# Effects of Individual Development Accounts on Household Saving Behavior:

## Evidence from a Controlled Experiment


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**Abstract**

This paper evaluates the first controlled social experiment on the effects of saving incentives in general and Individual Development Accounts (IDAs) in particular. We evaluate an IDA program administered in Tulsa, Oklahoma, as part of the American Dream Demonstration. Applicants randomly assigned to the treatment group could open IDAs; those assigned to the control group could not. Both groups were interviewed prior to random assignment and 18 months and 48 months later. Participants making full use of IDAs could accumulate $6,750 for home purchase or $4,500 for other allowed uses.

A very high percentage (85 percent) of treatment group members opened IDAs. Given their income levels, participants who made matched withdrawals were able to accumulate significant amounts -- more than $3,400 on average, including the match. But most accountholders withdrew all of their IDA funds for non-matchable purposes.

In the overall sample, the program had strong effects on homeownership, but almost no effect on accumulation of any other matched asset or financial wealth. Among African-Americans, the program increased the purchase of homes and the accumulation of retirement assets. But groups that were relatively disadvantaged in the baseline survey did not appear to have benefited much if at all from the IDA. This includes households with financial assets below $200, those without a bank account, and those on public assistance. Interpretation of these results requires care because of several special features of IDAs in general and this particular IDA demonstration in particular.

## I. Introduction

Tax incentives for private saving – including 401(k)-type plans and Individual Retirement Accounts – have become an increasingly prominent feature of public policy in the United States and other countries. Although most tax incentives for saving accrue to middle- and high-income households, other programs aim to provide special incentives to lower-income households for saving.

Individual development accounts (IDAs), for example, are saving accounts that receive matching contributions and are targeted to low-income households and for special purposes, such as retirement saving or home purchase (Sherraden 1991). Although IDAs differ considerably in certain design details from 401(k) plans or IRAs, the programs share key features, including the presence of a higher-than-otherwise-attainable rate of return (due to matching or tax considerations) on a limited amount of contributions whose withdrawals are dedicated to specific uses.

IDAs have been justified in several different ways. To the extent that they increase financial, real, and human capital among low-income households, IDAs may be more effective in combating poverty than conventional income-support policies. In particular, the availability of even a relatively small amount of financial capital as a buffer against emergencies or a fund with which to make downpayments for, or purchases of, major assets could have a significant impact on the life prospects of low-income households. In addition, the process of saving may in itself promote positive changes in individual attitudes and family- and community-related behavior. Finally, regardless of the efficacy of saving incentives, equity considerations may suggest that if affluent households receive such subsidies, low-income households should as well.

During the 1990s, several public policies and private initiatives increased attention and resources on IDAs. By 2001-2002, more than 500 IDA programs were in operation in the United States, serving more than 20,000 account holders.[1]

This paper reports the results of what is, to our knowledge, the first controlled social experiment of the effects of saving incentives in general and IDAs in particular. There has been virtually no formal analysis to date of any kind, much less experimental research, on how IDAs affect overall household wealth accumulation. Moreover, although a substantial research literature exists on the effects of 401(k)-type plans and IRAs, there have been no controlled experiments of the effects of saving incentives on wealth. As a result, significant controversy remains over the extent to which the research has successfully separated the influence of the saving incentive programs themselves from the effects of unobserved individual characteristics that affect saving and may be correlated with eligibility or participation in the program.[2]

We evaluate the effects of an experimental IDA program administered in Tulsa, Oklahoma, 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

[1] This estimate (see www.AssetBuilding.org) is derived from a survey conducted by the Corporation for Enterprise Development. The IDA programs include state and federal efforts linked to the 1996 federal welfare reform—the Personal Responsibility and Work Opportunity Reconciliation Act. More recently, IDA programs can now receive federal support through the Assets for Independence Act of 1998 and other federal grant programs, including Community Services Block Grants and funding from the Office of Refugee Resettlement and the Federal Home Loan Bank. A number of local community-based IDA initiatives have been launched around the country, with support from foundations, financial institutions, other corporate sponsors, and individual private donors.

[2] See Bernheim (1999), Engen, Gale, and Scholz (1996), and Poterba, Venti, and Wise (1996) for reviews of the literature. More recently, Attanasio and Brugiavini (2003) and Attanasio and Rohwedder (2003) have exploited legislative changes to generate natural experiments in which variations in pension wealth are plausibly exogenous with respect to unobserved individual characteristics that may be related to saving.

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 matched withdrawals for home repair/improvement, post-secondary education, microenterprise startup/expansion, or retirement saving at 1:1. For each of three years, up to $750 in deposits was subject to match. Participants could thus accumulate $6,750 for home purchase or $4,500 for other allowed uses.

A very high percentage (85 percent) of treatment group members opened IDAs. Given their income levels, participants who made matched withdrawals were able to apply significant amounts -- more than $3,400 on average, including the match -- toward their asset goals, typically home purchase or improvement. But about half of the IDA holders withdrew all of their funds for non-matchable purposes.

The econometric analysis generates four central findings. First, the strongest evidence in this IDA evaluation relates to homeownership. After 48 months, the overall homeownership rate, the rate of recent home purchase among non-homeowners, and several measures of preparation for home purchase among non-homeowners were significantly higher in the treatment group than in the control group.

Second, we find no sample-wide statistically significant effects of the IDA program on other targeted uses such as educational attainment and business ownership. Nor did we obtain significant impacts on financial outcomes, including net worth and its major components. The interpretation of the financial results, however, is not entirely straightforward because some successful uses of the IDA could plausibly result in no change, or even a decline, in measured net worth, even if the IDA contributions were financed by net additions to saving. For example, a home purchase entails transaction and moving costs that reduce net worth. Another concern discussed below is the high variance of the financial outcomes, which makes detection of significant effects difficult in some cases.

Third, one subgroup -- African-Americans -- showed positive treatment effects on two targeted investments -- homeownership and retirement savings -- and on real assets. The impact on homeownership for this group may reflect the fact that African-Americans were disproportionately non-homeowners at baseline.

Fourth, while all sample members were low-income, those who were relatively more disadvantaged in the baseline survey do not appear to have benefited as much from the IDA program. This includes households with financial assets below $200, those without a bank account, and those on public assistance.

Section II of this paper describes the Tulsa IDA program. Section III compares the treatment and control groups at baseline and analyzes sample attrition. Section IV discusses participants' IDA activity. Section V describes our econometric methods. Section VI presents the main results. Section VII examines additional specification issues. Section VIII concludes.

## II. The Tulsa IDA Program

The data analyzed in this paper come from an IDA experiment conducted in Tulsa, Oklahoma, as part of the American Dream Demonstration (ADD). The ADD is a national demonstration of IDA programs that was initiated in the late 1990s. The Tulsa evaluation was the only ADD site, out of 14 programs nationwide, to offer an experimental design.[3] 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. The financing, management, and oversight of the project are described in Abt Associates (2004).

A. Sample Recruitment and Baseline Survey

Enrollment occurred between October 1998 and December 1999. Information about the IDA Matched Savings Program (CAPTC 1998) was distributed through 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 asked about the program.

Interested individuals submitted an application and were interviewed to establish eligibility, which was verified with federal tax returns and pay stubs. Applicants signed a form providing their informed consent regarding random assignment and authorized the release of financial information. In all, 1,147 individuals were referred for baseline (Wave One) interviews; a total of 1,103 applicants (96 percent of referred applicants) completed the Wave One interview and were enrolled in the study sample.

B. Random Assignment

The foundation of the analysis is the random assignment of program-eligible IDA

[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 and with evaluation funding from the Ford Foundation and the Charles Stewart Mott Foundation. 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).

applicants to one of two groups: the treatment group, which was allowed to participate in the IDA program, and the control group, which was not. Formally, the treatment in this context is thus the *offer* to participate in the IDA program.

Within a week after the baseline interview, applicants were randomly assigned to the treatment or control group.[4] Of the 1,103 enrolled individuals, 537 were assigned to the treatment group and 566 were assigned to the control group. Careful attention was given to ensuring that treatment group members received a uniform, well-described IDA program and control group members were not allowed access to IDA program services.[5]

C. Wave Two and Wave Three Surveys

The Wave Two survey occurred about 18 months after random assignment, between May 2000 and August 2001.[6] For each case, the interview was attempted by telephone. If telephone attempts were unsuccessful, the case was referred to a field

[4] The treatment-control ratio was 5:6 from October 1998 through March 1999, and then became 1:1 until December 1999. The original treatment-control ratio (5:6) had been adopted under the expectation that survey response rates at the follow-up interview waves (Waves Two and Three) would be somewhat lower for control cases than for treatment cases. This would thus require more control cases in the initial sample to ultimately obtain an analysis sample with approximately equal numbers between the two groups. In early 1999, however, CAPTC staff expressed the view that program recruitment was hindered by applicants facing a less than 50 percent chance of entering the IDA program. To promote recruitment, the ratio was changed to 1:1 on March 16, 1999.

[5] Two additional restrictions applied to the control group. First, control group members were not allowed to receive direct financial assistance through any other (non-IDA) matched savings program from CAPTC. This included CAPTC's pre-existing homeownership assistance program, which provided 1:1 matching funds for down payment and closing costs. Second, control group members were not allowed to participate in the "Lease-Purchase" program offered by CAPTC's Housing Department. Control group members were not prohibited, however, from receiving homeownership counseling from CAPTC's Housing Department. If control group members, in the course of receiving non-IDA program services from CAPTC, requested information about financial assistance for homeownership, they were referred to services offered by other Tulsa-area providers. Control group (and treatment group) cases were allowed to receive a business loan through CAPTC's microenterprise program or a no-interest heating assistance loan, offered by CAPTC to meet home heating costs. Members of the control group were released from their demonstration status after completing the Wave Three interview (or, for Wave Three no respondents, after September 2003).

