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Evaluation of the American Dream Demonstration

Impacts of IDAs on Participant Savings and Asset Ownership

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Taking the Measure of the American Dream Demonstration: An Assessment of Knowledge Building and Impacts in Applied Social Research

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Chapter One:
Introduction and Background

This paper presents findings on the effects of individual development accounts (IDAs) on the savings and asset accumulation of program participants.[1] IDAs are subsidized savings accounts targeted for special purposes – typically for homeownership, business capitalization, and postsecondary education, but also (under some programs) for home repair, vehicle purchase, and retirement. The subsidy is provided in the form of match funds to supplement the accountholder's own deposits and interest, when withdrawn for allowable asset purchases.

The effects of IDAs are estimated here from data collected at the Tulsa, Oklahoma IDA program operated by the Community Action Project of Tulsa County (CAPTC) under the American Dream Demonstration (ADD).[2] To enable unbiased estimation of program effects, program applicants were randomly assigned to a treatment group, which was allowed to enter the program, or to a control group, which was not. Sample members in both the treatment and control groups were interviewed at three intervals: immediately prior to random assignment (Wave One), approximately 18 months after random assignment (Wave Two), and approximately 48 months after random assignment (Wave Three). An additional data source was the Management Information System for Individual Development Accounts (MIS IDA), which provided information on IDA transactions for treatment group accountholders.

1.1 Objectives of the Evaluation

The primary participant outcomes examined in this paper pertain to the forms of asset building that are specifically promoted by CAPTC's IDA program:

- Home ownership or improvement
- Business ownership
- Educational advancement or training
- Retirement savings

1 For the complete analysis on which this paper is based, see Mills (2004).

2 CAPTC implemented two IDA programs under the auspices of the American Dream Demonstration. The experimental program evaluated here is referred to as the "large-scale" CAPTC program. An earlier "small-scale" pilot program, which enrolled its first participant in February 1998, was nonexperimental, as were the ADD programs established at twelve other sites: two in Chicago (IL) and one each in Oakland (CA), Washington (DC), Indianapolis (IN), Berea (KY), Kansas City (MO), Ithaca (NY), Portland (OR), Austin (TX), Barre (VT), and Fond du Lac (WI). For a complete description of this demonstration, see Schreiner (2002).

A second set of participant outcomes pertain more generally to participants' assets, liabilities, and net worth.[3]

The first set of outcomes above is of obvious interest, to investigate the extent to which IDAs serve to promote the intended asset ownership and asset-building activities. The second set of outcomes was included in this research to understand the possible near-term and longer-term effects of IDAs on the economic behavior of participants. To make deposits into their IDAs, account holders may reduce their consumption expenditures, increase their work hours, draw down other assets ("asset shifting"), pay off debts more slowly, or increase their indebtedness.[4] Although consumption expenditures were not a focus of this study, the other indicated behavioral responses would be reflected in particular measured outcomes. For example, a slower pay-off of debts would lead to higher liabilities. Asset-shifting into IDAs from other liquid forms (such as checking or savings accounts) would suggest little or no observed change in total liquid assets (a measure that includes IDA balances); a shifting of assets into IDAs from financial or real assets would suggest reductions in these non-liquid outcome measures.

If IDAs have their intended result in promoting asset purchases among participants, positive treatment effects should be observed on homeownership, home repair, business ownership, educational attainment, and/or retirement savings. The effect on net worth will depend, however, on whether the IDA contributions are financed primarily from "new" savings into the IDA (that is, by reduced consumption expenditures or increased work effort) or are financed by shifting assets or saving or by increasing debt. These offsetting effects would mitigate (and possibly even reverse) the boost to participants' net worth.[5] For this reason, successful use of IDAs could well entail reductions in assets and/or increases in liabilities, and thus may not increase net worth in the short term. The impacts of IDAs are most likely to be evident through the estimated main effects on the incidence of asset purchases during the course of the demonstration, rather than through changes in net worth.

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3 The experimental evidence presented here focuses on the effects of IDAs on the savings, asset ownership, and asset-building activities of low-income individuals. As this research did not include any measurement of the costs of implementing and operating an IDA program, this paper makes no attempt to assess the cost-effectiveness of IDAs. For a detailed analysis of the costs associated with operation of CAPTC's IDA program, see Schreiner (2000).

4 For example, accountholders who make deposits into their IDAs without having increased their income or reduced their consumption may put more purchases on consumer credit cards and thus increase their liabilities.

