Evaluation of the American Dream Demonstration

Final Evaluation Report

August 19, 2004

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Acknowledgments

The authors of this report wish to acknowledge the many individuals whose guidance, support, and cooperation have been essential in conducting the research. Lisa Mensah and Kilolo Kijakazi of the Ford Foundation and Benita Melton of the Charles Stewart Mott Foundation have provided not only the necessary funds to conduct the work, but also important guidance on developing and presenting the findings. The following staff of the Center for Social Development (CSD) of Washington University in St. Louis played a central role in providing technical guidance and oversight for the study: Michael Sherraden (CSD founder and Director), Lissa Johnson, Mark Schreiner, and Margaret Clancy. Also integral to the research, especially in its early planning stages, were a number of current and former staff of the Corporation for Enterprise Development (CFED): Robert Friedman (CFED founder and Board Chair), Brian Grossman, Ray Boshara, and Rene Bryce-Laporte. The staff of the Community Action Project of Tulsa County all fully committed themselves to implementing the experimental design and facilitating the data collection. Most importantly, this included Steven Dow (CAPTC Executive Director), Jennifer Robey, Kimberly Cowden, Virilyaih Davis, Danny Snow, and Rachel Trares. William Gale of the Brookings Institution has offered very helpful advice on many technical aspects of the data analysis and on the interpretation of findings.

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Executive Summary

This report presents findings on the effects of individual development accounts (IDAs) on the savings and asset accumulation of low-income individuals. IDAs are subsidized savings accounts that are targeted for special purposes – typically for homeownership, business capitalization, and postsecondary education, but also (under some programs) for home repair or improvement, vehicle purchase, and retirement. The subsidy is provided in the form of funds that match the account holder's withdrawals for allowable asset purchases, at match rates that can exceed 1:1.

This research is based on an evaluation conducted at the Tulsa, Oklahoma IDA program operated by the Community Action Project of Tulsa County (CAPTC). CAPTC is a multi-service community action agency whose clients are low-income residents of the Tulsa metropolitan area. The Tulsa program was one of a series of local IDA projects initiated under the American Dream Demonstration (ADD). The Corporation for Enterprise Development (CFED) organized the demonstration and orchestrated its funding and implementation. The evaluation proceeded under the direction and oversight of the Center for Social Development of Washington University in St. Louis. Funding for the evaluation was provided by the Ford Foundation and the Charles Stewart Mott Foundation.

To allow unbiased estimation of program effects, program applicants in the Tulsa site were randomly assigned to a treatment group, which was allowed to enter the program, or to a control group, which was not. By randomizing the assignment of program-eligible individuals to the treatment and control groups, one can then attribute to the IDA treatment any systematic differences between the two groups in their subsequently measured outcomes.

The findings presented here are derived from data collected on a total sample of 1,103 program-eligible applicants. Of this total research sample, 537 applicants were randomly assigned to the treatment group; the other 566 applicants were assigned to the control group. Controls were not allowed to participate in either the IDA program or CAPTC's other homeownership assistance programs during the four-year demonstration period.

Context of This Study

Initiatives to encourage savings are becoming an increasingly prominent area of domestic policy in the U.S. Recent research suggests that Americans, particularly those at lower incomes, appear to be under-saving and under-investing in their economic futures.[1] Any programs that might effectively promote saving are generally viewed as offering favorable long-term economic effects, allocating greater resources to investment and thus boosting future incomes and living standards.

An emerging view is that carefully designed incentives for asset accumulation can also serve important social goals. In particular, efforts to encourage saving among low-income households may potentially be a more effective way to combat the cycle of poverty than more conventional income support policies. Prominent among the proposed strategies to encourage asset accumulation are

[1] See, for instance, Wolff (2001).

individual development accounts.[2] As a means of encouraging asset ownership and human capital development, it is also hypothesized that IDAs promote favorable changes in individual attitudes, family-related behavior, and community-oriented involvement.

During the 1990s a number of policy developments and private initiatives focused increasing attention and resources on IDAs as an instrument of economic and social policy. These include state and federal efforts linked to the 1996 federal welfare reform—the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). 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. The number of IDA programs operating nationwide was estimated in 2001-2002 to exceed 500, with more than 20,000 IDA account holders.[3]

Only a small portion of IDA programs has included any evaluation effort. The American Dream Demonstration has represented the first systematic attempt to measure the impacts of individual development accounts on patterns of savings and asset ownership.

Features of the Experimental IDA Program

In the Tulsa experimental IDA program, the allowable account uses were home purchase or repair/improvement, post-secondary education, microenterprise startup/expansion, or retirement. Authorized withdrawals were matched at 2:1 for home purchase and at 1:1 for all other allowable uses. To be eligible for the program, participants had to be employed, with family income below 150 percent of the federal poverty guideline.

Prior to a matched withdrawal, participants were required to take 12 hours of general financial education and (in most instances) additional training specific to the type of intended asset purchase. Participants were expected to make a minimum monthly deposit of $10 in at least nine months of each year. Under the program design, matching funds accrued to the accountholder for all IDA deposits made within 36 months after the account opening. The accountholder then typically had up to six additional months within which to make final matched withdrawals. Any remaining account balance could then be rolled over (with 1:1 match) into a Roth individual retirement account (IRA).

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 matchable savings amount was thus $2,250. 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). At the time of a matched withdrawal, 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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[2] See Sherraden (1991).

[3] This estimate, cited by the New America Foundation on its website (www.AssetBuilding.org), is derived from a survey conducted by the Corporation for Enterprise Development.

Evaluation Design

For the purpose of collecting baseline and follow-up data, the sample members were interviewed at three intervals: first at application ("baseline") and then at approximately 18 and 48 months after entry into the demonstration. An additional data source was the Management Information System for Individual Development Accounts (MIS IDA), which provided information on IDA transactions for treatment group account holders. The analysis sample—those for whom baseline and month 48 survey data were available—included 840 individuals: 428 in the control group and 412 in the treatment group.

The analysis of treatment effects examined a series of outcome measures relating to asset ownership and asset-building activities, net worth and its components, and other aspects of economic well-being. The first set of outcomes, relating to the major forms of asset-building specifically promoted by the IDA program, includes home ownership, home search activities, home improvement or repair, business ownership, and educational attainment.

A second set of outcomes measured total net worth and the components of net worth. Each of these outcomes was measured both at month 18 and at month 48. They include liquid assets, retirement savings, other financial assets, total financial assets (sum of liquid assets, retirement savings, and other financial assets), real assets, total assets (sum of total financial assets and real assets), total liabilities, and net worth (total assets minus total liabilities).

A third set of outcomes included additional measures of material well-being that were not explicitly captured elsewhere: vehicle ownership, ownership of property other than primary residence, employment, monthly earnings, monthly household income, household income-to-poverty ratio, and household receipt of public assistance.

Characteristics of the Analysis Sample at Baseline

More than three-fourths of the members of the analysis sample (80 percent) were female. At baseline (i.e., at the time of random assignment), nearly half (48 percent) of sample members were single parents with children; 30 percent were two-adult households with children; and the remaining 23 percent lived in households without children. Just over one-quarter of sample members (26 percent) were married, and 40 percent had never been married.

The average sample member was 36 years old at the start of the demonstration. About half of the sample members (47 percent) were Caucasian, and 41 percent were African-American. More than two-thirds (69 percent) of sample members had some post-secondary education, including 4 percent who were college graduates. Another quarter (26 percent) of the sample had a high school diploma or GED. Consistent with the requirements of program participation, nearly all sample members (99 percent) were employed at the time of the baseline survey.

Regarding the baseline financial circumstances of sample members, 23 percent owned a home, 7 percent owned a business, and 3 percent owned other property (real estate other than a primary residence). Over 80 percent owned a vehicle. Also at baseline, 58 percent already had a savings account, and 71 percent had a checking account. The sample averages for the major financial

measures were $909 for liquid assets, $751 for retirement savings, $456 for other financial assets, and $2,735 for net worth. Average monthly household income was $1,463.

For the analysis sample, statistical tests showed that the control group was well matched to the treatment group, with some exceptions. The number of baseline characteristics with a significant treatment-control difference was within the range expected by statistical fluctuation. (In the analysis, we controlled statistically for all observed treatment-control differences.)

Use of IDAs by the Treatment Cases

Among the 537 sample members assigned to the treatment group, 85 percent opened an IDA. Nearly one-quarter (24 percent) of treatment cases opened their accounts in the first month after random assignment; almost one-half (48 percent) opened accounts in the first three months. The average period over which participants kept their accounts open was 38 months. An account was considered closed when the balance was reduced to zero and there were no subsequent transactions. (Some account closures represent dropouts; others represent successful program completion.)

Based on MIS IDA data through the reporting period ending in October 2003, 34 percent of participants had made at least one matched withdrawal before closing their account. Fifty-three percent of participants had closed their account without ever making a matched withdrawal. The remaining 14 percent were still ongoing in the program, with positive balances remaining in their accounts.

Thirty-nine percent of participants had made at least one matched withdrawal by the end of October 2003. (This includes the 34 percent who had closed their accounts after one or more matched withdrawals and another 5 percent who remained ongoing.) Among those with at least one matched withdrawal, the amount of matched withdrawals averaged $1,480 per participant; matched withdrawals plus matches averaged $3,431 per participant. The largest share of matched withdrawals—35 percent—was for home repair or improvement. The next most common use of withdrawals was for home purchase, at 26 percent. Education/training and retirement savings were tied for the third most common use, at 17 percent each. The remaining 5 percent of matched withdrawals were for small business.

Estimated Effects of IDAs on Savings and Asset Accumulation

The findings from this evaluation provide important new evidence that an IDA program can have significant favorable impacts on asset-building among low-income persons. Most notably, as described below, the Tulsa IDA program resulted in a significantly higher rate of homeownership among treatment group members. The results show not only that participants were capable of understanding and responding to the incentives provided by IDAs, but also that – given incentives – they were capable of planning and implementing their financial goals over a multi-year time horizon, and of navigating the complexities associated with home purchase.

The results also provide compelling evidence that the medium-term effects of IDAs can be quite different than the short-term effects. After 18 months of follow-up, there was only one significant

effect, on debt repair among the baseline non-homeowners. There were no significant impacts on home purchases, on other activities preparatory to home purchase, or on any of the other program-targeted forms of asset purchase. After 48 months of follow-up, however, significant impacts were found among those who did not own homes at baseline, on their rate of home purchase and on the intensity of home search activities. There was also a significant effect on the rate at which sample members took non-degree educational courses during months 19 to 48. For the types of asset accumulation that are supported by IDA programs – including long-term, major investments such as homeownership – a multi-year time horizon appears to be necessary for program results to emerge.

The findings also indicate, as described below, that the positive effects on homeownership were concentrated among particular subgroups. Although no sample-wide effects were found on major categories of assets or on total assets, total liabilities, or net worth at month 18 or month 48, some subgroups did show significant treatment effects on these financial outcomes at month 48. No significant effects were found on business ownership.

Increase in Homeownership

The treatment had a significant positive effect on the rate of homeownership. After 48 months, the homeownership rate was 6.2 percentage points higher in the treatment group than in the control group. Proportionally, this was a 14 percent increase, relative to the homeownership rate for the control group (42.9 percent at month 48).

The favorable effect on homeownership was pronounced among the following subgroups (as defined at baseline): those who did not own a home, 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. Additionally, the extent to which baseline non-homeowners subsequently engaged in activities preparatory to home purchase (such as attending an open house or repairing credit to apply for a mortgage) was significantly higher among those in the treatment group.

Increase in Real Assets and Total 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. ("Real assets" includes the market value of the primary residence, any other properties, vehicles, and business assets.) The treatment had a positive impact on total assets at month 48 for those 36 years or older at baseline, consistent with the increase in their real assets. ("Total assets" include liquid assets, retirement savings, other financial assets, and real assets.)

Increase in Retirement Savings

The treatment had a positive impact on retirement savings at month 48 for African-Americans. The treatment effect for these participants amounted to $1,081 more in retirement savings than the $1,267 accumulated by their control group counterparts, an 85 percent increase. ("Retirement savings" includes amounts 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.)

Decrease in Liquid Assets and Other Financial Assets, Increase in Liabilities

Some subgroups, in making deposits to their IDAs and then making asset purchases, appear to have tapped other forms of assets or to have increased their liabilities in the process.

The treatment effect was negative on liquid assets for those with a four-year college degree or more. It was also negative 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 in order to purchase a home. ("Liquid assets" includes the IDA balance and amounts held in checking and savings accounts, money-market accounts, and certificates of deposit. "Other financial assets" includes 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.)

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 homeownership. ("Total liabilities" includes all indebtedness, such as mortgages, vehicle loans, credit card debt, personal loans, business loans, student loans, installment loans, consolidation loans, and overdue bills.)

Increase in Educational Attainment

There was a significant positive treatment effect on one educational outcome—whether one had taken a non-degree educational course during the latter part of the demonstration, during months 19 to 48. The percentage who took such a course during this time interval was 6.6 percentage points higher for treatment group members than the 19.1 percent for the control group, a proportionate increase of 35 percent. There were no statistically significant effects on other measures of educational attainment.

Multiple Impacts among African-Americans

It is noteworthy that African-Americans showed positive treatment effects on two targeted investments, homeownership and retirement savings, and on the value of their real assets. These effects were sizable in proportion to the respective control group means, more than 40 percent for both homeownership and real assets 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 other major subgroups. The pronounced impact on homeownership for the African-American subgroup may reflect the fact that these sample members were disproportionately non-homeowners at baseline.

Chapter One
Introduction and Background

This experimental evaluation is one component of the American Dream Demonstration (ADD), a comprehensive effort to assess the effects of individual development accounts (IDAs) on participant outcomes. IDAs are subsidized savings accounts that are 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 funds that match the amount of an account holder's withdrawal for an allowable asset purchase, with match rates that can exceed 1-to-1.

This report presents findings on the effects of a particular IDA program model on the savings and asset accumulation of program participants, as estimated from data collected at the Tulsa, Oklahoma ADD site operated by the Community Action Project of Tulsa County (CAPTC).[4] To allow 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 group 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 source of data was the Management Information System for Individual Development Accounts (MIS IDA), which provided information on IDA transactions for treatment group accountholders.

The American Dream Demonstration was orchestrated 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. With evaluation funding from the Ford Foundation and the Charles Stewart Mott Foundation, Abt Associates implemented random assignment, conducted the multi-wave survey data collection, and then analyzed the survey data and MIS IDA data. The findings reported here provide estimates of the impact of IDAs on participant savings and asset accumulation over a four-year follow-up period.

1.1 Objectives of the Evaluation

Initiatives to encourage savings are becoming an increasingly prominent focus of economic and social policy in the United States and other countries. Recent research suggests that Americans, particularly those at lower incomes, appear to be under-saving and under-investing in their economic futures.[5]

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[4] 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).

[5] See, for instance, Wolff (2001).

Programs that promote saving are generally viewed as offering favorable long-term economic effects, allocating greater resources to investment and thus boosting future incomes and living standards.

An emerging view is that carefully designed incentives for asset accumulation can also serve important social goals. In particular, efforts to encourage savings among low-income households, as through IDAs, are viewed by some as a more effective way to combat the culture of poverty than more conventional income support policies. In addition to fostering asset ownership and human capital development, IDAs are hypothesized to promote family-related behavior, community participation, and civic involvement.[6]

During the 1990s a number of policy developments and private initiatives focused increasing attention and resources on IDAs as a tool of public policy. These include state and federal efforts linked to the 1996 federal welfare reform—the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), under which IDAs are an allowed use of funds provided to states for Temporary Assistance to Needy Families (TANF). More recently, IDA programs now receive federal support through the Assets for Independence Act of 1998, Community Services Block Grants, 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. The number of IDA programs operating nationwide is estimated to exceed 500, with more than 20,000 IDA account holders.[7]

Only a small portion of IDA programs have included any evaluation effort. The American Dream Demonstration represents the first systematic attempt to measure the impacts of individual development accounts on patterns of savings and asset ownership.

As will be explained in detail in Chapter 3, the primary participant outcomes examined in this report pertain to the forms of asset building that are specifically promoted by CAPTC's IDA program:

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

A second set of participant outcomes pertain more generally to participants' assets, liabilities, and net worth. A final set of outcomes measure employment and household income.[8]

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6 See Sherraden (1991).

7 These estimates, cited by the New America Foundation on its website (www.AssetBuilding.org), are derived from a 2001-2002 survey conducted by the Corporation for Enterprise Development.

8 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, we make no attempt in this report 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).

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 and third sets of outcomes are 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 inadvertently increase their indebtedness.[9] Although consumption expenditures are not a focus of this study, the other indicated behavioral responses would be reflected in particular measured outcomes. For example, increased work effort would lead to higher monthly earnings, and a slower pay-off of debts would lead to higher liabilities. Asset-shifting could be observed in one of two ways: a shifting of assets into IDAs from other liquid forms (such as checking or savings accounts) would suggest little or no observed change in total liquid assets; a shifting of assets into IDAs from non-liquid categories would suggest reductions in financial assets or real assets.

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.[10] For this reason, successful use of IDAs could well entail reductions in assets and/or increases in liabilities, and may not increase net worth. The impacts of IDAs are thus 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.

1.2 Features of the Experimental IDA Program

CAPTC is a multi-service community action agency that targets the low-income population of the Tulsa metropolitan area. The organization was founded in 1973 and described itself in 1998, at the start of this demonstration, as follows:[11]

> "The Community Action Project of Tulsa County (CAPTC, formerly known as Project Get Together) is a comprehensive anti-poverty agency with a 24-year history of providing a variety of services to low-income people. CAPTC's mission is to help individuals and families in economic need achieve self-sufficiency

[9] 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.

[10] 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 increasing liabilities, measured net worth would drop.

[11] Community Action Project of Tulsa County (1998).

through emergency aid, medical care, housing, community development, education, and advocacy in an atmosphere of respect. Last year, our various programs served nearly 18,000 low-income households."

"CAPTC focuses intently on its mission: to help individuals and families in need achieve self-sufficiency. All programs and services – current and potentially future – are evaluated and assessed based on their capacity to contribute to the accomplishment of our self-sufficiency directive."

"One of the major priorities which the Board of Directors has established for CAPTC's future program expansion is the development of alternative financial services to those currently available to our low income clients. One of those new services is the Individual Development Accounts program."

CAPTC's IDA program was implemented in partnership with the Bank of Oklahoma, which held the IDAs and distributed regular monthly statements to clients. Participants had sole deposit and withdrawal authority regarding their IDA, into which they made their own deposits. 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.

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

- *Allowable uses*: To qualify for the program match, a participant's withdrawal from their IDA was to be used for home purchase or repair/improvement,[12] post-secondary education,[13] 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 eligibility*: At program entry, participants must have been currently employed, with family income below 150 percent of the federal poverty guideline. (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.
- *Asset eligibility*: There was no eligibility limit on assets.

[12] 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.

[13] 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.

- *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.[14] There was no similar requirement for withdrawals to be used for existing microenterprises or for home repair. 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.
- *Interest rate earned on deposits*: 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.
- *Account fees*: The Bank of Oklahoma waived all normal fees charged to open or maintain accounts.[15]
- *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.
- *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.[16] At the end of this "grace period," any remaining

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[14] 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.

[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.

[16] 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).

account balance could be rolled over (at the participant's request) into a Roth individual retirement account (IRA), with match provided at the 1:1 rate for retirement.

- *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" design. 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 (at a 2:1 match rate) and $750 for those planning for other allowed uses (at a 1:1 match rate).[17]
- *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).

CAPTC's IDA program received its funding from a number of sources. These included the Corporation for Enterprise Development, which administered the funds supporting ADD (for both site operating costs and match funds) from its eleven foundation sponsors.[18] Additional funding was also provided through funds raised by CAPTC from five in-state sponsors.[19] The City of Tulsa allocated some of its Community Development Block Grant (CDBG) funds to operating costs. A number of other in-state organizations also supported the implementation of the program through in-kind contributions.[20]

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[17] 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.

[18] These foundations were: Ford Foundation, Charles Stewart Mott Foundation, Joyce Foundation, F.B. Heron Foundation, John D. and Catherine T. MacArthur Foundation, Citigroup Foundation, Fannie Mae Foundation, Levi Strauss Foundation, Ewing Marion Kauffman Foundation, Rockefeller Foundation, and the Moriah Fund.

[19] These in-state sponsors were the Bank of Oklahoma, Kaiser Foundation, Zarrow Foundation, Federal Home Loan Bank of Topeka (Affordable Housing Program), and City of Tulsa HOME Funds. The first three provided support for both operating and match funds. The latter two provided match funds for home purchase.

[20] These in-state partners included: Metropolitan Tulsa Chamber of Commerce, Neighbor for Neighbor, Oklahoma State University Cooperative Extension Service, Robert B. Kerns and Associates, Rogers University, Tulsa Community College, Tulsa Housing Authority, Tulsa Technology Center, and Tulsa University's Economics Department.

To plan, develop, and implement the CAPTC program, a project team was formed. The team included CAPTC's Executive Director and IDA Program Coordinator and the following staff from the Bank of Oklahoma: the Project Manager, District Branch Manager, Community Reinvestment Act (CRA) Manager, Applications Support Manager, Compliance Officer, and Account Development Manager. Program design decisions were complex, given the need to meet the program requirements of ADD as established by CFED and CSD, and to take account of the organizational requirements and capabilities of both CAPTC and the Bank of Oklahoma. An advisory board for the IDA program was also established, to oversee the program's operations.

1.3 Organization of This Report

This report includes four chapters and five appendices, organized as follows. Chapter One introduces the study and its background. Chapter Two describes the enrollment of the research sample and the collection of sample data at the Tulsa site. Chapter Three provides a statistical profile of the analysis sample, with respect to baseline demographic characteristics and baseline values of outcome measures, for both the treatment and control groups. Chapter Four presents estimates of the effects of the IDA program on asset ownership and other participant outcomes. Appendix A shows information on the timing of sample enrollment and survey interviews. Appendix B provides an analysis of sample attrition. Appendix C examines the sensitivity of the impact estimates to the post-interview data verification efforts. Appendix D presents the minimum detectable effects for the impact estimates.

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. Abt Associates' role in this evaluation was to collect the experimental data at the Tulsa site and to estimate the effects of IDAs on participant outcomes that relate to savings and asset ownership. It is important to note that the 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.[21]

The foundation of this impact analysis is the random assignment of program-eligible IDA 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 allowed to participate in the IDA program (nor in CAPTC's other homeownership assistance programs) during the four-year demonstration period. Formally, the treatment in this context is thus the *offer* to participate in the IDA program. By randomizing the assignment of program-eligible individuals to the treatment and control groups, one can then attribute to the IDA treatment any systematic differences between the two groups in their subsequent outcomes.

2.1 Sample Recruitment 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. CAPTC used a variety of methods to distribute information about the IDA program among the Tulsa-area working poor population. These channels included:

- CAPTC's tax assistance program, which focuses on assisting individuals in applying for the federal Earned Income Tax Credit;
- CAPTC's homeownership assistance program;
- Distribution of flyers to clients of other local social service agencies, some of whom were represented on the IDA advisory board;
- Media outreach through press conferences, interviews with broadcast and print media, public service announcements, and radio advertising;
- Mailings to individuals calling CAPTC to enquire about the program; and
- Mass mailings of a postcard flyer to current and former CAPTC clients.

[21] For a description of the evaluation activities associated with ADD, see Appendix A in Schreiner et al. (October 2002).

Individuals interested in the program—which CAPTC referred to as the IDA Matched Savings Program—were asked to submit an application. This was used to identify those who appeared to be program eligible (i.e., currently employed with prior-year adjusted gross income of below 150 percent of the federal poverty guideline, based on their most recent federal income tax return). These individuals were then scheduled for a 45-minute "application review." During the interview, CAPTC staff members explained the IDA program, verified eligibility (using driver's license, Social Security card, federal tax returns, W-2 forms, and pay stubs), and described the procedures for baseline interviewing and random assignment. Applicants were asked to sign a consent form by which they:

- attested to the accuracy of the information that they had provided in applying for the program;
- indicated their understanding that participation in the demonstration was voluntary;
- provided their informed consent regarding random assignment (acknowledging the implications of being assigned to the control group); and
- authorized the release of financial information for evaluation purposes.

Those applicants found program-eligible by CAPTC staff were referred to Abt Associates for a baseline (Wave One) 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 survey staff for the baseline (Wave One) interview. The Wave One interviews were conducted by staff at the Abt Associates Telephone Center in Amherst, Massachusetts, using computer-assisted telephone interviewing. A total of 1,103 applicants (96 percent of the referred applicants) completed the Wave One interview.

Within a week of completing the baseline interview, the applicant was then randomly assigned by Abt Associates to the treatment or control group. The treatment-control ratio was 5:6 from late October 1998 through mid-March 1999, and then became 1:1 thereafter.[22] The first random assignment was made on November 2, 1998; the last was made on December 10, 1999. Abt Associates staff provided to CAPTC an updated weekly listing of applicants, showing the applicants most recently assigned to the treatment and control groups. CAPTC staff then notified each applicant of his/her assignment. Those in the treatment group were asked to call the CAPTC office to schedule their first two Money Management Sessions. Those in the control group were informed that they were not selected to enter the IDA program but would be compensated for their cooperation with Abt Associates in being interviewed on two occasions over the upcoming four years.

[22] 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.