[6] Cases were interviewed in 13 monthly cohorts, defined according to their month of random assignment The four last-enrolled cases, who entered the sample during the first week of December 1999, were grouped with the November 1999 enrollees.

interviewer who attempted to arrange an in-person interview at the respondent's residence. Interviews were conducted using computer-assisted telephone and personal interviewing methods.[7] A total of 933 interviews were completed, for an overall completion rate of 84.6 percent (Table 1), with the rate somewhat higher for treatment cases than for controls. Respondents received $35 for completing the interview.

The Wave Three survey occurred about 48 months after random assignment, from January 2003 to September 2003, and followed the same process of attempted telephone interviews followed by in-field interviews. The completion rate was 76.2 percent for the baseline sample, and somewhat higher for treatments than for controls (Table 2).[8] Respondents again received $35 for completing the interview.[9]

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[7] To maintain updated locating information on each sample case and thus enable a high response rate for the Wave Two survey, Abt Associates implemented interwove tracking efforts. These activities included a series of three separate tracking letters. These were mailed to each sample member 6, 11, and 16 months after random assignment. Each tracking letter reminded the sample members of the importance of their continued cooperation in the study. The letter asked the sample members to review and update records on contact information. The sample members used either a postage-paid envelope (enclosed with the tracking letter) or a toll-free telephone number (available seven days a week) to confirm or update their locating information. Those responding to the Month 16 letter received a $10 payment for their cooperation. All updated information was entered into a tracking database for use by the telephone and field interviewers in conducting the Month 18 survey. The response rate for the 16$^{th}$ month tracking letter was 45.1 percent, for the entire research sample of 1,103. Even if the sample member did not respond to a tracking letter, useful information came back through "postal updates" (i.e., letters returned by the post office with a forwarding address noted). In other instances, letters were returned by the post office as "undeliverable" (i.e., with no forwarding address). This identified the sample member as one requiring additional locating efforts, including contacts to CAPTC and possible use of secondary sources such as directory assistance and commercial services that compile address and telephone information from credit bureaus, employment agencies, and other automated lists.

[8] To the extent that there was variation among cases in the elapsed interval between random assignment and the Wave Three interview (targeted at 48 months, equal to 1,460 days), we also examined whether the timing of the Wave Three interview differed systematically between treatment and control cases. We found that the follow-up interval at Wave Three averaged 1,449 days for treatment cases and 1,456 days for control cases. The treatment-control difference was not statistically significant.

[9] The interwove tracking efforts included tracking letters mailed to each sample member approximately 26, 33, and 45 months after random assignment. The response rate for the Month 45 tracking letter was 40.1 percent, for the entire research sample of 1,103.

D. Data Verification

The difficulties of obtaining accurate household data on components of net worth and other financial circumstances, especially for low-income households, are well documented.[10] Unusually extensive efforts were made to ensure the accuracy of the survey data, especially for financial variables. Several criteria were developed to identify and verify financial variables that might have been misreported or misrecorded: values were verified if they fell outside a specified range each question; if the change in the recorded value between one wave and the next fell outside a specified range; or if the value was inconsistent with another response in the same wave.[11]

For data identified by these criteria, measures were taken to verify the correct values. For Wave One and Two values identified for verification, respondents were asked to correct or confirm the previously recorded 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 at the close of the Wave Three interview. Wave Three interviewers immediately verified all out-of-range item-specific values using range checks incorporated directly into the CATI/CAPI software. For other Wave Three data values identified for verification (involving a between-wave or within-wave inconsistency), a Survey Quality Form was either administered by telephone during November 2003 or mailed to the respondent. The results presented in this paper use the data that was revised to reflect the results of these verification efforts. We discuss the effects of using the original data in section VII.

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[10] These difficulties have been experienced for many years in major federal surveys, such as the Survey of Income and Program Participation (SIPP). See Bureau of the Census (1998).

[11] See Abt Associates (2004) for details on the criteria for verifying unusual financial values.

E. IDA Data

The Management Information System for Individual Development Accounts (MIS IDA), developed and supported by the CSD, provided information by month on IDA deposits, withdrawals, interest, and match funds, through October 31, 2003.[12]

F. IDA Rules

At program entry, participants had to be employed, with family income below 150 percent of the federal poverty guideline.[13] There were no limits on assets.

There was no minimum opening balance. Participants were expected to make a minimum monthly deposit of $10 in at least nine months of each year, but failure to do so did not normally result in dismissal from the program. Deposits earned the market rate of interest offered by the Bank of Oklahoma on passbook savings accounts, which was typically in the range of 2 to 3 percent during this period.[14] The Bank of Oklahoma waived all normal fees charged to open or maintain accounts.[15]

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[12] The MIS IDA information for the Tulsa site was provided to Abt Associates by CSD. MIS IDA was used by all ADD sites and is used by numerous other IDA programs nationwide. IDA demonstration projects that receive federal funding under the Assets for Independence Act are required to use MIS IDA or an equivalent software package.

[13] For a family of four in 1999, 150 percent of the federal poverty guideline was $25,050. Income was measured by CAPTC as the amount of adjusted gross income in the applicant's most recent federal tax return. Until February 15, 1999, federal tax returns for calendar year 1997 were used as verification. For later enrollees, calendar year 1998 tax returns were used. Current employment was verified by a pay stub.

[14] The Bank of Oklahoma held the IDAs and distributed regular monthly statements to clients. Participants had sole authority for deposits and withdrawals regarding their IDA. CAPTC controlled the separate custodial account in which match funds (and associated interest) accrued to the participant. The accounts could be opened at any of four local branch offices of the Bank of Oklahoma, and ongoing transactions could then be made at any of the bank's branches statewide.

[15] IDA account holders were not exempt from other service charges, however. For example, if the participant made more than three withdrawals within a twelve-month period, $3 was charged for each additional withdrawal. Additionally, a $15 charge was assessed if the account holder moved without notifying the bank of the address change.

IDA withdrawals used for home purchase were matched at 2:1. Withdrawals for home repair/improvement,[16] post-secondary education,[17] microenterprise expansion or startup, or retirement saving (funding 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).

Matching funds accrued for IDA deposits made within 36 months after the account opening. The accountholder had up to six additional months to make final matched withdrawals.[18] 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 matchable saving for the next year, but those who contributed less than $750 in one year could not match more than $750 in a subsequent year.[19]

At least four hours of general financial education (called Money Management sessions) were required before opening an account. Prior to a matched withdrawal,

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[16] Matching funds for home purchase were allowable only for a primary residence, but were not restricted to first-time homebuyers. An account holder who currently owned a home could thus upgrade (or downsize) their primary residence. Home repairs or improvements were matchable only for one's primary residence.

[17] 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 (GED) certificate.

[18] There were some exceptions to this provision. First, those participants who did not open their IDAs within 12 months of random assignment had only until the 48th month after random assignment to accumulate savings *and* make matched withdrawals. Second, for those participants opening their accounts after June 30, 2000, the last deposit date was June 30, 2003, and the final announced deadline for withdrawals was December 15, 2003 (although CAPTC allowed some participants to make subsequent matched withdrawals).

[19] This is referred to as an "annual match cap" design and is similar to how IRAs and 401(k)s operate. Other IDA programs with multi-year savings periods have used a "lifetime match cap" whereby the participant's accrued match is subject to a total cumulative limit instead of a yearly maximum.

participants were required to have taken 12 hours of general financial education as well as additional training specific to the type of intended asset purchase. Participants could not make a matched withdrawal until six months after opening their account, but were allowed to make up to three withdrawals for non-matched purposes every twelve months.

IDA balances in this program did not affect eligibility for TANF programs, but could affect eligibility for other programs, such as food stamps and medicaid.

## III. Sample Characteristics

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.

### A. Baseline Characteristics of the Analysis Sample

Table 3 presents the baseline demographic and economic characteristics of the analysis sample. About 80 percent sample members were female. Nearly half were single parents with children; 30 percent lived in two-adult households with children; and the remaining 23 percent lived in households without children. About one-quarter of sample members were married and 40 percent had never been married.

The average age was 36 years. Nearly half of the sample members were non-Hispanic Caucasian, and 41 percent were African-American. Just over one quarter had a high-school degree or GED but no further education, and 65 percent had some college education (including 8 percent who had attained an Associates degree). Only 4 percent were college graduates and only 6 percent had neither a high school diploma nor a GED.

Consistent with the eligibility requirements, nearly all sample members (99 percent) were employed at the time of the baseline survey,[20] and average monthly household income was $1,463, with an average income-to-poverty ratio of 126 percent. About 43 percent of the sample reported receiving "some" or "a lot of" government assistance during the prior month. Despite the very high employment rate, a large minority of respondents had no health insurance coverage.

Table 4 shows that average wealth was modest at baseline: $909 for liquid assets, $751 for retirement savings, $456 for other financial assets, and $2,735 for net worth. One quarter owned a home, and 7 percent owned a business.[21] About 84 percent owned a vehicle, 71 percent had a checking account, and 58 percent owned a savings account.

In tables 3 and 4, statistically significant differences in baseline characteristics of treatment and control group members were less frequent than would be expected based on chance alone. Relative to controls, treatment group members were more likely to have been married at some point and more likely to have two children, although there was no significant difference in the average number of children. There were no significant differences between the two groups in total financial assets, but treatment group members allocated less of their financial assets to liquid forms, and more to retirement savings. Likewise, there were no significant differences in real assets, liabilities, or net worth. Treatment group members were more likely to own other property, but very few

[20] Note that the program requirement was to be employed at the time of the eligibility interview with CAPTC. A small percentage of applicants become unemployed in the time between the eligibility interview and the baseline interview with Abt Associates.

[21] In contrast, about 20 percent reported some household income from self-employment. This difference may reflect the self-employment income of other family members or uncertainty about whether operating a microenterprise should be counted as "owning a business."

members of either group owned such property.[22]

B. Sample Balance: Implications of Random Assignment and Sample Attrition

Although baseline characteristics of treatment and control group members in the analysis sample are comparable, random assignment and attrition may have affected the composition of the analysis sample. This section summarizes tests that examine these issues with the goal of informing the regression model.