5 Under some scenarios, measured net worth could actually decline for a successful IDA participant. Consider, for instance, a participant who uses their IDA in combination with a student loan to enroll in a college course. The investment in human capital would not increase measured assets. (There could be a *decline* in assets if the IDA deposits were funded by reducing liquid assets or shifting other savings.) With the student loan causing an increase in liabilities, measured net worth would drop.

1.2 Features of the Experimental IDA Program

The key features of the experimental IDA program at CAPTC were as follows:

- *Allowable uses*: a participant's withdrawals from their IDA qualified for the program match only if used for home purchase or repair/improvement,[6] post-secondary education or training,[7] microenterprise startup or expansion, or retirement (funding an IRA).
- *Match rate*: Authorized withdrawals for home purchase were matched at 2:1. For all other allowable uses, the match rate was 1:1.
- *Income and asset eligibility*: At program entry, participants must have been currently employed, with family income below 150 percent of the federal poverty guideline. There was no eligibility limit on assets.
- *General financial education*: Prior to a matched withdrawal, participants were required to take 12 hours of financial education, by attending six two-hour classes (called Money Management sessions). At least two classes (four hours) were required before opening an account.
- *Asset-specific training*: Prior to a matched withdrawal, participants were required to take additional training specific to the type of intended asset purchase (with approximate hours as follows): 8 hours for home purchase, 2 hours for post-secondary education, 16 hours for business startup, and 2 hours for retirement.[8] There was no similar requirement for withdrawals to be used for existing microenterprises or for home repair/improvement. A business plan was required, however, for those planning to use funds for an existing business.
- *Minimum expected deposits*: 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. Noncompliance with this guideline, however, did not normally result in dismissal from the program.
- *Minimum period prior to matched withdrawal (wait period)*: Participants could not make a matched withdrawal until six months after their account opening date (having also completed the six financial education sessions and any asset-specific training, as indicated above).
- *Unauthorized withdrawals*: Participants were allowed to make up to three unauthorized withdrawals every twelve months.

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6 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.

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

8 During the demonstration, the required asset-specific training for homebuyers increased from 5 to 8 hours, as a result of CAPTC's lengthening the class time for its basic homeownership course.

- *Time interval within which matchable deposits could be made (time cap)*: Matching funds accrued to the accountholder for all IDA deposits made within 36 months after the account opening. The accountholder then had up to six additional months within which to make final matched withdrawals.[9] At the end of this grace period, any remaining account balance could be rolled over (at the participant's request) into a Roth individual retirement account (IRA), with match provided at a 1:1 rate.

- *Maximum savings amount subject to match (savings target)*: For each account year (measured from the month of account opening), up to $750 in deposits was subject to match, when withdrawn for an allowable use. Over the three-year savings period, the maximum was thus $2,250. On a monthly basis, this amounted to $62.50. Participants who exceeded the $750 in one year could carry forward their excess matchable savings into the following year. (For example, someone who saved $1,000 in one year could apply the $250 excess to the next year.) The reverse was not true, however. That is, someone who saved $500 in one year was not allowed to accumulate $1,000 in matchable deposits the following year.

- *Maximum available match amount (match cap)*: CAPTC used an annual match cap. Consistent with the above-described annual savings target of $750, one's accrued match was limited each year to $1,500 for those planning to make a home purchase (2:1 match) and $750 for those planning for other allowed uses (1:1 match).[10]

- *Maximum asset accumulation (sum of matchable savings and match payments)*: Participants making full use of their accounts over three years could accumulate $6,750 for home purchase ($2,250 in savings plus $4,500 in match) or $4,500 for other allowed uses ($2,250 in savings plus $2,250 in match).

- *Form of payment of match funds*: At the time of a matched withdrawal from the accountholder's own balance, the match was provided in the form of a check made out to the vendor (e.g., a home mortgage lender).

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9 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 (i.e., less than 36 months in total) 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).

10 Other IDA programs with multi-year savings periods use a "lifetime match cap" whereby the participant's accrued match is subject to a total cumulative limit instead of a yearly maximum.

Chapter Two:
Sample Enrollment and Data Collection

This chapter describes the implementation of the experimental research at the Tulsa IDA site, including the enrollment of the research sample and the survey data collection.

2.1 Sample Recruitment, Baseline Data Collection, and Random Assignment

The enrollment of the research sample proceeded over the course of 15 months. The first cases were recruited by CAPTC in late October 1998 and were randomly assigned by Abt Associates in early November 1998; the last-recruited cases were randomly assigned in early December 1999.