A total of 1,103 individuals were enrolled into the research sample, with 537 assigned to the treatment group and 566 assigned to the control group. Appendix Exhibit A.1 shows the weekly pattern of referrals and random assignment.

Careful attention was given to ensuring that those randomly assigned to the treatment group were offered a uniform, well-described IDA program intervention and that those assigned to the control group were not allowed access to IDA program services. This was essential to ensuring that the experiment would provide a fair test of the IDA program and that the estimated treatment effects would be attributable to the program.

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

- 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.
- 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 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) from treatment and control group members and the collection of data on IDA use by treatment group members throughout the demonstration period.

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.

To maintain updated locating information on each sample case and thus enable a high response rate for the Wave Two survey, Abt Associates implemented interwave 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 for our records the following locating information:

- the respondent's address and telephone number;
- a second telephone number where they could be reached (if possible); and
- names, addresses, and telephone numbers of two friends or relatives outside their household who would know where to reach them (if necessary) at the time of the Wave Two interview.

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.[23]

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.

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.

Exhibit 2.1 shows the completion rate by treatment/control status. As expected, the completion rate was somewhat higher for treatment cases (86.0 percent) than for control cases (83.2 percent). Appendix Exhibit A.2 shows the completion rates by sample cohort. The completion rates were higher for the earlier-enrolled cohorts (90 to 96 percent for the cohorts enrolled between October

[23] The response rate for the 16th month tracking letter was 45.1 percent, for the entire research sample of 1,103.

1998 and April 1999) than for the later-enrolled cohorts (73 to 87 percent for the cohorts enrolled between May and December 1999).[24]

Exhibit 2.1: 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).

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.[25]

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. 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.

Exhibit 2.2 shows the completion rate by treatment/control status. As at Wave Two, the Wave Three completion rate was expectedly somewhat higher for treatment cases (76.7 percent) than for control cases (75.6 percent). The response rates by cohort are shown in Appendix Exhibit A.3. Also as in the previous wave, the completion rates were higher for the earlier-enrolled cohorts (79 to 92 percent for the cohorts enrolled between October 1998 and April 1999) than for the later-enrolled cohorts (63 to 77 percent for the cohorts enrolled between May and December 1999).

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[24] One possible explanation for this pattern of response rates is that the earlier enrollees (both treatment and control group members) were more closely connected to CAPTC through other program services than were the later enrollees. As explained in Chapter 1, the early IDA sample recruitment occurred largely through the referral of individuals already receiving services from CAPTC. Such individuals (whether in the treatment or control group) may then have been more responsive to the requests from CAPTC staff to cooperate with the survey data collection. It is also possible that the earlier enrollees tended to be individuals who were instinctively more motivated by financial incentives—first the prospect of IDA match funds (if assigned to the treatment group) and later (for both treatment and control cases) the prospect of a $35 incentive payment for competing a follow-up interview.

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

Exhibit 2.2: 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).

As described later (in Section 3.4), steps were taken in the impact analysis to address the imbalance in the analysis sample caused by the differential Wave Three response rates by cohort. (In specifying the estimating models, we included cohort dummy variables and treatment-cohort interaction terms to the estimating models.)[26]

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.[27] 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 for a specific survey question.
- Items for which the change in the recorded values between one wave and the next fell outside a specified range for a specific survey question.
- Items for which there was an apparent inconsistency in 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

[26] 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.

[27] 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).

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.[28]

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

2.3 Definition of 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 a targeted set of investments. As noted earlier in this chapter, the matchable uses included home purchase, home repairs and improvements, educational coursework or training programs, microenterprise development, and retirement.

In this evaluation we analyze the impact of the demonstration on both the targeted investments and on a set of additional outcomes that measure individuals' total net worth and the components of net worth. Outcomes that are measured at a point in time, such as individuals' net worth, are evaluated at the time of both the month 18 and month 48 follow-up surveys. Comparison of the two sets of impacts indicates whether the treatment had a short-term and/or longer-term effect on these "point-in-time" outcomes. A smaller set of outcomes is measured over a specified time interval. For example, questions about home improvement were posed as "Have you made any home improvements since [the date of the last survey]?" These outcomes are 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. Again, comparison of the early and later sets of impacts indicates whether the treatment had short term and/or longer term effects on each outcome.[30]

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[28] As discussed further in Section 4.4, Appendix C contains an analysis of the sensitivity of the impact estimates to the data revisions resulting from the post-interview verification efforts.

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

[30] 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.

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 search.*** Home search activities, although not matchable uses of participants' IDAs, represent important early steps towards homeownership. Home search is measured using six survey items, which were asked only of persons who did not own a home at the time of the survey. In addition, any person who did not own a home at baseline but bought a home over the time interval in question was classified as responding "yes" to each of the six home search questions. In addition to dummy variables (valued at 0 or 1) for each of the six individual home search questions, we constructed a seventh measure that captures the intensity of home search. This measure indicates the summed number of home search activities, plus one if the respondent had purchased a home over the time interval; the values thus range from 0 to 7. The six specific home search activities are:
  - Looked through home listings in the newspaper
  - Drove to look at houses for sale
  - Attended an open house
  - Talked to someone about borrowing money for a home
  - Cleared up old debts to apply for a home loan
  - Talked with a real estate agent about buying a home
- ***Home improvement or repair***. This outcome is coded as "yes" only for respondents who owned a home at the time of the survey, who had undertaken home improvements, and who indicated that they paid for at least part of the cost of these home improvements. Home improvement or repair is measured in two ways:
  - Any home improvement or repair over each time interval
  - Any "major" home improvement or repair (a repair for which the respondent paid over $200) over each time interval
- ***Business ownership***, measured as:
  - Ownership of a business at Month 18 and Month 48
  - Purchase or startup of a business over each time interval (analyzed only for persons who did not own a business at baseline)
- ***Business startup activity.*** Activities taken to enquire about or plan for starting a business, although not matchable IDA activities, are important preliminary steps toward business ownership. The following four types of activity were defined as outcomes (each measured for Months 1-48, Months 1-18, and Months 19-48):
  - Talked about starting his/her own business
  - Prepared a business plan or similar document
  - Applied for a business license
  - Talked to a banker or someone else about a business loan

- ***Educational attainment***, measured with the following four outcomes, evaluated over each time interval:
  - Took (or was still taking) a course that did not count towards a degree or certificate
  - Took (or was still taking) a class that did count towards a degree or certificate
  - Completed a job training program with a certificate
  - Graduated from school with a degree

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

- ***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 like 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

Finally, a third set of outcomes includes additional measures of material well-being that were not explicitly captured in any of the other outcomes:

- ***Vehicle ownership***
- ***Ownership of property other than primary residence***
- ***Employment***
- ***Monthly earnings***
- ***Monthly household income***
- ***Household income-to-poverty ratio***
- ***Household receipt of public assistance***

This final set of outcomes was designed to measure the impact of the treatment on general measures of economic well being that are not specifically targeted by the program, but may be affected by the program and are of interest to policy-makers and researchers.

Chapter Three
Statistical Profile of the Research Sample

This chapter describes the characteristics of the research sample, with particular attention to the analysis sample of 840 treatment and control cases on which program impacts have been estimated. A portrait of the demographic and economic characteristics and financial circumstances of this sample at the time of random assignment ("at baseline") provides the context within which the goals and achievements of the IDA program participants can be understood and evaluated.

As indicated in Chapter 1, the final (Wave Three) survey was administered to the survey sample during January-September 2003. The survey was timed so that each sample member was interviewed approximately 48 months after he or she entered the demonstration. Among the 1,103 individuals who were randomly assigned (the "baseline" sample), 840 (or 76 percent of the total) completed the Month 48 survey. The analysis sample used in this report consists of these 840 sample members, including 428 control group members and 412 treatment group members. Section 3.1 presents the baseline demographic and economic characteristics of the analysis sample. Section 3.2 discusses the baseline financial circumstances of these cases. Section 3.3 provides further analysis of sample balance—i.e., comparability between the treatment and control groups—as resulting from the process of random assignment and sample attrition. Section 3.4 focuses on treatment group members and their patterns of IDA participation over the course of the demonstration.

3.1 Baseline Demographic and Economic Characteristics of Analysis Sample

The IDA experiment at the Tulsa site was designed to test whether IDAs enable low-income persons to increase their savings and acquire assets. The basic criterion for program eligibility was that participants were employed with prior-year income below 150 percent of the poverty level. The program did not otherwise target particular demographic groups. The descriptive profile of the analysis sample thus indicates the characteristics of the Tulsa-area low-income workers who voluntarily applied for the announced "matched savings program" aimed at homeownership and other asset building.

Exhibit 3.1 presents the baseline demographic and economic characteristics of the analysis sample. The exhibit presents several pieces of information: the distribution (and mean value, in some instances) of each characteristic in the analysis sample overall, the distribution (and mean) separately for the control and treatment groups, the treatment-control difference for each baseline characteristic, and an indication of whether the difference was statistically significant (with one or more asterisks denoting the level of significance).

Exhibit 3.1: Baseline Demographic and Economic Characteristics of the Analysis Sample

	Control Group (n=428)	Treatment Group (n=412)	Difference[a]	Analysis Sample (n=840)
	Percent / Mean	Percent / Mean	(Treatment-Control)	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%

Exhibit 3.1: Baseline Demographic and Economic Characteristics of the Analysis Sample *(Continued)*

	Control Group (n=428)	Treatment Group (n=412)	Difference[a]	Analysis Sample (n=840)
	Percent / Mean	Percent / Mean	(Treatment-Control)	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.

The overwhelming majority of sample members – 80 percent – were female. Nearly half (48 percent) were single parents with children; 30 percent were two-adult households with children; and the remaining 23 percent lived in households without children. Just over one-quarter of sample members (26 percent) were married at the time of the baseline survey, and 40 percent had never been married.

The average sample member was 36 years old at the start of the demonstration. Nearly half of sample members (47 percent) were non-Hispanic Caucasian, and 41 percent were African-American. There were few Asians, Hispanics, or Native Americans in the sample. Sample members were most likely to have a high-school diploma or several years of college: just over one quarter (26 percent) 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). The percentage was small for college graduates (4 percent) and for those with neither a high school diploma nor GED (6 percent).

Consistent with the requirements of program participation, nearly all sample members (99 percent) were employed at the time of the baseline survey. In addition, 7 percent reported that they owned their own business. A higher percentage – 20 percent – reported that their household received some income from self-employment in the past month. This may reflect some uncertainty among respondents about whether operating a microenterprise should be counted as "owning a business". However, this higher percentage could also reflect the self-employment of other family members.

Consistent with the low-income levels of the sample, 43 percent reported that they received "some" or "a lot of" government assistance in making ends meet during the prior month. With 58 percent covered by health insurance, a large minority of these predominantly working individuals thus had no health insurance coverage. Average monthly household income was $1,463, with an average income-to-poverty ratio of 126 percent.

There were very few statistically significant differences between the treatment and control groups along any of these baseline characteristics, based on bivariate comparisons. Treatment group members were less likely than control group members to have been never married at baseline. In addition, treatment group members were more likely (than controls) to have two children, although there was no significant difference in the average number of children between the two groups. These differences are fewer in number than one would have expected based on chance alone. Overall, this bivariate tabulation indicates that the treatment and control groups were highly comparable in their baseline characteristics.

3.2 Baseline Financial Circumstances of Analysis Sample

Just as it is important that the treatment and control groups reveal few differences in baseline demographic characteristics, it is extremely desirable that the two groups be well matched on baseline values of the key financial outcome variables. The experimental design was intended to create a control group that would be comparable in its financial circumstances to the treatment group, particularly for those financial variables that the program intended to influence.

Exhibit 3.2 presents the baseline values for each of the point-in-time outcome variables, which were defined in Section 2.3. As in Exhibit 3.1, Exhibit 3.2 presents several pieces of information: the baseline value of each outcome separately for the control group and the treatment group, the treatment-control difference and a test for the statistical significance of the treatment-control difference, and the baseline value of the outcome for the sample as a whole.

At baseline, the sample averages for the major financial measures were as follows: $909 for liquid assets, $751 for retirement savings, $456 for other financial assets, and $2,735 for net worth. Just under one-quarter (23 percent) owned a home at baseline, about 7 percent owned a business, and 3 percent owned other property (real estate other than the primary residence). Over 84 percent owned a vehicle. We found that 71 percent had some money in a checking account, and 58 percent had some money in a savings account.

The average value of sample members' real assets at baseline was over $15,000. However, there were large differences between homeowners and non-homeowners (not shown in Exhibit 3.2). The average value of real assets was about $3,800 for non-homeowners, compared to approximately $53,000 for homeowners. A similar difference occurs for net worth, with homeowners having markedly higher baseline net worth than non-homeowners, approximately $24,000 versus -$3,800. Thus, although all sample members had low annual incomes prior to random assignment, there was considerable variation in the baseline wealth levels according to homeownership.

Exhibit 3.2: 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.

The baseline treatment-control difference was statistically significant in several instances—in number, no more than would be expected based on chance alone. On average, treatment group members had lower levels of liquid assets; however, they also had higher levels of retirement savings. There were no significant differences between the two groups in other financial assets, total financial assets, real assets, liabilities, or net worth. There was a small but significant treatment-control difference at baseline for one other outcome: ownership of other property (real estate other than the primary residence). Treatment group members were more likely to own such property. Note that, in estimating program impacts, the analysis controlled statistically for the observed baseline value of the outcome being analyzed.

3.3 IDA Participation Among Treatment Group Members

The sample members randomly assigned to the treatment group were given the opportunity to participate in the IDA program. In this section, we present information drawn from MIS IDA data on the extent to which these individuals opened IDAs and used them. These findings are not impact estimates, as we are not comparing the treatment group to the control group. However, the results are useful for understanding the dynamics of IDA saving and withdrawals among treatment group members. The findings presented here are for the entire treatment group, not just those who completed the month–48 follow-up survey. Chapter Four will then present estimates of the impact that access to the IDA program had on the economic well-being of individuals and their households, obtained by comparing outcomes between the treatment group and control group, for the analysis sample.

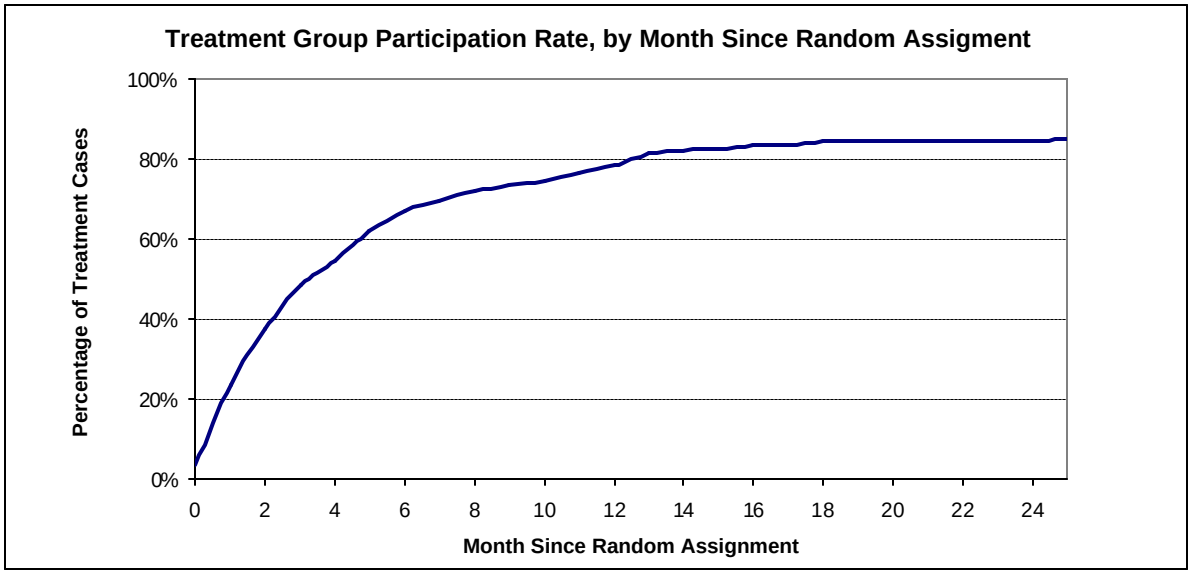
IDA Openings among the Treatment Group

Of the 1,103 sample members in the full research sample, 537 were randomly assigned to the treatment group and thus had the opportunity to open an IDA. A very high percentage of this group – 456, or 85 percent – chose to participate in the IDA program and open an account.[31] We refer to these individuals as "participants" and to their percentage of the treatment group as the "participation rate." (Those who never opened accounts may nonetheless have received some program services, such as the Money Management sessions.)

Exhibit 3.3 shows the upward trend in the participation rate, by month since random assignment. Participation among treatment cases rose steeply in their initial months after entering the demonstration. Nearly a quarter (24 percent) of all treatment cases opened an IDA in the first month of random assignment. Almost half of all treatment group members (48 percent) had opened an account within three months of random assignment. Thereafter, the pace of account openings moderated. By month 18, nearly all treatment group members who were ever to open an IDA had done so. No new accounts were opened after month 25.

[31] This number does not count as participants 16 treatment group members who opened an account but were subsequently found to be ineligible to participate. As detailed later, 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.

Exhibit 3.3



Length of Program Participation

Participants kept their accounts open, on average, for 38 months once opened. An account was considered closed (with participation thus having ended) when the balance was reduced to zero and there were no subsequent transactions. (As described later, some account closures represent dropouts; others represent successful program completion.) At 12 months after opening, 97 percent of accounts remained open; at 24 months, 87 percent remained open; at 36 months, 66 percent were still open; and at 48 months, 16 percent remained (see Exhibit 3.4). It should be noted that the demonstration was designed to last four years; it is thus impossible to know what percentage of participants would have kept their accounts open for longer than 48 months given on-going access to their IDA. A small number of participants (19 cases, or 4 percent of all 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.

Savings Accumulated by Participants

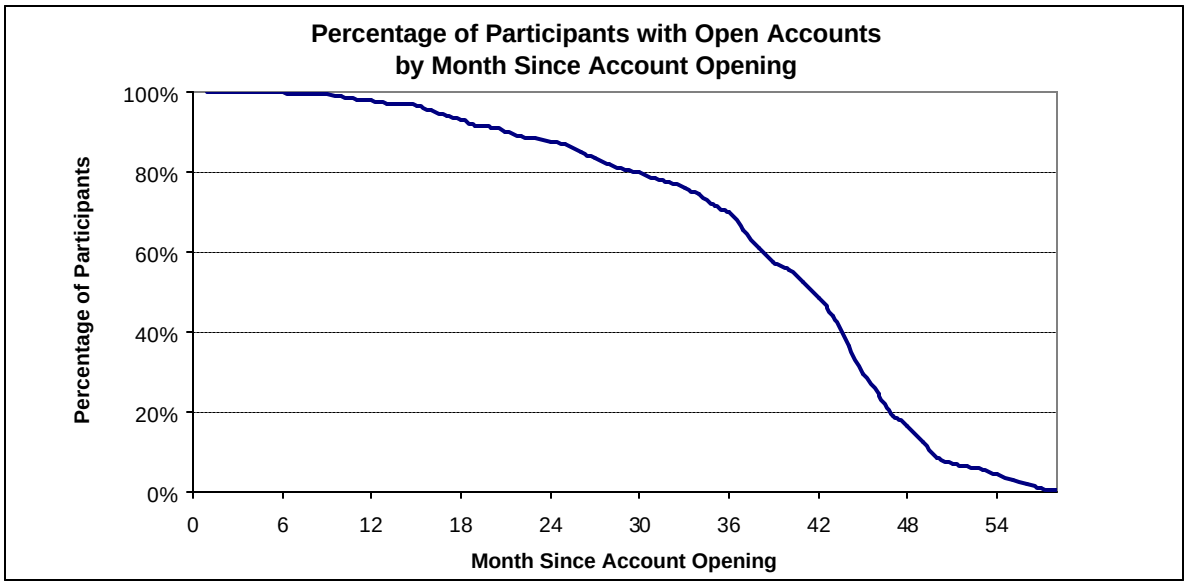
As mentioned above, the average length of participation was 38 months. Average ***gross deposits*** – all deposits, both matchable and unmatchable – were $2,150 per participant over the entire period. Two additional measures of savings performance among IDA participants are *net deposits* and *average monthly net deposits*.[32] For each participant, ***net deposits*** equal cumulative matchable deposits (including interest, but net of fees), minus unmatched withdrawals.[33] Note that deposits in excess of the matchable amount are not included, but that *matched* withdrawals to date are not subtracted from this measure. Thus, the net deposits measure captures the amount of *matched or matchable* deposits

[32] These measures, as developed by the Center for Social Development, are detailed in Schreiner (2002), Chapter 3.

[33] 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.

ever made into the IDA by a participant. For each participant, their ***average monthly net deposit*** is the average monthly amount deposited during their program participation, net of any unmatched withdrawals or excess (unmatchable) deposits. Stated otherwise, average monthly net deposits equals net deposits divided by months of participation.

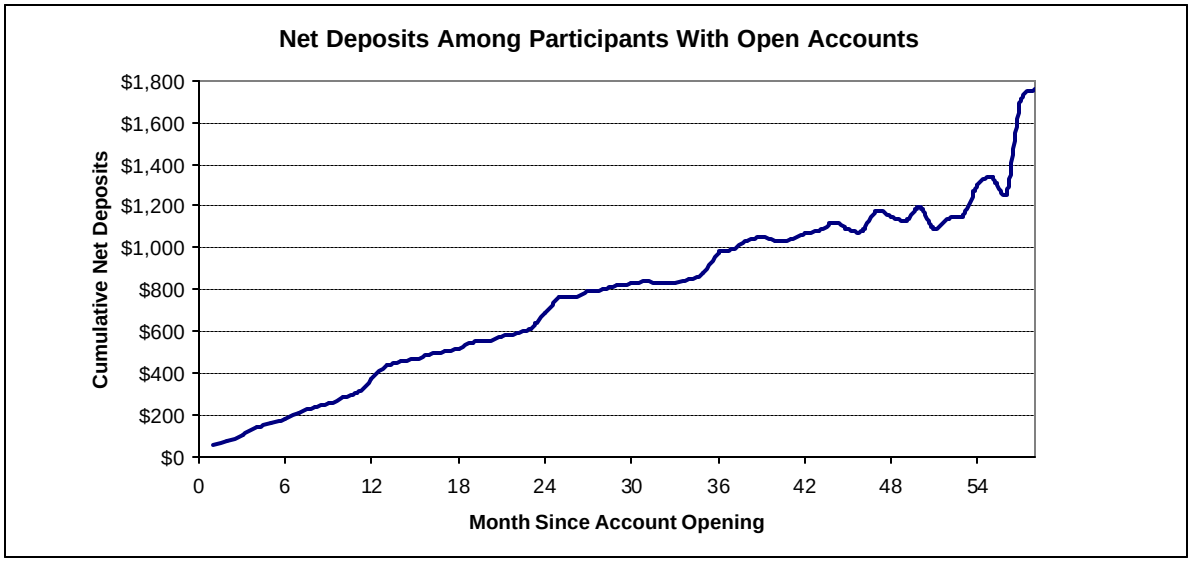
Exhibit 3.4



By the end of the reporting period (October 2003), approximately one-half of all participants (51 percent) had made positive net deposits. The remainder had chosen to withdraw all of their deposits for unmatchable uses. The average participant had made $655 in net deposits. Among those participants with positive net deposits, however, the average participant saved much more: $1,300. The average match rate in the CAPTC program, across all uses (weighted by use) was 2.54. If all net deposits as of October 2003 were ultimately matched, the average program participant with positive net deposits would thus have accumulated $3,303 in savings plus match.

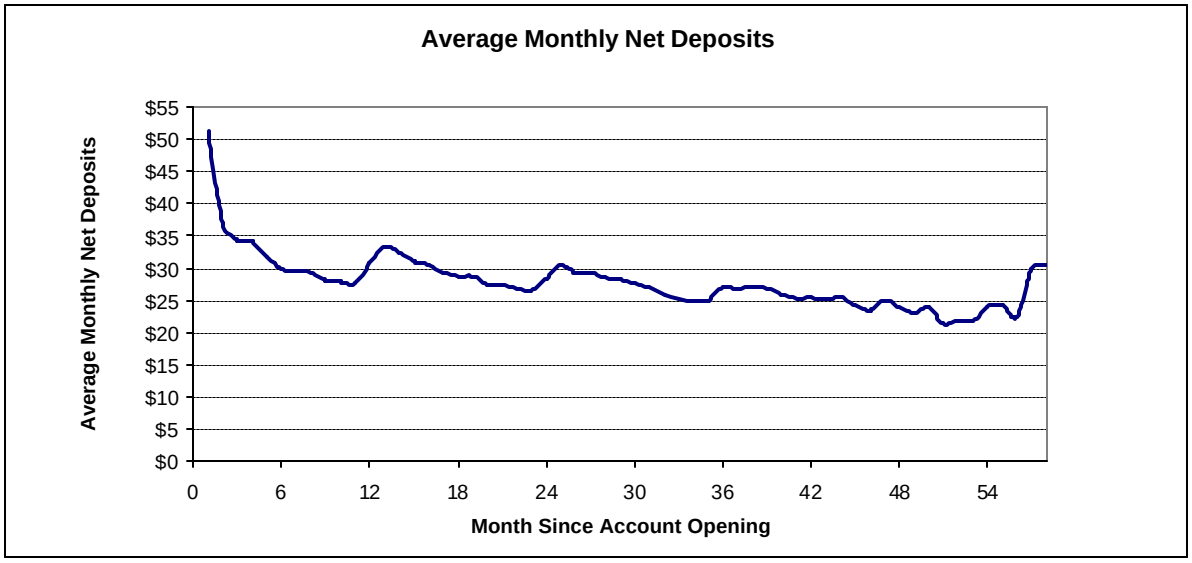
Net deposits, averaged across participants, rose steadily by month since account opening. (See Exhibit 3.5, as computed each month for accounts remaining open.) Among participants with accounts still open at month 12, net deposits averaged $368; among participants with accounts still open at month 24, net deposits averaged $684; and among participants with accounts still open at month 36, net deposits averaged $973. Those participants who stayed longer generally accumulated greater net deposits. The rising average thus reflects the fact that the sample is changing over the follow-up period, with the "bigger savers" most likely to remain in the sample towards the end of the program, as well as increasing deposits over time. (Note that these averages do not include the matched deposits of those who previously closed their accounts.)