Regarding random assignment, we examined – in the baseline sample and the analysis sample – whether particular baseline characteristics were correlated with treatment group status, controlling for other factors. A few characteristics were significantly correlated with treatment group assignment.[23] Similarly, we examined whether sample attrition by the Wave 3 (month 48) interview was correlated with any baseline characteristics, adjusting for other factors. These attrition tests were run for the baseline sample and within the treatment group. Within the baseline sample, we did find significant correlations between a few characteristics and sample attrition.[24] To control for the effects of these imbalances on the estimation of program impacts, our regression

[22] There were large wealth differences between homeowners and non-homeowners (not shown in Table 4). The average value of real assets was about $3,800 for non-homeowners, compared to $53,000 for homeowners. A similar difference occurs for net worth, approximately -$3,800 versus $24,000.

[23] It is important to note that such sample imbalance is present to some degree in any randomized experiment and does not indicate any failure of random assignment. The steps taken here are ones that could (and perhaps should) be taken routinely to rebalance an experimental sample statistically in estimating program effects.

[24] For example, completion rates in both follow-up surveys were higher for earlier-enrolled cohorts than for later-enrolled cohorts. Earlier enrollees (both treatments and controls) may have been more closely connected to CAPTC – the early IDA sample recruitment occurred largely through the referral of individuals already receiving services from CAPTC – and thus more responsive to requests from CAPTC staff to interview later. Alternatively, earlier enrollees may have been instinctively more motivated by financial incentives—first the prospect of IDA match funds and later the prospect of a $35 incentive payment for competing a follow-up interview.

specification (see section V) includes each characteristic that is a source of imbalance as a covariate and in an interaction with the treatment dummy.[25] By including "treatment interactions" in the model, we protect against the possibility that sample imbalances bias the estimates. (For further details, see Abt Associates 2004.)

Among treatment cases, we also found that IDA deposits of those who completed the Wave Three Survey exceeded the deposits of those who did not. We have no way of adjusting for this difference in the analysis.

## IV. IDA Activity

Information on the dynamics of IDA saving and withdrawals among treatment group members is of some interest in its own right and is presented below. These findings refer to the entire treatment group in the baseline sample.

<u>Participation.</u> Of the 537 treatment group members, 456 – or 85 percent – opened an IDA.[26] We refer to these individuals as "participants." Almost half of participants opened their IDA in the first three months in which they were eligible. Participants kept their accounts open an average of 38 months.[27] An account was considered closed when the balance was reduced to zero and there were no subsequent transactions. As described

[25] In addition, any outcome variables that were imbalanced in the baseline sample – even if they were not imbalanced in the analysis sample, as measured by a T-test at the 95 percent confidence level – were entered in the model, both as a baseline covariate and in interaction with the treatment dummy.

[26] This number does not count as participants 16 treatment group members who opened an account but were subsequently found to be ineligible to participate. Among the 412 treatment group members who completed a month-48 follow-up interview and were thus included in the analysis sample, 369 (90 percent) opened an IDA.

[27] At 12 months after opening, 97 percent of accounts remained open; at 36 months, 66 percent were still open; and at 48 months, 16 percent remained. Because the demonstration was designed to last four years; it is impossible to know how many participants would have kept their accounts open beyond 48 months given on-going access to their IDA. (In 19 cases, participants, had their accounts open for 54 months or longer, as CAPTC did not require that participants close accounts with positive balances, as long as the demonstration was still operating.)

later, some closures represent dropouts; others represent successful program completion.

Contributions Following Schreiner (2002), we define *net deposits* as cumulative matchable deposits (including interest, net of fees) minus cumulative unmatched withdrawals. This measures net matchable deposits ever made into the IDA, but excludes the match. By October 2003, about one-half of participants had positive net deposits. The remainder had chosen to withdraw all of their deposits for unmatchable uses. The average net deposit was $655 for all participants and $1,300 for those with positive net deposits.[28]

Withdrawals Through October 2003, 39 percent of participants had made at least one matched withdrawal, including 34 percent who closed their accounts and 5 percent whose accounts remained open. But more than half – 53 percent – of participants closed their account without ever making a matched withdrawal. The remaining 9 percent continued in the program without having made a matched withdrawal. Among those with at least one matched withdrawal, the amount of matched withdrawals averaged $1,380 per participant; matched withdrawals plus matches averaged $3,431 per participant. Separately, 87 percent of participants had made at least one unmatched withdrawal through October 2003. Among these participants, the amount of unmatched withdrawals averaged $885.

Account uses The most prevalent use of matched withdrawals was home repair or improvement, and home purchase, which accounted for 35 and 26 percent of withdrawal transactions, respectively. Education and retirement each accounted for 17

[28] Those participants who had not withdrawn their matchable deposits by the end of the experiment could request that their deposits (plus match) be rolled over into a Roth IRA. Matchable balances that remain at the end of the reporting period are therefore included in net deposits.

percent. The remaining 5 percent were for small business. The distribution of funds allocated (the matched withdrawals plus the match) by use was somewhat different, with 41 percent for home purchase, 27 percent for home improvement or repair, 21 percent for retirement, 7 percent for education and training, and 5 percent for small business.

## V. Econometric Methodology

To examine the effects of the offer of an IDA on various aspects of household saving behavior, we estimate ordinary least squares equations of the form:

$$(1) \qquad \mathbf{Y_i} = \beta_0 + \beta_1 * \mathbf{X}_i + \beta_2 * \mathbf{T}_i + \beta_3 * \mathbf{Z_i} * \mathbf{T}_i + \varepsilon_i \,,$$

where the subscript i refers to the individual sample member, $\mathbf{Y_i}$ is an outcome variable, $\mathbf{X}_i$ is a vector of covariates measuring baseline demographic characteristics and baseline values of every outcome variable,[29] $\mathbf{Z}_i$ is the subset of the covariate vector $X_i$ identified as sources of sample imbalance as discussed in section IV,[30] $\mathbf{T}_i$ is an indicator variable taking the value 1 for treatment group members and zero otherwise, the β's are parameters and ε is the individual-specific error term.[31] The estimated treatment effect is

[29] Covariates capturing all of the baseline values of demographic variables and outcomes described in Section III are included in each model. Some categories are specified slightly differently in the models than they are presented in Section III.

[30] Based on the analyses of sample balance with the baseline sample and the analysis sample, described in Section III, a series of 23 variables were identified as sources of sample imbalance. These variables, which comprised the vector $Z_i$, were as follows: homeownership, property ownership, number of children in household, 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, liquid assets, retirement savings, African-American, monthly household income, cohorts 4-6, 7-9, 10-12, and 13.

[31] All of the estimates in the paper weight the sample to adjust for the change in the random assignment ratio early in the project. As discussed in section II, the random assignment ratio was changed early in the course of sample enrollment. The random assignment (treatment: control) ratio was 5:6 for those enrolled through March 15, 1999. Subsequently, the random assignment ratio was 1:1. Weights were constructed such that the weighted populations contain a 1:1 ratio of treatment to control group members in each month of random assignment. All of the results in this section reflect weighted samples.

given by $\hat{\beta}_2 + \hat{\beta}_3 * \bar{\mathbf{Z}}_\mathbf{i}$, and is evaluated at the mean of Z for the estimation sample.

For dichotomous outcomes (e.g., homeownership), probit models were estimated:

(2) $\mathbf{Pr(Y_i} = 1) = \Phi\ (\beta_0 + \beta_1 * \mathbf{X}_i + \beta_2 * \mathbf{T}_i + \beta_3 * \mathbf{Z_i} * \mathbf{T}_i)$,

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

We also examine whether treatment effects vary across subgroups, defined at the baseline. A separate equation was estimated for each outcome, for each set of mutually exclusive categories of a given characteristic (e.g., racial categories), using interaction terms between the baseline subgroup characteristic and the treatment indicator variable. If, for example, two subgroups were constructed, the estimating equation would be:

$$(3) \qquad \mathbf{Y_i} = \beta_0 + \beta_{11} * \mathbf{X_{i1}} + \beta_{12} * \mathbf{X_{i2}} + \beta_{21} * \mathbf{D_{i1}} * \mathbf{T_i} + \beta_{22} * \mathbf{D_{i2}} * \mathbf{T_i}$$

$$+ \beta_{31} * \mathbf{D_{i1}} * \mathbf{Z}_i * \mathbf{T_i} + \beta_{32} * \mathbf{D_{i2}} * \mathbf{Z}_i * \mathbf{T_i} + \varepsilon_i$$

where the $\mathbf{D}_{ij,}$ j = 1, 2, are dummy variables indicating the subgroup.[32] The treatment effect for group j is $\beta_{2j} + \beta_{3j} * \mathbf{Z}_i$, evaluated at the mean of **Z** for the *subgroup* estimation sample.[33]

[32] Because $\mathbf{D}_{i1} + \mathbf{D}_{i2} = 1$, the treatment dummy, $\mathbf{T_i}$, is not entered separately in the subgroup models.

[33] The estimates in this paper pertain to the treatment effect for the entire treatment group, including those who did not open an IDA. These are conventionally called "Intent-to-Treat" (ITT) estimates. The effect of IDA *participation*—i.e., the treatment effect on those who opened an IDA—may also be of interest and is conventionally referred to as the "Treatment-on-Treated" (TOT) estimate. TOT estimates can be generated easily (Orr 1999). If the treatment effect on eligible non-participants is zero and if ITT is the overall impact effect evaluated at the sample mean values of the covariates in $Z_i$, then the TOT estimate is ITT/p, where p is the proportion of the treatment group who participated. In the IDA experiment, this should be viewed as an upper bound, as one might reasonably expect that the early Money Management classes that all treatment group members received could have had a favorable effect on saving by eligible nonparticipants. Because the participation rate among treatment group members was so high, and because the TOT impacts can be estimated by applying the multiplier (1/p) to the ITT estimates, we present only the ITT estimates. To derive the TOT for a particular subgroup from its estimated ITT, one should use the participation rate among treatment group members in that subgroup (see Table 8).

