impact on the transition to home ownership by race among all low-income households would be smaller, since many or most low-income households would not apply for the IDA in the first place, as suggested by the difficulties in attracting recruits to the program to begin with.<sup>24</sup>

## **IX. Conclusion**

This paper presents the first experimental evidence on how Individual Development Accounts affect economic behavior. The effects of the IDA program

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<sup>24</sup> An additional issue is that, despite continual efforts by program staff to prohibit such behavior, 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.

differed across groups. Black renters at baseline transitioned to home ownership, to some extent at the expense of lower financial assets and fewer business start-ups. White renters increased business equity. Home owners enrolled in classes and purchased computers. Households with more formal education and who owned bank accounts at the time of the baseline survey made more extensive use of the IDA option and used the IDA balances in different ways than other households. The results raise two broader issues about the efficacy of IDAs that provide an important framework for future research.

First, how do IDAs work? 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. 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 encouragement and attention from program staff.

Second, how do the costs and benefits of IDAs stack up against other policy options? Experimental evaluation of the benefits of IDAs compared to other programs has not yet begun. Comparison of the costs of IDAs and other programs is equally difficult but imperative. Schreiner, Ng, and Sherraden (2002) estimate the costs of running the Tulsa IDA program at \$595,000, excluding the matching funds. Total matchable contributions were about \$457,000, with – as noted earlier – more than half of

these funds withdrawn for unqualified uses. 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.

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**Table 1**  
**Sample Size by Treatment Status and Survey Wave**

<b>Sample Group</b>	<b>Wave 1 Sample Size</b>	<b>Completed Interviews</b>			<b>Completion Rate<sup>a</sup></b>
		<b>Telephone</b>	<b>Field</b>	<b>Total</b>	
<b><u>Month 18 (Wave Two) Survey</u></b>					
Treatment Group	537	407	55	462	86.0%
Control Group	566	403	68	471	83.2%
<b>Total</b>	<b>1,103</b>	<b>810</b>	<b>123</b>	<b>933</b>	<b>84.6%</b>
<b><u>Month 48 (Wave Three) Survey</u></b>					
Treatment Group	537	384	28	412	76.7%
Control Group	566	381	47	428	75.6%
<b>Total</b>	<b>1,103</b>	<b>765</b>	<b>75</b>	<b>840</b>	<b>76.2%</b>

<sup>a</sup> Total completed interviews (fourth column) as a percentage of corresponding total sample (first column).

**Table 2**  
**Baseline Demographic and Economic Characteristics: Analysis Sample and 1998 Survey of Consumer Finances (SCF)**

Sample Characteristics	Analysis Sample (n=840)	Treatment Group (n=412)	Control Group (n=428)	Difference <sup>a</sup> Treatment-Control	1998 SCF <sup>b</sup> (n=1,927)	Difference <sup>a</sup> Analysis Sample - SCF
Age	36.3	36.3	36.3	0.0	35.5	0.8 ***
Monthly Household Income	\$1,453	\$1,488	\$1,418	\$70	\$1,120	\$334 ***
Female (%)	80.0%	79.0%	81.0%	-2.1%	38.9%	41.1% ***
Number of Children in Household	1.7	1.7	1.6	0.1	1.4	0.3 ***
<b>Marital Status (%)</b>						
Never Married	39.9%	35.7%	44.3%	-8.6% ***	40.3%	-0.4% ***
Married	26.2%	28.3%	24.1%	4.1%	38.4%	-12.2% ***
Divorced or Separated	31.1%	33.4%	28.8%	4.6%	18.0%	13.1% ***
Widowed	2.7%	2.7%	2.8%	-0.1%	3.3%	-0.6% ***
<b>Race/Ethnicity (%)</b>						
Caucasian, Non-Hispanic	47.0%	45.0%	49.0%	-4.0%	55.3%	-8.3% ***
African-American, Non-Hispanic	40.9%	42.8%	39.0%	3.8%	19.9%	21.1% ***
Other	12.1%	12.2%	12.0%	0.2%	24.9%	-12.8% ***
<b>Educational Attainment (%)</b>						
Less than High School	5.5%	6.3%	4.7%	1.7%	23.5%	-18.0% ***
High School Diploma or GED	25.8%	25.1%	26.5%	-1.3%	39.7%	-13.9% ***
Less than BA	57.1%	56.4%	57.7%	-1.3%	22.1%	35.0% ***
BA or more	11.5%	12.1%	10.9%	1.2%	14.7%	-3.2% ***
Receive Government Assistance (%)	42.5%	42.9%	42.1%	0.8%	18.3%	24.2% ***
With Health Insurance (%)	58.1%	58.8%	57.5%	1.3%	71.0%	-12.9% ***