Exhibit 3.5



As shown in Exhibit 3.6, the average monthly net deposit was highest in the first few months after a participant had opened their account, and then leveled off. In the first month of account opening, the average monthly net deposit (AMND) was $51. This declined to $31 by month 12, to $28 by month 24.

Exhibit 3.6



Matched and Unmatched Withdrawals by Participants

Based on MIS IDA data through October 2003, 34 percent of the 472 participants had made at least one matched withdrawal before closing their account. Another 5 percent of the participants had made at least one matched withdrawal and had not yet closed their accounts (i.e., had a remaining positive

balance). Fully 53 percent of participants had closed their account without ever making a matched withdrawal. The remaining 9 percent of participants remained ongoing in the program without having made a matched withdrawal.

Thirty-nine percent of participants had thus made at least one matched withdrawal through October 2003. (This includes the 34 percent who had closed their accounts and the 5 percent who remained ongoing.) Among those with at least one matched withdrawal, the amount of matched withdrawals averaged $1,480 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 by participants was home repair or improvement, accounting for 35 percent of withdrawal transactions. The next most common use of matched withdrawals was home purchase, at 26 percent. Education and retirement each accounted for 17 percent. The remaining 5 percent of withdrawals were for small business.

A somewhat different picture emerges as to the distribution of dollars spent on asset purchases by type of use. The distribution of total asset purchases (matched withdrawals plus match) by use was as follows: 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.

In summary, a very high percentage (85 percent) of the sample members randomly assigned to the treatment group opened IDAs. Nearly half of the treatment cases (48 percent) opened accounts within three months of their random assignment date. As the demonstration approached its end, nearly one-half of these participants had succeeded either in making one or more matched withdrawals (39 percent) or in accumulating a savings balance (9 percent). On average, those participants who did make matched withdrawals were able to commit more than $3,400 in funds (including match) toward their asset goals, typically for either home purchase or home improvement. The key question for this research, as addressed in Chapter 4, is whether the savings and asset-building outcomes of the treatment group, all of whom had the opportunity to open and use an IDA, were significantly improved by the existence of these accounts.

3.4 Sample Balance: Implications of Random Assignment and Sample Attrition

An experimental design offers the strongest possible basis for measuring the effects of a program intervention such as IDAs. As noted earlier, the fundamental advantage of a randomized demonstration (versus nonexperimental approaches) is that the estimated program effects can be attributed with greater confidence to the intervention itself, apart from the confounding influence of pre-existing differences (either observable or unobservable) between those who were exposed to the intervention and those who were not (the treatment and control groups, respectively).

As described in the previous section, there is a high degree of comparability in the baseline characteristics of the treatment and control group members of the analysis sample—the sample for which both Wave One and Wave Three interviews were completed. This sample forms the basis for estimating program effects over the full four-year period of the demonstration. Only for a small number of baseline characteristics is the treatment-control difference statistically significant; given the number of baseline characteristics that were subject to such tests, the number of significant differences is within the range that one might expect through statistical fluctuations only.

Although the analysis sample is reasonably well balanced, we undertook (at the suggestion of CSD) a series of statistical tests to examine the way in which the composition of the analysis sample was influenced by two separate processes. The first process was random assignment, by which cases entered the demonstration as treatment or control group members. The second process was sample attrition, by which cases became unavailable for the impact analysis because they were not located and interviewed at Wave Three. The aim of these tests was to inform the specification of the regression models to be used in estimating program impacts. The results of these tests, detailed in Appendix B for sample attrition, are summarized below.

An initial set of ***bivariate*** tests, similar to those described in Section 3.2, explored the treatment-control differences in baseline characteristics for the full baseline sample of 1,103.

- For the vast majority of variables measured at baseline, there was no statistically significant treatment-control difference for *either* the full baseline sample or the analysis sample.
- Where a significant treatment-control difference was present in *both* the analysis sample and the baseline sample, one can attribute it primarily to random assignment (although attrition may have accentuated the difference). This was the situation for two of the previously noted variables with significant treatment-control differences in the analysis sample: the percentage of cases with two children in the household (higher for the treatment group in both samples) and the level of liquid assets at baseline (lower for the treatment group in both samples).
- Where a significant treatment-control difference in the analysis sample was *not* present in the baseline sample, one can attribute it primarily to sample attrition. This was the case for three of the previously noted variables with significant differences in the analysis sample: the percentage never-married at baseline (lower for the treatment group in the analysis sample), the level of retirement savings at baseline (higher for the treatment group in the analysis sample), and the percentage who owned other property at baseline (higher for the treatment group in the analysis sample).
- In those instances where a difference was significant in the baseline sample, but not in the analysis sample, the random assignment and sample attrition processes appear to have acted in offsetting directions. This occurred for the percentage of cases with an income-to-poverty ratio of 1.00 to 1.49 (lower for treatment cases in the baseline sample). More importantly, this also occurred for the homeownership rate (lower for treatment cases in the baseline sample) and for other variables closely related to homeownership (all of which were also lower for treatment cases in the baseline sample): the percentage having made recent home improvements and the levels of real assets, total assets, and net worth.

In addition to these bivariate tests of treatment-control differences, ***multivariate*** tests were also conducted on the patterns of random assignment and sample attrition. This was done so that we could identify the particular types of cases that, all other things equal, were over-or under-represented in the sample.

Regarding random assignment, we examined whether cases of particular baseline characteristics stood an equal chance of being assigned to the treatment group, adjusting for other baseline characteristics. These tests were run for both the baseline sample and the analysis sample. As discussed in Appendix B, for a small number of the tested baseline characteristics, the probability of being assigned to the treatment group was significantly different than among all other cases.

The combination of bivariate and multivariate tests, on both the baseline sample and the analysis sample, thus provided some evidence of sample imbalance. Perhaps most notably, in the analysis sample the multivariate tests showed that, for some given baseline characteristics, cases were not equally likely to have been assigned to the treatment group or the control group. To minimize the influence of this imbalance on the estimation of program impacts, we adopted a strategy, as further detailed in Section 4.1, that explicitly took account of those baseline characteristics that were identified as sources of imbalance. (In technical terms, the estimating equations were specified so that each of these identified baseline characteristics was included in the model, not only as a separate covariate but also in an interaction term with the treatment dummy.[34]) This strategy was applied to any variable that appeared as a source of imbalance in either the bivariate or multivariate analysis, for the analysis sample.[35]

Regarding sample attrition, we similarly considered whether cases of particular baseline characteristics were more likely than others to have completed the Wave Three interview, adjusting for other baseline characteristics (including the sample cohort). We looked at these patterns across the entire baseline sample (combining treatment and control groups) and within the treatment group. For a small number of the tested baseline characteristics, the probability of completing the Wave Three interview was significantly different than among all other cases.

Among treatment cases, we also examined whether those who completed the Wave Three interview had systematically higher or lower levels of net deposits, as measured by the MIS IDA information (described above in Section 3.3). Wave Three respondents within the treatment group did have a significantly higher level of net deposits than Wave Three nonrespondents, as measured over the course of their participation (at month 48 or at the month of account closure, if earlier than month 48).[36]

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[34] 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.

[35] 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.

[36] One might have expected a response rate at Wave Three among treatment cases who used their IDAs to purchase homes (versus other treatment cases), for the following reason. The survey locating efforts included requests to the IDA program staff for updated address information on sample members. Case

In summary, we found that the random assignment process, coupled with sample attrition, resulted in some degree of treatment-control imbalance in the analysis sample. To address this, we have adopted an approach that statistically re-balances the sample in the estimation of treatment effects. Specifically, for those observable baseline case characteristics on which there was some treatment-control imbalance, we have allowed for the possibility that the treatment effect may indeed differ according to such characteristics. By explicitly including these "treatment interactions" in the statistical models, we have taken steps to protect against the possibility that sample imbalance would bias the estimates of program impact.

records would have shown the addresses of homes purchased by treatment cases, enabling interviewers to locate these respondents more readily.

Chapter Four
Estimates of IDA Program Impacts

Many treatment group members made substantial deposits into their IDAs, as shown in Chapter Three, and many accumulated significant matching funds. 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. Following a summary of key results, we describe the methodology, the impact estimates for the entire sample, and the subgroup-level impact estimates.

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, 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. Additionally, the extent to which sample members who did not own a home at baseline subsequently engaged in activities preparatory to home purchase (such as attending an open house or repairing credit to apply for a mortgage) was significantly higher among those in the treatment group.
- **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. ("Real assets" includes the market value of the primary residence, any other properties, vehicles, and business assets.)
- **Increase in retirement savings:** The treatment yielded a positive impact on retirement savings at month 48 for African-Americans. The treatment effect for these participants amounted to $1,081 more in retirement savings than the $1,267 accumulated by their control group counterparts. ("Retirement savings" includes amounts 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.)
- **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 (as explained further below) reflect the family's need to draw down such assets in order to purchase a home. ("Liquid assets"

includes the IDA balance and amounts held in checking and savings accounts, money-market accounts, and certificates of deposit. "Other financial assets" includes 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.)

- **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. ("Total assets" include liquid assets, retirement savings, other financial assets, and real assets. "Total liabilities" includes all indebtedness, such as mortgages, vehicle loans, credit card debt, personal loans, business loans, student loans, installment loans, consolidation loans, and overdue bills.)
- **Increase in non-degree educational coursework:** There was a significant positive treatment effect on one educational outcome—whether one had taken a non-degree educational course during the latter part of the demonstration, during months 19 to 48. The percentage who took such a course during this time interval was 6.6 percentage points higher for treatment group members than the 19.1 percent for the control group.

There was no strong evidence of treatment effects for other outcome measures, including business startup or purchase, home repair or improvement, or net worth.

Expected Program Impacts

The American Dream Demonstration provided significant financial incentives for participants to make five forms of investment: home purchase, home improvement or repair, small business startup or expansion, postsecondary education ("human capital investment"), and retirement saving. The match formula offered by CAPTC provided the strongest incentives for home purchase: participants who used their IDA funds to buy homes received a match of $2 for every $1 they invested. For the other four allowable uses the match rate was $1 for every $1 the participant invested. Consistent with the incentives offered by the CAPTC program, 58 percent of participants reported that they intended to use their IDA for home purchase.[37] (The percentage was presumably even higher among those who did not own a home at the time they entered the program.)

Given the incentives offered by the demonstration, one would expect the impacts of the treatment to be found most directly in the higher likelihood of treatment cases engaging in the subsidized forms of investment. The other outcomes examined in this report will be influenced indirectly by the treatment in ways that reflect how these outcomes interact with the asset accumulation goals for which the program provides direct incentives. We outline these interactions below.

For all participants who are seeking to make deposits into their IDAs, regardless of the asset they intend to purchase, we expect to see the following changes in other outcomes:

- *Earnings* may increase, if participants respond to the increased incentives for saving by increasing their work effort to fund their IDA deposits, or *consumption* may decrease, if

[37] Schreiner (2002), p. 80.

participants opt to economize on their consumption purchases, thus enabling "new savings."

- *Financial assets* may decrease, if participants have available funds that they can transfer into their IDAs from other sources—especially *liquid assets* such as money in checking and savings accounts, or *other financial assets* such as money held with family or friends.
- *Liabilities* may increase through either a slower paydown of debt or an increased buildup of debt (e.g., by financing more consumption through credit cards), to free up funds for IDA deposits.
- *Net worth* may increase or decrease, depending on whether participants fund their IDAs primarily through new savings, asset shifting, or debt.

For participants seeking to purchase a home or start a small business, we might expect to see the following changes in other outcomes:

- *Total liabilities* may increase, as participants must take out mortgages or business loans to purchase their home or invest in their business.
- *Financial assets* may decrease, as participants must deplete savings to make their down payments or invest in their business. Most likely this decrease would occur in the areas of *liquid assets* or *other financial assets*, as these sources are more readily accessible (and withdrawals are less likely to incur penalties) than funds that are in *retirement savings*.
- *Real assets* should increase, as the value of a new home or business would be counted in this measure.
- There is likely to be a negative effect on *net worth*. If all assets were perfectly measured and their were no transaction costs, we would expect home purchase and small business purchase or startup to produce little impact on net worth in the short run, as the increase in debt and decrease in financial assets should be offset by the increase in the value of real or business assets. Given the fixed transaction costs associated with buying a home or starting a business – closing costs in the case of home purchase – the impact on net worth is likely to be negative in the short run.

For participants who intended to use their IDAs to fund home improvements or repairs, the impacts on other outcomes are likely to be similar to, but smaller in magnitude than, the impacts associated with home purchase or business purchase/startup. To the extent that participants are motivated to engage in home improvement in excess of the value of their IDA savings plus match, those participants' liabilities and financial assets may decline accordingly. However, because home improvement and repairs also increase home values, there is likely to be little impact on net worth; if assets were perfectly measured, the increase in home value is expected to partially, though not fully, offset the cost of home improvement and repairs in the short run.[38]

[38] Note that it was not possible to estimate these effects separately for subgroups defined by their intended IDA use, as such questions were not asked at the Wave One interview. The information was thus not available for control group members or treatment group nonparticipants.

For participants who invest in further education, the incentives offered by the demonstration may motivate treatment group members to invest in education whose cost is higher than their savings plus IDA match. If so, we might expect to see the following changes to other outcomes:

- *Total liabilities* may increase, if participants invest in education that requires taking out student loans.
- *Financial assets* may decrease, if participants must dip into other savings to fully fund their additional education.
- *Net worth* may well decrease. Because human capital does not have a monetized value in the survey-measured outcomes, investments in human capital are reflected as declines in net worth in the short run, as funds are spent on acquiring education that has yet to be translated into higher earnings and subsequent asset accumulation.

For participants who invest in retirement savings, there are no predictable interactions with other outcomes beyond those already described for all participants seeking to fund their IDAs.

To summarize, the incentives provided by the IDA lead us to expect the treatment to produce positive impacts on all of the subsidized forms of asset purchase, but particularly on homeownership—the asset for which the program offered the highest subsidy and for which the majority of participants were intending to use their IDA. A naïve assessment of the program might lead to the assumption that the treatment should also produce positive impacts on financial assets, negative impacts (reductions) on total liabilities, and positive impacts on net worth. As presented above, however, the relationships are complex between the impacts of the treatment on the subsidized outcomes and its impacts on components of net worth. Thus, if one finds seemingly adverse effects in terms of reduced financial assets, increased liabilities, or reduced net worth, it may simply reflect the way in which IDA participants have chosen to fund their IDA deposits and their matchable asset purchases.

4.1 Methodology

Our basic impact estimates were obtained using the following methodology. Throughout, the subscript **i** refers to the individual sample member, and $\mathbf{T}_i$ refers to the individual's treatment group status (a dummy variable equal to 1 for the treatment group and 0 for the control group).

For each outcome measure, the outcome variable $\mathbf{Y_i}$ is regressed on $\mathbf{T}_i$, $\mathbf{X}_i$, and $\mathbf{Z}_i*\mathbf{T}_i$, where:

> $\mathbf{X}_i$ = a vector of covariates measuring baseline demographic characteristics and baseline values of every outcome variable [39], and

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

$\mathbf{Z_i}*\mathbf{T_i}$ = a set of interactions between treatment status and Z_i, the subset of the covariate vector X_i identified as sources of sample imbalance, as explained in Section 3.4.[40]

The estimation uses ordinary least squares (OLS), with the sample weighted to adjust for the change in the random assignment ratio early in the demonstration.[41] The basic model, then, is:

Eq. 4.1: $$\mathbf{Y_i} = \beta_0 + \beta_1*\mathbf{X_i} + \beta_2*\mathbf{T_i} + \beta_3*\mathbf{Z_i}*\mathbf{T_i} + \varepsilon_i ,$$

where β_0 is the constant term, $(\beta_2 + \beta_3*\mathbf{Z_i})$ is the treatment effect, and ε_i is the individual-specific error term. The estimated treatment effect, $(\hat{\beta_2} + \hat{\beta_3}*\bar{\mathbf{Z}}_\mathbf{i})$, is evaluated at the mean of Z for the estimation sample. If $(\hat{\beta_2} + \hat{\beta_3}*\bar{\mathbf{Z}}_\mathbf{i})$ is significantly different from zero at the 0.05 level, then we conclude that the treatment affected the outcome variable, controlling for baseline characteristics (including the baseline value of the outcome variable).

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

Eq. 4.2: $$\mathbf{Pr(Y_i} = 1) = \theta\ (\beta_0 + \beta_1*\mathbf{X_i} + \beta_2*\mathbf{T_i} + \beta_3*\mathbf{Z_i}*\mathbf{T_i}),$$

where θ is the standard cumulative normal distribution.

Treatment Effects for Baseline Subgroups

We also examined whether treatment effects vary across major subgroups defined by demographic and socioeconomic characteristics at baseline, including homeownership, race/ethnicity, age, gender, family structure, education, financial assets, and whether the individual had a bank account at baseline. (The specific subgroup definitions are described in Section 4.3 below.)

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 on the basis of the baseline characteristic in question, the estimating equation would be:

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[40] Based on the bivariate and multivariate analyses of sample balance with the baseline sample and the analysis sample, as described in Section 3.4, 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, cohorts 7-9, cohorts 10-12, and cohort 13.

[41] As discussed in Chapter One, 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 tables presented in this report reflect weighted results.

Eq. 4.3: $$\mathbf{Y_i} = \beta_0 + \beta_{11} * \mathbf{X_{i1}} + \beta_{12} * \mathbf{X_{i2}} + \beta_{21} * \mathbf{D_{i1}} * \mathbf{T_i} + \beta_{22} * \mathbf{D_{i2}} * \mathbf{T_i} + \beta_{31} * \mathbf{D_{i1}} * \mathbf{Z_i} * \mathbf{T_i} + \beta_{32} * \mathbf{D_{i2}} * \mathbf{Z_i} * \mathbf{T_i} + \varepsilon_i$$

where $\mathbf{D}_{i1}$ and $\mathbf{D}_{i2}$ are dummy variables indicating the subgroup categories for the baseline characteristic. Note that $(\mathbf{D}_{i1} + \mathbf{D}_{i2}) = 1$; therefore, the treatment dummy, $\mathbf{T_i}$, is not entered separately in the subgroup models.

Under this model, $(\beta_{21} + \beta_{31} * \mathbf{Z}_i)$ is the treatment effect for subgroup 1, and $(\beta_{22} + \beta_{32} * \mathbf{Z}_i)$ is the treatment effect for subgroup 2. The estimated treatment effects for each subgroup are evaluated at the mean of **Z** for the *subgroup* estimation sample; thus, treatment effects for different subgroups are evaluated at different values of $\bar{\mathbf{Z}}_\mathbf{i}$. .

Some of the tested baseline characteristics had three categories and were estimated with the following equation:

Eq. 4.4: $$\mathbf{Y_i} = \beta_0 + \beta_{11} * \mathbf{X_{i1}} + \beta_{12} * \mathbf{X_{i2}} + \beta_{13} * \mathbf{X_{i3}} + \beta_{21} * \mathbf{D_{i1}} * \mathbf{T_i} + \beta_{22} * \mathbf{D_{i2}} * \mathbf{T_i} + \beta_{23} * \mathbf{D_{i3}} * \mathbf{T_i} + \beta_{31} * \mathbf{D_{i1}} * \mathbf{Z_i} * \mathbf{T_i} + \beta_{32} * \mathbf{D_{i2}} * \mathbf{Z_i} * \mathbf{T_i} + \beta_{33} * \mathbf{D_{i3}} * \mathbf{Z_i} * \mathbf{T_i} + \varepsilon_i$$

Under this model, $(\beta_{21} + \beta_{31} * \mathbf{Z}_i)$ is the treatment effect for subgroup 1, and $(\beta_{22} + \beta_{32} * \mathbf{Z}_i)$ is the treatment effect for subgroup 2, and $(\beta_{23} + \beta_{33} * \mathbf{Z}_i)$ is the treatment effect for subgroup 3.

Estimating the Effects of Treatment on Participants

The impact estimates discussed above and presented throughout this report pertain to the treatment effect for the entire treatment group, including those who did not open an IDA. These estimates are called the "Intent-to-Treat" (ITT) estimates. Also of interest is the effect of IDA *participation*—i.e., the treatment effect on those who opened an IDA. Conventionally, the estimated treatment effect on participants is called the "Treatment-on-Treated" (TOT) estimate.

TOT estimates, although not shown in this report, can be generated using a standard methodology.[42] One first obtains the ITT estimate from the multivariate model described above. Let ITT represent the overall impact effect. In Equation 4.1, the ITT estimate is $\beta_2 + \beta_3 * Z_i$, evaluated at the sample mean values of the covariates included in Z_i. One can express the ITT estimate as a weighted average of the treatment effect on participants (the TOT, or "treatment on treated" effect) and the treatment effect on nonparticipants (represented by I_n). If r is the proportion of the treatment group who participated (the "participation rate" defined in Chapter Three), and (1-r) is the proportion of nonparticipants, then:

Eq. 4.5: $$ITT = r * TOT + (1-r) * I_n$$

Solving for TOT, which is the treatment effect on participants, one obtains:

Eq. 4.6: $$TOT = (ITT - (1-r) * I_n) / r$$

[42] See Orr (1999).

If one assumes that the treatment effect on nonparticipants is zero, this reduces to

Eq. 4.7: $TOT = ITT / r$

Of the 412 treatment group members in the analysis sample, 369 opened an IDA, yielding a participation rate of 90 percent. To derive the TOT impact for a particular subgroup from its estimated ITT effect, one should use the participation rate among treatment group members in that subgroup.

This derivation of the impact on participants makes no assumption about the similarity of participants and nonparticipants. The only assumption made is that the treatment had no impact on nonparticipants. This should be viewed as a bounding assumption, as one might reasonably expect some favorable effect on nonparticipants of early Money Management classes. The derivation above thus provides an upper-bound estimate of the treatment effect on IDA participants.

Because the participation rate among treatment group members was so high, and because the TOT impacts can be estimated by applying a multiplier (1/r) to the ITT estimates, we present only the ITT estimates in the exhibits of this chapter.

4.2 Impact Estimates for the Entire Analysis Sample

In this section we present estimates of the impacts of the IDA program on the entire analysis sample of 412 treatment group members. Program impacts on ownership of real assets are presented in Exhibit 4.1. Real assets include homes, businesses, "other property" (real estate other than the primary residence), and vehicles. 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).

Program impacts on asset-building activities undertaken at any time during the four-year demonstration period are presented in Exhibit 4.2. The outcomes examined include home search and home purchase, home improvement (both "any" improvement and "major" improvement to one's own home), business purchase or startup, and education and training.

Home purchase was measured only for sample members who did not own a home at baseline.[43] 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. 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.

[43] 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 4.1: 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 4.2: 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 or Related Activities‡									
Home purchase	643	0.302	0.089 **	579	0.166	-0.006	579	0.148	0.092 ***
			(0.037)			0.030			(0.032)
Looked through home listings in newspaper	643	0.764	0.045	579	0.539	0.030	579	0.540	0.044
			(0.032)			(0.042)			(0.042)
Drove to look at houses for sale	643	0.751	0.033	579	0.563	-0.026	579	0.528	0.079 *
			(0.032)			(0.041)			(0.043)
Attended open house	643	0.503	0.079 **	579	0.320	-0.036	579	0.304	0.107 ***
			(0.039)			(0.038)			(0.040)
Talked about borrowing money for a home	643	0.559	0.067 *	579	0.393	-0.022	579	0.336	0.095 **
			(0.039)			(0.042)			(0.041)
Cleared up old debts to apply for home loan	643	0.592	0.117 ***	579	0.399	0.094 **	579	0.373	0.100 **
			(0.038)			(0.042)			(0.043)
Talked with realtor about buying home	643	0.681	0.034	579	0.504	-0.029	579	0.428	0.075 *
			(0.035)			(0.042)			(0.042)
Intensity of home search	643	4.15	0.465 **	579	2.88	0.005	579	2.66	0.591 ***
			(0.185)			(0.204)			(0.223)
Home Improvement									
Any home improvement	840	0.343	0.053 *	764	0.208	0.022	764	0.304	0.038
			(0.031)			(0.026)			(0.031)
Major home improvement (over $200)	840	0.299	0.032	764	0.157	0.017	764	0.265	0.025
			(0.030)			(0.025)			(0.031)
Business Startup or Related Activities†									
Business startup or purchase	784	0.106	-0.016	710	0.066	-0.018	710	0.049	-0.002
			(0.022)			(0.017)			(0.017)
Talked about starting his/her own business	784	0.501	0.025	710	0.375	0.007	710	0.342	0.063 *
			(0.037)			(0.037)			(0.038)
Prepared business plan or similar document	784	0.217	0.001	710	0.136	0.001	710	0.137	0.011
			(0.031)			(0.025)			(0.027)

Exhibit 4.2: 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 (0.024)	710	0.082	-0.027 (0.019)	710	0.060	0.011 (0.019)
Talked about obtaining business loan	784	0.153	-0.009 (0.026)	710	0.112	-0.021 (0.022)	710	0.074	0.015 (0.020)
Education or Training									
Took non-degree course	840	0.373	0.009 (0.035)	764	0.247	0.006 (0.035)	764	0.191	0.066 ** (0.031)
Took course toward degree	840	0.502	-0.010 (0.033)	764	0.384	0.001 (0.035)	764	0.381	0.013 (0.034)
Finished job training program with certificate	840	0.373	-0.001 (0.035)	764	0.243	-0.012 (0.033)	764	0.266	-0.021 (0.034)
Graduated from school	840	0.220	-0.037 (0.029)	764	0.134	-0.023 (0.025)	764	0.134	-0.023 (0.026)
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)

† 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.