## VI. Results

A. Full Sample

Homeownership Over the first 18 months, the IDA had no effects on homeownership (Table 5) or home purchase (Table 6). During that period, however, treatment group members were significantly more likely to report clearing up old debts in order to apply for a home loan (Table 6).

Over months 19 to 48, treatment group members were more likely to engage in a range of activities in preparation for home purchase (attend open house, talk with a realtor, talk about borrowing money, clear up debts).[34] By month 48, the homeownership rate for treatment group members was 6.2 percentage points higher than the control group mean of 42.9 percent (Table 5). Among non-homeowners at baseline, treatment group members were 8.9 percentage points more likely than control group members to have purchased a home over the 48-month horizon (Table 6).[35]

Because the effects on home purchase and home ownership are the most striking in the study, they are worth discussing further. First, it may not be surprising that the results change over time, since participants may well have wanted to take maximal advantage of matching contributions before making a downpayment. But the difference does suggest that the effects of IDA experiment can differ over time.

[34] Because the home search questions were also asked only of people who did not own a home at the time of the survey, we assign people who purchased a home (but did not own one at baseline) a "yes" to each of the home search questions. Thus, the variables can be interpreted as "searched for or purchased a home." There were also significant positive impacts on the cumulative measure of "home search intensity," but the measure is ordinal, which implies that the point estimates do not have intrinsic meaning.

[35] The 8.9 percentage point increase in homeownership among non-homeowners at baseline translates directly into about a 6.6 percentage point increase in the overall homeownership rate, since about three quarters of households did not own homes at baseline. The difference between the 6.2 and 6.6 figures represents changes in homeownership among homeowners at baseline.

Second, the result may be spurious. In particular, buying a house entails moving. The survey team could track closely the treatment group members who moved, because the IDA matching contribution was sent directly to the mortgage lender. Control group members who bought a house during the sample period, however, may have been more likely to have lost touch with the survey team. If so, the resulting differential attrition would bias the home purchase and home ownership effects upward.

Third, even to the extent that the result is sound, it may represent an acceleration of home purchase rather than a permanent difference in home ownership rates. The IDA program essentially matched downpayments made during a four-year period at a 2:1 rate and downpayments made in future years 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 household that knew it was planning on buying a home at some point in the future therefore may have accelerated its buying decision due to the program. This is especially salient since the underlying sample appears to be a relatively highly motivated group of savers.

<u>Other subsidized uses</u> The treatment had a marginally significant effect on the incidence of "any home improvement" over 48 months. The home improvement rate for the treatment group was 5.3 percentage points higher than the control group mean of 34.3 percent (Table 6).

The effects on other subsidized uses of IDA funds that involved real assets were virtually nonexistent. There were no significant effects on ownership of businesses or business start-up at either time horizon.

There is only one statistically significant impact on education (out of 15 outcomes) – whether participants had taken a non-degree course during the latter part of the demonstration. Notably, this is the educational outcome that takes the least time to complete. It may be that participants who were unable to use their IDAs to invest in homes or businesses used the program as a vehicle for taking classes to avoid losing the matching funds; in this context, non-degree classes might be more appropriately viewed as a consumption item rather than an investment.

Unsubsidized Uses There were no effects on purchases or ownership of vehicles or other property at either the 18-month or 48-month horizon.

Table 7 shows effects on financial and overall wealth outcomes. By month 48, the IDA had increased retirement saving, one of the qualified uses, by $581, an effect that is statistically significant at the 10 percent level. The IDA program also increased real assets, at 48 months, consistent with the increase in homeownership.

The effects on total financial assets were negative but not significant after 18 months, and negative and 6-7 times as large but still not significant at 48 months.[36] The effect on liabilities was positive but also not significant. The effect on net worth was negative but not significant at 18 months, and generated a point estimate of essentially zero, with a large standard error, at 48 months.

Interpretation of the financial and net worth results is made difficult by the fact that the IDA program emphasized the purchase of real assets. For example, in analysis of IRA or 401(k) programs, there is a close link between the source of the contributions and the effect on net worth. Contributions financed by reductions in consumption represent increases in net worth. Contributions financed by shifting existing assets, by diverting

[36] Other financial assets actually fell for treatment group members over the first 18 months, a result consistent with using financial assets to pay down debt, as described above. However, the results do not show a reduction in liabilities for the treatment group over the first 18 months.

current saving that would have been undertaken even in the absence of the program, or by increasing debt would not represent an increase in net worth.

Thus, one interpretation of the net worth result above could be that it shows that the contributions were not funded by reductions in consumption and therefore that IDAs did not increase saving. But the situation may well be more complex, because many of the allowable uses of the funds could generate no increases in *measured* net worth even if the contributions *were* financed by reductions in consumption. Using the funds to take educational classes is one example, since an increase in human capital would not appear in measured net worth. Using the funds to purchase a house is another example, because expenses such as moving and closing costs reduce net worth in the first year or two of home purchase. Thus, it is difficult to conclude that this IDA program did or did not lead to increases in the overall level of saving.[37]

B. Impact Estimates by Subgroup

We also examine the effects of the IDA program on various subgroups based on homeownership status, race, age, sex, family structure, education, total financial assets, receipt of public assistance, and banking status. Subgroups are defined by characteristics measured at baseline. Table 8 reports the results. Our discussion focuses primarily on impacts that are statistically different from zero at the 0.05 level, particularly where an F-test indicates that the impacts are unequal across subgroups.

Homeownership For six of the 21 tested subgroups, there was a significant positive impact on month-48 homeownership: those who did not own a home at baseline,

[37] Although not shown in this paper, we have also estimated the effects of the IDA program on monthly household income, the income-to-poverty ratio, monthly earnings, employment, and receipt of public assistance. Of the 10 estimates (5 measured at each of two surveys), only one is statistically significant at the 10 percent level, a negative effect on employment at month 48.

African-American non-Hispanics, families comprised of two or more adults with children, those with financial assets above $1,100, those not on public assistance, and those with a checking or savings account. The positive result for households that did not own homes echoes the results above for home purchase. The positive effect for African-Americans may be related to the finding that African-American sample members had a much lower rate of homeownership in the baseline survey than others (15.1 percent versus 29.2 percent). Each of the other four groups with positive treatment effects might be regarded as having economic advantages at baseline (relative to the rest of the sample). Families with multiple adults potentially had multiple earners or one earner who was not also balancing weekday child-care responsibilities. Similarly, those with financial assets above $1,100 or with a checking or savings account may simply have been better off financially and thus better able to accumulate sufficient savings in their IDAs to afford a home purchase.

<u>Preparation for home purchase</u> Although there were a number of subgroups with individually significant impact estimates for preparation for home purchase, the lack of significance for the associated F-tests indicates that there was no systematic concentration of this effect in any particular subgroup. Nonetheless, it is notable that the treatment had a favorable impact on home search activities for several subgroups where no impact had been found on homeownership itself at month 48: those 35 or younger, females, single parents, and those with high school education or less.

<u>Business ownership</u> The IDA appears to have had no effect on the rate of business ownership. There is a marginally significant positive impact for Caucasian non-Hispanics, but this evidence is weak (as the F-test on race/ethnicity is not significant).

Education/training  There were no subgroup differences in treatment effects on coursework/training.

Liquid assets  Households that have a four-year college degree or more experienced significant *negative* effects on this outcome. As discussed previously, this may reflect the use of IDA funds for purchases encouraged by the IDA program. For this group, however, no treatment effects were found on any of the subsidized forms of asset ownership. Also, none of the six groups that had an increase in homeownership experienced a significant decline in liquid assets.

Retirement savings  Two subgroup differences emerged for impacts on retirement saving. For African-Americans and for older participants (those 36 or older at baseline), the treatment served to increase retirement savings by approximately $1,100 (proportionally, by more than 70 percent of the respective control group mean). The effect associated with age was perhaps not surprising. Older participants would naturally have a stronger incentive to use the IDA program as a means of boosting their retirement accounts. The effect among African-Americans is striking in combination with the earlier-mentioned impact on homeownership.

Other financial assets and total financial assets  The most interesting aspect of the results for other financial assets and total financial assets is that virtually all of the estimates are negative. However, few of them are significant, and for no subgroups are the associated t and F tests both statistically significant (at the 0.05 level). Two notable findings are the negative impacts on other financial assets for males and for families with two or more adults with children. The latter subgroup showed a significant impact on homeownership. The reduction in other financial assets may indicate that these families needed to use such assets to purchase their homes.

Real assets and total assets Significant increases in real assets were found for four subgroups: African-Americans, those 36 or older, those not receiving public assistance, and those with checking or savings accounts. We previously noted a significant treatment effect on homeownership for three of these subgroups (all but the 36 or older subgroup). It is thus not surprising that these groups would show increases in real assets, an outcome category that is dominated by home value.

For the 36 or older subgroup, the treatment had a positive effect not only on real assets but also on total assets. These effects are substantial in magnitude, approximately $13,000 (proportionally, more than 30 percent of the respective control group means). For this subgroup, the previously noted effect on retirement savings would have contributed to the effect on total assets. The effect on real assets is somewhat surprising, however, as the treatment appeared to have no impact on either homeownership or business ownership for this subgroup.

Total liabilities For those not owning a home at baseline, the treatment had a positive effect on liabilities. This is consistent with the finding of increased homeownership for this subgroup. The increase in their liabilities presumably reflected their home mortgage loans.

Net worth For net worth, the F-test showed statistical significance (at the 0.05 level) for several baseline characteristics: age, public assistance receipt, and banking status. Within these categories, however, there were no individual subgroups for which the estimated treatment effect was also significantly nonzero.

## VII. Further Examination of Impact Estimates

This section addresses several issues that arose in the course of collecting and analyzing the data and interpreting the estimated treatment effects.[38]

A. Control group crossover

Despite continual efforts throughout the evaluation to prohibit such behavior, it appears that up to 31 control group members (7.2 percent of the control group) may have received access to *some* (not all) of the educational services and financial assistance with housing that violated the eligibility rules of the evaluation. None of the 31 members, however, was allowed to open an IDA.