<sup>a</sup> Statistical significance is indicated as follows: \*\*\* = p<.001; \*\* = p<0.05; \* = p<0.10.

<sup>b</sup> The SCF sample is limited to those who are employed, 65 or younger, and with income below 150% of the poverty line.

**Table 3**  
**Baseline Financial Status: Analysis Sample and 1998 Survey of Consumer Finances (SCF)**

<b>Sample Characteristics</b>	<b>Analysis Sample (n=840)</b>	<b>Treatment Group (n=412)</b>	<b>Control Group (n=428)</b>	<b>Difference<sup>a</sup> Treatment-Control</b>	<b>1998 SCF<sup>b</sup> (n=1,927)</b>	<b>Difference<sup>a</sup> Analysis Sample - SCF</b>
<b>Ownership Probabilities (%)</b>						
Own Home	23.4%	22.5%	24.3%	-1.8%	34.7%	-11.2% ***
Own Business	6.8%	7.7%	5.9%	1.8%	8.9%	-2.1% ***
Have Retirement Saving	21.0%	23.0%	19.1%	3.9%	18.5%	2.5% ***
Have Checking or Saving Account	85.6%	88.5%	82.7%	5.9% **	74.8%	10.8% ***
Own Car	84.1%	84.3%	83.9%	0.4%	79.6%	4.5% ***
<b>Average Holdings<sup>c</sup></b>						
Home Equity	\$4,696	\$4,208	\$5,195	-\$987	\$15,447	-\$10,751 ***
Business Equity	\$467	\$385	\$551	-\$166	\$10,654	-\$10,187 ***
Non-Retirement Financial Assets	\$1,366	\$1,256	\$1,478	-\$222	\$8,659	-\$7,293 ***
Retirement Account Balances	\$751	\$934	\$563	\$372 *	\$1,958	-\$1,208 ***
Financial Assets	\$2,116	\$2,190	\$2,041	\$149	\$10,617	-\$8,501 ***
Net Worth	\$2,735	\$2,090	\$3,394	-\$1,304	\$43,456	-\$40,721 ***

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

<sup>b</sup> The SCF sample is limited to those who are employed, 65 or younger, and with income below 150% of the poverty line.

<sup>c</sup> Including non-owners.