Those applicants found program-eligible by CAPTC staff were referred to Abt Associates for a baseline interview. CAPTC referred applicants to Abt on a twice-weekly basis from late October 1998 through mid-March 1999 and on a once-weekly basis from mid-March 1999 through early December 1999. A total of 1,147 cases were referred by CAPTC to Abt. Within two weeks of their application review, each eligible applicant was contacted by Abt Associates' telephone survey staff for the baseline (Wave One) interview. A total of 1,103 applicants (96 percent of the referred applicants) completed the Wave One interview.

Of the 1,103 individuals enrolled into the research sample, 537 were assigned to the treatment group, and 566 were assigned to the control group. The treatment-control ratio was 5:6 from late October 1998 through mid-March 1999 and 1:1 thereafter.[11]

Under the experimental design, the following restrictions applied to the control group:

- Control group members were not allowed to enter the IDA program.
- 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 (First-Time Home Buyer's) program, which provided 1:1 matching funds for down payment and closing costs.
- Control group members were not allowed to participate in the "Lease-Purchase" program offered by CAPTC's Housing Department.

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[11] 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.

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 nonrespondents, after September 2003).

2.2 Follow-up Data Collection

We describe below the collection of follow-up survey data (at approximately 18 and 48 months after random assignment) for treatment and control group members and the collection of data on IDA use by treatment group members.

Month 18 Follow-up Survey

To obtain the information necessary to estimate interim treatment effects, members of the enrolled research sample were interviewed in a Wave Two follow-up survey timed to occur approximately 18 months after random assignment. Unlike the Wave One survey, which was conducted entirely by telephone, the Wave Two survey employed a mixed-mode format. For each sample member the interview was first attempted by telephone. If telephone attempts were unsuccessful, the case was referred to one of several Tulsa-area field interviewers who then attempted to arrange an in-person interview at the respondent's residence. Interviews were conducted using computer-assisted telephone and personal interviewing methods.

Wave Two interviewing began in May 2000, when the earliest enrollees reached their 18th month after random assignment. Cases were released for interviewing 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.) Wave Two interviewing was completed in August 2001. A total of 933 interviews were completed, 810 by telephone and 123 by field interviewers, for an overall completion rate of 84.6 percent. Respondents received a $35 incentive payment for completing the interview.

Month 48 Follow-up Survey

A final round of follow-up interviews was conducted as sample members neared the end of the four-year demonstration period, to obtain the information necessary to estimate final program effects. Sample members were interviewed in this Wave Three follow-up survey approximately 48 months after random assignment. As at Wave Two, the month 48 survey employed a mixed-mode format. The interwave tracking efforts included tracking letters mailed to each sample member approximately 26, 33, and 45 months after random assignment.[12]

Wave Three interviewing began in January 2003, with cases again released for interviewing according to the timing of their random assignment. Interviewing was completed in September 2003.

[12] The response rate for the month 45 tracking letter was 40.1 percent, for the entire research sample of 1,103.

A total of 840 interviews were completed, 765 by telephone and 75 by field interviewers. As later described, these 840 cases comprise the "analysis sample" on the basis of which program impacts were estimated. The overall Wave Three completion rate was 76.2 percent. As in Wave Two, the respondents received a $35 incentive payment.

Post-Interview Verification of Survey Data

The difficulties of obtaining accurate household data on components of net worth and other financial circumstances, especially for low-income households, are well documented in survey literature.[13] Extensive efforts were made in this study to ensure the accuracy of the survey data, especially for financial variables. Criteria were first established, in collaboration with CSD, for identifying data values that might have been misreported by respondents or misrecorded by interviewers. Data items of the following types were identified for verification:

- Items for which the respondent's recorded value fell outside a specified range.
- Items for which the change in the recorded values between one wave and the next, for the same survey question, fell outside a specified range.
- Items for which there was an apparent inconsistency in the responses to related survey questions within the same wave.

For all individual data items identified by these criteria, measures were taken to verify the recorded data values. For all Wave One and Two data values identified for verification, the associated survey respondent was asked to correct or confirm the previously recorded value by responding to questions on an individualized Survey Quality Form. This form was mailed to sample members with their month 45 tracking letter. For those not responding to this mail-out, the Survey Quality Form was then administered at the close of the Wave Three interview. In conducting the Wave Three interviews, the interviewers immediately verified all out-of-range item-specific values, as detected through 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 was subsequently mailed to the respondent.