When control group members receive services that are part of the treatment, the unadjusted analysis will understate the true treatment effect. The appropriate adjustment for such "crossover" is the Bloom (1984) correction, which calls for the estimated treatment effect to be multiplied by 1/(1-r), where r is the rate of crossover. In this instance, the adjustment factor is small, changing the estimates by 8 percent.[39] It is almost certainly an overcorrection, too; it assumes that *all* 31 cases received *all* of the services intended for treatments, including the option of opening an IDA. The adjustment should therefore be seen as providing an upper bound, not a better estimate.

B. Sensitivity of estimates to outlier data values

As discussed above, we undertook several efforts to verify a variety of types of outlier data values. The findings presented in this report were based on survey datasets that we refer to as the "revised data," making use of the post-interview verifications. For

[38] Abt Associates (2004) contains more detailed discussion and analysis of these issues.

[39] For example, the effect on home ownership would rise from 6.2 percentage points to 6.7 percentage points. Because it applies to both the point estimate and standard error of the treatment effect, the adjustment does not alter statistical significance.

the full-sample analysis of major outcomes, we have also generated an alternative set of findings based on the "original data," suppressing any revisions that occurred through the post-interview verifications, but retaining the same econometric specification.[40]

We summarize here the findings of this sensitivity analysis. Among all 94 pairs of impact estimates for which the "revised" and "original" results were compared, only two pairs of impacts showed a change in the significance level of the treatment effect. These estimates pertain to the month-18 effects on total liabilities and net worth. Both effects were statistically significant using the original data (positive for liabilities and negative for net worth). Neither effect was statistically significant using the revised data. At month 48, no significant treatment effect was found for either of these outcomes, using either the revised data (as already reported) or the original data.

Based on these comparisons, it seems reasonable to conclude that, without conducting the post-interview data verification, one would likely have obtained the same general pattern of significant effects as reported here. By removing erroneous values in the survey data, however, the post-verification efforts almost certainly improved somewhat the accuracy of the point estimates.

A separate sensitivity analysis examines whether the estimated treatment effects are sensitive to alternative methods of dealing with item-specific out-of-range values. Specifically, we imputed these values to their respective (treatment or control) group mean. We then specified (in combination with the indicated handling of out-of-range covariates) several possible rules for handing out-of-range dependent variables (outcomes, as measured at Wave Three): deleting cases entirely from the analysis if the

[40] See Abt Associates (2004) for more details. Similar tests were not performed at the subgroup level.

dependent variable is out of range or deleting cases entirely from the analysis if the dependent variable falls in the extreme tail of the distribution of sample values (defined as the top 3 percent for positive financial values, or the top 1.5 percent and bottom 1.5 percent for net worth). Using different combinations of these rules, we estimated treatment effects on real, financial, and total assets; total liabilities; and net worth.

The findings, available upon request, can be summarized as follows: Point estimates of some treatment effects become slightly larger, while the standard errors are little affected. This causes the treatment effect to become statistically significant and positive for both real assets and liabilities, with effects remaining insignificant on the other outcomes. Similar results are obtained when combining imputation of out-of-range covariates with the deletion of observations having out-of-range dependent variables.

Generally, however, we concluded that such strategies yield results that are less valid than the findings presented above because at Wave Three, all out-of-range financial variables were subject to a real-time verification procedure, with range checks incorporated into the CATI/CAPI interviewing software. This meant that all out-of-range outcome values in the dataset had been explicitly confirmed by the respondent. Relying on values explicitly confirmed by the respondent seems preferable to relying on estimation methods that would delete such observations from the analysis.[41]

C. Minimum detectable effects and precision of estimates

It is important to consider the findings above in the context of the study's ability to detect treatment effects, as measured by its "minimum detectable effects" (or MDEs). The MDEs are the smallest true impacts that one would have been confident of detecting

[41] See Bollinger and Chandra (2003) for further discussion of the statistical bias that may be introduced through removing observations whose values lie outside a specified range.

as statistically significant, adjusting for sample size and the inherent variability of the outcome measures.[42] To the extent feasible, one always wants the minimum detectable effects to be within the plausible range of impacts for the intervention in question.

Appendix Table 1 shows the MDEs for the full-sample impacts on all major outcomes at month 48 or reflecting asset-building activities during months 1 to 48. For many of the outcomes under investigation, our ability to detect a treatment effect was reasonably good. For about two-thirds of the outcomes we could be confident of detecting an effect of less than 25 percent of the control mean. Among these outcomes were: homeownership, vehicle ownership, each of the separate activities preparatory to home purchase, home improvement, each of the indicators of education or training (other than school graduation), real assets, total assets, total liabilities, and each of the indicators of employment and income.

For other outcomes—typically, those corresponding to rare events or highly variable financial components—impacts needed to be considerably larger, in the range of 25 to 50 percent of the control mean, to be detectable with confidence. Such was the case for: home purchase (among those not owing a home at baseline), business ownership, business startup or purchase, activities preparatory to business startup (preparing a business plan, applying for a license, discussing a business loan), liquid assets, and retirement savings. Program effects of this magnitude, although quite large, might still have been considered plausible.

On all other outcomes, including other financial assets, total financial assets, and net worth, effects would have needed to be well above 50 percent of the control mean for

[42] The MDEs presented here are the minimum true effects detectable with 80 percent power.

us to be confident of detecting them. Normally, proportional effects of 50 percent or more would be regarded as implausibly large for a program intervention.

For each of the outcomes in the second and third categories above, the study may well have failed to detect as significant a true program effect. To have reduced this risk, however, one would have needed a much larger sample.[43]

While we cannot completely rule out program effects in those cases where the estimated impacts were statistically insignificant, the 95 percent confidence intervals shown in the table allow us to place an upper bound on the likely magnitude of the impact. The upper and lower bounds of the confidence interval indicate the likely range of the estimates that one would obtain in repeated sampling. For some of the outcomes that had insignificant impact estimates, the upper limit of the confidence interval suggests that the actual impact was probably small relative to the control mean. For example, the upper limits of the confidence intervals suggest that the impacts on business startups and total financial assets, which have large MDEs, were probably no more than 25 percent of the control mean, and the impact on other financial assets was probably no more than 20 percent of the control mean. Similarly, the impacts on vehicle ownership, school graduation, any postsecondary education or training, employment, earnings, income, and the income-to-poverty ratio were all probably less than 10 percent of the control mean.

## VIII. Conclusion

The results presented above constitute the first true experimental evidence on how saving incentives for low-income families can affect household wealth accumulation; indeed, the results provide the first experimental evidence of the effects of savings

[43] The ability to detect effects is primarily a matter of sample size.

incentives on wealth accumulation for any population. Subject to caveats discussed above, the IDA program positively affected home ownership rates in the overall study sample and retirement savings for older adults.

These results thus stem from an identification strategy that is superior to others found in the previous literature. At least in this case, however, the same identification strategy limits the generalizability of the results, for at least two reasons. First, the sample involved is a highly motivated group of savers; the 85 percent IDA participation rate exceeds the 401(k) participation rate among 401(k)-eligible workers, who tend to have much higher income than the treatment group in this evaluation. Second, an IDA bundles together a significant number of features (contribution limits, allowable uses, match rates, financial education, etc.). We have no way of sorting out the relative impacts of particular factors or components of the Tulsa IDA. Moreover, to the extent that the program succeeded in helping households obtain particular assets or in raising wealth, the experiment provides little evidence on the *mechanisms* through which such changes occurred.

Efforts to clarify these issues would be important avenues for future research. In addition, future research could usefully return to the original motivation for IDAs – that is, as a substitute for traditional income-support programs. An experimental test of the benefits of IDAs relative to more traditional programs would be of significant interest. Finally, the hypothesis noted above that the increase in homeownership might represent an acceleration of home purchases rather than a permanent change in the homeownership rate could be tested by resurveying the treatment and control group in a number of years.

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**Table 1: Month 18 (Wave Two) Survey**

| | Total Sample | Completed Interviews | | | Completion Rate[a] |
|---|---|---|---|---|---|
| | | Telephone | Field | Total | |
| Treatment Group | 537 | 407 | 55 | 462 | 86.0% |
| Control Group | 566 | 403 | 68 | 471 | 83.2% |
| **Total** | **1,103** | **810** | **123** | **933** | **84.6%** |

[a] Total completed interviews (fourth column) as a percentage of corresponding total sample (first column).

**Table 2: Month 48 (Wave Three) Survey**

| | Total Sample | Completed Interviews | | | Completion Rate[a] |
|---|---|---|---|---|---|
| | | Telephone | Field | Total | |
| Treatment Group | 537 | 384 | 28 | 412 | 76.7% |
| Control Group | 566 | 381 | 47 | 428 | 75.6% |
| **Total** | **1,103** | **765** | **75** | **840** | **76.2%** |

[a] Total completed interviews (fourth column) as a percentage of corresponding total sample (first column).