**Table 4**  
**Determinants of IDA Participation, Contributions, and Withdrawals**

Baseline Characteristic	Prob. Contribute if in Treatment (Probit)		Cumulative Matchable Contributions by Program End if in Treatment (Tobit)		Prob. Matched Withdrawal if Contributed (Probit)		Prob. Unmatched Withdrawal if Contributed (Probit)	
	dF/dx <sup>a</sup>	Rob.SE	Coef.	SE	dF/dx	Rob.SE	dF/dx	Rob.SE
	Age 30-39	-0.011	0.030	-145	124	-0.104	0.077	-0.076
Age 40-49	0.083	0.025 ***	97	139	0.000	0.087	-0.103	0.059 *
Age 50 +	-0.030	0.061	-32	203	0.110	0.124	-0.229	0.105 **
Annual Income: 10k-20k	0.030	0.033	179	137	0.036	0.087	-0.050	0.043
Annual Income: 20k-30k	0.039	0.033	311	163 *	0.049	0.102	-0.032	0.058
Annual Income: 30k +	0.047	0.031	367	198 *	0.071	0.119	-0.076	0.082
Some college	0.073	0.030 **	223	109 **	0.077	0.066	-0.020	0.034
4-year degree or more	0.046	0.027 *	769	170 ***	0.249	0.096 ***	0.012	0.047
Have bank account	-0.001	0.039	402	154 ***	0.247	0.082 ***	-0.027	0.046
Own home	0.054	0.028 *	447	138 ***	0.274	0.080 ***	0.007	0.042
Net worth (in \$10,000s)	0.002	0.009	36	26	0.020	0.015	-0.015	0.009 *
On government assistance	-0.020	0.026	-112	103	0.002	0.065	0.011	0.031
Have health insurance	-0.023	0.026	-158	100	-0.090	0.061	0.023	0.032
Own car	0.001	0.031	160	135	0.117	0.079	0.002	0.042
Married	-0.046	0.047	44	143	0.050	0.086	0.018	0.041
Divorced	-0.063	0.037 *	53	124	0.212	0.075 ***	0.000	0.039
Have children	-0.039	0.028	-406	141 ***	-0.145	0.083 *	0.052	0.047
Female	-0.021	0.033	67	137	-0.168	0.082 **	0.048	0.048
Black	-0.057	0.031 *	-231	109 **	-0.145	0.062 **	0.060	0.031 *
Other non-white	-0.061	0.051	3	156	-0.068	0.095	0.031	0.042
Cohort 4-6	-0.102	0.059 *	-615	155 ***	-0.241	0.085 ***	-0.054	0.067
Cohort 7-9	-0.001	0.045	-348	163 **	-0.124	0.097	-0.171	0.080 **
Cohort 10-12	0.004	0.041	-430	150 ***	-0.165	0.088	-0.120	0.072 *
Cohort 13 +	-0.070	0.060	-525	165 ***	-0.217	0.090 **	-0.184	0.089 **

a. dF/dx is for discrete change of dummy variable from 0 to 1  
Statistical significance is indicated as follows: \*\*\* = p<.0.01; \*\* = p<0.05; \* = p<0.10.  
Data are weighted.

**Table 5**  
**Effects on the Transition to Home Ownership**

Sample	All (n = 840)		Renters <sup>a</sup> (n = 643)		Black Renters <sup>a</sup> (n = 292)		White Renters <sup>a</sup> (n = 273)	
<u>Homeownership Rates</u>	<u>Mean</u>	<u>St. Error</u>	<u>Mean</u>	<u>St. Error</u>	<u>Mean</u>	<u>St. Error</u>	<u>Mean</u>	<u>St. Error</u>
<b>Wave 1</b>								
Treatment	0.225	0.021 ***	0	0	0	0	0	0
Control	0.243	0.021 ***	0	0	0	0	0	0
T-C Difference	-0.018	0.029	0	0	0	0	0	0
<b>Wave 2</b>								
Treatment	0.343	0.025 ***	0.165	0.022 ***	0.132	0.029 ***	0.221	0.039 ***
Control	0.349	0.024 ***	0.166	0.022 ***	0.143	0.031 ***	0.180	0.033 ***
T-C Difference	-0.006	0.034	-0.001	0.031	-0.012	0.043	0.041	0.051
<b>Wave 3</b>								
Treatment	0.457	0.025 ***	0.349	0.027 ***	0.291	0.037 ***	0.409	0.044 ***
Control	0.429	0.024 ***	0.278	0.025 ***	0.168	0.032 ***	0.374	0.040 ***
T-C Difference	0.028	0.034	0.07	0.037 *	0.123	0.049 **	0.036	0.059
<b>Wave 3 - Wave 1</b>								
Treatment	0.231	0.025 ***	0.349	0.027 ***	0.291	0.037 ***	0.409	0.044 ***
Control	0.185	0.022 ***	0.278	0.025 ***	0.168	0.032 ***	0.374	0.040 ***
T-C Difference	<b>0.046</b>	<b>0.033</b>	<b>0.070</b>	<b>0.037 *</b>	<b>0.123</b>	<b>0.049 **</b>	<b>0.036</b>	<b>0.059</b>
<b>Estimated Treatment Effects (Probit)</b>								
	<u>dF/dX<sup>b</sup></u>	<u>Robust St. Error</u>	<u>dF/dX</u>	<u>Robust St. Error</u>	<u>dF/dX</u>	<u>Robust St. Error</u>	<u>dF/dX</u>	<u>Robust St. Error</u>
Controls = X (except home1)	0.010	0.036	0.053	0.038	0.087	0.049 *	0.001	0.062
Controls = X, Z (except home1)	0.030	0.038	0.057	0.039	0.097	0.051 *	0.007	0.066
Controls = X	0.021	0.038						
Controls = X, Z	0.035	0.040						