Use of Administrative Data on IDA Accountholders

The other data source for this analysis was the Management Information System for Individual Development Accounts (MIS IDA). This software system, developed and supported by the Center for Social Development, was used by all ADD sites and is used by numerous other IDA programs nationwide.[14] The MIS IDA information for the Tulsa site was provided to Abt Associates by CSD. For the treatment group members, this data set provided month-by-month information on IDA

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

[14] IDA demonstration projects that receive federal funding under the Assets for Independence Act are required to use MIS IDA or a similar software package.

transactions, including account holder deposits, withdrawals, accrued interest, and match funds, through October 31, 2003.[15]

2.3 Outcome Variables

The American Dream Demonstration provided financial incentives (through the match funds) and program services (through the financial education, asset-specific training, and case management) to encourage low-income people to save money and to use those savings for targeted investments. As noted earlier in this chapter, the matchable uses included home purchase or improvement, educational coursework or training, microenterprise development, and retirement.

In this evaluation we analyzed the impact of the demonstration on both the targeted investments and on a set of additional outcomes that measure individuals' total net worth and major components of net worth. Outcomes that are measured at a point in time, such as individuals' net worth, were evaluated at the time of both the month 18 and month 48 follow-up surveys. A smaller set of outcomes was measured over a specified time interval. For example, questions about home repair or improvement were posed as "Since [the date of the last interview], did you or anyone in your household do any maintenance or improvement to your home or apartment?" These outcomes were evaluated over three time intervals: the entire interval between the baseline survey and the month 48 survey; the early interval between the baseline survey and the month 18 survey; and the later interval between the month 18 survey and the month 48 survey.[16]

The first set of outcomes, relating to the investments promoted by the program, included the following variables:

- ***Homeownership***, measured as:
  - Homeownership at month 18
  - Homeownership at month 48
  - Purchase of a home over each time interval, months 1-48, 1-18, and 19-48 (analyzed only for persons who did not own a home at baseline)
- ***Home repair or improvement***, a binary outcome coded as 1 ("yes") only for respondents who owned a home at the time of the survey, who had undertaken home repairs or improvements, and who indicated that they paid for at least part of the cost of these repairs or improvements, evaluated separately for each time interval.
- ***Business ownership***, measured as:
  - Ownership of a business at month 18 and month 48

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15 Note that, at the end of October 2003, the program was phasing out its operations, but had not completely closed down. Some treatment group members were allowed to make matched withdrawals during the following several months. These transactions are not included in the analysis reported here.

16 The analysis of impacts at the 18th month was restricted to those members of the analysis sample who completed interviews at both month 18 and month 48. This included 764 of the 840 observations in the analysis sample.

- Purchase or startup of a business over each time interval (analyzed only for persons who did not own a business at baseline)

- ***Educational advancement or training***, a binary outcome evaluated over each time interval, coded as 1 (yes) if the respondent engaged in any of the following activities:
  - Took (or was still taking) a course that did not count toward a degree or certificate
  - Took (or was still taking) a class that did count toward a degree or certificate
  - Completed a job training program with a certificate
  - Graduated from school with a degree

The second set of outcomes measured total net worth and the components of net worth. Each of these outcomes was measured at month 18 and at month 48. They included:

- ***Liquid assets***

  Amount held in checking and savings accounts (including IDA balances), money-market accounts, and certificates of deposit

- ***Retirement savings***

  Amount held in personal retirement plans such as IRAs, and retirement plans through work such as 401(k) plans, 403(b) plans, or other pension accounts

- ***Other financial assets***

  Additional forms of savings or investment, such as stocks, bonds, mutual funds, educational accounts, savings held with family or friends or at home, savings in Christmas or vacation clubs, or any other kinds of savings

- ***Total financial assets***

  Sum of liquid assets, retirement savings, and other financial assets

- ***Real assets***

  Market value of the primary residence, any other properties, vehicles, and business assets

- ***Total assets***

  Sum of total financial assets and real assets

- ***Total liabilities***

  Total indebtedness, including mortgages; vehicle loans; credit card debt; personal loans from banks, friends, or relatives; business loans from banks, friends, or relatives; medical bills; student loans; installment loans on furniture and major appliances; consolidation loans or bills owed to collection agencies; over-due rent payments; overdue phone or utility bills; overdue bills on record or book clubs; any other bills more than one month past due

- ***Net worth***

  Total assets minus total liabilities

Chapter Three: Estimates of IDA Program Impacts

In this chapter we analyze whether the savings and investments achieved by the treatment group differed significantly from the outcomes for the control group over the four-year follow-up period. Impact estimates are presented for the entire sample and for subgroups.