**Table 3: Baseline Demographic and Economic Characteristics of the Analysis Sample**

| | Control Group (n=428) Percent / Mean | Treatment Group (n=412) Percent / Mean | Difference[a] (Treatment-Control) | Analysis Sample (n=840) Percent / Mean |
|---|---|---|---|---|
| **Gender** | | | | |
| Female | 81.0% | 79.0% | -2.1% | 80.0% |
| Male | 19.0% | 21.0% | 2.1% | 20.0% |
| **Race/Ethnicity** | | | | |
| Caucasian, Non-Hispanic | 49.0% | 45.0% | -4.0% | 47.0% |
| African-American, Non-Hispanic | 39.0% | 42.8% | 3.8% | 40.9% |
| Hispanic | 2.6% | 1.7% | -0.9% | 2.1% |
| Asian, Non-Hispanic | 0.7% | 1.2% | 0.5% | 1.0% |
| Native American / Other, Non-Hispanic | 5.5% | 5.6% | 0.1% | 5.6% |
| **Age** | | | | |
| Average Age | 36.3 | 36.3 | -0.1 | 36.3 |
| Less than 30 | 29.6% | 30.3% | 0.8% | 29.9% |
| 30 to 39 | 33.9% | 34.4% | 0.5% | 34.1% |
| 40 to 49 | 26.1% | 25.0% | -1.2% | 25.4% |
| 50 and Older | 10.5% | 10.5% | -0.1% | 19.5% |
| **Marital Status** | | | | |
| Never Married | 44.3% | 35.7% | -8.6%** | 39.9% |
| Married | 24.1% | 28.3% | 4.1% | 26.2% |
| Divorced or Separated | 28.8% | 33.4% | 4.6% | 31.1% |
| Widowed | 2.8% | 2.7% | -0.1% | 2.7% |
| **Household Type** | | | | |
| One Adult With Children | 47.5% | 49.2% | 1.7% | 48.3% |
| One Adult Without Children | 11.6% | 11.7% | 0.0% | 11.6% |
| Two or More Adults With Children | 28.9% | 30.3% | 1.4% | 29.6% |
| Two or More Adults Without Children | 12.0% | 8.9% | -3.1% | 10.5% |
| **Adults in Household** | | | | |
| Average Number of Adults | 1.51 | 1.49 | 0.02 | 1.50 |
| 1 | 59.1% | 60.8% | 1.7% | 60.0% |
| 2 | 32.2% | 30.7% | -1.5% | 31.5% |
| 3 | 7.0% | 7.5% | 0.5% | 7.3% |
| 4 or More | 1.6% | 0.9% | -0.7% | 1.3% |
| **Children in Household** | | | | |
| Average Number of Children | 1.61 | 1.75 | -0.14 | 1.68 |
| None | 23.7% | 20.6% | -3.1% | 22.1% |
| 1 | 27.5% | 22.3% | -5.1%* | 24.9% |
| 2 | 22.6% | 31.9% | 9.3%*** | 27.3% |
| 3 or More | 26.3% | 25.2% | -1.1% | 25.7% |

**Table 3: Baseline Demographic and Economic Characteristics of the Analysis Sample *(Continued)***

| | Control Group (n=428) Percent / Mean | Treatment Group (n=412) Percent / Mean | Difference[a] (Treatment-Control) | Analysis Sample (n=840) Percent / Mean |
|---|---|---|---|---|
| **Education** | | | | |
| Less than High School | 4.7% | 6.3% | 1.6% | 5.5% |
| High School Diploma or GED | 26.5% | 25.1% | -1.4% | 25.8% |
| Some College | 57.7% | 56.4% | -1.3% | 57.1% |
| Graduated From 2-year College | 7.3% | 7.7% | 0.4% | 7.5% |
| Graduated From 4-year College | 3.7% | 4.4% | 0.7% | 4.0% |
| Missing/Refused/Don't Know | 0.2% | 0.0% | -0.2% | 0.1% |
| **Employment** | | | | |
| Employed | 98.1% | 99.3% | 1.2% | 98.7% |
| **Self-Employment** | | | | |
| Owned Business | 5.9% | 7.7% | 1.8% | 6.8% |
| Had Household Income from Self-Employment | 19.0% | 20.2% | 1.2% | 19.6% |
| **Received Government Assistance** | | | | |
| "Some" or "A Lot of" Government Assistance | 42.1% | 42.9% | 0.8% | 42.5% |
| **Health Insurance Coverage** | | | | |
| With Health Insurance | 57.5% | 58.8% | 1.3% | 58.1% |
| **Monthly Household Income** | $1,416 | $1,508 | $93 | $1,463 |
| **Household Income-to-Poverty Ratio** | 125% | 128% | 3.2% | 126% |

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

**Table 4: Baseline Financial Circumstances of the Analysis Sample**

| | Control Group (n=428) | Treatment Group (n=412) | Difference[a] (Treatment-Control | Analysis Sample (n=840) |
|---|---|---|---|---|
| **Liquid Assets** | | | | |
| Amount held in checking and savings accounts {including IDAs}, money market accounts, and CDs | $1,069 | $753 | -$316 * | $909 |
| **Retirement Savings** | | | | |
| Amount held in pensions, IRAs, 401(k)s | $563 | $934 | $372 * | $751 |
| **Other Financial Assets** | | | | |
| All other savings: stocks and bonds, savings at home or with friends, educational savings accounts | $409 | $503 | $94 | $456 |
| **Total Financial Assets** | | | | |
| Sum of liquid assets, retirement savings, and other financial assets | $2,041 | $2,190 | $150 | $2,116 |
| **Real Assets** | | | | |
| Market value of primary residence, other property, vehicles, and business assets | $16,368 | $14,465 | -$1,904 | $15,406 |
| **Total Assets** | | | | |
| Sum of total financial assets and real assets | $18,409 | $16,655 | -$1,754 | $17,523 |
| **Total Liabilities** | | | | |
| Total indebtedness: mortgages, car loans, credit card debt, educational loans, medical bills, personal and business loans | $15,015 | $14,565 | -$450 | $14,788 |
| **Net Worth** | | | | |
| Total assets minus total liabilities | $3,394 | $2,090 | -$1,304 | $2,735 |
| **Home Ownership** | 24.3% | 22.6% | -1.8% | 23.4% |
| **Business Ownership** | 5.9% | 7.7% | 1.8% | 6.8% |
| **Other Property Ownership** | 2.1% | 4.6% | 2.5% ** | 3.4% |
| **Vehicle Ownership** | 84.0% | 84.3% | 0.4% | 84.1% |
| **Any Recent Home Improvement** | 5.7% | 5.0% | 0.7% | 5.3% |
| **Major (>$200) Recent Home Improvement** | 4.5% | 4.0% | 0.5% | 4.2% |
| **Owned a Checking Account** | | | | |
| With Money in a Checking Account | 69.1% | 73.2% | 4.1% | 71.2% |
| **Owned a Savings Account** | | | | |
| With Money in a Savings Account | 57.1% | 59.5% | 2.4% | 58.3% |

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

## Table 5: Impacts on Ownership of Real Assets

| Outcome | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect at Month 48[a] (Standard Error) | Sample Size at Month 18 | Control Mean at Month 18 | Treatment Effect at Month 18[a] (Standard Error) |
|---|---|---|---|---|---|---|
| **Homeownership** | 839 | 0.429 | 0.062 ** (0.031) | 764 | 0.349 | 0.004 (0.025) |
| **Business Ownership** | 840 | 0.105 | -0.002 (0.020) | 764 | 0.100 | -0.006 (0.018) |
| **Other Property Ownership** | 840 | 0.047 | 0.010 (0.018) | 764 | 0.036 | -0.004 (0.013) |
| **Vehicle Ownership** | 840 | 0.903 | -0.004 (0.023) | 764 | 0.901 | 0.002 (0.022) |

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

**Table 6: Impacts on Asset-Building Activities**

| Outcome | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect on Activity in Months 1 to 48[a] (Standard Error) | Sample Size at Month 18 | Control Mean at Month 18 | Treatment Effect on Activity in Months 1 to 18[a] (Standard Error) | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect on Activity in Months 19 to 48[a] (Standard Error) |
|---|---|---|---|---|---|---|---|---|---|
| **Home Purchase or Related Activities‡** | | | | | | | | | |
| Home purchase | 643 | 0.302 | 0.089 ** (0.037) | 579 | 0.166 | -0.006 0.030 | 579 | 0.148 | 0.092 *** (0.032) |
| Looked through home listings in newspaper | 643 | 0.764 | 0.045 (0.032) | 579 | 0.539 | 0.030 (0.042) | 579 | 0.540 | 0.044 (0.042) |
| Drove to look at houses for sale | 643 | 0.751 | 0.033 (0.032) | 579 | 0.563 | -0.026 (0.041) | 579 | 0.528 | 0.079 * (0.043) |
| Attended open house | 643 | 0.503 | 0.079 ** (0.039) | 579 | 0.320 | -0.036 (0.038) | 579 | 0.304 | 0.107 *** (0.040) |
| Talked about borrowing money for a home | 643 | 0.559 | 0.067 * (0.039) | 579 | 0.393 | -0.022 (0.042) | 579 | 0.336 | 0.095 ** (0.041) |
| Cleared up old debts to apply for home loan | 643 | 0.592 | 0.117 *** (0.038) | 579 | 0.399 | 0.094 ** (0.042) | 579 | 0.373 | 0.100 ** (0.043) |
| Talked with realtor about buying home | 643 | 0.681 | 0.034 (0.035) | 579 | 0.504 | -0.029 (0.042) | 579 | 0.428 | 0.075 * (0.042) |
| Intensity of home search | 643 | 4.15 | 0.465 ** (0.185) | 579 | 2.88 | 0.005 (0.204) | 579 | 2.66 | 0.591 *** (0.223) |
| **Home Improvement** | | | | | | | | | |
| Any home improvement | 840 | 0.343 | 0.053 * (0.031) | 764 | 0.208 | 0.022 (0.026) | 764 | 0.304 | 0.038 (0.031) |
| Major home improve-ment (over $200) | 840 | 0.299 | 0.032 (0.030) | 764 | 0.157 | 0.017 (0.025) | 764 | 0.265 | 0.025 (0.031) |
| **Business Startup or Related Activities†** | | | | | | | | | |
| Business startup or purchase | 784 | 0.106 | -0.016 (0.022) | 710 | 0.066 | -0.018 (0.017) | 710 | 0.049 | -0.002 (0.017) |
| Talked about starting his/her own business | 784 | 0.501 | 0.025 (0.037) | 710 | 0.375 | 0.007 (0.037) | 710 | 0.342 | 0.063 * (0.038) |
| Prepared business plan or similar document | 784 | 0.217 | 0.001 (0.031) | 710 | 0.136 | 0.001 (0.025) | 710 | 0.137 | 0.011 (0.027) |