a. Defined by status in the baseline survey.  
b. dF/dx is for discrete change of dummy variable from 0 to 1.  
Statistical significance is indicated as follows: \*\*\* = p<.0.01; \*\* = p<.0.05; \* = p<.0.10.  
Data are weighted.

**Table 6**  
**Percent of Race-Specific Home Ownership Gap Eliminated by the Treatment Effect**

<b>Sample</b>	<b>No Controls</b>		<b>Controls for X, Z</b>	
	<b>dF/dx<sup>a</sup></b>	<b>Robust SE</b>	<b>dF/dX</b>	<b>Robust SE</b>
All renters, control group	-0.193	0.048 ***	-0.175	0.053 ***
All renters, treatment group	-0.111	0.053 **	-0.049	0.061
Percent of gap eliminated by treatment <sup>b</sup>	42.5		72.1	
Black and white renters, control group	-0.206	0.051 ***	-0.191	0.058 ***
Black and white renters, treatment group	-0.119	0.057 **	-0.046	0.068
Percent of gap eliminated by treatment <sup>c</sup>	42.4		75.9	

a. dF/dx is for discrete change of African American indicator from 0 to 1.

b.  $(1 - \text{Row 2}/\text{Row 1}) * 100$

c.  $(1 - \text{Row 5}/\text{Row 4}) * 100$

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

Data are weighted.

**Table 7**  
**Other Treatment Effects<sup>a</sup>**

<b>Outcome</b>	<b>All</b>		<b>Renters<sup>b</sup></b>		<b>Black Renters<sup>b</sup></b>		<b>White Renters<sup>b</sup></b>		<b>Owners<sup>b</sup></b>	
	<b>Coef.<sup>c</sup></b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>
Home equity	1443	1472	1386	1292	4073	1609 **	-251	2250	4476	4605
Non-retirement financial assets	-2365	1509	-712	400 *	-1348	700 *	-215	533	-3218	5629
Own Business (0,1)	-0.005	0.018	-0.015	0.018	-0.035	0.019 *	0.020	0.029	0.007	0.009
Business equity	1207	1686	-833	1181	-2336	1931	1747	872 **	2049	4676
Non-degree course (0,1)	0.060	0.030 **	0.037	0.036	0.068	0.052	0.010	0.054	0.169	0.056 ***
Degree course (0,1)	0.007	0.038	-0.002	0.045	-0.001	0.071	0.024	0.068	0.041	0.070
Computer Purchase (0,1)	0.071	0.034 **	0.021	0.040	0.014	0.066	0.000	0.055	0.297	0.069 ***
Have Retirement Saving (0,1)	-0.015	0.037	0.002	0.042	-0.057	0.061	0.037	0.073	-0.059	0.099
Retirement Saving Balance	425	339	132	241	-69	297	74	435	1553	1275
Home improvement (0,1)	0.028	0.038	0.042	0.036	-0.012	0.036	0.078	0.062	-0.065	0.067
Net worth	2118	3565	2222	2569	1673	3347	3977	4590	10226	11711

a. OLS for continuous and Probit for dichotomous variables. The regressions control for X and Z as discussed in the text.

b. Defined by status in the baseline survey.

c. For Probit estimates, reported value is dF/dx for discrete change of dummy variable from 0 to 1.

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

Data are weighted.