3.1 Impact Estimates for the Entire Analysis Sample

In this section we present estimates of the impacts of the IDA program, using data on the entire analysis sample of 412 treatment group members and 428 control group members. Program impacts on ownership of real assets are presented in Exhibit 3.1. Impacts measured at both months 18 and 48 are presented. ***The program effect on homeownership at month 48 was statistically significant and substantial in magnitude.*** The homeownership rate for treatment group members at month 48 was found to be 6.2 percentage points higher than the control group mean of 42.9 percent (proportionally, 14 percent higher).

Home purchase (as distinct from homeownership) was measured for sample members who did not own a home at baseline.[17] Consistent with the above-cited finding on homeownership, the results indicate that the IDA treatment had a significant positive impact on home purchase during months 1 to 48. See Exhibit 3.2. The incidence of home purchase among treatment group members was 8.9 percentage points higher over the demonstration period than the 30.2 percent rate among control group members. This represents a proportional impact of more than 29 percent. The pattern of point estimates and significance levels for the early and later periods of the demonstration indicates that the impact was concentrated in the latter interval, during months 19 to 48.

The treatment had a marginally significant positive effect on the incidence of any home improvement during months 1 to 48. For the treatment group, the home improvement rate was 5.3 percentage points higher than the control group mean of 34.3 percent.

The analysis of business startup or purchase was restricted to sample members who did not own a business at baseline. There were no significant program impacts on this outcome measured over the entire period of months 1 to 48 or separately during months 1 to 18 or months 19 to 48.

Similarly, for education and training there were no statistically significant program effects on the most general measure—encompassing any form of postsecondary education or training--over the entire demonstration period or separately during months 1 to 18 or months 19 to 48. This measure included completion of any postsecondary degree or certificate program, any degree-related or certificate- related coursework, or any other coursework.

[17] To the extent that some of these individuals may have previously owned homes, the home purchase outcome does not necessarily indicate first-time homeownership.

Exhibit 3.1: 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.

Exhibit 3.2: 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[b]									
Any home purchase	643	0.302	0.089 ** (0.037)	579	0.166	-0.006 0.030	579	0.148	0.092 *** (0.032)
Home Repair or Improvement[c]									
Any home repair of improvement	840	0.343	0.053 * (0.031)	764	0.208	0.022 (0.026)	764	0.304	0.038 (0.031)
Business Startup or Purchase[d]									
Any 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)
Postsecondary Education or Training									
Any postsecondary education or training	840	0.690	-0.002 (0.030)	764	0.569	0.004 (0.035)	764	0.514	0.045 (0.035)

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

[b] Sample restricted to those who did not own a home at baseline.

[c] Outcome measure indicates whether the sample member owned their primary residence and engaged in any repair or improvement of the home.

[d] Sample restricted to those who did not own a business at baseline.

Treatment effects on financial outcomes are presented in Exhibit 3.3. These outcomes include liquid assets, retirement savings, other financial assets, total financial assets, real assets, total assets, total liabilities, and net worth. Impacts measured at the time of follow-up months 18 and 48 are presented. No impacts are significant at the 0.05 level.

Among these full-sample outcomes, only one treatment effect is even marginally significant (i.e., significant at the 0.10 level): a *negative* effect on other financial assets (stocks, bonds, and other forms of savings) at month 18. For this financial outcome, treatment group members had $361 less at month 18 than the control group mean of $683. This finding may indicate short-term asset shifting; with treatment group members converting these financial assets into IDA deposits to maximize their use of match funds. Alternatively, those in the treatment group who made a major asset purchase with their IDA may simply have needed to also draw down their financial assets to afford the asset purchase.

To summarize, the experimental evidence indicates significant treatment effects on several key outcomes related to homeownership. The full-sample rate of homeownership, the full-sample rate of home improvement, and the rate of recent home purchase among non-homeowners were significantly higher in the treatment group than in the control group. No significant impacts were found on the advancement of postsecondary education or training over the demonstration period, nor on the rate of business ownership. No statistically significant treatment effects (at the 0.05 level) were found on financial outcomes, including net worth and its major components, when measured over the full sample.

3.2 Impact Estimates by Subgroup

Exhibit 3.4 summarizes the patterns of estimated effects by subgroup.[18] For each subgroup, the exhibit shows those outcomes for which the treatment effect was estimated to be significantly positive (+++, ++, or +) or negative (---, --, or -), according to the level of statistical significance (0.01, 0.05, or 0.10). As previously in this chapter, we focus here on the effects that were significant at the 0.05 level or better.

Subgroups with impacts on financial outcomes, but not on homeownership or other targeted forms of asset purchase

Those 36 or older showed a significant increase in both real assets and total assets, but this was not accompanied by an increase in homeownership or purchase of other targeted assets. It may be that IDAs prompted these cases to increase their savings and then make unmatched withdrawals for the purchase of assets such as vehicles.