Six measures of "home search" (activities preparatory to home purchase), plus one cumulative measure, are presented. These outcomes are measured only for sample members who did not own a home at baseline. 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 significant positive treatment effects on several of the home search measures. Over the entire period, there were significant effects on the percentage of participants who attended an open house, talked to someone about borrowing money for a home, and who cleared up old debts to apply for a home loan. There were also significant positive impacts on the cumulative measure of "home search intensity." Although the home search intensity measure is an ordinal scale and therefore the point estimates do not have intrinsic meaning, the impacts indicate that home search intensity was higher in the treatment group. Note that all but one of the home search measures were significant (or least marginally) in the latter part of the demonstration, during months 19 to 48. Interest in home purchase appeared to intensify toward the latter part of the demonstration, as participants accumulated more in matchable deposits. The one significant impact in the *early* part of the demonstration was "cleared up old debts to apply for a home loan." As credit repair is an important early step along the path toward home purchase, it is encouraging that the treatment had an early effect on this outcome.

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.

Treatment effects on financial outcomes are presented in Exhibit 4.3, as estimated for the entire analysis sample. 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. None of the 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 estimate must be considered in the context of the impact on other outcomes such as homeownership, business ownership, and educational attainment. This finding may indicate short-term asset shifting; as treatment group members may have deposited miscellaneous savings into their IDA to maximize their use of match funds. Alternatively, those in the treatment group who made a major asset purchase with their IDA may have needed to draw also from their miscellaneous savings, funds that (unlike their liquid assets) they may have been reserving for such use.

Exhibit 4.3: 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.

Turning to educational outcomes, there are no statistically significant program effects over the entire demonstration period. Interestingly, however, there is a significant impact on one educational outcome – whether participants' had taken a non-degree course – during the latter part of the demonstration. It is notable that the one educational outcome for which a significant program impact was found is the outcome that takes the least time to complete—a single course, without the requirement that participants enroll in a degree program. 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 adult education classes or continuing education courses. This was one way to avoid losing the accrued match funds.

Estimated impacts for a series of additional economic outcomes are presented in Exhibit 4.4. These additional measures include monthly household income, household income-to-poverty ratio, monthly earnings and employment of the sample member, and household receipt of public assistance. The only statistically significant treatment effect on any of these outcomes was a marginally significant negative effect on the sample member's employment at month 48.

To summarize, the experimental evidence indicates significant treatment effects on several key outcomes related to homeownership. The full-sample rate of homeownership, the rate of recent home purchase among non-homeowners, and several measures of home search among non-homeowners were all significantly higher in the treatment group than in the control group. Although no significant impacts were found on educational attainment over the entire demonstration period, there is evidence to suggest that an impact on one educational outcome emerged in the latter follow-up months. The treatment did not significantly affect the rate of business ownership or (for nonbusiness owners at baseline) the incidence of recent business start-up or purchase. No statistically significant treatment effects were found on financial outcomes, including net worth and its major components, when measured over the full sample.

One expects, however, that the treatment might have different effects on different groups of program enrollees. For example, as mentioned in Chapter Three, real assets and net worth differed dramatically at baseline between homeowners and non-homeowners. For such subgroups, the patterns of subsequent savings and asset purchase may also differ. We therefore turn next to examine whether treatment effects on each of the major outcomes differed across subgroups.

Exhibit 4.4: Impacts on Employment and Income

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)
Monthly Household Income	840	2256	-118 (151)	764	1891	232 (255)
Household Income-to-Poverty Ratio	840	1.786	-0.134 (0.120)	764	1.551	0.136 (0.210)
Monthly Earnings	840	1382	-78 (75)	764	1488	-62 (77)
Employment	840	0.781	-0.053 * (0.028)	764	0.871	0.017 (0.024)
Household Receipt of Public Assistance	840	0.362	0.009 (0.033)	764	0.330	0.016 (0.034)

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

4.3 Impact Estimates by Subgroup

CAPTC's IDA program provided a sufficiently broad set of permissible uses for IDAs that a diverse group of applicants were attracted to it. Given this diversity, different groups of enrollees may be expected to have responded differently to the treatment. The analysis presented in Section 4.2 has indicated one subgroup of crucial importance: sample members who did not own a home at baseline. For those participants, homeownership was the most widely cited economic goal.[44] In contrast enrollees who owned a home already, particularly those who were older, may well have had quite different goals, such as saving for retirement. In this section of the evaluation we examine whether the effects differed across a number of different subgroups. We examine impacts on each of the major outcomes for the following 21 subgroups, all defined by their status at baseline:

- Homeownership
  - Owned home
  - Did not own home
- Race/ethnicity[45]
  - African-American non-Hispanic
  - Caucasian non-Hispanic
- Age
  - 35 or younger
  - 36 or older
- Gender
  - Male
  - Female
- Family structure
  - No children
  - Single parent with children
  - Two or more adults with children
- Education
  - High school diploma, GED, or less
  - Some college (including a two-year degree)
  - Four-year degree or more
- Total financial assets
  - $200 or less
  - $201 to $1,100

[44] Note that participants who already owned a home when entering the program were permitted to use their IDA funds to "upgrade" to a new home if they desired.

[45] The subgroup analysis of race/ethnicity includes only the sample members that were African-American non-Hispanic or Caucasian non-Hispanic. Other racial/ethnic subgroups were too small in number to analyze separately.

  - $1,101 or more

- Receipt of public assistance
  - Public assistance
  - No public assistance
- Banking status
  - Checking or savings account
  - No checking or savings account

As we turn to the analysis of subgroup impacts, note that we will focus primarily on those impacts that are statistically significant at the 0.05 level, particularly where an F-test on the equality of the treatment effects by subgroup also indicates that the impacts are unequal. A significant F-test indicates that one can reject the hypothesis of equal treatment effects across the tested subgroups.

As in the previous section, the results presented in this section are all ITT (intent-to-treat) estimates. As discussed above, ITT estimates show the impact of the intervention on the entire treatment group. The impact on those who opened an IDA is estimated by the TOT (treatment-on-treated) estimate. As derived earlier, an upper bound of the TOT estimate can be derived as the ITT estimate divided by the participation rate for the treatment group.

As discussed previously, the participation rate in the treatment group is very high; fully 90 percent of treatment group members in the analysis sample opened an IDA. Readers who wish to know the TOT impact for any "main effect" presented above can simply divide the ITT impact estimate by 0.9 (or multiply by 1.1). Because the participation rate is very high, the TOT and ITT impacts will not differ greatly in magnitude. Because the standard errors for TOT impacts are multiplied by the same factor as the point estimates, the estimated significance levels are identical for both the ITT and TOT impacts.

For the subgroup estimates presented below, readers who wish to know the TOT impacts can divide the ITT impacts presented by the participation rate *for the particular subgroup.* As shown in Exhibit 4.5, the participation rate in each defined subgroup was very high. The lowest participation rate among these subgroups was 84 percent, for those with a high school education or less. The highest participation rate was 96 percent among treatment group members who owned a home at baseline.

Homeownership at month 48

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, 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. (See Exhibit 4.6.)

For sample members who did not own a home at baseline, the treatment produced an 8.5 percentage point increase in homeownership relative to the control group mean of 27.8 percent (proportionally, an increase of 31 percent). We have already discussed, in Section 4.2, the significant positive impact on home *purchase* during months 1 to 48 for those not owning a home at baseline. The result presented in Exhibit 4.6 shows the corollary of that finding: a significant positive impact on month 48 homeownership rates for sample members who did not own a home at baseline. Although the use of

the IDA for home purchase was not restricted to first-time homeownership, one can presume that baseline non-homeowners were disproportionately intending to use their IDA to buy a home.

Exhibit 4.5: IDA Program Participation Rate among Treatment Group Members by Subgroup

Characteristic/Subgroup	Sample Size	IDA Program Participation Rate
Total Analysis Sample	412	90%
Homeownership		
Owned Home	93	96%
Did Not Own Home	319	88%
Age		
35 or Younger	207	87%
36 or Older	205	92%
Gender		
Male	87	94%
Female	325	88%
Race/Ethnicity		
African-American Non-Hispanic	177	87%
Caucasian Non-Hispanic	185	92%
Family Structure		
No Children	85	94%
Single Parent	202	88%
Two or More Adults With Children	125	90%
Education		
High School or Less	129	84%
Some College	233	92%
Four-Year Degree or Higher	50	92%
Total Financial Assets		
$200 or Less	133	86%
$201 to $1,100	148	89%
$1,101 or More	131	94%

Exhibit 4.6: Impacts by Subgroup on Homeownership and Business Ownership

Characteristic/Subgroup (Defined at Baseline)	Homeownership at Month 48				Business Ownership at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership				†				
Owned Home	197	0.895	-0.017 (0.057)		197	0.156	0.036 (0.045)	
Did Not Own Home	642	0.278	0.085 ** (0.036)		643	0.089	-0.007 (0.022)	
Race/Ethnicity				†				
African-American (Non-Hispanic)	343	0.284	0.114 ** (0.046)		344	0.083	-0.020 (0.027)	
Caucasian (Non-Hispanic)	395	0.528	0.028 (0.045)		395	0.100	0.061 * (0.031)	
Age								
35 or Younger	425	0.380	0.063 (0.043)		425	0.096	-0.029 (0.028)	
36 or Older	414	0.480	0.061 (0.041)		415	0.115	0.034 (0.027)	
Gender								
Male	169	0.513	0.071 (0.071)		169	0.193	0.000 (0.051)	
Female	670	0.409	0.061* (0.034)		671	0.084	0.002 (0.022)	
Family Structure								
No Children	186	0.397	0.053 (0.062)		186	0.089	0.032 (0.044)	
Single Parent	404	0.038	0.038 (0.043)		405	0.089	-0.003 (0.027)	
Two or More Adults With Children	249	0.506	0.107 ** (0.053)		249	0.145	-0.010 (0.035)	
Education								
High School or Less	262	0.395	0.059 (0.053)		262	0.060	0.24 (0.032)	
Some College	479	0.420	0.066 (0.040)		480	0.113	0.003 (0.028)	
Four-Year Degree or Higher	97	0.556	0.057 (0.087)		97	0.193	-0.055 (0.060)	
Total Financial Assets								
$200 or Less	290	0.317	0.007 (0.044)		291	0.081	-0.039 (0.026)	
$201 to $1,100	276	0.465	0.064 (0.044)		276	0.101	0.000 (0.028)	
$1,101 or More	273	0.520	0.116 ** (0.049)		273	0.135	0.047 (0.033)	
Public Assistance Receipt								
Public Assistance	356	0.332	0.028 (0.041)		356	0.089	-0.020 (0.026)	
No Public Assistance	483	0.499	0.087 ** (0.036)		484	0.117	0.018 (0.024)	

Exhibit 4.6: Impacts by Subgroup on Homeownership and Business Ownership *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Homeownership at Month 48				Business Ownership at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status				††				
Checking or Savings Account	719	0.463	0.076 ** (0.032)		719	0.113	0.000 (0.021)	
No Checking or Savings Account	120	0.261	-.042 (0.064)		121	0.067	0.016 (0.043)	

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

The positive treatment effect for African-Americans appears related to the above finding. At baseline, African-American sample members had a much lower rate of homeownership at baseline than others (15.1 percent versus 29.2 percent).

Among the four other subgroups with positive treatment effects, all might be regarded as having economic advantages at baseline. 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.

Business ownership at month 48

Impacts on business ownership by subgroup are also shown in Exhibit 4.6. 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). The treatment appears to have had no effect on the rate of business ownership.

Liquid assets at month 48

Impacts on liquid assets by subgroup are shown in Exhibit 4.7. Sample members with a four-year college degree or more experienced significant negative effects on this outcome. As discussed previously, a negative program impact of IDAs on liquid assets should not be surprising; it may reflect the use of such funds for asset purchases, as sample members moved their balances out of liquid sources (checking and savings accounts, money market accounts, or CDs) to invest in homeownership, education, or other uses encouraged by the IDA program. For the highly educated subgroup, however, no treatment effects were found on any of the subsidized forms of asset ownership. As this subgroup did show some reduction in liabilities (although not significant), one possible explanation is that the money management courses prompted these individuals to use liquid assets to pay off loans (including student loans) and other outstanding debt in order to reduce interest costs or in preparation for major asset purchases that they did not make by the end of the demonstration.

Exhibit 4.7: Impacts by Subgroup on Liquid Assets and Retirement Savings

Characteristic/Subgroup (Defined at Baseline)	Liquid Assets at Month 48				Retirement Savings at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership				†				†
Owned Home	197	3819	1335 (1040)		197	3205	1750 (1139)	
Did Not Own Home	643	1754	-460 (412)		643	1296	240 (298)	
Race/Ethnicity								†
African-American (Non-Hispanic)	344	1687	-516 (653)		344	1267	1081 ** (471)	
Caucasian (Non-Hispanic)	395	2711	154 (573)		395	2355	-37 (541)	
Age								††
35 or Younger	425	2064	-312 (479)		425	1938	33 (428)	
36 or Older	415	2457	204 (592)		415	1574	1139 * (593)	
Gender								
Male	169	4121	-1621 (996)		169	3276	486 (908)	
Female	671	1820	307 (409)		671	1405	599 * (357)	
Family Structure								
No Children	186	2605	-187 (754)		186	2094	684 (837)	
Single Parent	405	1583	343 (491)		405	1233	165 (388)	
Two or More Adults With Children	249	3078	-609 (758)		249	2352	1175 (761)	
Education				†				
High School or Less	262	1730	-21 (611)		262	1462	1100 * (617)	
Some College	480	2059	462 (465)		480	1653	288 (420	
Four-Year Degree or More	97	4455	-2806 ** (1251)		97	3096	732 (1294)	
Total Financial Assets								
$200 or Less	291	656	-236 (398)		291	439	650 (441)	
$201 to $1,100	276	1643	553 (589)		276	1557	538 (495)	
$1,101 or More	273	4598	-572 (934)		273	3417	574 (860)	
Public Assistance								
Public Assistance	356	1345	-313 (393)		356	782	534 (399)	
No Public Assistance	484	2917	133 (474)		484	2768	616 (492)	

Exhibit 4.7: Impacts by Subgroup on Liquid Assets and Retirement Savings *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Liquid Assets at Month 48				Retirement Savings at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status								†
Checking or Savings Account	719	2633	-66 (391)		719	2039	666 * (373)	
No Checking or Savings Account	121	462	28 (469)		121	428	-77 (458)	

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

Retirement savings at month 48

Exhibit 4.7 shows two notable subgroup differences in impacts on retirement savings (amounts held in pensions, IRAs, and 401k accounts). 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 (indicated by a significant F-test on age and a marginally significant t-test for the older subgroup) 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 (receiving the match for IDA savings rolled over to a Roth IRA). The effect among African-Americans is striking in combination with the earlier-mentioned impact on homeownership. It may indicate that African-Americans who did not use their IDA for a specific asset purchase tended to take advantage of the IRA rollover provision, to still utilize the IDA match.

Other financial assets and total financial assets at month 48

Exhibit 4.8 shows limited evidence of effects on other financial assets (stocks, bonds, savings at home or with friends, educational savings accounts) or on total financial assets (liquid assets, retirement savings, and other financial assets). For no subgroup 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. Recall that 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 at month 48

Significant increases in real assets (the value of primary residence, other property, vehicles, and business assets) were found for four subgroups: African-Americans, those 36 or older, those not receiving public assistance, and those with checking or savings accounts. (See Exhibit 4.9.) 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.

Exhibit 4.8: Impacts by Subgroup on Other Financial Assets and Total Financial Assets

Characteristic/Subgroup (Defined at Baseline)	Other Financial Assets at Month 48				Total Financial Assets at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership								
Owned Home	197	8280	-6601 (4272)		197	15304	-3515 (5159)	
Did Not Own Home	643	785	-1499 (922)		643	3835	-1719 (1159)	
Race/Ethnicity								
African-American (Non-Hispanic)	344	1510	-1536 * (929)		344	4465	-971 (1463)	
Caucasian (Non-Hispanic)	395	3857	-4311 (2964)		395	8924	-4195 (3419)	
Age								†
35 or Younger	425	3736	-4297 (2750)		425	7738	-4576 (3146)	
36 or Older	415	1431	-975 (1060)		415	5462	368 (1705)	
Gender				†				†
Male	169	4336	-5180 ** (2344)		169	11733	-6314 * (3391)	
Female	671	2203	-2113 (1623)		671	5427	-1207 (1908)	
Family Structure								
No Children	186	896	-150 (940)		186	5595	347 (1685)	
Single Parent	405	3193	-3404 (2761)		405	6009	-2896 (3109)	
Two or More Adults With Children	249	3047	-3144 ** (1435)		249	8477	-2578 (2305)	
Education								
High School or Less	262	1159	-1207 (1176)		262	4350	-128 (1652)	
Some College	480	3626	-3935 * (2386)		480	7338	-3186 (2754)	
Four-Year Degree or More	97	1411	226 (1684)		97	8961	-1849 (3016)	
Total Financial Assets								
$200 or Less	291	454	-1443 (1269)		291	1549	-1029 (1620)	
$201 to $1,100	276	5307	-3049 (2429)		276	8507	-1959 (2854)	
$1,101 or More	273	2578	-3434 * (1771)		273	10593	-3432 (2916)	
Public Assistance								
Public Assistance	356	673	-1976 * (1165)		356	2800	-1755 (1468)	
No Public Assistance	484	4009	-3158 (1985)		484	9394	-2409 (2362)	

Exhibit 4.8: Impacts by Subgroup on Other Financial Assets and Total Financial Assets *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Other Financial Assets at Month 48				Total Financial Assets at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status								
Checking or Savings Account	719	3087	-2693 * (1597)		719	7759	-2093 (1900)	
No Checking or Savings Account	121	323	-2315 (1900)		121	428	-2363 (2261)	

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

Exhibit 4.9: Impacts by Subgroup on Real Assets and Total Assets

Characteristic/Subgroup (Defined at Baseline)	Real Assets at Month 48				Total Assets at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership								
Owned Home	197	76944	4565 (11066)		197	92248	1050 (12830)	
Did Not Own Home	643	26901	6818 * (3639)		643	30763	5100 (3979)	
Race/Ethnicity								
African-American (Non-Hispanic)	344	23559	9784 ** (4668)		344	28024	8813 * (5020)	
Caucasian (Non-Hispanic)	395	50253	5968 (5642)		395	59176	1773 (7136)	
Age				††				††
35 or Younger	425	40166	-255 (4587)		425	47904	-4831 (6069)	
36 or Older	415	37928	12979 ** (5645)		415	43391	13348 ** (6052)	
Gender								
Male	169	53538	4319 (7918)		169	65271	-1995 (9295)	
Female	671	35681	6565 (4083)		671	41108	5358 (4739)	
Family Structure								
No Children	186	32094	16865 (10350)		186	37689	17212 (10630)	
Single Parent	405	36903	-117 (4434)		405	42912	-3014 (5850	
Two or More Adults With Children	249	48351	9632 (5948)		249	56828	7054 (6801)	
Education								
High School or Less	262	31342	5214 (4749)		262	35692	5086 (5234)	
Some College	480	38186	7822 (5193)		480	45524	4636 (6272)	
Four-Year Degree or More	97	64860	760 (10348)		97	73821	-1089 (11657)	
Total Financial Assets								
$200 or Less	291	24571	-5739 (3696)		291	26120	-6768 (4235)	
$201 to $1,100	276	42945	14465 (6910)		276	51452	12507 (7846)	
$1,101 or More	273	51771	9122 (5184)		273	62364	5689 (6234)	
Public Assistance								
Public Assistance	356	27184	-158 (4147)		356	29984	-1913 (4673)	
No Public Assistance	484	47683	11049 ** (5012)		484	57078	8640 (5830)	

Exhibit 4.9: Impacts by Subgroup on Real Assets and Total Assets *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Real Assets at Month 48				Total Assets at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status				†				†
Checking or Savings Account	719	43175	7591 ** (3703)		719	50934	5498 (4435)	
No Checking or Savings Account	121	19498	-3590 (5480)		121	20711	-5954 (6204)	

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

For the 36 or older subgroup, the treatment had a positive effect not only on real assets but also on total assets (the sum of financial assets and real assets, also shown in Exhibit 4.9). 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 and net worth at month 48

Exhibit 4.10 presents impacts on total liabilities and net worth by subgroup. For these outcomes, there was only one specific subgroup for which the treatment had a significant effect. 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. For net worth, there were several instances in which the F-test showed statistical significance (at the 0.05 level) for a baseline characteristic: 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.

Education/training and intensity of home search during months 1 to 48

Exhibit 4.11 presents treatment impacts by subgroup on two outcomes that are measured over the demonstration period: whether the respondent took any courses or training, and the intensity with which the respondent engaged in home search (including home purchase). There were no subgroup differences in treatment effects on coursework/training. Although there were a number of subgroups with individually significant impact estimates for intensity of home search, 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 during months 1 to 48 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.

Exhibit 4.10: Impacts by Subgroup on Total Liabilities and Net Worth

Characteristic/Subgroup (Defined at Baseline)	Total Liabilities at Month 48				Net Worth at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership				††				
Owned Home	197	49244	-2620 (6452)		197	43004	3670 (10746)	
Did Not Own Home	643	30221	6131 ** (2926)		643	515	-1031 (2907)	
Race/Ethnicity								
African-American (Non-Hispanic)	344	24169	6955 * (3693)		344	3855	1858 (4060)	
Caucasian (Non-Hispanic)	395	43870	1452 (4588)		395	15306	322 (5897)	
Age								††
35 or Younger	425	37318	2099 (3788)		425	10586	-6930 (4783)	
36 or Older	415	32271	6248 * (3756)		415	11120	7100 (4999)	
Gender								†
Male	169	44803	7323 (7886)		169	20468	-9318 (8190)	
Female	671	32515	3444 (2866)		671	8594	1914 (3789)	
Family Structure								
No Children	186	30898	4325 (5893)		186	6792	12887 (8350)	
Single Parent	405	32955	1179 (3297)		405	9957	-4193 (4854)	
Two or More Adults With Children	249	41195	8884 (5759)		249	15634	-1830 (5532)	
Education								
High School or Less	262	25831	3115 (4556)		262	9861	1970 (4460)	
Some College	480	32839	6234 (3721)		480	12685	-1598 (4894)	
Four-Year Degree or More	97	69933	-3864 (9797)		97	3888	2775 (9285)	
Total Financial Assets								
$200 or Less	291	26800	-3061 (3316)		291	-680	-3708 (3329)	
$201 to $1,100	276	38366	5474 (4223)		276	13086	7033 (6215)	
$1,101 or More	273	40661	9907 (4356)		273	21704	-4218 (5438)	
Public Assistance								††
Public Assistance	356	28314	4081 (3703)		356	1670	-5994 * (3388)	
No Public Assistance	484	39581	4218 (3211)		484	17497	4422 (4684)	

Exhibit 4.10: Impacts by Subgroup on Total Liabilities and Net Worth *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Total Liabilities at Month 48				Net Worth at Month 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status								††
Checking or Savings Account	719	38441	5051 * (2761)		719	12493	448 (3523)	
No Checking or Savings Account	121	17711	-2750 (5767)		121	3000	-3204 (4843)	

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

Exhibit 4.11: Impacts by Subgroup on Asset-Building Activities: Education/Training and Intensity of Home Search

Characteristic/Subgroup (Defined at Baseline)	Any Education/Training During Months 1 to 48				Intensity of Home Search During Months 1 to 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Homeownership								
Owned Home	197	0.588	0.087 (0.066)					
Did Not Own Home	643	0.723	-0.028 (0.034)					
Race/Ethnicity								
African-American (Non-Hispanic)	344	0.765	-0.012 (0.045)		292	3.880	0.243 (0.269)	
Caucasian (Non-Hispanic)	395	0.614	0.027 (0.045)		273	4.467	0.435 (0.273)	
Age								
35 or Younger	425	0.778	-0.021 (0.040)		365	4.430	0.464 (0.231)	**
36 or Older	415	0.599	0.017 (0.044)		278	3.778	0.467 (0.291)	
Gender								
Male	169	0.572	-0.062 (0.075)		116	4.495	0.831 (0.434)	*
Female	671	0.718	0.011 (0.032)		527	4.408	0.414 (0.205)	**
Family Structure								
No Children	186	0.593	0.035 (0.065)		138	4.232	-0.127 (0.373)	
Single Parent	405	0.768	-0.043 (0.041)		336	3.951	0.542 (0.250)	**
Two or More Adults With Children	249	0.644	0.038 (0.054)		169	4.478	0.710 (0.323)	**
Education								
High School or Less	262	0.519	0.040 (0.059)		197	3.713	0.857 (0.352)	**
Some College	480	0.764	-0.020 (0.037)		374	4.226	0.290 (0.236)	
Four-Year Degree or More	97	0.806	-0.017 (0.078)		72	5.094	0.436 (0.530)	
Total Financial Assets								
$200 or Less	291	0.644	-0.062 (0.046)		250	3.499	0.424 (0.268)	
$201 to $1,100	276	0.758	0.020 (0.041)		211	4.602	0.756 (0.248)	***
$1,101 or More	273	0.681	0.033 (0.045)		182	4.637	0.165 (0.311)	
Public Assistance								†††
Public Assistance	356	0.744	-0.016 (0.039)		312	3.848	0.333 (.233)	
No Public Assistance	484	0.652	0.008 (0.035)		331	4.429	.588 (.218)	

Exhibit 4.11: Impacts by Subgroup on Education/Training and Intensity of Home Search *(Continued)*

Characteristic/Subgroup (Defined at Baseline)	Any Education/Training During Months 1 to 48				Intensity of Home Search During Months 1 to 48			
	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]	Sample Size	Control Mean	Estimate[a] (Std Err)	F-Test[b]
Banking Status								
Checking or Savings Account	719	.0694	.003 (.031)		540	4.361	0.528 *** (0.192)	
No Checking or Savings Account	121	0.674	-0.041 (0.534)		103	3.349	-0.009 (0.401)	

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

4.4 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 (FTHB) 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 2, 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 3 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.[46] 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 the results shown throughout this report.