**Table 8**  
**Other Treatment Effects for Black Renters<sup>a</sup>**

<b>Outcome</b>		<b>Black Renters with Bank Account<sup>b</sup> (n = 240)</b>		<b>Black Renters without Bank Account<sup>b</sup> (n = 52)</b>		<b>Black Renters with Some College or More<sup>b</sup> (n = 209)</b>		<b>Black Renters with High School or Less<sup>b</sup> (n = 83)</b>	
		<b>Coef.<sup>c</sup></b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>	<b>Coef.</b>	<b>St.Error</b>
<b>Home Ownership (0, 1)</b>	<b>Probit</b>	0.146	0.059 **	†		0.100	0.057 *	0.073	0.062
	<b>OLS</b>	0.123	0.056 **	-0.111	0.085	0.099	0.056 *	0.091	0.086
<b>Home Equity</b>	<b>OLS</b>	5476	1948 ***	-2813	1576 *	4408	2089 **	2279	2205
<b>Non-retirement Financial Assets</b>	<b>OLS</b>	-1874	825 **	1193	571 **	-775	600	-1996	1181 *
<b>Business Ownership (0, 1)</b>	<b>Probit</b>	-0.052	0.030 *	†		-0.051	0.029 *	†	
	<b>OLS</b>	-0.046	0.032	-0.030	0.029	-0.054	0.033 *	0.066	0.028 **

a. For dichotomous (0,1) variables, the first estimate is Probit and the second is OLS; OLS for continuous variables. The regressions control for X and Z as discussed in the text.

b. Defined by status in the baseline survey.

c. For Probit estimates, reported value is dF/dx for discrete change of dummy variable from 0 to 1.

†. Outcome predicts data perfectly.

Statistical significance is indicated as follows: \*\*\* = p<.001; \*\* = p<.05; \* = p<.10.

Data are weighted.



**Appendix Table 1**  
**Distribution of IDA Contributions and Withdrawals**

<b>Sample</b>	<b>Sample Size</b>	<b>Percent of Treatment Group</b>	<b>Percent of Total Matchable Contributions</b>	<b>Percent of Total Withdrawals</b>	<b>Percent of Total Matched Withdrawals</b>	<b>Percent of Total Unmatched Withdrawals</b>	
<b>Did not contribute</b>	46	11.2	0	0	0	0	
<b>Contributed, no remaining balance</b>	288	69.9	74.6	76.9	82.1	72.4	
<b>Matched withdrawal only</b>	28	6.8	12.2	10.6	22.8	0	
<b>Unmatched withdrawal only</b>	159	38.6	22.1	22.5	0	41.8	
<b>Both matched and unmatched withdrawal</b>	101	24.5	40.3	43.8	59.2	30.6	
<b>Contributed, remaining balance</b>	78	18.9	25.4	23.1	17.9	27.6	
<b>Matched withdrawal only</b>	8	1.9	3.1	2.2	4.7	0	
<b>Unmatched withdrawal only</b>	33	8.0	9.1	9.3	0	17.4	
<b>Both matched and unmatched withdrawal</b>	26	6.3	11.0	11.6	13.3	10.2	
<b>Neither matched nor unmatched withdrawal</b>	11	2.7	2.2	0	0	0	
			<b>Average Cumulative Matchable Contribution</b>	<b>Average Withdrawal</b>	<b>Average Matched Withdrawal</b>	<b>Average Unmatched Withdrawal</b>	<b>Average Remaining Matchable Balance</b>
<b>Contributed, no remaining balance</b>	288	1185	274	645	176	0	
<b>Matched withdrawal only</b>	28	1994	828	828	0	0	
<b>Unmatched withdrawal only</b>	159	635	151	0	151	0	
<b>Both matched and unmatched withdrawal</b>	101	1827	367	594	225	0	
<b>Contributed, remaining balance</b>	78	1489	333	598	266	433	
<b>Matched withdrawal only</b>	8	1758	766	766	0	338	
<b>Unmatched withdrawal only</b>	33	1259	267	0	267	421	
<b>Both matched and unmatched withdrawal</b>	26	1934	367	555	266	270	
<b>Neither matched nor unmatched withdrawal</b>	11	921	0	0	0	932	

Data are weighted.