[18] See Section 4.3 in Mills (2004) for a detailed discussion of the subgroup estimates. Impacts by subgroup were not estimated for home repair or improvement, an outcome linked to homeownership.

Exhibit 3.3: 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.

Exhibit 3.4: Summary of Estimated Impacts at Month 48 by Subgroup

Subgroup (Baseline characteristic)	Outcome: Homeownership	Business Ownership	Any Education/ Training
Total sample	++		
Homeownership			
Owned home			
Did not own home	++		
Race/ethnicity			
African-American, non-Hispanic	++		
Caucasian, non-Hispanic		+	
Age			
35 or younger			
36 or older			
Gender			
Male			
Female			
Family structure			
No children			
Single parent			
Two or more adults with children	++		
Education			
High school or less			
Some college			
Four-year degree or higher			
Total financial assets			
$200 or less			
$201 to $1,100			
$1,101 or more	++		
Public assistance receipt			
Public assistance			
No public assistance	++		
Banking status			
Checking or savings account	++		
No checking or savings account			

See explanatory notes at end of exhibit.

Exhibit 3.4: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)*

Subgroup (Baseline characteristic)	Outcome: Liquid Assets	Retirement Savings	Other Financial Assets	Total Financial Assets
Total sample		+		
Homeownership				
Owned home				
Did not own home				
Race/ethnicity				
African-American, non-Hispanic		++	-	
Caucasian, non-Hispanic				
Age				
35 or younger				
36 or older		+		
Gender				
Male			--	-
Female		+		
Family structure				
No children				
Single parent				
Two or more adults with children			--	
Education				
High school or less		+		
Some college			-	
Four-year degree or higher	--			
Total financial assets				
$200 or less				
$201 to $1,100				
$1,101 or more				
Public assistance receipt				
Public assistance			-	
No public assistance				
Banking status				
Checking or savings account		+	-	
No checking or savings account				

See explanatory notes at end of exhibit.

Exhibit 3.4: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)*

Subgroup (baseline characteristic)	Outcome			
	Real Assets	Total Assets	Total Liabilities	Net Worth
Total sample	+			
Homeownership				
Owned home				
Did not own home	+		++	
Race/ethnicity				
African-American, non-Hispanic	++	+	+	
Caucasian, non-Hispanic				
Age				
35 or younger				
36 or older	++	++	+	
Gender				
Male				
Female				
Family structure				
No children				
Single parent				
Two or more adults with children				
Education				
High school or less				
Some college				
Four-year degree or higher				
Total financial assets				
$200 or less				
$201 to $1,100				
$1,101 or more				
Public assistance receipt				
Public assistance				-
No public assistance	++			
Banking status				
Checking or savings account	++		+	
No checking or savings account				

Explanatory notes:
Entries in the exhibit indicate the outcomes and subgroups for which the treatment effect was estimated to be statistically significant at the 0.01 level (+++ or ---), the 0.05 level (++ or --), or the 0.10 level (+ or -). "Any education/training" pertains to activities during months 1-48. All other outcomes pertain to month 48.

Subgroups with impacts on homeownership, but not on other asset-building or financial outcomes

Cases with two or more adults and children and *cases with $1,101 or more in financial assets* showed an increased rate of homeownership, but no other financial effects related to home purchase or other targeted asset purchases. These findings may indicate that home purchases were enabled by matched IDA withdrawals and a liquidation of real assets.[19]

Subgroups with impacts on homeownership and related financial outcomes

Other subgroups experienced positive impacts on their rate of homeownership, combined with effects on other financial outcomes that were seemingly related to the financing of their home purchase. For *those not owning a home at baseline*, the positive effect on homeownership was combined with an increase in their amount of liabilities, presumably a result of their home mortgage loans. For *those not on public assistance* and *those with a checking or savings account* at baseline, increased homeownership was combined with an increase in real assets, suggesting little or no liquidation of real assets. Instead, these cases may have financed their home purchase in part through a drawdown of financial assets and in part through increased liabilities.[20]

Subgroups with impacts on homeownership and retirement savings

African-Americans showed positive treatment effects on two targeted investments, homeownership and retirement savings. These effects were sizable in proportion to the respective control group means, more than 40 percent for homeownership and more than 85 percent for retirement savings. African-Americans, who comprised more than 40 percent of the analysis sample, thus appear to have benefited from IDAs to an extent well beyond that of other major subgroups. As noted earlier in this chapter, the pronounced impact on homeownership for the African-American subgroup may reflect the fact that these sample members were disproportionately non-homeowners at baseline.