**Table 6: Impacts on Asset-Building Activities *(Continued)***

| Outcome | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect on Activity in Months 1 to 48[a] (Standard Error) | Sample Size at Month 18 | Control Mean at Month 18 | Treatment Effect on Activity in Months 1 to 18[a] (Standard Error) | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect on Activity in Months 19 to 48[a] (Standard Error) |
|---|---|---|---|---|---|---|---|---|---|
| **Business Startup or Related Activities† *(Continued)*** | | | | | | | | | |
| Applied for business license | 784 | 0.124 | -0.001<br>(0.024) | 710 | 0.082 | -0.027<br>(0.019) | 710 | 0.060 | 0.011<br>(0.019) |
| Talked about obtaining business loan | 784 | 0.153 | -0.009<br>(0.026) | 710 | 0.112 | -0.021<br>(0.022) | 710 | 0.074 | 0.015<br>(0.020) |
| **Education or Training** | | | | | | | | | |
| Took non-degree course | 840 | 0.373 | 0.009<br>(0.035) | 764 | 0.247 | 0.006<br>(0.035) | 764 | 0.191 | 0.066 **<br>(0.031) |
| Took course toward degree | 840 | 0.502 | -0.010<br>(0.033) | 764 | 0.384 | 0.001<br>(0.035) | 764 | 0.381 | 0.013<br>(0.034) |
| Finished job training program with certificate | 840 | 0.373 | -0.001<br>(0.035) | 764 | 0.243 | -0.012<br>(0.033) | 764 | 0.266 | -0.021<br>(0.034) |
| Graduated from school | 840 | 0.220 | -0.037<br>(0.029) | 764 | 0.134 | -0.023<br>(0.025) | 764 | 0.134 | -0.023<br>(0.026) |
| Any postsecondary education or training | 840 | 0.690 | -0.002<br>(0.030) | 764 | 0.569 | 0.004<br>(0.035) | 764 | 0.514 | 0.045<br>(0.035) |

† Sample restricted to those who did not own a business at baseline. Such persons who started or purchased business during a follow-up interval were included in the numerator for the business-related activities.

‡ Sample restricted to those who did not own a home at baseline. Note that such persons who purchased homes during a follow-up interval were included in the numerator for the "Home Purchase or Search" outcome measures.

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

## Table 7: Impacts on Components of Net Worth

| Outcome | Sample Size at Month 48 | Control Mean at Month 48 | Treatment Effect at Month 48[a] (Standard Error) | Sample Size at Month 18 | Control Mean at Month 18 | Treatment Effect at Month 18[a] (Standard Error) |
|---|---|---|---|---|---|---|
| **Liquid Assets** | 840 | 2257 | -55 | 764 | 1678 | 280 |
| Amount held in checking and savings accounts {including IDAs}, money market accounts, and CDs | | | (367) | | | (212) |
| **Retirement Savings** | 840 | 1760 | 581 * | 764 | 1207 | -358 |
| Amount held in pensions, IRAs, 401(k)s | | | (338) | | | (228) |
| **Other Financial Assets** | 840 | 2608 | -2650 | 764 | 683 | -361 * |
| Stocks and bonds, educational accounts, Christmas clubs, savings held with family and friends, and all other savings | | | (1608) | | | (214) |
| **Total Financial Assets** | 840 | 6624 | -2124 | 764 | 3568 | -438 |
| Sum of liquid assets, retirement savings, and other financial assets | | | (1890) | | | (455) |
| **Real Assets** | 840 | 39071 | 6310 * | 764 | 29561 | -719 |
| Market value of primary residence, other property, vehicles, and business assets | | | (3552) | | | (2481) |
| **Total Assets** | 840 | 45694 | 4186 | 764 | 33129 | -1157 |
| Sum of total financial assets and real assets | | | (4292) | | | (2622) |
| **Total Liabilities** | 840 | 34847 | 4157 | 764 | 23132 | 1529 |
| Total indebtedness: mortgage(s), car loans, credit card debt, educational loans, medical bills, personal and business loans. | | | (2672) | | | (1547) |
| **Net Worth** | 840 | 10847 | 29 | 764 | 9997 | -2686 |
| Total assets minus total liabilities | | | (3433) | | | (2188) |

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

## Table 8: Summary of Estimated Impacts at Month 48 by Subgroup

| Subgroup | | Outcome | | | |
|---|---|---|---|---|---|
| | | Home-ownership | Intensity of home search | Business ownership | Any education/ training |
| **Homeownership:** | Owned home | -0.017† | | 0.036 | 0.087 |
| | | -0.057 | | -0.045 | -0.066 |
| | Did not own home | 0.085**† | | -0.007 | -0.028 |
| | | -0.036 | | -0.022 | -0.034 |
| **Race/ethnicity:** | African-American | 0.114**† | 0.243 | -0.02 | -0.012 |
| | | -0.046 | -0.269 | -0.027 | -0.045 |
| | Caucasian | 0.028† | 0.435 | 0.061* | 0.027 |
| | | -0.045 | -0.273 | -0.031 | -0.045 |
| **Age:** | 35 or younger | 0.063 | 0.464** | -0.029 | -0.021 |
| | | -0.043 | -0.231 | -0.028 | -0.04 |
| | 36 or older | 0.061 | 0.467 | 0.034 | 0.017 |
| | | -0.041 | -0.291 | -0.027 | -0.044 |
| **Gender:** | Male | 0.071 | 0.831* | 0 | -0.062 |
| | | -0.071 | -0.434 | -0.051 | -0.075 |
| | Female | 0.061* | 0.414** | 0.002 | 0.011 |
| | | -0.034 | -0.205 | -0.022 | -0.032 |
| **Family structure:** | No children | 0.053 | -0.127 | 0.032 | 0.035 |
| | | -0.062 | -0.373 | -0.044 | -0.065 |
| | Single parent | 0.038 | 0.542** | -0.003 | -0.043 |
| | | -0.043 | -0.25 | -0.027 | -0.041 |
| | Two or more adults with children | 0.107** | 0.710** | -0.01 | 0.038 |
| | | -0.053 | -0.323 | -0.035 | -0.054 |
| **Education:** | High school or less | 0.059 | 0.857** | 0.24 | 0.04 |
| | | -0.053 | -0.352 | -0.032 | -0.059 |
| | Some college | 0.066 | 0.29 | 0.003 | -0.02 |
| | | -0.04 | -0.236 | -0.028 | -0.037 |
| | Four-year degree or higher | 0.057 | 0.436 | -0.055 | -0.017 |
| | | -0.087 | -0.53 | -0.06 | -0.078 |
| **Financial assets:** | $200 or less | 0.007 | 0.424 | -0.039 | -0.062 |
| | | -0.044 | -0.268 | -0.026 | -0.046 |
| | $201 to $1,100 | 0.064 | 0.756*** | 0 | 0.02 |
| | | -0.044 | -0.248 | -0.028 | -0.041 |
| | $1,101 or more | 0.116** | 0.165 | 0.047 | 0.033 |
| | | -0.049 | -0.311 | -0.033 | -0.045 |
| **Public Assistance:** | Public Assistance | 0.028 | 0.333††† | -0.02 | -0.016 |
| | | -0.041 | -0.233 | -0.026 | -0.039 |
| | No public assistance | 0.087** | .588††† | 0.018 | 0.008 |
| | | -0.036 | -0.218 | -0.024 | -0.035 |
| **Banking status:** | Checking or savings account | 0.076**†† | 0.528*** | 0 | 0.003 |
| | | -0.032 | -0.192 | -0.021 | -0.031 |
| | No checking or savings account | -.042†† | -0.009 | 0.016 | -0.041 |
| | | -0.064 | -0.401 | -0.043 | -0.534 |

See explanatory notes at end of exhibit

## Table 8: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)*

| | | Outcome | | | |
|---|---|---|---|---|---|
| **Subgroup** | | **Liquid assets** | **Retirement savings** | **Other financial assets** | **Total financial assets** |
| **Homeownership:** | Owned home | 1335† | 1750† | -6601 | -3515 |
| | | -1040 | -1139 | -4272 | -5159 |
| | Did not own home | -460† | 240† | -1499 | -1719 |
| | | -412 | -298 | -922 | -1159 |
| **Race/ethnicity:** | African-American | -516 | 1081**† | -1536* | -971 |
| | | -653 | -471 | -929 | -1463 |
| | Caucasian | 154 | -37† | -4311 | -4195 |
| | | -573 | -541 | -2964 | -3419 |
| **Age:** | 35 or younger | -312 | 33†† | -4297 | -4576† |
| | | -479 | -428 | -2750 | -3146 |
| | 36 or older | 204 | 1139*†† | -975 | 368† |
| | | -592 | -593 | -1060 | -1705 |
| **Gender:** | Male | -1621 | 486 | -5180**† | -6314*† |
| | | -996 | -908 | -2344 | -3391 |
| | Female | 307 | 599* | -2113† | -1207† |
| | | -409 | -357 | -1623 | -1908 |
| **Family structure:** | No children | -187 | 684 | -150 | 347 |
| | | -754 | -837 | -940 | -1685 |
| | Single parent | 343 | 165 | -3404 | -2896 |
| | | -491 | -388 | -2761 | -3109 |
| | Two or more adults with children | -609 | 1175 | -3144** | -2578 |
| | | -758 | -761 | -1435 | -2305 |
| **Education:** | High school or less | -21† | 1100* | -1207 | -128 |
| | | -611 | -617 | -1176 | -1652 |
| | Some college | 462† | 288 | -3935* | -3186 |
| | | -465 | (420 | -2386 | -2754 |
| | Four-year degree or higher | -2806**† | 732 | 226 | -1849 |
| | | -1251 | -1294 | -1684 | -3016 |
| **Financial assets:** | $200 or less | -236 | 650 | -1443 | -1029 |
| | | -398 | -441 | -1269 | -1620 |
| | $201 to $1,100 | 553 | 538 | -3049 | -1959 |
| | | -589 | -495 | -2429 | -2854 |
| | $1,101 or more | -572 | 574 | -3434* | -3432 |
| | | -934 | -860 | -1771 | -2916 |
| **Public assistance:** | Public assistance | -313 | 534 | -1976* | -1755 |
| | | -393 | -399 | -1165 | -1468 |
| | No public assistance | 133 | 616 | -3158 | -2409 |
| | | -474 | -492 | -1985 | -2362 |
| **Banking status:** | Checking or savings account | -66 | 666*† | -2693* | -2093 |
| | | -391 | -373 | -1597 | -1900 |
| | No checking or savings account | 28 | -77† | -2315 | -2363 |
| | | -469 | -458 | -1900 | -2261 |