Sensitivity of impact estimates to outlier data values

In Chapter Two and in Appendix C, we have 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*. As detailed in Appendix C, we have 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 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.[47] We summarize here the findings of this sensitivity analysis. The comparison of estimates from the revised data and the original data are shown in Exhibits C.1 through C.4.

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

[46] See Bloom (1984).

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

either of these outcomes, using either the revised data (as already reported in Exhibit 4.3) 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.

In a separate sensitivity analysis, suggested by the Center for Social Development, we 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, available upon request, 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. Similar results were obtained using strategies that combine the 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 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.[48]

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

[48] 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.

variability of the outcome measures.[49] To the extent feasible, one always wants the minimum detectable effects to be within the plausible range of impacts for the intervention in question.

Exhibit D.1 in Appendix D shows the MDEs for the full-sample impacts on all major outcomes measured at month 48 or reflecting asset-building activities during months 1 to 48. For each outcome, this exhibit also shows the control group's mean value and the following properties of the estimated treatment effect: the point estimate, its standard error, and the upper and lower bounds of the 95 percent confidence interval around the point estimate. To enable comparisons across outcomes, the exhibit also shows the upper bound of the confidence interval and the MDE as percentages of the corresponding control mean.

Based on the MDEs provided in Exhibit D.1, 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, vehicle ownership, each of the separate activities preparatory to home purchase (although not home purchase itself), 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, 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 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. 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.

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 Exhibit D.1 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,

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

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 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.

4.5 Summary and Conclusion

The results presented in this evaluation indicate that access to the IDA program had a significant influence on the savings and asset accumulation of those served by the Tulsa IDA program, especially in promoting their rate of homeownership. Favorable program effects were especially pronounced in particular subgroups, including African-Americans.

Evidence of Significant Sample-Wide Impacts

The single most notable sample-wide effect of the IDA program was to increase the treatment group's rate of homeownership at month 48 by 6.2 percentage points, a proportional increase of 14 percent relative to the control group mean. Among the other allowable IDA uses under the Tulsa program, the only other sample-wide effect that reached statistical significance (at the 0.05 level) pertained to postsecondary education. The proportion of the treatment group that took a nondegree course during months 19 to 48 was 6.6 percentage points higher for the treatment group (proportionally, an increase of 35 percent over the 19.1 percent rate in the control group).

The results estimated for the entire analysis sample indicate that the effects of IDAs require several years to develop and emerge. Neither of the effects noted above was present at month 18, nor were there significant treatment impacts on any other subsidized outcomes. It is noteworthy, however, that by month 18 a significantly higher proportion of the treatment group members indicated that they had cleared up old debts to apply for a home loan. Perhaps not surprisingly, for the types of asset accumulation that are supported by IDA programs – including long-term, major investments such as homeownership – a multi-year time horizon appears necessary for asset purchases to occur.

The sample-wide estimates provided no statistically significant evidence of effects on business startup or expansion, on retirement savings or other components of assets, on liabilities or net worth, or on employment or income, at either the interim stage (month 18) or final stage (month 48) of the demonstration. Underlying this, however, was a series of significant effects on subgroups within the sample, as discussed below.

Evidence of Significant Subgroup-Specific Impacts

Exhibit 4.12 summarizes the patterns of estimated effects by subgroup. 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 (0.05, or 0.01). As throughout this chapter, we focus here on the effects that were significant at the 0.05 level or better.

Exhibit 4.12: Summary of Estimated Impacts at Month 48 by Subgroup

Subgroup	Outcome			
	Homeownership	Intensity of home search	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 4.12: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)*

Subgroup	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 4.12: Summary of Estimated Impacts at Month 48 by Subgroup *(Continued)*

Subgroup	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 0.05 level (++ or --) or the 0.01 level (+++ or ---). Intensity of home search and "any education/training" pertain to activities during months 1-48. All other outcomes pertain to month 48.

Subgroups with impacts on home search activity only. Some subgroups did not experience an increase in homeownership or other targeted forms of asset purchase, but nonetheless showed a significant increase in their home search activity. These subgroups included *those 35 or younger, females, single parents, those with a high school education or less, and those with total financial assets between $201 and $1,100.* Seemingly, the treatment enabled these groups to become more focused on the goal of homeownership, but they may have had insufficient income or resources to make homeownership affordable.

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 seemingly not related to increased 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 real assets, such as vehicles or real estate.

Subgroups with impacts on homeownership, but not on other asset-building or financial outcomes. *Cases with two or more adults and children, cases with $1,101 or more in financial assets, and cases with a checking or savings account at baseline* 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.[50]

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* increased homeownership was combined with an increase in real assets, suggesting no necessary 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.[51]

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.

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[50] 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.

[51] 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.

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Appendix A

Patterns of Sample Enrollment

and Survey Completion

Exhibit A.1: Weekly Referrals and Random Assignment: October 1998-December 1999

Week Of:	Number of Cases Received From CAPTC	Number of Cases Randomly Assigned by Abt Associates		
	Weekly Total	Treatment	Control	Weekly Total
October 26 – October 30, 1998	19	--	--	--
November 2 – November 6	7	2	5	7
November 9 – November 13	9	5	6	11
November 16 – November 20	12	4	6	10
November 23 – November 27	11	3	1	4
November 30 - December 4	7	6	8	14
December 7 – December 11	22	4	5	9
December 14 – December 18	29	5	7	12
December 21 – December 25	13	0	0	0
December 28 – January 1, 1999	10	17	20	37
January 4 – January 8	7	6	4	10
January 11 – January 15	9	7	11	18
January 18 – January 22	17	4	4	8
January 25 – January 29	17	8	5	13
February 1 – February 5	17	2	7	9
February 8 – February 12	14	13	16	29
February 15 – February 19	33	7	9	16
February 22 – February 26	24	9	10	19
March 1 – March 5	30	9	7	16
March 8 – March 12	26	15	21	36
March 15 – March 19	22	16	19	35
March 22 – March 26[1]	0	8	9	17
March 29 – April 2	46	6	4	10
April 5 – April 9	12	10	11	21
April 12 – April 16	11	13	14	27
April 19 – April 23	17	6	3	9
April 26 – April 30	21	0	1	1
May 3 – May 7	7	8	7	15

[1] The treatment-control ratio was switched from 5:6 to 1:1 on March 16, 1999, and CAPTC made a change from twice-weekly to once-weekly referrals.

Exhibit A.1: Weekly Referrals and Random Assignment: October 1998-December 1999 *(Continued)*

Week Of:	Number of Cases Received From CAPTC	Number of Cases Randomly Assigned by Abt Associates		
	Weekly Total	Treatment	Control	Weekly Total
May 10 – May 14	10	4	4	8
May 17 – May 21	15	10	12	22
May 24 – May 28	20	12	11	23
May 31- June 4, 1999	5	4	6	10
June 7 – June 11	20	3	3	6
June 14 – June 18	13	9	6	15
June 21 – June 25	13	6	8	14
June 28 – July 2	11	9	9	18
July 5 – July 9	12	3	3	6
July 12 – July 16	16	7	7	14
July 19 – July 23	20	11	11	22
July 26 – July 30	21	6	4	10
August 2 – August 6	17	8	10	18
August 9 – August 13	21	9	8	17
August 16 – August 20	34	2	3	5
August 23 – August 28	25	17	17	34
August 30 – September 3	22	14	12	26
September 6 – September 10	19	11	12	23
September 13 – September 17	20	8	8	16
September 20 – September 24	20	14	16	30
September 27 – October 1	23	4	3	7
October 4 – October 8	36	8	7	15
October 11 – October 15	30	6	6	12
October 18 – October 22	31	8	9	17
October 25 – October 29	39	22	21	43
November 1 – November 5	50	20	20	40
November 8 – November 12	115	13	13	26
November 15 – November 19	--	50	52	102
November 22 – November 26	--	27	27	54
November 29 – December 3	--	17	16	33
December 6 – December 10	--	2	2	4
Totals	**1,147**	**537**	**566**	**1,103**

Exhibit A.2: Completion Rates by Sample Cohort: Month 18 (Wave Two) Survey

Sample Cohort	Month of Enrollment	Treatment Group			Control Group			Total		
		Total Sample	Completed Interviews	Completion Rate	Total Sample	Completed Interviews	Completion Rate	Total Sample	Completed Interviews	Completion Rate
1	Nov-98	22	21	95.5%	27	23	85.2%	49	44	89.8%
2	Dec-98	25	24	96.0%	32	30	93.8%	57	54	94.7%
3	Jan-99	30	29	96.7%	40	38	95.0%	70	67	95.7%
4	Feb-99	42	39	92.9%	37	36	97.3%	79	75	94.9%
5	Mar-99	39	36	92.3%	48	42	87.5%	87	78	89.7%
6	Apr-99	32	30	93.8%	35	30	85.7%	67	60	89.6%
7	May-99	33	30	90.9%	34	28	82.4%	67	58	86.6%
8	Jun-99	27	22	81.5%	26	24	92.3%	53	46	86.8%
9	Jul-99	34	31	91.2%	29	21	72.4%	63	52	82.5%
10	Aug-99	49	37	75.5%	47	33	70.2%	96	70	72.9%
11	Sep-99	37	32	86.5%	44	37	84.1%	81	69	85.2%
12	Oct-99	55	45	81.8%	56	39	69.6%	111	84	75.7%
13	Nov/Dec-99	112	86	76.8%	111	90	81.1%	223	176	78.9%
Total		537	462	86.0%	566	471	83.2%	1103	933	84.6%

Exhibit A.3: Completion Rates by Sample Cohort: Month 48 (Wave Three) Survey

Sample Cohort	Month of Enrollment	Treatment Group			Control Group			Total		
		Total Sample	Completed Interviews	Completion Rate	Total Sample	Completed Interviews	Completion Rate	Total Sample	Completed Interviews	Completion Rate
1	Nov-98	22	21	95.5%	27	24	88.9%	49	45	91.8%
2	Dec-98	25	21	84.0%	32	29	90.6%	57	50	87.7%
3	Jan-99	30	26	86.7%	40	37	92.5%	70	63	90.0%
4	Feb-99	42	37	88.1%	37	36	97.3%	79	73	92.4%
5	Mar-99	39	33	84.6%	48	38	79.2%	87	71	81.6%
6	Apr-99	32	25	78.1%	35	28	80.0%	67	53	79.1%
7	May-99	33	26	78.8%	34	24	70.6%	67	50	74.6%
8	Jun-99	27	19	70.4%	26	22	84.6%	53	41	77.4%
9	Jul-99	34	27	79.4%	29	20	69.0%	63	47	74.6%
10	Aug-99	49	36	73.5%	47	37	78.7%	96	73	76.0%
11	Sep-99	37	27	73.0%	44	33	75.0%	81	60	74.1%
12	Oct-99	55	38	69.1%	56	32	57.1%	111	70	63.1%
13	Nov/Dec-99	112	76	67.9%	111	68	61.3%	223	144	64.6%
1-3								176	158	89.8%
4-6								233	197	84.6%
7-9								183	138	75.4%
10-12								288	203	70.5%
13								223	144	64.6%
Total		537	412	76.7%	566	428	75.6%	1103	840	76.2%

Appendix B

Analysis of Sample Attrition

The full experimental sample consisted of 1,103 sample members who were surveyed at the start of the demonstration (the "baseline sample"). This entering sample consisted of 566 control group members and 537 treatment group members. As in any longitudinal survey, a number of persons who were in the initial sample could not be reached or surveyed at the time of the final follow-up interviews. A total of 840 persons, or 76 percent of the original sample, were surveyed in the month 48 (Wave Three) follow-up survey. The analysis sample used in this report consists of these 840 sample members. The analysis sample contains 428 control group members and 412 treatment group members.

This appendix examines whether sample attrition produced an analysis sample that was different along any important dimensions compared to the baseline sample. We present baseline values of demographic characteristics and baseline values of the outcome variables analyzed in the report for both the baseline sample and analysis sample.

Exhibit B.1 presents baseline demographic characteristics for both the baseline sample and the analysis sample. The exhibit provides several pieces of information for each variable. First, the exhibit shows the overall mean of each variable for the baseline sample and for the analysis sample, and the percentage difference in these means (calculated as the analysis sample mean minus the baseline sample mean, divided by the baseline sample mean). Second, the exhibit shows the mean of each variable for the treatment group and the control group, and the difference between the treatment and control means, for both the baseline sample and the analysis sample. Third, the exhibit provides statistical tests for the difference in means between the control and treatment groups, for both the baseline sample and the analysis sample. The comparison of means between control and treatment groups in the baseline sample indicates the extent to which, at the outset of the demonstration, random assignment yielded two groups that were similar in their measured characteristics.

Two pieces of information are relevant for considering the impact of attrition. First, whether the overall sample means for the baseline sample and the analysis sample are similar in magnitude; and second, whether any statistically significant differences between treatment and control groups are present in the analysis sample that were not present in the baseline sample, or vice versa. Differences in the overall sample means between the baseline and analysis sample suggest that attrition was non-random – that particular characteristics were correlated with the probability of sample members being lost to follow-up. A finding that significant differences between the treatment and control groups are present in one sample but not the other suggests that the intersection of a particular characteristic with treatment status was correlated with the probability of sample members being lost to follow-up. Note that this is not identical to attrition being non-random with respect to treatment status overall; we already know that the percentage of treatment group members was virtually identical in the baseline sample and the analysis sample. The raw percentage of treatment group members is 48.7 percent in the baseline sample and 49.0 percent in the analysis sample; the weighted percentage of treatment group members is 50.0 percent in the baseline sample and 50.5 percent in the analysis sample.

Exhibit B.1 shows that on every demographic variable the means in the entire baseline sample and the entire analysis sample are extremely close. For most variables the mean in the analysis sample is within 5 percent of the mean of the baseline sample, and for nearly all variables the mean in the analysis sample is with 10 percent of the mean of the baseline sample. The few exceptions are for the percentage of the sample that is Hispanic; the percentage of the sample that has four or more children; and the percentage of the sample with less than a high-school diploma. The percentage of Hispanics is 2.8 percent in the baseline sample, and 2.1 percent in the analysis sample; the percentage of the

sample with four or more children is 1.5 in the baseline sample, and 1.3 in the analysis sample; and the percentage of the sample with less than a high-school diploma is 7.1 in the baseline sample, and 5.5 in the analysis sample. Thus, even though the percentage difference in the means is quite large across the two samples (24 percent, 16 percent, and 23 percent respectively), the absolute differences in sample means are very small.

There are two instances where a treatment-control difference in means was detected in the analysis sample, but was not detected in the baseline sample: for the percentage of sample members who were "never married" and for the percentage of sample members with one child. However, there was also one instance where a treatment-control difference in means was detected in the baseline sample that was *not* detected in the analysis sample: the percentage of sample members whose income-to-poverty ratio was in the range of 1.00 to 1.49. Finally, there was one instance where a treatment-control difference in means was detected in the baseline sample that remained in the analysis sample: the percentage of sample members with two children. Given the very large number of statistical tests presented in Exhibit B.1, these few examples of treatment-control differences in means, and the extent to which these findings differ between the baseline sample and the analysis sample, are fewer in number than would be expected to occur by chance alone.

Exhibit B.1: Baseline Value of Demographic Characteristics in the Baseline Sample and the Analysis Sample

	Baseline Sample				Analysis Sample				
	Control Group (N=566)	Treatment Group (N=537)	Difference[a]	Total (N=1103)	Control Group (N=428)	Treatment Group (N=412)	Difference[a]	Total (N=840)	Difference in Overall Means
Gender									
Female	78.9%	78.1%	-0.8%	78.5%	81.0%	79.0%	-2.1%	80.0%	1.9%
Male	21.1%	22.0%	0.8%	21.5%	19.0%	21.0%	2.1%	20.0%	-7.0%
Race / Ethnicity									
Caucasian, non-Hispanic	45.0%	43.1%	-1.8%	44.1%	49.0%	45.0%	-4.0%	47.0%	6.7%
African-American, non-Hispanic	41.2%	43.3%	2.1%	42.2%	39.0%	42.8%	3.8%	40.9%	-3.0%
Hispanic	3.2%	2.4%	-0.8%	2.8%	2.6%	1.7%	-0.9%	2.1%	-24.3%
Asian, non-Hispanic	0.7%	1.1%	0.4%	0.9%	0.7%	1.2%	0.5%	1.0%	4.3%
Native American / Other, non-Hispanic	6.3%	6.0%	-0.3%	6.2%	5.5%	5.6%	0.1%	5.6%	-9.2%
Age									
Average Age	40.1	39.7	-0.3	39.9	40.5	40.4	-5.5%	40.4	1.3%
Age less than 30	15.7%	18.2%	2.5%	17.0%	15.2%	16.1%	1.0%	15.7%	-7.8%
Aged 30 - 39	37.1%	34.7%	-2.3%	35.9%	35.9%	34.0%	-1.9%	34.9%	-2.6%
Aged 40 - 49	29.1%	31.8%	2.7%	30.4%	29.6%	33.6%	4.0%	31.6%	3.9%
Age 50 and older	18.1%	15.3%	-2.8%	16.7%	19.4%	16.3%	-3.1%	17.8%	6.5%
Marital Status									
Never Married	42.3%	37.9%	-4.4%	40.1%	44.3%	35.7%	-8.6% **	39.9%	-0.4%
Married	27.9%	27.9%	0.0%	27.8%	24.1%	28.3%	4.1%	26.2%	-5.9%
Divorced or Separated	27.4%	31.2%	3.8%	29.3%	28.8%	33.4%	4.6%	31.1%	6.1%
Widowed	2.5%	3.0%	0.5%	2.7%	2.8%	2.7%	-0.1%	2.7%	1.0%
Household Type									
One Adult with Children	44.5%	48.0%	3.4%	46.2%	47.5%	49.2%	1.7%	48.3%	4.5%
One Adult without Children	12.5%	11.6%	-0.9%	12.0%	11.6%	11.7%	0.0%	11.6%	-3.2%
Two or more Adults with Children	31.4%	30.9%	-0.5%	31.2%	28.9%	30.3%	1.4%	29.6%	-5.1%
Two or more Adults without Children	11.6%	9.6%	-2.0%	10.6%	12.0%	8.9%	-3.1%	10.5%	-1.2%

Exhibit B.1: Baseline Value of Demographic Characteristics in the Baseline Sample and the Analysis Sample
(Continued)

	Baseline Sample				Analysis Sample				
	Control Group (N=566)	Treatment Group (N=537)	Difference[a]	Total (N=1103)	Control Group (N=428)	Treatment Group (N=412)	Difference[a]	Total (N=840)	Difference in Overall Means
Adults in Household									
Average number of adults	1.53	1.51	-0.02	1.52	1.50	1.49	0.02	1.50	-1.4%
1	57.0%	59.5%	2.5%	58.3%	59.1%	60.8%	1.7%	60.0%	2.9%
2	34.5%	31.8%	-2.7%	33.1%	32.2%	30.7%	-1.5%	31.5%	-5.0%
3	6.9%	7.3%	0.4%	7.1%	7.0%	7.5%	0.5%	7.3%	2.5%
4 or more	1.6%	1.5%	-0.1%	1.5%	1.6%	0.9%	-0.7%	1.3%	-15.7%
Children in Household									
Average number of children	1.65	1.74	0.09	1.70	1.61	1.75	-0.14	1.68	-0.9%
None	24.1%	21.1%	-2.9%	22.6%	23.7%	20.6%	-3.1%	22.1%	-2.3%
1	26.2%	23.3%	-3.0%	24.7%	27.5%	22.3%	-5.1% *	24.9%	0.6%
2	23.3%	30.1%	6.8% **	26.7%	22.6%	31.9%	9.3% ***	27.3%	2.3%
3 or more	26.4%	25.5%	-0.9%	26.0%	26.3%	25.2%	-1.1%	25.7%	-0.9%
Education									
Less than High School	6.4%	7.9%	1.5%	7.1%	4.7%	6.3%	1.6%	5.5%	-22.9%
High School Degree or GED	27.7%	25.8%	-1.9%	26.8%	26.5%	25.1%	-1.4%	25.8%	-3.4%
Some College (including 2 year degree)	54.3%	54.2%	-0.1%	54.3%	57.7%	56.4%	-1.3%	57.1%	5.2%
Graduate from 4 - Year College	7.6%	7.8%	0.2%	7.7%	7.3%	7.7%	0.4%	7.5%	-2.7%
More than 4 years of college	3.9%	4.3%	0.5%	4.1%	3.7%	4.4%	0.7%	4.0%	-1.3%
Missing / Refused / Don't Know	0.2%	0.0%	-0.2%	0.1%	0.2%	0.0%	-0.2%	0.1%	
Employment									
Employed	98.2%	99.1%	0.9%	98.6%	98.1%	99.3%	1.2%	98.7%	0.1%
Self Employment									
Owned Business	7.8%	7.0%	-0.8%	7.4%	5.9%	7.7%	1.8%	6.8%	-8.8%
Any Income from Self-Employment	19.5%	18.8%	-0.6%	19.2%	19.0%	20.2%	1.2%	19.6%	2.3%

Exhibit B.1: Baseline Value of Demographic Characteristics in the Baseline Sample and the Analysis Sample
(Continued)

	Baseline Sample				Analysis Sample				
	Control Group (N=566)	Treatment Group (N=537)	Difference[a]	Total (N=1103)	Control Group (N=428)	Treatment Group (N=412)	Difference[a]	Total (N=840)	Difference in Overall Means
Income-to-Poverty Ratio									
Average income-to-poverty ratio	1.19	1.22	0.03	1.21	1.21	1.23	-0.02	1.22	1.2%
Ratio: 0 to .99	40.9%	42.4%	1.4%	41.6%	40.3%	40.6%	0.2%	40.5%	-2.9%
Ratio: 1.00 to 1.49	36.2%	31.4%	-4.8% *	33.8%	36.2%	34.1%	-2.1%	35.1%	4.0%
Ratio: 1.50 - 1.99	12.6%	15.9%	3.3%	14.2%	12.2%	14.9%	2.7%	13.6%	-4.6%
Ratio: 2.00 or higher	10.3%	10.4%	0.1%	10.3%	11.3%	10.4%	-0.9%	10.8%	4.9%
Received Government Assistance									
"Some" or "A Lot" of Government Assistance	41.3%	42.4%	1.2%	41.8%	42.1%	42.9%	0.8%	42.5%	1.6%
Health Insurance Coverage									
Percent with Health Insurance	57.2%	59.1%	1.8%	58.1%	57.5%	58.8%	1.3%	58.1%	0.0%
Owned a Checking Account									
Percent with money in a checking account	67.6%	70.3%	2.7%	68.9%	69.1%	73.2%	4.1%	71.2%	3.3%
Owned a Savings Account									
Percent with money in a savings account	57.5%	57.0%	-0.5%	57.3%	57.1%	59.5%	2.4%	58.3%	1.9%

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

Exhibit B.2 presents baseline values of the outcome variables for both the baseline sample and the analysis sample. This Exhibit, like Exhibit B.1, provides three pieces of information for each variable. First, the overall mean of each outcome for the baseline sample and for the analysis sample (and the percent difference between these means); second, for both samples, the mean of each outcome for the treatment group and the control group, and the difference between the treatment and control means; and third, for both samples, statistical tests for the difference in means between the control and treatment groups.

The average value of most outcomes was very similar in the full baseline sample and the full analysis sample. Excluding net worth, every outcome in the analysis sample was within 10 percent of the value in the baseline sample. The largest negative difference was found among baseline business ownership, which was 9 percent lower in the analysis sample (6.8 percent of the analysis sample reported owning a business) than in the baseline sample (7.4 percent of the baseline sample reported owning a business.) The largest positive difference, excluding net worth, was found for retirement savings, which was 9.8 percent higher in the analysis sample. Average baseline retirement savings were $684 in the full baseline sample, compared to $751 in the analysis sample.