3.3 Further Examination of Impact Estimates

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

Control group crossover

Throughout the demonstration, efforts were made through discussions with the IDA program staff at CAPTC to enforce the program rules that prohibited control group members from receiving down payment assistance from CAPTC's Housing Department. Nonetheless, it was learned at the close of the demonstration that three control cases (in the analysis sample) had received such financial assistance in purchasing homes through the Housing Department's First-Time Home Buyer's (FTHB)

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[19] If first-time homes were purchased *without* liquidating real assets, one would have expected to find a positive treatment effect on real assets, as this measure includes the value of one's primary residence.

[20] Although the estimated impacts on financial measures were not statistically significant for this subgroup, the estimated impact on total financial assets was negative, consistent with this interpretation.

Program. This was a program that consisted of an orientation session, financial analysis, counseling seminar, and financial assistance for down payment and closing costs. As noted in Chapter Two, control cases were not prohibited from receiving homeownership counseling from CAPTC, but they were not to receive direct financial assistance through the FTHB program.

Based on the survey responses to questions asked at Waves Two and Three, a total of 30 control cases in the analysis sample (including the three identified above) indicated that they received some services from the First-Time Homebuyer's Program during the demonstration period. At each of these waves, control cases were asked whether "you or a member of your household received any of the following services from CAPTC" since the previous interview, with one of the listed response categories as "the First-Time Homebuyer's Program, including help with down payment and closing costs." Among the control cases in the analysis sample, 30 responded affirmatively to this item at either Wave Two or Wave Three. CAPTC lists of those attending the FTHB seminars included 9 of these 30 respondents, plus one additional control case not among the 30.

The survey data and administrative data thus indicate that a total of 31 control group cases in the analysis sample received some services from the FTHB program, including 3 cases who received down payment assistance in purchasing homes. The 31 cases represent 7.2 percent of the 428 control cases in the analysis sample.

The concern raised by these cases is that, if they indeed received (and potentially benefited from) services that were part of the IDA program intervention, the impact analysis may have understated the true treatment effect. The appropriate adjustment for such "crossover" is commonly referred to as the Bloom correction.[21] Simply stated, this correction calls for the estimated treatment effect to be multiplied be the factor 1/(1-r), where r is the rate of crossover. In this instance, the adjustment factor is 1/(1-0.072) or 1.08. Because this factor applies to both the point estimate and its standard error of the treatment effect, the adjustment does not alter the statistical significance of the estimated effect. Note that this adjustment is almost certainly an over-correction, as it assumes that *all* 31 cases received services that were intended solely for the treatment group. Under the rules of the demonstration, those control cases that received homeownership counseling, but not direct financial assistance toward a home purchase, should *not* be regarded as crossovers. The adjusted estimate should therefore be viewed as an upper bound on the treatment effect, not as a more accurate estimate.

Applying the adjustment factor (1.08) to the point estimate of 0.062 (or 6.2 percent) for the treatment effect on the rate of homeownership, one obtains an adjusted treatment effect of 0.067 (or 6.7 percent). The same adjustment factor (1.08) would be the appropriate value to use in adjusting all estimated full-sample treatment effects.

Because this adjustment is small in magnitude, and almost certainly over-corrects for the 31 instances of crossover in the control group, we have not applied the adjustment to other results shown throughout this report.

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[21] See Bloom (1984).

Sensitivity of impact estimates to outlier data values

Chapter Two described the efforts undertaken in this study to verify outlier data values of three types: those identified as out-of-range for a specific item response, those identified as seemingly inconsistent with other information collected from the same respondent *at the same wave*, and those identified as seemingly inconsistent with information collected from the same respondent *on the same item at a different wave*. In a separate analysis, we examined whether the estimated treatment effects are sensitive to the data revisions that resulted from the post-interview verification. 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 the full-sample analysis of major outcomes, we 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.[22] We summarize here the findings of this sensitivity analysis.[23]

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 significant using the original data (positive for liabilities and negative for net worth). Neither effect was 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 reported in Exhibit 3.3) 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.

We also have examined whether the estimated treatment effects are sensitive to alternative methods of dealing with item-specific out-of-range values (i.e., the first type of outlier identified above). Specifically, we considered one alternative rule for handling out-of-range values for the independent variables (covariates, as measured at Wave One): imputing these values to their respective group mean (treatment or control group). 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 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 assets, financial assets, total assets, total liabilities, and net worth.