See explanatory notes at end of exhibit

**Table 8: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)***

| Subgroup | | Outcome: Real assets | Total assets | Total liabilities | Net worth |
|---|---|---|---|---|---|
| **Homeownership:** | Owned home | 4565 | 1050 | -2620†† | 3670 |
| | | -11066 | -12830 | -6452 | -10746 |
| | Did not own home | 6818* | 5100 | 6131**†† | -1031 |
| | | -3639 | -3979 | -2926 | -2907 |
| **Race/ethnicity:** | African-American | 9784** | 8813* | 6955* | 1858 |
| | | -4668 | -5020 | -3693 | -4060 |
| | Caucasian | 5968 | 1773 | 1452 | 322 |
| | | -5642 | -7136 | -4588 | -5897 |
| **Age:** | 35 or younger | -255†† | -4831†† | 2099 | -6930†† |
| | | -4587 | -6069 | -3788 | -4783 |
| | 36 or older | 12979**†† | 13348**†† | 6248* | 7100†† |
| | | -5645 | -6052 | -3756 | -4999 |
| **Gender:** | Male | 4319 | -1995 | 7323 | -9318† |
| | | -7918 | -9295 | -7886 | -8190 |
| | Female | 6565 | 5358 | 3444 | 1914† |
| | | -4083 | -4739 | -2866 | -3789 |
| **Family structure:** | No children | 16865 | 17212 | 4325 | 12887 |
| | | -10350 | -10630 | -5893 | -8350 |
| | Single parent | -117 | -3014 | 1179 | -4193 |
| | | -4434 | (5850 | -3297 | -4854 |
| | Two or more adults with children | 9632 | 7054 | 8884 | -1830 |
| | | -5948 | -6801 | -5759 | -5532 |
| **Education:** | High school or less | 5214 | 5086 | 3115 | 1970 |
| | | -4749 | -5234 | -4556 | -4460 |
| | Some college | 7822 | 4636 | 6234 | -1598 |
| | | -5193 | -6272 | -3721 | -4894 |
| | Four-year degree or higher | 760 | -1089 | -3864 | 2775 |
| | | -10348 | -11657 | -9797 | -9285 |
| **Financial assets:** | $200 or less | -5739 | -6768 | -3061 | -3708 |
| | | -3696 | -4235 | -3316 | -3329 |
| | $201 to $1,100 | 14465 | 12507 | 5474 | 7033 |
| | | -6910 | -7846 | -4223 | -6215 |
| | $1,101 or more | 9122 | 5689 | 9907 | -4218 |
| | | -5184 | -6234 | -4356 | -5438 |
| **Public assistance:** | Public assistance | -158 | -1913 | 4081 | -5994*†† |
| | | -4147 | -4673 | -3703 | -3388 |
| | No public assistance | 11049** | 8640 | 4218 | 4422†† |
| | | -5012 | -5830 | -3211 | -4684 |
| **Banking status:** | Checking or savings account | 7591**† | 5498† | 5051* | 448†† |
| | | -3703 | -4435 | -2761 | -3523 |
| | No checking or savings account | -3590† | -5954† | -2750 | -3204†† |
| | | -5480 | -6204 | -5767 | -4843 |

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

[b] Statistical significance of the F-test is indicated as follows: ††† = p<.0.01; †† = p<0.05; † = p<0.10.

**Appendix Table 1: Treatment Effects—Point Estimates, Confidence Intervals, and Minimum Detectable Effects**

| | | Estimated treatment effect | | | | | As % of control mean | |
|---|---|---|---|---|---|---|---|---|
| | | | | 95 percent confidence interval | | | | |
| Outcome | Control mean | Point estimate[a] | Standard error of estimate | Lower bound | Upper bound | MDE[b] | Upper bound | MDE |
| **Ownership of real assets (month 48)** | | | | | | | | |
| Homeownership | 0.429 | **0.062** | 0.031 | 0.017 | 0.123 | 0.077 | 29 | 18 |
| Business ownership | 0.105 | -0.002 | 0.020 | -0.038 | 0.042 | 0.050 | 40 | 47 |
| Other property ownership | 0.047 | 0.01 | 0.018 | -0.025 | 0.044 | 0.045 | 94 | 95 |
| Vehicle ownership | 0.903 | -0.004 | 0.023 | -0.041 | 0.048 | 0.057 | 5 | 6 |
| **Home purchase or related activities (months 1-48)** | | | | | | | | |
| Home purchase | 0.302 | **0.089** | 0.037 | 0.016 | 0.162 | 0.092 | 54 | 30 |
| Looked through home listings in newspaper | 0.764 | 0.045 | 0.032 | -0.017 | 0.107 | 0.080 | 14 | 10 |
| Drove to look at houses for sale | 0.751 | 0.033 | 0.032 | -0.030 | 0.096 | 0.080 | 13 | 11 |
| Attended open house | 0.503 | **0.079** | 0.039 | 0.004 | 0.155 | 0.097 | 31 | 19 |
| Talked about borrowing money for a home | 0.559 | 0.067 | 0.039 | -0.009 | 0.144 | 0.097 | 26 | 17 |
| Cleared up old debts to apply for home loan | 0.592 | **0.117** | 0.038 | 0.043 | 0.192 | 0.094 | 32 | 16 |
| Talked with realtor about buying home | 0.681 | 0.034 | 0.035 | -0.035 | 0.103 | 0.087 | 15 | 13 |
| Intensity of home search | 4.15 | **0.465** | 0.185 | 0.101 | 0.828 | 0.460 | 20 | 11 |
| **Home improvement (months 1 to 48)** | | | | | | | | |
| Any home improvement | 0.343 | 0.053 | 0.031 | -0.007 | 0.114 | 0.077 | 33 | 22 |
| **Business startup or related activities (months 1 to 48)** | | | | | | | | |
| Business startup or purchase | 0.106 | -0.016 | 0.022 | -0.059 | 0.027 | 0.055 | 25 | 52 |
| Talked about starting his/her own business | 0.501 | 0.025 | 0.037 | -0.047 | 0.099 | 0.092 | 20 | 18 |
| Prepared business plan or similar document | 0.217 | 0.001 | 0.031 | -0.059 | 0.061 | 0.077 | 28 | 36 |
| Applied for business license | 0.124 | -0.001 | 0.024 | -0.048 | 0.047 | 0.060 | 38 | 48 |
| Talked about obtaining business loan | 0.153 | -0.009 | 0.026 | -0.060 | 0.041 | 0.065 | 27 | 42 |

## Appendix Table 1: Treatment Effects—Point Estimates, Confidence Intervals, and Minimum Detectable Effects *(Continued)*

| Outcome | Control mean | Estimated treatment effect: Point estimate[a] | Estimated treatment effect: Standard error of estimate | 95 percent confidence interval: Lower bound | 95 percent confidence interval: Upper bound | Minimum detectable effect[b] | As % of control mean: Upper bound of conf. int. | As % of control mean: Minimum detectable effect |
|---|---|---|---|---|---|---|---|---|
| **Education or training (months 1 to 48)** | | | | | | | | |
| Took non-degree course | 0.373 | 0.009 | 0.035 | -0.060 | 0.079 | 0.087 | 21 | 23 |
| Took course toward degree | 0.502 | -0.010 | 0.033 | -0.076 | 0.055 | 0.082 | 11 | 16 |
| Finished job training program with certificate | 0.373 | -0.001 | 0.035 | -0.071 | 0.068 | 0.087 | 18 | 23 |
| Graduated from school | 0.220 | -0.037 | 0.029 | -0.094 | 0.020 | 0.072 | 9 | 33 |
| Any postsecondary education or training | 0.690 | -0.002 | 0.030 | -0.061 | 0.057 | 0.075 | 8 | 11 |
| **Components of net worth (month 48)** | | | | | | | | |
| Liquid assets | 2257 | -55 | 367 | -775 | 664 | 912 | 29 | 40 |
| Retirement savings | 1760 | 581 | 338 | -83 | 1244 | 840 | 71 | 48 |
| Other financial assets | 2608 | -2650 | 1608 | -5806 | 506 | 3996 | 19 | 153 |
| Total financial assets | 6624 | -2124 | 1890 | -5834 | 1586 | 4697 | 24 | 71 |
| Real assets | 39071 | 6310 | 3552 | -662 | 13283 | 8827 | 34 | 23 |
| Total assets | 45694 | 4186 | 4292 | -4239 | 12612 | 10666 | 28 | 23 |
| Total liabilities | 34847 | 4157 | 2672 | -1088 | 9402 | 6640 | 27 | 19 |
| Net worth | 10847 | 29 | 3433 | -6709 | 6767 | 8531 | 62 | 79 |
| **Employment and income (month 48)** | | | | | | | | |
| Employment | 0.781 | -0.053 | 0.028 | -0.107 | 0.002 | 0.070 | 0 | 9 |
| Monthly earnings | 1382 | -78 | 75 | -225 | 68 | 186 | 5 | 13 |
| Household receipt of public assistance | 0.362 | 0.009 | 0.033 | -0.055 | 0.073 | 0.082 | 20 | 23 |
| Monthly household income | 2256 | -118 | 151 | -415 | 179 | 375 | 8 | 17 |
| Household income-to-poverty ratio | 1.786 | -0.134 | 0.120 | -0.370 | 0.102 | 0.298 | 6 | 17 |

Notes:

a. Point estimates shown in bold are statistically significant at the 0.05 level.

b. Minimum detectable effects (or MDEs) are the minimum true effects detectable with 80 percent power at the 0.10 significance level (two-tailed test).