One notable exception, however, was baseline net worth, which was 20 percent higher ($2,735) in the analysis sample than in the baseline sample ($2,285.) This difference most likely reflects the fact that the baseline homeownership rate was 8.3 percent higher in the analysis sample, as homeownership by far the largest correlate of net worth in this sample. A total of 23.4 percent of the analysis sample owned a home at baseline, compared to 21.6 percent of the baseline sample.

The fact that the analysis sample had a slightly higher homeownership rate than the baseline sample also had an impact on treatment-control differences in means. In the baseline sample, there was a significantly higher rate of homeownership in the control group (24.2 percent) than in the treatment group (19.1 percent). Because of this difference in homeownership rates, there were also significant treatment-control differences in several other variables that are highly correlated with homeownership: the value of real assets, total assets, net worth, and recent home improvements were all lower in the treatment group than in the control group, in the baseline sample.

In the analysis sample, however, homeownership rates in the treatment and control groups are very similar. The homeownership rate among control group members is 24.3 percent, and the homeownership rate among treatment group members is 22.6 percent. The difference is not statistically significant. As a consequence, we also find that treatment-control differences in the value of real assets, the value of total assets, and net worth are not statistically significant in the analysis sample. Thus, it appears that there has been some differential attrition from the sample. Treatment group members who owned a home at baseline were slightly more likely to remain in the sample than treatment group members who did not own a home at baseline, which had the effect of reducing the difference in baseline homeownership rates between the treatment and control groups.

There are two treatment-control differences in the analysis sample that were not statistically significant in the baseline sample. First, there is a treatment-control difference in baseline retirement savings in the analysis sample, with treatments having baseline retirement savings that are $372 higher than controls. Second, there is a treatment-control difference in the rate of ownership of other property (besides a home), with treatments having baseline rates 2.5 percentage points higher than controls.

Finally, there is one treatment-control difference that was present in the baseline sample and remains in the analysis sample. This is a difference in liquid assets. In both samples, treatment group members are found to have baseline levels of liquid assets that are about $310 to $315 higher than control group members.

The number of baseline treatment-control differences found in the analysis sample (three out of the eighteen outcomes tested) is about what one would expect by chance. Precisely because random assignment can, by chance, produce a few differences in baseline characteristics between the treatment and control groups, the estimation of treatment impacts always includes controls for baseline values of these three outcomes – liquid assets, retirement savings, and ownership of other property besides the primary residence – along with baseline values of all the outcomes for which there was no treatment-control difference in baseline means. In this study, we have also included treatment interaction terms for variables where the baseline treatment-control difference was significant in the analysis sample. The multivariate analyses should prevent these baseline differences from contaminating the impact estimates.

To summarize, it appears that there was indeed some non-random attrition. Most importantly, it appears that homeowners were slightly more likely to have remained in the sample than non-homeowners, and in particular, that treatment group homeowners were more likely to have remained in the sample than treatment group non-homeowners. In addition to reducing the treatment-control difference in homeownership rates, this non-random attrition also reduced the treatment-control differences in several correlated baseline variables: real assets, total assets, home improvements, and net worth.

Exhibit B.2: Baseline Values of Outcomes in the Baseline Sample and the Analysis Sample

	Baseline Sample				Analysis Sample				
	Control Group (N=566)	Treatment Group (N=537)	Difference[a]	Total (N=1103)	Control Group (N=428)	Treatment Group (N=412)	Difference[a]	Total (N=840)	Difference in Overall Means
Liquid Assets									
Amount held in Checking and Savings Accounts {including IDAs}, Money Market Accounts, and CDs	$984	$674	-$310 **	$829	$1,069	$753	-$316 *	$909	9.7%
Retirement Savings									
Amount held in pensions, IRAs, 401(k)s	$631	$736	$105	$684	$563	$934	$372 *	$751	9.8%
Other Financial Assets									
Stocks and bonds, any other forms of savings	$428	$453	$25	$441	$409	$503	$94	$456	3.5%
Total Financial Assets									
Sum of Liquid Assets, Retirement Savings, and Other Financial Assets	$2,043	$1,864	-$180	$1,954	$2,041	$2,190	$150	$2,116	8.3%
Real Assets									
Market value of primary residence, other property, automobiles, and business assets	$16,759	$12,620	-$4,139 ***	$14,691	$16,368	$14,465	-$1,904	$15,406	4.9%
Total Assets									
Sum of Total Financial Assets and Real Assets	$18,802	$14,484	-$4,319 **	$16,644	$18,409	$16,655	-$1,754	$17,523	5.3%
Total Liabilities									
Total indebtedness: mortgage(s), car loans, credit card debt, educational loans, medical bills, personal and business loans	$15,058	$13,659	-$1,399	$14,359	$15,015	$14,565	-$450	$14,788	3.0%
Net Worth									
Total Assets minus Total Liabilities	$3,744	$825	-$2,919 **	$2,285	$3,394	$2,090	-$1,304	$2,735	19.7%

Exhibit B.2: Baseline Values of Outcomes in the Baseline Sample and the Analysis Sample *(Continued)*

	Baseline Sample				Analysis Sample				
	Control Group (N=566)	Treatment Group (N=537)	Difference[a]	Total (N=1103)	Control Group (N=428)	Treatment Group (N=412)	Difference[a]	Total (N=840)	Difference in Overall Means
Monthly Household Income	$1,364	$1,428	$64	$1,396	$1,368	$1,443	$75	$1,406	0.7%
Homeownership	24.2%	19.1%	-5.2% **	21.6%	24.3%	22.6%	-1.8%	23.4%	8.3%
Business Ownership	7.8%	7.0%	-0.8%	7.4%	5.9%	7.7%	1.8%	6.8%	-8.8%
Other Property Ownership	3.2%	4.1%	0.9%	3.7%	2.1%	4.6%	2.5% **	3.4%	-8.0%
Vehicle Ownership	83.3%	81.1%	-2.2%	82.2%	84.0%	84.3%	0.4%	84.1%	2.4%
Household Income-to-Poverty Ratio	1.19	1.22	0.03	1.21	121%	123%	2.2%	122%	0.9%
Employed	98.2%	99.1%	0.9%	98.6%	98.1%	99.3%	1.2%	98.7%	0.1%
Household Receipt of Public Assistance	41.1%	42.4%	1.3%	41.8%	42.0%	42.9%	0.9%	42.5%	1.7%
Any Home Improvement Recently	6.5%	4.2%	-2.3% *	5.3%	5.7%	5.0%	0.7%	5.3%	-0.2%
Major Home Improvement (> $200) Recently	5.2%	3.4%	-1.8%	4.3%	4.5%	4.0%	0.5%	4.2%	-2.4%

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

Appendix C

Sensitivity of Impact Estimates to Post-Interview Data Verification

As described in Chapter Two, Abt Associates undertook extensive post-interview data verification efforts to check the accuracy of survey data values that did not meet specified edit checks. In consultation with the Center for Social Development, specific criteria were developed to identify out-of-range or inconsistent values for financial variables collected at Wave One (baseline), Wave Two (month 18), and Wave Three (month 48). For any data value identified by these criteria, the respondent received a follow-up telephone call or mail questionnaire (the Survey Quality Form) requesting confirmation or correction of the value, or the verification was conducted at the Wave Three interview.

For the data collected at Waves One and Two, 4,840 data values were identified for verification, with one or more items for each of 944 respondents.[1] Verification was attempted with all 944 sample members. These efforts resulted in the receipt of verification data from 732 respondents (78 percent of the 944 sample members) for 3,460 data values (71 percent of the 4,840 values). For the 3,460 data values that were verified, 82 percent (2,852) were confirmed as correct by the respondent and remained unchanged; 14 percent (497) were indicated by the respondent to be incorrect and were revised to reflect new values provided by the respondent. For the remaining 3 percent of values (111), the respondent either indicated "don't know" or refused to answer the verification question. The originally recorded value of these items was retained in the survey data.

We were unable to obtain verification data from 212 respondents (944 - 732), on a total of 1,380 values (4,840 - 3,460). Assuming that, had we been able to reach these respondents, they would have considered 14 percent of the 1,380 values to be incorrect, there would be an additional 193 errors identified in the Wave One and Two datasets, for a total of 690 errors (497 + 193). This number implies an error rate of approximately 1.3 percent among all Wave One and Two financial variables subject to data checks.[2]

At Wave Three, some of the range checks used in Waves One and Two were incorporated into the survey software. Among the 840 respondents, any collected values subject to this real-time verification process that were confirmed by the respondent were not subject to further verification. Therefore, the number of follow-up telephone calls and mail questionnaires was much lower for the final wave of data collection. A total of 390 respondents received follow-up telephone calls or mail questionnaires, pertaining to a total of 613 identified data values. Verification data were obtained from 179 sample members, or 46 percent of the attempted contacts. The lower response rate for the Wave Three verification (versus the 78 percent for Waves One and Two) resulted from the shorter post-survey period available to the survey staff to complete the verification in advance of the January 30, 2004 Draft Final Report. All verification data obtained via telephone or mail by January 9, 2004 were included in the analysis. Of the 613 data values subject to verification, information was obtained on 321 (or 52 percent). For these 321 verified Wave Three data items, 84 percent were confirmed by the respondent as correct and remained unchanged; 15 percent were indicated by the

[1] More than 85 percent of all sample members (944 of 1,103) thus had one or more financial values identified for verification. The range and consistency checks were applied to approximately 25,978 values in Wave One and 26,734 values in Wave Two, a combined total of approximately 52,712 values. Of these values, 9.2 percent (the 4,840 discussed above) were identified as not meeting a data check and were therefore subject to verification.

[2] 1.3% = 690 / 52,712.

respondent as incorrect and were revised; and the remaining 1 percent were unchanged as a result of "don't know" responses or refusals to answer.

This appendix assesses the sensitivity of the impact estimates to the data revisions made through the post-interview verification process. In Exhibits C.1 – C.4 we present all of the impact estimates shown in Exhibits 4.1 through 4.4, which were based on a data file that incorporated the post-interview data revisions, along with a separate set of estimates obtained by estimating the same models (on the same analysis sample) using an "original" data file that did not incorporate the post-interview data revisions. In both the revised and original data files, missing data values were imputed using the mean value for the corresponding (treatment or control) group.[3]

Exhibit C.1 shows estimated treatment effects for the set of real outcomes presented in Exhibit 4.1: homeownership, business ownership, ownership of property other than a primary residence, and vehicle ownership. There are virtually no differences in the estimated magnitude or significance levels of the impact estimates based on the revised and original data.

Exhibit C.2 shows estimated treatment impacts on the set of asset-building activities presented in Exhibit 4.2: home purchase or related activities, home improvement, business startup or related activities, and education or training. Outcomes are measured over three time intervals: months 1 to 48, 1 to 18, and 19 to 48.

Exhibit C.2 indicates virtually no differences in the magnitude or significance of the impact estimates based on the revised and original data. Of the 60 pairs of impact estimates presented in this exhibit (a total of 120 separate impact estimates), there is no situation in which an estimated effect was statistically significant (at the 0.05 level) using one data set but not statistically significant using the other data set.

Exhibit C.3 shows re-estimated treatment impacts on the set of financial outcomes presented in Exhibit 4.3. The outcomes are liquid assets, retirement savings, other financial assets, total financial assets, real assets, total assets, total liabilities, and net worth.

Turning first to the impacts on asset measures, the estimates based on the revised data are all of the same sign and of similar magnitude and significance levels as the estimates based on the original data, both at month 18 and month 48. The pattern is different, however, in comparing the month 18 findings for liabilities and net worth. Using the original data, the effect of the IDA program on total liabilities at month 18 is significantly positive (i.e., greater liabilities), and the effect on net worth at month 18 is significantly negative (i.e., lower net worth). Neither of these short-term impacts was significant using the revised data. For both outcomes, there was no significant effect at month 48

[3] For persons who answered "yes" to the preliminary question "do you have any [income/savings/debt] from [source X]", but then answered "don't know" or "refused" to the follow-up question "how much [income/savings/debt] did you have from [source X] last month?" the missing value was imputed using the mean of non-missing values among respondents who answered "yes" to the preliminary question. For persons who answered "don't know" or "refused" to the preliminary question, the missing value for the follow-up question was imputed using the mean of non-missing values among all respondents in the sample. In both cases, means were conditioned on treatment group status. Specifically, the imputations for observations in the treatment group were based on treatment group means, and imputations for observations in the control group were based on control group means.

using either data set. With no apparent end-of-demonstration effect of the IDA program on either of these outcomes, it would be difficult to assert that one's overall interpretation of program effects is meaningfully influenced by the use of one data set versus the other.

Exhibit C.4 shows estimated treatment impacts on the set of outcomes presented in Exhibit 4.4: monthly household income, household income-to-poverty ratio, respondents' monthly earnings, respondents' employment, and household receipt of public assistance. The exhibit shows that there are virtually no differences in the estimated magnitude or significance levels of the impact estimates based on the revised data and the original data.

To summarize, among all pairs of impacts presented in the four tables that compare results using the original data and the revised data, only two pairs of impacts showed a change in the significance level (using the conventional 0.05 threshold). 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, and neither was significant at 48 months using either data set.

The remaining exhibits in this appendix provide further details for each financial variable, comparing the original and revised data values for treatment and control cases in the analysis sample (using a format suggested by the Center for Social Development):

- Exhibits C.5 through C.10 show, by sample group (treatment or control) and by wave (One, Two, and Three) for each financial variable: the number of cases for which the variable's value was changed through post-interview verification, and the associated minimum change, mean change, and maximum change in value (computed only for those with changes, retaining the direction of each change in the data.) Each table also shows, for cases in the analysis sample, the financial variable's minimum, mean, and maximum value in the original data (without post-interview verification) and in the revised data.
- Exhibits C.11 through C.13 show, for each wave (One, Two, and Three), the effect of post-interview verification on the treatment-control difference in raw means for each financial variable, computed across all cases in the analysis sample.[4] Each table shows the raw mean treatment-control difference before verification, the raw mean treatment-control difference after verification, and the associated change.

[4] In the impact analysis, treatment-control differences were adjusted for differences in baseline characteristics between the treatment and control groups.

Exhibit C.1: Impacts on Ownership of Real Assets

Outcome	Estimates Using Revised Data				Estimates Using Original Data			
	Month 48		Month 18		Month 48		Month 18	
	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value
Homeownership	**0.062**	0.04	0.004	0.86	**0.067**	0.03	0.009	0.73
	(0.031)		(0.025)		(0.031)		(0.025)	
Business Ownership	-0.002	0.92	-0.006	0.72	0.003	0.87	-0.007	0.72
	(0.020)		(0.018)		(0.020)		(0.018)	
Other Property Ownership	0.010	0.58	-0.004	0.79	0.011	0.55	-0.002	0.85
	(0.018)		(0.013)		(0.018)		(0.013)	
Vehicle Ownership	-0.004	0.87	0.002	0.94	0.006	0.80	-0.009	0.69
	(0.023)		(0.022)		(0.023)		(0.024)	

Note: Estimates in bold are statistically significant at the 0.05 level.

Exhibit C.2: Impacts on Asset-Building Activities

	Estimates Using Revised Data						Estimates Using Original Data					
	Months 1 - 48		Months 1 - 18		Months 19 - 48		Months 1 - 48		Months 1 - 18		Months 19-48	
Outcome	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value
Business Startup or Related Activities												
Business startup or purchase	-0.016 (0.022)	0.46	-0.018 (0.017)	0.29	-0.002 (.017)	0.89	-0.014 (0.022)	0.52	-0.018 (0.017)	0.28	0.000 (0.017)	1.00
Talked about starting his/her own business	0.025 (0.037)	0.50	0.007 (0.037)	0.85	0.063 (0.038)	0.09	0.029 (0.037)	0.44	0.008 (0.037)	0.83	0.068 (0.038)	0.08
Prepared business plan or similar document	0.001 (0.031)	0.97	0.001 (0.025)	0.98	0.011 (0.027)	0.69	0.003 (0.031)	0.92	0.002 (0.025)	0.94	0.013 (0.027)	0.62
Applied for business license	-0.001 (0.024)	0.98	-0.027 (0.019)	0.16	0.011 (0.019)	0.55	0.001 (0.024)	0.98	-0.029 (0.019)	0.14	0.013 (0.019)	0.47
Talked about obtaining business loan	-0.009 (0.026)	0.72	-0.021 (0.022)	0.33	0.015 (0.020)	0.47	-0.007 (0.026)	0.79	-0.020 (0.022)	0.36	0.018 (0.020)	0.39
Home Improvements												
Any Home Improvement	0.053 (0.031)	0.09	0.022 (0.026)	0.40	0.038 (0.031)	0.24	0.056 (0.031)	0.07	0.029 (0.026)	0.27	0.041 (0.032)	0.20
Major Home Improvement (over $200)	0.032 (0.030)	0.29	0.017 (0.025)	0.50	0.025 (0.031)	0.41	0.035 (0.030)	0.24	0.022 (0.025)	0.38	0.029 (0.031)	0.34
Home Purchase or Related Activities												
Home Purchase	**0.089** (0.037)	0.02	-0.006 (0.030)	0.84	**0.092** (0.032)	0.00	**0.095** (0.037)	0.01	-0.002 (0.030)	0.94	**0.095** (0.032)	0.00
Looked through Home Listings in Newspaper*	0.045 (0.032)	0.16	0.030 (0.042)	0.47	0.044 (0.042)	0.30	0.049 (0.032)	0.13	0.035 (0.042)	0.40	0.045 (0.043)	0.30
Drove to Look at Houses for Sale*	0.033 (0.032)	0.30	-0.026 (0.041)	0.52	0.079 (0.043)	0.07	0.037 (0.032)	0.25	-0.026 (0.040)	0.52	0.083 (0.043)	0.05
Attended an Open House*	**0.079** (0.039)	0.04	-0.036 (0.038)	0.34	**0.107** (0.040)	0.01	**0.088** (0.038	0.02	-0.030 (0.038)	0.44	**0.112** (0.041)	0.01
Talked to Anyone about Borrowing Money for a Home*	0.067 (0.039)	0.08	-0.022 (0.042)	0.60	**0.095** (0.041)	0.02	0.073 (0.039)	0.06	-0.019 (0.042)	0.65	**0.099** (0.041)	0.02
Cleared up Old Debts to Apply for Home Loan*	**0.117** (0.038)	0.00	0.094 (0.042)	0.02	**0.100** (0.043)	0.02	**0.122** (0.038)	0.00	**0.096** (0.042)	0.02	**0.106** (0.043)	0.01
Talked with a Realtor about buying a Home*	0.034 (0.035)	0.34	-0.029 (0.042)	0.49	0.075 (0.042)	0.08	0.039 (0.035)	0.27	-0.021 (0.042)	0.62	0.079 (0.042)	0.06
Intensity of Home Search	**0.465** (0.185)	0.01	0.005 (0.204)	0.98	**0.591** (0.223)	0.01	**0.504** (0.185)	0.01	0.034 (0.205)	0.87	**0.619** (0.225)	0.01

Exhibit C.2: Impacts on Asset-Building Activities *(Continued)*

	Estimates Using Revised Data						Estimates Using Original Data					
	Months 1 - 48		Months 1 - 18		Months 19 - 48		Months 1 - 48		Months 1 - 18		Months 19-48	
Outcome	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value
Education/Training												
Took Non-Degree Course	0.009 (0.035)	0.80	0.006 (0.035)	0.87	**0.066** (0.031)	0.04	0.017 (0.036)	0.64	0.006 (0.035)	0.85	**0.071** (0.031)	0.02
Took Course Toward Degree	-0.010 (0.033)	0.75	0.001 (0.035)	0.97	0.013 (0.034)	0.70	-0.006 (0.033)	0.86	0.001 (0.035)	0.97	0.016 (0.035)	0.65
Finished Job Training Program with Certificate	-0.001 (0.035)	0.97	-0.012 (0.033)	0.60	-0.021 (0.034)	0.54	0.002 (0.035)	0.95	-0.015 (0.033)	0.66	-0.019 (0.034)	0.57
Graduated from School	-0.037 (0.029)	0.21	-0.023 (0.025)	0.37	-0.023 (0.026)	0.37	-0.039 (0.029)	0.18	-0.024 (0.025)	0.34	-0.024 (0.026)	0.36
Any Postsecondary Training or Education	-0.002 (0.030)	0.94	0.004 (0.035)	0.91	0.045 (0.035)	0.20	0.009 (0.030)	0.77	0.006 (0.035)	0.85	0.053 (0.035)	0.13

* Respondents who either purchased a home or conducted this type of home search in the specified period are counted as “Yes”

Note: Estimates in bold are statistically significant at the 0.05 level.

Exhibit C.3: Impacts on Financial Outcomes

Outcome	Estimates Using Revised Data				Estimates Using Original Data			
	Month 48		Month 18		Month 48		Month 18	
	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value
Liquid Assets								
Amount held in Checking and Savings Accounts {including IDAs}, Money Market Accounts, and CDs	-55 (367)	0.88	280 (212)	0.19	-93 (369)	0.80	388 (338)	0.25
Retirement Savings								
Amount held in pensions, IRAs, 401(k)s	581 (338)	0.09	-358 (228)	0.12	523 (339)	0.12	-348 (264)	0.19
Other Financial Assets								
Stocks and bonds, educational accounts, Christmas clubs, savings held with family and friends, and all other savings	-2650 (1608)	0.10	-361 (214)	0.09	-2608 (1562)	0.10	-725 (370)	0.05
Total Financial Assets								
Sum of Liquid Assets, Retirement Savings, and Other Financial Assets	-2124 (1890)	0.26	-438 (455)	0.34	-2179 (1854)	0.24	-686 (669)	0.31
Real Assets								
Market value of primary residence, other property, vehicles, and business assets	6310 (3552)	0.08	-719 (2481)	0.77	6946 (3551)	0.05	-291 (2525)	0.91
Total Assets								
Sum of Total Financial Assets and Real Assets	4186 (4292)	0.33	-1157 (2622)	0.66	4768 (4268)	0.26	-977 (2741)	0.72
Total Liabilities								
Total indebtedness: mortgage(s), vehicle loans, credit card debt, educational loans, medical bills, personal and business loans, etc.	4157 (2672)	0.12	1529 (1547)	0.32	5105 (2679)	0.06	**3658** (1848)	0.05
Net Worth								
Total Assets minus Total Liabilities	29 (3433)	0.99	-2686 (2188)	0.22	-337 (3426)	0.92	**-4635** (2316)	0.05

Note: Estimates in bold are statistically significant at the 0.05 level.