The findings of this sensitivity analysis can be summarized as follows. If one imputes out-of-range covariates to their respective group mean, point estimates of the treatment effect become larger (more positive or less negative) 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 not significant on financial assets, total assets, and net worth. Using strategies that

[22] No similar comparisons were done for impact estimates at the subgroup level.

[23] See Appendix C of Mills (2004) for the details of this analysis.

combine the imputation of out-of-range covariates with the deletion of observations having out-of-range dependent variables, the estimated effects on total assets and net worth also become statistically significant and positive. Generally, however, we concluded that such strategies yield results that are less valid than the findings presented in this chapter, for the following reason. 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. It thus appeared counterproductive to rely on estimation methods that would delete such observations from the analysis.[24]

Minimum detectable effects and precision of estimates

As described in this chapter, significant program effects were found on a number of key program outcomes—most importantly, on the rate of homeownership. Across the wide array of estimated effects, however, the predominant finding was a lack of significance. It is important to consider such findings 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 as statistically significant, taking into account the sample size and the inherent variability of the outcome measures.[25] To the extent feasible, one always wants the minimum detectable effects to be within the plausible range of impacts for the intervention in question.

A separate analysis was conducted in which MDEs were calculated for the full-sample impacts on all major outcomes measured at month 48 or reflecting asset-building activities during months 1 to 48.[26] Based in this analysis of MDEs, the following observations can be made:

- For many of the outcomes under investigation, our ability to detect a treatment effect was reasonably good. Specifically, 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, home repair/improvement, education/training, real assets, total assets, and total liabilities.
- 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, business ownership, business startup or purchase, 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 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.

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24 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.

25 In statistical terms, the MDEs presented here are the minimum true effects detectable with 80 percent power.

26 See Appendix D of Mills (2004) for the details of this analysis.

- 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. Indeed, it is important to note that in a study of this kind one's ability to detect effects is primarily a matter of sample size, not of data quality.

Although we cannot completely rule out program effects in those cases where the estimated impacts were statistically insignificant, the 95 percent confidence intervals (computed in the analysis of MDEs) 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 relevant 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 impact on any postsecondary education or training was probably less than 10 percent of the control mean.

3.4 Summary and Conclusion

Among the estimated program impacts presented in this chapter, the major findings (statistically significant at the 0.05 level or better) are as follows:

- **Increase in homeownership:** There was a large positive impact on the rate of homeownership. At month 48 the rate of homeownership was 6.2 percentage points higher for the treatment group members than the 42.9 percent rate among their control group counterparts. (The proportional increase in homeownership was thus 14 percent.) The favorable effect on homeownership at month 48 was pronounced among the following subgroups (as defined at baseline): those who did not own a home (i.e., those for whom homeownership at month 48 implied a home purchase during months 1 to 48), African-Americans, families comprised of two or more adults with children, those with more than $1,100 in total financial assets, those not on public assistance, and those with a checking or savings account.

- **Increase in real assets:** Because home value typically comprises a large share of the real assets owned by low-income households, it is not surprising that a positive impact on real assets was found for several of the subgroups that experienced an increase in homeownership – African-Americans, those not on public assistance, and those with a checking or savings account – and also for those 36 years or older at baseline.

- **Increase in retirement savings:** The treatment yielded a positive impact on retirement savings at month 48 for African-Americans.

- **Decrease in liquid assets and other financial assets:** The treatment reduced liquid assets for those with a four-year college degree or more. There was also a negative effect on other financial assets for two subgroups: males and families comprised of two or more adults with children. For this last subgroup, where the treatment had a positive impact on homeownership, the decline in financial assets may reflect the family's need to draw down such assets to purchase a home.

- **Increase in total assets and total liabilities:** The treatment had a positive impact on total assets at month 48 for those 36 years or older at baseline, consistent with the above-mentioned increase in their real assets. The treatment was found to increase total liabilities at month 48 for those who were not homeowners at baseline, presumably a result of the higher mortgage debt associated with their higher rate of home purchase.

Under the basic estimating approach used throughout this report, no sample-wide impacts (with statistical significance at the 0.05 level) were found for other outcomes, including home repair or improvement, business startup or purchase, postsecondary education or training, or net worth. As noted earlier, under some alternative specifications involving the imputation or deletion of outlier values and observations, the impacts on net worth and its components became positive and statistically significant. We do not, however, regard those alternative approaches as appropriate in light of the steps taken to verify the data outliers. For those outcomes on which no significant program impact was found in the basic analysis, one should not regard this as definitive evidence that the program has no impact on those outcomes. As with any impact evaluation, especially one focusing on the financial circumstances of low-income households, the ability to detect program effects is limited by sample size and the inherent variability of the outcomes under study.

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