Exhibit C.4: Impacts on Employment and Income

	Estimates Using Revised Data				Estimates Using Original Data			
	Month 48		Month 18		Month 48		Month 18	
Outcome	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value	Estimate (Std.Err)	p-value
Monthly Household Income	-118	0.44	232	0.36	-100	0.52	290	0.27
	(151)		(255)		(153)		(260)	
Household Income-to-Poverty Ratio	-0.134	0.27	0.136	0.52	-0.127	0.30	0.172	0.85
	(0.120)		(0.210)		(0.122)		(0.213)	
Respondent's Monthly Earnings	-78	0.29	-62	0.42	-62	0.41	-165	0.45
	(75)		(77)		(76)		(218)	
Respondent's Employment	-0.053	0.06	0.017	0.47	-0.053	0.06	0.009	0.69
	(0.028)		(0.024)		(0.028)		(0.023)	
Household Receipt of Public Assistance	0.009	0.79	0.016	0.64	0.002	0.94	0.012	0.73
	(0.033)		(0.033)		(0.033)		(0.035)	

Exhibit C.5: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Treatment Cases, by Variable

	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	8	-1600	946.4	11360	0	1469.6	5480	0	1488.3	14990
Income to Poverty Ratio	8	-1.1	0.4	6.1	0.0	1.3	4.1	0	1.3	8.1
Monthly Earnings	59	-8400	27.5	1269	0	1274.8	9000	0	1278.7	3998
Liquid Assets	7	-35000	-5830.0	-93	0	852.1	35000	0	753.2	29600
Retirement Savings	29	-40000	-2164.2	1200	0	1083.8	50000	0	934.4	50000
Total Financial Assets	36	-55000	-3542.9	-54	0	2495.7	63370	0	2190.2	63316
Real Assets	36	-25200	1262.4	33500	0	14355.4	168500	0	14464.9	168500
Total Assets	67	-55000	-1218.4	33500	0	16851.1	181700	0	16655.0	181700
Total Liabilities	37	-13000	4692.7	79920	0	14143.8	108500	0	14565.1	108500
Net Worth	90	-79920	-2844.8	30000	-94093	2707.3	136000	-94047	2089.9	136000
Homeownership	0	.	.	.	0	0.2	1	0	0.2	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	1	2.0	2	1	2.0	2
Vehicle Ownership	0	.	.	.	1	1.2	2	1	1.2	2
Savings in Bonds	4	-20000	-5781.4	-54	0	73.3	20000	0	16.1	2600
Savings in CDs	0	.	.	.	0	49.9	10500	0	49.9	10500
Savings in Checking Accounts	0	.	.	.	0	277.0	4500	0	277.0	4500
Savings in Christmas Clubs	0	.	.	.	0	4.8	500	0	4.8	500
Savings in Educational Accounts	0	.	.	.	0	98.5	10000	0	98.5	10000
Savings in Savings Accounts	7	-35000	-5830.0	-93	0	434.4	35000	0	335.5	14500
Savings in Money Markets	0	.	.	.	0	90.9	14500	0	90.9	14500
Savings in Retirement Accounts	9	-20000	-3696.0	1200	0	594.7	50000	0	515.5	50000
Savings in Pensions	23	-20000	-1278.0	-67	0	489.1	30000	0	418.9	30000
Savings in Stocks	0	.	.	.	0	320.3	62000	0	320.3	62000
Savings with Family	0	.	.	.	0	3.7	300	0	3.7	300
Savings in the Home	0	.	.	.	0	52.0	7000	0	52.0	7000
Other Savings	0	.	.	.	0	7.2	1000	0	7.2	1000
Value of Business Assets	0	.	.	.	0	467.7	65000	0	467.7	65000
Value of Vehicles	25	-25200	755.3	33500	0	3845.7	30000	0	3890.9	37000

Exhibit C.5: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Treatment Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	5	-3000	-628.5	-7	0	757.9	50000	0	750.1	50000
Current Value of Home	9	74.2574257	3362.8	30000	0	9284.1	110000	0	9356.2	110000
Debt: Home Mortgage	7	-10000	5051.5	40000	0	5052.6	90000	0	5138.3	90000
Debt: Auto Loans	6	-5300	4136.8	27000	0	2055.5	30000	0	2114.0	30000
Debt: Home Improvement	0	.	.	.	0	186.7	20000	0	186.7	20000
Debt: Business Loans from Banks	0	.	.	.	0	0.0	0	0	0.0	0
Debt: Business Loans from Family	0	.	.	.	0	82.5	15000	0	82.5	15000
Debt: Credit cards	3	60.2678571	4540.2	13500	0	1098.6	16000	0	1130.6	16000
Debt: Installment Plans	0	.	.	.	0	137.7	5200	0	137.7	5200
Debt: Educational Loans	9	-200	13808.6	79920	0	3347.4	90000	0	3653.2	90000
Debt: Collection Agencies	0	.	.	.	0	640.7	40000	0	640.7	40000
Debt: Loans for Other Properties	0	.	.	.	0	267.6	55000	0	267.6	55000
Debt: Personal Loans from Banks	2	-10800	-5413.1	-26	0	102.2	12000	0	74.1	7300
Debt: Personal Loans from Friends	0	.	.	.	0	89.6	5000	0	89.6	5000
Debt: Medical	12	-13000	-1121.8	-34	0	850.3	40000	0	817.6	40000
Debt: Past Due Rent	0	.	.	.	0	64.6	3000	0	64.6	3000
Debt: Overdue Phone Bills	0	.	.	.	0	30.3	600	0	30.3	600
Debt: Overdue Utility Bills	0	.	.	.	0	39.0	1500	0	39.0	1500
Debt: Book Clubs etc	0	.	.	.	0	5.3	495	0	5.3	495
Debt: Other	0	.	.	.	0	93.3	10000	0	93.3	10000
Income: Wages	0	.	.	.	0	1008.6	3692	0	1008.6	3692
Income: Self-Employment	0	.	.	.	0	156.0	3200	0	156.0	3200
Income: TANF	0	.	.	.	0	4.7	300	0	4.7	300
Income: SSI	2	-1000	-525.0	-2	0	26.3	1400	0	23.7	1400
Income: Social Security	2	-200	-103.2	-6	0	47.3	1800	0	46.8	1800
Income: UI	0	.	.	.	0	8.4	600	0	8.4	600
Income: Veterans benefits	0	.	.	.	0	4.5	490	0	4.5	490

Exhibit C.5: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Treatment Cases, by Variable *(Continued)*

Variable	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	1	11360	11360.0	11360	0	9.8	1500	0	36.5	12000
Income: Child Support	0	.	.	.	0	65.4	800	0	65.4	800
Income: Alimony	0	.	.	.	0	3.0	600	0	3.0	600
Income: Selling things	0	.	.	.	0	2.3	400	0	2.3	400
Income: Odd Jobs	0	.	.	.	0	6.3	400	0	6.3	400
Income: Taking People Places	0	.	.	.	0	1.1	50	0	1.1	50
Income: Investments	0	.	.	.	0	1.4	390	0	1.4	390
Income: Family	0	.	.	.	0	42.4	2000	0	42.4	2000
Income: Food Stamps	0	.	.	.	0	44.6	600	0	44.6	600
Income: Other Sources	4	-1600	-488.7	-107	0	23.6	3600	0	18.6	2000
Income: Parents	0	.	.	.	0	1.6	100	0	1.6	100
Income: Partner	0	.	.	.	0	6.5	400	0	6.5	400
Income: Spouse	0	.	.	.	0	6.1	500	0	6.1	500

Exhibit C.6: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Control Cases, by Variable

	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	10	-605	-128.5	-2	0	1420.9	5000	0	1417.9	5000
Income to Poverty Ratio	10	-0.4	-0.1	0.0	0.0	1.3	5.4	0	1.2	5.4
Monthly Earnings	50	-19917	-619.2	1489	0	1323.2	21650	0	1250.6	5000
Liquid Assets	2	-3000	-1568.6	-7	0	1076.1	31200	0	1068.8	31200
Retirement Savings	10	-200	668.1	6600	0	547.1	22000	0	562.9	22000
Total Financial Assets	13	-4800	-116.8	6600	0	2044.3	39000	0	2040.7	39000
Real Assets	27	-58500	-1078.2	65000	0	16436.6	221000	0	16368.5	221000
Total Assets	39	-58500	-784.0	65000	0	18480.9	237800	0	18409.2	237800
Total Liabilities	29	-4750	5201.8	74995	0	14664.4	102400	0	15015.0	102400
Net Worth	67	-74995	-2699.1	65000	-98370	3816.6	156800	-98370	3394.1	156800
Homeownership	0	.	.	.	0	0.2	1	0	0.2	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	1	2.0	2	1	2.0	2
Vehicle Ownership	0	.	.	.	1	1.2	2	1	1.2	2
Savings in Bonds	0	.	.	.	0	35.0	3000	0	35.0	3000
Savings in CDs	0	.	.	.	0	122.6	15000	0	122.6	15000
Savings in Checking Accounts	0	.	.	.	0	337.8	8000	0	337.8	8000
Savings in Christmas Clubs	0	.	.	.	0	2.8	600	0	2.8	600
Savings in Educational Accounts	0	.	.	.	0	151.9	36000	0	151.9	36000
Savings in Savings Accounts	2	-3000	-1568.6	-7	0	563.0	18000	0	555.8	15000
Savings in Money Markets	0	.	.	.	0	52.7	12000	0	52.7	12000
Savings in Retirement Accounts	0	.	.	.	0	326.0	22000	0	326.0	22000
Savings in Pensions	10	-200	668.1	6600	0	221.1	12000	0	236.9	12000
Savings in Stocks	0	.	.	.	0	136.2	15000	0	136.2	15000
Savings with Family	2	-4800	-2526.3	-253	0	56.0	5000	0	43.8	3000
Savings in the Home	0	.	.	.	0	34.7	1600	0	34.7	1600
Other Savings	0	.	.	.	0	4.6	1000	0	4.6	1000
Value of Business Assets	0	.	.	.	0	651.0	100000	0	651.0	100000
Value of Vehicles	22	-58500	-5886.2	-301	0	4449.9	65000	0	4146.4	30000

Exhibit C.6: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Control Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	1	-2000	-2000.0	-2000	0	444.7	60000	0	440.2	60000
Current Value of Home	6	233	17073.0	65000	0	10891.0	100000	0	11130.8	100000
Debt: Home Mortgage	2	1	244.6	488	0	5934.9	84000	0	5936.1	84000
Debt: Auto Loans	0	.	.	.	0	2323.2	20000	0	2323.2	20000
Debt: Home Improvement	2	70	15035.1	30000	0	156.8	23000	0	223.1	30000
Debt: Business Loans from Banks	0	.	.	.	0	77.0	10000	0	77.0	10000
Debt: Business Loans from Family	0	.	.	.	0	22.7	5000	0	22.7	5000
Debt: Credit cards	7	-4750	-661.2	-11	0	1034.6	22000	0	1023.9	22000
Debt: Installment Plans	0	.	.	.	0	167.5	7000	0	167.5	7000
Debt: Educational Loans	7	419	7675.4	30000	0	3390.6	98000	0	3515.3	98000
Debt: Collection Agencies	0	.	.	.	0	557.7	40000	0	557.7	40000
Debt: Loans for Other Properties	0	.	.	.	0	48.1	15000	0	48.1	15000
Debt: Personal Loans from Banks	0	.	.	.	0	112.1	9000	0	112.1	9000
Debt: Personal Loans from Friends	0	.	.	.	0	132.8	14000	0	132.8	14000
Debt: Medical	8	183	9267.8	74995	0	563.6	17000	0	736.5	75000
Debt: Past Due Rent	0	.	.	.	0	24.1	1200	0	24.1	1200
Debt: Overdue Phone Bills	3	-770	-265.2	-2	0	25.8	900	0	23.9	575
Debt: Overdue Utility Bills	2	-800	-400.9	-2	0	35.1	1200	0	33.2	800
Debt: Book Clubs etc	0	.	.	.	0	4.5	200	0	4.5	200
Debt: Other	0	.	.	.	0	53.1	5000	0	53.1	5000
Income: Wages	6	-605	-105.0	-2	0	1002.7	3900	0	1001.2	3900
Income: Self-Employment	0	.	.	.	0	141.3	2500	0	141.3	2500
Income: TANF	0	.	.	.	0	5.7	440	0	5.7	440
Income: SSI	0	.	.	.	0	38.5	1500	0	38.5	1500
Income: Social Security	4	-600	-163.4	-30	0	38.1	3000	0	36.6	3000
Income: UI	0	.	.	.	0	6.0	808	0	6.0	808
Income: Veterans benefits	0	.	.	.	0	8.0	900	0	8.0	900

Exhibit C.6: Changes to Financial Variables Due to Post-Interview Verification—Wave One, Control Cases, by Variable *(Continued)*

Variable	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	0	.	.	.	0	7.1	1500	0	7.1	1500
Income: Child Support	0	.	.	.	0	47.6	1700	0	47.6	1700
Income: Alimony	0	.	.	.	0	0.7	300	0	0.7	300
Income: Selling things	0	.	.	.	0	3.6	800	0	3.6	800
Income: Odd Jobs	0	.	.	.	0	3.7	200	0	3.7	200
Income: Taking People Places	0	.	.	.	0	2.0	60	0	2.0	60
Income: Investments	0	.	.	.	0	6.9	1800	0	6.9	1800
Income: Family	0	.	.	.	0	26.0	500	0	26.0	500
Income: Food Stamps	0	.	.	.	0	44.2	546	0	44.2	546
Income: Other Sources	0	.	.	.	0	11.4	604	0	11.4	604
Income: Parents	0	.	.	.	0	3.9	300	0	3.9	300
Income: Partner	0	.	.	.	0	10.9	1000	0	10.9	1000
Income: Spouse	0	.	.	.	0	12.7	2500	0	12.7	2500

Exhibit C.7: Changes to Financial Variables Due to Post-Interview Verification—Wave Two, Treatment Cases, by Variable

	For cases with changed values:				For all cases (n=376):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	32	-14400	-306.0	2000	0	2128.9	85200	0	2103.2	85200
Income to Poverty Ratio	32	-9.8	-0.2	2.8	0.0	1.7	69.9	0	1.7	69.9
Monthly Earnings	14	-60726	-3289.5	1980	0	1602.4	63326	0	1483.6	13071
Liquid Assets	14	-3000	190.1	6000	0	1730.8	40209	0	1737.9	40209
Retirement Savings	24	-60771	-3644.0	10900	0	1413.0	60771	0	1183.6	50000
Total Financial Assets	36	-60771	-2534.8	10900	0	3863.2	86551	0	3622.6	86551
Real Assets	49	-42000	-1454.3	57500	0	28508.2	322000	0	28319.1	322000
Total Assets	75	-60771	-2169.0	57500	0	32371.5	336516	0	31941.7	336516
Total Liabilities	56	-180000	-6254.6	20000	0	24275.8	315000	0	23347.9	313200
Net Worth	107	-60771	1759.9	180000	-113866	8095.7	327053	-101474	8593.8	334253
Homeownership	0	.	.	.	0	0.3	1	0	0.3	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Vehicle Ownership	6	1	1.0	1	0	0.9	1	0	0.9	1
Savings in Bonds	0	.	.	.	0	27.7	2500	0	27.7	2500
Savings in CDs	0	.	.	.	0	105.7	10000	0	105.7	10000
Savings in Checking Accounts	10	-3000	-321.6	-8	0	396.6	5000	0	387.8	5000
Savings in Christmas Clubs	0	.	.	.	0	8.4	1000	0	8.4	1000
Savings in Educational Accounts	0	.	.	.	0	76.1	10000	0	76.1	10000
Savings in Savings Accounts	9	16	666.7	6000	0	699.0	38000	0	714.8	38000
Savings in Money Markets	0	.	.	.	0	177.3	12000	0	177.3	12000
Savings in Retirement Accounts	11	-60771	-5605.2	10900	0	710.6	60771	0	548.6	17000
Savings in Pensions	16	-10000	-1603.5	-67	0	702.4	50000	0	635.0	50000
Savings in Stocks	6	-5800	-1167.8	-149	0	505.4	75000	0	487.0	75000
Savings with Family	0	.	.	.	0	18.7	2300	0	18.7	2300
Savings in the Home	0	.	.	.	0	62.4	5000	0	62.4	5000
Other Savings	0	.	.	.	0	20.7	2500	0	20.7	2500
Value of Business Assets	0	.	.	.	0	1418.8	150000	0	1418.8	150000
Value of Vehicles	30	-42000	-2998.9	9000	0	6004.1	47000	0	5767.3	45000

Exhibit C.7: Changes to Financial Variables Due to Post-Interview Verification—Wave Two, Treatment Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=376):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	16	-30000	-2141.2	-86	0	1230.8	300000	0	1139.7	300000
Current Value of Home	6	-10000	8406.1	57500	0	20170.6	250000	0	20309.4	250000
Debt: Home Mortgage	13	-180000	-13544.6	4000	0	11781.9	300000	0	11313.6	300000
Debt: Auto Loans	18	-7200	1848.4	16000	0	3355.0	50000	0	3441.8	50000
Debt: Home Improvement	4	-74000	-19998.8	-198	0	539.2	74000	0	327.7	42000
Debt: Business Loans from Banks	6	-30000	-6512.2	79	0	121.7	30000	0	20.9	5000
Debt: Business Loans from Family	2	-2000	-1200.0	-400	0	31.4	5000	0	25.2	5000
Debt: Credit cards	9	-18900	-3234.1	-79	0	1701.1	25000	0	1624.4	25000
Debt: Installment Plans	0	.	.	.	0	104.8	3000	0	104.8	3000
Debt: Educational Loans	5	-11700	1981.6	20000	0	4548.3	80000	0	4574.9	100000
Debt: Collection Agencies	0	.	.	.	0	497.7	33000	0	497.7	33000
Debt: Loans for Other Properties	1	-15000	-15000.0	-15000	0	260.4	60000	0	217.8	45000
Debt: Personal Loans from Banks	0	.	.	.	0	99.7	10000	0	99.7	10000
Debt: Personal Loans from Friends	2	-11000	-5657.1	-314	0	154.3	14000	0	125.1	14000
Debt: Medical	9	-28000	-4321.3	-277	0	885.6	36000	0	779.7	36000
Debt: Past Due Rent	0	.	.	.	0	19.4	2000	0	19.4	2000
Debt: Overdue Phone Bills	0	.	.	.	0	36.2	1000	0	36.2	1000
Debt: Overdue Utility Bills	0	.	.	.	0	43.1	1000	0	43.1	1000
Debt: Book Clubs etc	0	.	.	.	0	3.5	200	0	3.5	200
Debt: Other	0	.	.	.	0	92.3	11000	0	92.3	11000
Income: Wages	18	-14400	-376.0	2000	0	1237.3	16000	0	1219.6	6000
Income: Self-Employment	2	-537	-318.4	-100	0	229.8	5820	0	228.1	5820
Income: TANF	0	.	.	.	0	10.2	800	0	10.2	800
Income: SSI	2	-1058	-545.5	-33	0	52.2	1800	0	49.4	1800
Income: Social Security Disability	0	.	.	.	0	29.2	1580	0	29.2	1580
Income: Social Security Retirement	4	-900	-267.2	-3	0	29.4	1150	0	26.6	1150
Income: UI	0	.	.	.	0	11.4	680	0	11.4	680
Income: Veterans benefits	0	.	.	.	0	3.7	565	0	3.7	565

Exhibit C.7: Changes to Financial Variables Due to Post-Interview Verification—Wave Two, Treatment Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=376):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	4	-25	400.2	1000	0	17.2	3000	0	21.4	3000
Income: Child Support	4	-900	-231.1	-2	0	74.9	900	0	72.5	800
Income: Alimony	0	.	.	.	0	3.0	600	0	3.0	600
Income: Selling things	0	.	.	.	0	3.7	800	0	3.7	800
Income: Odd Jobs	0	.	.	.	0	14.9	1500	0	14.9	1500
Income: Taking People Places	0	.	.	.	0	4.8	1400	0	4.8	1400
Income: Investments	0	.	.	.	0	5.4	800	0	5.4	800
Income: Family	1	-900	-900.0	-900	0	52.4	5000	0	49.8	5000
Income: Food Stamps	0	.	.	.	0	38.4	850	0	38.4	850
Income: Other Sources	0	.	.	.	0	298.2	83000	0	298.2	83000
Income: Parents	0	.	.	.	0	3.2	200	0	3.2	200
Income: Partner	0	.	.	.	0	7.6	400	0	7.6	400
Income: Spouse	0	.	.	.	0	2.0	500	0	2.0	500

Exhibit C.8: Changes to Financial Variables Due to Post-Interview Verification--Wave Two, Control Cases, by Variable

	For cases with changed values:				For all cases (n=388):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	35	-7000	230.0	3000	0	1870.1	14000	0	1891.0	14000
Income to Poverty Ratio	35	-5.7	0.2	2.0	0	1.5	14.5	0	1.6	14.5
Monthly Earnings	24	-95477	-3440.9	2394	0	1705.9	97425	0	1487.8	18186
Liquid Assets	26	-135000	-8622.3	11000	0	2249.4	150500	0	1677.9	31000
Retirement Savings	22	-8000	-1201.2	3200	0	1274.1	30000	0	1207.3	30000
Total Financial Assets	50	-135000	-6495.4	5000	0	4394.5	150500	0	3568.0	72000
Real Assets	27	-36000	-1735.5	39000	0	29682.1	426147	0	29561.3	426147
Total Assets	66	-135000	-5615.4	39000	0	34076.6	451847	0	33129.3	451847
Total Liabilities	40	-24000	5051.1	70000	0	22618.4	204000	0	23132.4	204000
Net Worth	91	-135000	-6283.9	39000	-124400	11458.2	380234	-125716	9996.9	380234
Homeownership	0	.	.	.	0	0.3	1	0	0.3	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	0	0.0	1	0	0.0	1
Vehicle Ownership	4	1	1.0	1	0	0.9	1	0	0.9	1
Savings in Bonds	0	.	.	.	0	41.8	5000	0	41.8	5000
Savings in CDs	0	.	.	.	0	74.7	5500	0	74.7	5500
Savings in Checking Accounts	14	-3500	504.8	11000	0	561.8	10000	0	579.9	15000
Savings in Christmas Clubs	0	.	.	.	0	12.4	1100	0	12.4	1100
Savings in Educational Accounts	0	.	.	.	0	224.6	30000	0	224.6	30000
Savings in Savings Accounts	16	-135000	-13916.1	5000	0	1333.8	150000	0	763.1	15000
Savings in Money Markets	6	-7000	-1274.2	-20	0	279.2	12000	0	260.3	12000
Savings in Retirement Accounts	14	-8000	-1291.0	3200	0	774.8	30000	0	729.1	30000
Savings in Pensions	13	-7000	-641.6	-21	0	499.2	15000	0	478.2	15000
Savings in Stocks	11	-28000	-5886.1	-165	0	468.4	50000	0	305.2	25000
Savings with Family	0	.	.	.	0	34.6	5000	0	34.6	5000
Savings in the Home	4	-10000	-2568.8	-26	0	83.1	11000	0	58.0	5000
Other Savings	0	.	.	.	0	6.2	600	0	6.2	600
Value of Business Assets	0	.	.	.	0	4465.6	600000	0	4465.6	600000
Value of Vehicles	24	-36000	-3890.5	6330	0	6565.8	50000	0	6325.2	50000

Exhibit C.8: Changes to Financial Variables Due to Post-Interview Verification--Wave Two, Control Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=388):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	0	.	.	.	0	139.1	55000	0	139.1	55000
Current Value of Home	6	-10000	7833.5	39000	0	20377.5	205000	0	20497.3	205000
Debt: Home Mortgage	12	326	10094.3	70000	0	10775.8	203000	0	11084.6	203000
Debt: Auto Loans	12	-24000	-628.2	12000	0	3713.7	38000	0	3694.2	36000
Debt: Home Improvement	6	-12000	-2591.3	-39	0	239.6	27000	0	200.0	24000
Debt: Business Loans from Banks	0	.	.	.	0	38.0	8000	0	38.0	8000
Debt: Business Loans from Family	1	900	900.0	900	0	17.2	3000	0	19.6	3000
Debt: Credit cards	6	83	3314.5	17600	0	1577.3	24000	0	1626.6	24000
Debt: Installment Plans	0	.	.	.	0	207.7	10000	0	207.7	10000
Debt: Educational Loans	3	10000	20191.2	31500	0	4099.5	90000	0	4251.6	90000
Debt: Collection Agencies	0	.	.	.	0	426.0	19800	0	426.0	19800
Debt: Loans for Other Properties	0	.	.	.	0	85.3	35000	0	85.3	35000
Debt: Personal Loans from Banks	0	.	.	.	0	69.5	8000	0	69.5	8000
Debt: Personal Loans from Friends	0	.	.	.	0	227.8	8000	0	227.8	8000
Debt: Medical	7	203	4351.7	30000	0	989.6	80000	0	1065.8	80000
Debt: Past Due Rent	0	.	.	.	0	20.5	1230	0	20.5	1230
Debt: Overdue Phone Bills	0	.	.	.	0	17.9	1000	0	17.9	1000
Debt: Overdue Utility Bills	0	.	.	.	0	36.1	800	0	36.1	800
Debt: Book Clubs etc	0	.	.	.	0	5.2	500	0	5.2	500
Debt: Other	3	-6000	-2027.6	-207	0	71.7	6000	0	56.0	5012
Income: Wages	19	-7000	317.2	3000	0	1235.6	9000	0	1251.3	7000
Income: Self-Employment	8	8	587.7	2000	0	278.8	12800	0	291.2	12800
Income: TANF	0	.	.	.	0	2.5	292	0	2.5	292
Income: SSI	0	.	.	.	0	40.4	1600	0	40.4	1600
Income: Social Security Disability	0	.	.	.	0	29.6	1285	0	29.6	1285
Income: Social Security Retirement	6	-2275	-465.3	62	0	35.2	2277	0	28.1	1550
Income: UI	0	.	.	.	0	13.4	900	0	13.4	900
Income: Veterans benefits	1	-200	-200.0	-200	0	12.9	2100	0	12.4	1900

Exhibit C.8: Changes to Financial Variables Due to Post-Interview Verification--Wave Two, Control Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=388):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	0	.	.	.	0	9.4	1400	0	9.4	1400
Income: Child Support	0	.	.	.	0	59.1	1700	0	59.1	1700
Income: Alimony	0	.	.	.	0	0.8	220	0	0.8	220
Income: Selling things	0	.	.	.	0	7.7	1000	0	7.7	1000
Income: Odd Jobs	0	.	.	.	0	21.4	2000	0	21.4	2000
Income: Taking People Places	0	.	.	.	0	1.0	150	0	1.0	150
Income: Investments	0	.	.	.	0	7.8	1200	0	7.8	1200
Income: Family	2	1	261.5	500	0	40.5	3000	0	41.8	3000
Income: Food Stamps	3	-284	-98.4	-6	0	25.7	600	0	24.9	500
Income: Other Sources	0	.	.	.	0	29.3	4300	0	29.3	4300
Income: Parents	0	.	.	.	0	6.7	800	0	6.7	800
Income: Partner	0	.	.	.	0	8.7	1000	0	8.7	1000
Income: Spouse	0	.	.	.	0	3.3	600	0	3.3	600

Exhibit C.9: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Treatment Cases, by Variable

	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	2	-265	-140.2	-3	25	2256.6	40290	25	2256.0	40290
Income to Poverty Ratio	2	-0.4	-0.2	0.0	0	1.7	31.7	0	1.7	31.7
Monthly Earnings	0	.	.	.	0	1360.0	6000	0	1360.0	6000
Liquid Assets	6	-3500	-587.7	-17	-860	1904.8	58000	-860	1896.3	58000
Retirement Savings	0	.	.	.	0	2393.7	62000	0	2393.7	62000
Total Financial Assets	6	-3500	-587.7	-17	-860	5090.2	94106	-860	5081.8	94106
Real Assets	6	-5000	-804.0	-12	0	43062.0	922000	0	43050.1	922000
Total Assets	11	-5000	-758.8	-12	0	48152.3	927400	0	48131.9	927400
Total Liabilities	19	-16500	-938.7	24	0	37097.0	312500	0	37053.9	312500
Net Worth	26	-3500	361.6	11500	-107589	11055.3	761400	-107589	11078.0	761400
Homeownership	0	.	.	.	0	0.5	1	0	0.5	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Vehicle Ownership	0	.	.	.	0	0.9	1	0	0.9	1
Savings in Bonds	0	.	.	.	0	53.3	5400	0	53.3	5400
Savings in CDs	0	.	.	.	0	130.3	8900	0	130.3	8900
Savings in Checking Accounts	0	.	.	.	0	224.8	5000	0	224.8	5000
Savings in Christmas Clubs	0	.	.	.	0	8.7	900	0	8.7	900
Savings in Educational Accounts	0	.	.	.	0	151.0	11000	0	151.0	11000
Savings in Savings Accounts	6	-3500	-587.7	-17	0	726.3	26000	0	717.8	26000
Savings in Money Markets	0	.	.	.	0	238.4	29000	0	238.4	29000
Savings in Retirement Accounts	0	.	.	.	0	1250.6	50000	0	1250.6	50000
Savings in Pensions	0	.	.	.	0	1143.1	42000	0	1143.1	42000
Savings in Stocks	0	.	.	.	0	424.6	36000	0	424.6	36000
Savings with Family	0	.	.	.	0	31.4	4000	0	31.4	4000
Savings in the Home	0	.	.	.	0	80.6	7000	0	80.6	7000
Other Savings	0	.	.	.	0	42.0	10000	0	42.0	10000
Value of Business Assets	0	.	.	.	0	3127.4	500000	0	3127.4	500000
Value of Vehicles	0	.	.	.	0	7594.2	62000	0	7594.2	62000

Exhibit C.9: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Treatment Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	0	.	.	.	0	878.1	72000	0	878.1	72000
Current Value of Home	6	-5000	-804.0	-12	0	31604.8	400000	0	31592.9	400000
Debt: Home Mortgage	13	-20000	-1637.1	-156	0	20008.1	300000	0	19956.5	300000
Debt: Auto Loans	0	.	.	.	0	4536.7	80000	0	4536.7	80000
Debt: Home Improvement	0	.	.	.	0	367.7	30000	0	367.7	30000
Debt: Business Loans from Banks	0	.	.	.	0	639.1	120000	0	639.1	120000
Debt: Business Loans from Family	0	.	.	.	0	83.2	30000	0	83.2	30000
Debt: Credit cards	0	.	.	.	0	2232.2	32000	0	2232.2	32000
Debt: Installment Plans	0	.	.	.	0	162.9	17000	0	162.9	17000
Debt: Educational Loans	0	.	.	.	0	5873.2	101000	0	5873.2	101000
Debt: Collection Agencies	0	.	.	.	0	785.8	60000	0	785.8	60000
Debt: Loans for Other Properties	0	.	.	.	0	520.0	122000	0	520.0	122000
Debt: Personal Loans from Banks	0	.	.	.	0	112.5	15000	0	112.5	15000
Debt: Personal Loans from Friends	0	.	.	.	0	158.0	7000	0	158.0	7000
Debt: Medical	7	9	511.8	3500	0	1406.6	70000	0	1415.1	70000
Debt: Past Due Rent	0	.	.	.	0	32.3	5000	0	32.3	5000
Debt: Overdue Phone Bills	0	.	.	.	0	30.7	1600	0	30.7	1600
Debt: Overdue Utility Bills	0	.	.	.	0	47.0	600	0	47.0	600
Debt: Book Clubs etc	0	.	.	.	0	1.5	150	0	1.5	150
Debt: Other	0	.	.	.	0	99.6	19000	0	99.6	19000
Income: Wages	0	.	.	.	0	1364.3	8000	0	1364.3	8000
Income: Self-Employment	0	.	.	.	0	357.4	8000	0	357.4	8000
Income: TANF	0	.	.	.	0	5.6	400	0	5.6	400
Income: SSI	0	.	.	.	0	40.9	2000	0	40.9	2000
Income: Social Security Disability	0	.	.	.	0	24.1	700	0	24.1	700
Income: Social Security Retirement	0	.	.	.	0	30.9	1100	0	30.9	1100
Income: UI	0	.	.	.	0	45.6	1444	0	45.6	1444
Income: Veterans benefits	0	.	.	.	0	12.3	2681	0	12.3	2681

Exhibit C.9: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Treatment Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=412):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	0	.	.	.	0	13.9	1700	0	13.9	1700
Income: Child Support	0	.	.	.	0	74.4	1150	0	74.4	1150
Income: Alimony	0	.	.	.	0	2.7	600	0	2.7	600
Income: Selling things	0	.	.	.	0	3.7	500	0	3.7	500
Income: Odd Jobs	0	.	.	.	0	6.9	200	0	6.9	200
Income: Taking People Places	0	.	.	.	0	1.5	70	0	1.5	70
Income: Investments	0	.	.	.	0	3.1	510	0	3.1	510
Income: Family	0	.	.	.	0	55.6	1700	0	55.6	1700
Income: Food Stamps	2	-265	-140.2	-3	0	55.1	800	0	54.4	800
Income: Other Sources	0	.	.	.	0	141.1	40000	0	141.1	40000
Income: Parents	0	.	.	.	0	1.3	200	0	1.3	200
Income: Partner	0	.	.	.	0	14.8	2000	0	14.8	2000
Income: Spouse	0	.	.	.	0	1.5	300	0	1.5	300

Exhibit C.10: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Control Cases, by Variable

	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Monthly Household Income	0	.	.	.	139	2255.9	22800	139	2255.9	22800
Income to Poverty Ratio	0	.	.	.	0.1	1.8	18	0	1.8	17.9
Monthly Earnings	0	.	.	.	0	1381.6	6500	0	1381.6	6500
Liquid Assets	11	-700	-69.4	-2	0	2258.3	80305	0	2256.5	80305
Retirement Savings	0	.	.	.	0	1759.8	60000	0	1759.8	60000
Total Financial Assets	11	-700	-69.4	-2	0	6625.6	497200	0	6623.8	497200
Real Assets	8	-25000	-2966.7	300	0	39125.9	391500	0	39070.6	391500
Total Assets	18	-25000	-1375.1	300	0	45751.5	736700	0	45694.4	736700
Total Liabilities	12	9	8068.3	71000	0	34622.0	351200	0	34847.0	351200
Net Worth	24	-71000	-5094.7	300	-351000	11129.5	573200	-351000	10847.4	573200
Homeownership	0	.	.	.	0	0.4	1	0	0.4	1
Business Ownership	0	.	.	.	0	0.1	1	0	0.1	1
Other Property Ownership	0	.	.	.	0	0.0	1	0	0.0	1
Vehicle Ownership	0	.	.	.	0	0.9	1	0	0.9	1
Savings in Bonds	0	.	.	.	0	79.3	12500	0	79.3	12500
Savings in CDs	0	.	.	.	0	373.8	53000	0	373.8	53000
Savings in Checking Accounts	0	.	.	.	0	258.2	17000	0	258.2	17000
Savings in Christmas Clubs	0	.	.	.	0	7.1	650	0	7.1	650
Savings in Educational Accounts	0	.	.	.	0	807.7	100000	0	807.7	100000
Savings in Savings Accounts	11	-700	-69.4	-2	0	861.6	21000	0	859.9	21000
Savings in Money Markets	0	.	.	.	0	356.7	30000	0	356.7	30000
Savings in Retirement Accounts	0	.	.	.	0	834.8	60000	0	834.8	60000
Savings in Pensions	0	.	.	.	0	924.9	30000	0	924.9	30000
Savings in Stocks	0	.	.	.	0	1467.8	450000	0	1467.8	450000
Savings with Family	0	.	.	.	0	47.8	6500	0	47.8	6500
Savings in the Home	0	.	.	.	0	48.7	2000	0	48.7	2000
Other Savings	0	.	.	.	0	149.1	40000	0	149.1	40000
Value of Business Assets	0	.	.	.	0	3206.9	500000	0	3206.9	500000
Value of Vehicles	1	300	300.0	300	0	6836.4	51000	0	6837.0	51000

Exhibit C.10: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Control Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Value of Other Property	0	.	.	.	0	814.3	95000	0	814.3	95000
Current Value of Home	7	-25000	-3405.0	-59	0	28938.1	210000	0	28882.1	210000
Debt: Home Mortgage	0	.	.	.	0	17757.8	250000	0	17757.8	250000
Debt: Auto Loans	0	.	.	.	0	3902.7	54000	0	3902.7	54000
Debt: Home Improvement	0	.	.	.	0	415.3	40000	0	415.3	40000
Debt: Business Loans from Banks	0	.	.	.	0	719.1	200000	0	719.1	200000
Debt: Business Loans from Family	0	.	.	.	0	495.5	100000	0	495.5	100000
Debt: Credit cards	9	9	451.3	3800	0	1953.5	35000	0	1962.9	35000
Debt: Installment Plans	0	.	.	.	0	89.4	8000	0	89.4	8000
Debt: Educational Loans	3	213	30701.6	71000	0	5168.6	92000	0	5384.2	92000
Debt: Collection Agencies	0	.	.	.	0	693.5	60000	0	693.5	60000
Debt: Loans for Other Properties	0	.	.	.	0	333.3	95000	0	333.3	95000
Debt: Personal Loans from Banks	0	.	.	.	0	102.3	6000	0	102.3	6000
Debt: Personal Loans from Friends	0	.	.	.	0	849.8	120000	0	849.8	120000
Debt: Medical	0	.	.	.	0	1984.3	350000	0	1984.3	350000
Debt: Past Due Rent	0	.	.	.	0	23.9	2500	0	23.9	2500
Debt: Overdue Phone Bills	0	.	.	.	0	38.8	5000	0	38.8	5000
Debt: Overdue Utility Bills	0	.	.	.	0	44.4	900	0	44.4	900
Debt: Book Clubs etc	0	.	.	.	0	1.0	200	0	1.0	200
Debt: Other	0	.	.	.	0	48.8	11000	0	48.8	11000
Income: Wages	0	.	.	.	0	1517.1	16000	0	1517.1	16000
Income: Self-Employment	0	.	.	.	0	269.0	6000	0	269.0	6000
Income: TANF	0	.	.	.	0	5.0	351	0	5.0	351
Income: SSI	0	.	.	.	0	33.0	1600	0	33.0	1600
Income: Social Security Disability	0	.	.	.	0	22.1	494	0	22.1	494
Income: Social Security Retirement	0	.	.	.	0	68.2	988	0	68.2	988
Income: UI	0	.	.	.	0	42.5	1800	0	42.5	1800
Income: Veterans benefits	0	.	.	.	0	9.4	1900	0	9.4	1900

Exhibit C.10: Changes to Financial Variables Due to Post-Interview Verification—Wave Three, Control Cases, by Variable *(Continued)*

	For cases with changed values:				For all cases (n=428):					
					Before verification			After verification		
Variable	Number of cases with change	Minimum change	Mean change	Maximum change	Minimum Value	Mean Value	Maximum Value	Minimum Value	Mean Value	Maximum Value
Income: Pension	0	.	.	.	0	16.7	1500	0	16.7	1500
Income: Child Support	0	.	.	.	0	58.1	1700	0	58.1	1700
Income: Alimony	0	.	.	.	0	0.9	400	0	0.9	400
Income: Selling things	0	.	.	.	0	7.5	1500	0	7.5	1500
Income: Odd Jobs	0	.	.	.	0	10.0	1200	0	10.0	1200
Income: Taking People Places	0	.	.	.	0	1.0	100	0	1.0	100
Income: Investments	0	.	.	.	0	66.7	20000	0	66.7	20000
Income: Family	0	.	.	.	0	42.4	3000	0	42.4	3000
Income: Food Stamps	0	.	.	.	0	47.6	630	0	47.6	630
Income: Other Sources	0	.	.	.	0	29.7	3000	0	29.7	3000
Income: Parents	0	.	.	.	0	1.8	400	0	1.8	400
Income: Partner	0	.	.	.	0	4.2	400	0	4.2	400
Income: Spouse	0	.	.	.	0	3.1	400	0	3.1	400

Exhibit C.11: Effect of Post-Interview Verification on Treatment-Control Differences—Wave One, Analysis Sample, by Variable

Variable	Mean treatment-control difference (n=840): Before verification	After verification	Change due to verification
Monthly Household Income	48.67	70.41	21.74
Income to Poverty Ratio	0.00	0.01	0.01
Monthly Earnings	-48.39	28.11	76.50
Liquid Assets	-223.93	-315.60	-91.67
Retirement Savings	536.74	371.50	-165.24
Total Financial Assets	451.43	149.49	-301.94
Real Assets	-2081.22	-1903.61	177.61
Total Assets	-1629.78	-1754.13	-124.35
Total Liabilities	-520.54	-449.93	70.61
Net Worth	-1109.25	-1304.20	-194.95
Homeownership	-0.02	-0.02	0.00
Business Ownership	0.02	0.02	0.00
Other Property Ownership	-0.03	-0.03	0.00
Vehicle Ownership	0.00	0.00	0.00
Savings in Bonds	38.33	-18.88	-57.21
Savings in CDs	-72.74	-72.74	0.00
Savings in Checking Accounts	-60.71	-60.71	0.00
Savings in Christmas Clubs	1.91	1.91	0.00
Savings in Educational Accounts	-53.39	-53.39	0.00
Savings in Savings Accounts	-128.67	-220.34	-91.67
Savings in Money Markets	38.19	38.19	0.00
Savings in Retirement Accounts	268.72	189.47	-79.25
Savings in Pensions	268.02	182.04	-85.98
Savings in Stocks	184.09	184.09	0.00
Savings with Family	-52.20	-40.04	12.17
Savings in the Home	17.28	17.28	0.00
Other Savings	2.60	2.60	0.00
Value of Business Assets	-183.33	-183.33	0.00
Value of Vehicles	-604.21	-255.49	348.72
Value of Other Property	313.28	309.84	-3.43
Current Value of Home	-1606.95	-1774.64	-167.69
Debt: Home Mortgage	-882.27	-797.75	84.52
Debt: Auto Loans	-267.70	-209.22	58.48
Debt: Home Improvement	29.89	-36.47	-66.37
Debt: Business Loans from Banks	-77.00	-77.00	0.00
Debt: Business Loans from Family	59.76	59.76	0.00
Debt: Credit cards	63.94	106.77	42.83
Debt: Installment Plans	-29.78	-29.78	0.00
Debt: Educational Loans	-43.18	137.92	181.10
Debt: Collection Agencies	82.92	82.92	0.00
Debt: Loans for Other Properties	219.48	219.48	0.00

Exhibit C.11: Effect of Post-Interview Verification on Treatment-Control Differences—Wave One, Analysis Sample, by Variable *(Continued)*

Variable	Mean treatment-control difference (n=840): Before verification	After verification	Change due to verification
Debt: Personal Loans from Banks	-9.97	-38.03	-28.06
Debt: Personal Loans from Friends	-43.25	-43.25	0.00
Debt: Medical	286.78	81.07	-205.71
Debt: Past Due Rent	40.51	40.51	0.00
Debt: Overdue Phone Bills	4.49	6.35	1.86
Debt: Overdue Utility Bills	3.85	5.78	1.93
Debt: Book Clubs etc	0.84	0.84	0.00
Debt: Other	40.14	40.14	0.00
Income: Wages	5.86	7.34	1.48
Income: Self-Employment	14.64	14.64	0.00
Income: TANF	-1.00	-1.00	0.00
Income: SSI	-12.22	-14.82	-2.60
Income: Social Security	9.17	10.22	1.05
Income: UI	2.48	2.48	0.00
Income: Veterans benefits	-3.52	-3.52	0.00
Income: Pension	2.65	29.42	26.77
Income: Child Support	17.83	17.83	0.00
Income: Alimony	2.31	2.31	0.00
Income: Selling things	-1.32	-1.32	0.00
Income: Odd Jobs	2.53	2.53	0.00
Income: Taking People Places	-0.92	-0.92	0.00
Income: Investments	-5.58	-5.58	0.00
Income: Family	16.38	16.38	0.00
Income: Food Stamps	0.33	0.33	0.00
Income: Other Sources	12.21	7.26	-4.95
Income: Parents	-2.29	-2.29	0.00
Income: Partner	-4.33	-4.33	0.00
Income: Spouse	-6.54	-6.54	0.00

Exhibit C.12: Effect of Post-Interview Verification on Treatment-Control Differences—Wave Two, Analysis Sample, by Variable

Variable	Mean treatment-control difference (n=764): Before verification	After verification	Change due to verification
Monthly Household Income	258.87	212.17	-46.70
Income to Poverty Ratio	0.13	0.10	-0.03
Monthly Earnings	-103.52	-4.18	99.34
Liquid Assets	-518.60	60.02	578.62
Retirement Savings	138.91	-23.65	-162.56
Total Financial Assets	-531.31	54.54	585.85
Real Assets	-1173.83	-1242.14	-68.31
Total Assets	-1705.14	-1187.60	517.54
Total Liabilities	1657.36	215.53	-1441.83
Net Worth	-3362.49	-1403.13	1959.36
Homeownership	-0.01	-0.01	0.00
Business Ownership	0.00	0.00	0.00
Other Property Ownership	0.02	0.02	0.00
Vehicle Ownership	-0.01	-0.01	0.01
Savings in Bonds	-14.15	-14.15	0.00
Savings in CDs	31.01	31.01	0.00
Savings in Checking Accounts	-165.21	-192.05	-26.84
Savings in Christmas Clubs	-3.98	-3.98	0.00
Savings in Educational Accounts	-148.46	-148.46	0.00
Savings in Savings Accounts	-634.81	-48.27	586.54
Savings in Money Markets	-101.86	-82.94	18.93
Savings in Retirement Accounts	-64.23	-180.41	-116.18
Savings in Pensions	203.15	156.76	-46.38
Savings in Stocks	37.04	181.77	144.73
Savings with Family	-15.91	-15.91	0.00
Savings in the Home	-20.65	4.41	25.06
Other Savings	14.49	14.49	0.00
Value of Business Assets	-3046.79	-3046.79	0.00
Value of Vehicles	-561.76	-557.90	3.86
Value of Other Property	1091.74	1000.59	-91.15
Current Value of Home	-206.86	-187.88	18.98
Debt: Home Mortgage	1006.13	229.07	-777.06
Debt: Auto Loans	-358.63	-252.35	106.28
Debt: Home Improvement	299.60	127.69	-171.91
Debt: Business Loans from Banks	83.76	-17.05	-100.81
Debt: Business Loans from Family	14.16	5.58	-8.59
Debt: Credit cards	123.77	-2.22	-125.99
Debt: Installment Plans	-102.89	-102.89	0.00
Debt: Educational Loans	448.87	323.28	-125.59
Debt: Collection Agencies	71.71	71.71	0.00
Debt: Loans for Other Properties	175.05	132.48	-42.57

Exhibit C.12: Effect of Post-Interview Verification on Treatment-Control Differences—Wave Two, Analysis Sample, by Variable *(Continued)*

Variable	Mean treatment-control difference (n=764): Before verification	After verification	Change due to verification
Debt: Personal Loans from Banks	30.14	30.14	0.00
Debt: Personal Loans from Friends	-73.45	-102.64	-29.19
Debt: Medical	-103.93	-286.05	-182.12
Debt: Past Due Rent	-1.11	-1.11	0.00
Debt: Overdue Phone Bills	18.32	18.32	0.00
Debt: Overdue Utility Bills	7.04	7.04	0.00
Debt: Book Clubs etc	-1.77	-1.77	0.00
Debt: Other	20.59	36.32	15.73
Income: Wages	1.67	-31.76	-33.43
Income: Self-Employment	-49.06	-63.08	-14.02
Income: TANF	7.73	7.73	0.00
Income: SSI	11.80	8.99	-2.81
Income: Social Security Disability	-0.36	-0.36	0.00
Income: Social Security Retirement	-5.81	-1.52	4.29
Income: UI	-1.96	-1.96	0.00
Income: Veterans benefits	-9.21	-8.68	0.53
Income: Pension	7.74	11.98	4.23
Income: Child Support	15.80	13.41	-2.38
Income: Alimony	2.21	2.21	0.00
Income: Selling things	-4.07	-4.07	0.00
Income: Odd Jobs	-6.53	-6.53	0.00
Income: Taking People Places	3.75	3.75	0.00
Income: Investments	-2.34	-2.34	0.00
Income: Family	11.91	8.02	-3.89
Income: Food Stamps	12.70	13.49	0.79
Income: Other Sources	268.88	268.88	0.00
Income: Parents	-3.49	-3.49	0.00
Income: Partner	-1.17	-1.17	0.00
Income: Spouse	-1.34	-1.34	0.00

Exhibit C.13: Effect of Post-Interview Verification on Treatment-Control Differences—Wave Three, Analysis Sample, by Variable

Variable	Mean treatment-control difference (n=840): Before verification	After verification	Change due to verification
Monthly Household Income	0.70	0.01	-0.69
Income to Poverty Ratio	-0.05	-0.05	0.00
Monthly Earnings	-21.63	-21.63	0.00
Liquid Assets	-353.53	-360.22	-6.69
Retirement Savings	633.92	633.92	0.00
Total Financial Assets	-1535.35	-1542.05	-6.70
Real Assets	3936.10	3979.53	43.43
Total Assets	2400.75	2437.48	36.73
Total Liabilities	2475.00	2206.85	-268.15
Net Worth	-74.25	230.62	304.87
Homeownership	0.03	0.03	0.00
Business Ownership	0.01	0.01	0.00
Other Property Ownership	0.02	0.02	0.00
Vehicle Ownership	-0.01	-0.01	0.00
Savings in Bonds	-25.97	-25.97	0.00
Savings in CDs	-243.44	-243.44	0.00
Savings in Checking Accounts	-33.43	-33.43	0.00
Savings in Christmas Clubs	1.61	1.61	0.00
Savings in Educational Accounts	-656.64	-656.64	0.00
Savings in Savings Accounts	-135.33	-142.03	-6.69
Savings in Money Markets	-118.32	-118.32	0.00
Savings in Retirement Accounts	415.76	415.76	0.00
Savings in Pensions	218.15	218.15	0.00
Savings in Stocks	-1043.16	-1043.16	0.00
Savings with Family	-16.37	-16.37	0.00
Savings in the Home	31.90	31.90	0.00
Other Savings	-107.12	-107.12	0.00
Value of Business Assets	-79.50	-79.50	0.00
Value of Vehicles	757.87	757.21	-0.66
Value of Other Property	63.75	63.75	0.00
Current Value of Home	2666.68	2710.76	44.08
Debt: Home Mortgage	2250.31	2198.62	-51.69
Debt: Auto Loans	633.95	633.95	0.00
Debt: Home Improvement	-47.65	-47.65	0.00
Debt: Business Loans from Banks	-79.94	-79.94	0.00
Debt: Business Loans from Family	-412.29	-412.29	0.00
Debt: Credit cards	278.65	269.23	-9.42
Debt: Installment Plans	73.55	73.55	0.00
Debt: Educational Loans	704.53	488.93	-215.60
Debt: Collection Agencies	92.29	92.29	0.00
Debt: Loans for Other Properties	186.75	186.75	0.00

Exhibit C.13: Effect of Post-Interview Verification on Treatment-Control Differences—Wave Three, Analysis Sample, by Variable *(Continued)*

Variable	Mean treatment-control difference (n=840): Before verification	After verification	Change due to verification
Debt: Personal Loans from Banks	10.22	10.22	0.00
Debt: Personal Loans from Friends	-691.74	-691.74	0.00
Debt: Medical	-577.73	-569.17	8.56
Debt: Past Due Rent	8.34	8.34	0.00
Debt: Overdue Phone Bills	-8.18	-8.18	0.00
Debt: Overdue Utility Bills	2.65	2.65	0.00
Debt: Book Clubs etc	0.54	0.54	0.00
Debt: Other	50.78	50.78	0.00
Income: Wages	-152.84	-152.84	0.00
Income: Self-Employment	88.46	88.46	0.00
Income: TANF	0.61	0.61	0.00
Income: SSI	7.93	7.93	0.00
Income: Social Security disability	2.00	2.00	0.00
Income: Social Security retirement	-37.27	-37.27	0.00
Income: UI	3.05	3.05	0.00
Income: Veterans benefits	2.88	2.88	0.00
Income: Pension	-2.83	-2.83	0.00
Income: Child Support	16.31	16.31	0.00
Income: Alimony	1.84	1.84	0.00
Income: Selling things	-3.86	-3.86	0.00
Income: Odd Jobs	-3.09	-3.09	0.00
Income: Taking People Places	0.47	0.47	0.00
Income: Investments	-63.54	-63.54	0.00
Income: Family	13.18	13.18	0.00
Income: Food Stamps	7.48	6.79	-0.69
Income: Other Sources	111.32	111.32	0.00
Income: Parents	-0.47	-0.47	0.00
Income: Partner	10.61	10.61	0.00
Income: Spouse	-1.53	-1.53	0.00

Appendix D

Minimum Detectable Effects and Precision of Impact Estimates

Exhibit D.1: Treatment Effects—Point Estimates, Confidence Intervals, and Minimum Detectable Effects

Outcome	Control mean	Estimated treatment effect				MDE[b]	As % of control mean	
		Point estimate[a]	Standard error of estimate	95 percent confidence interval				
				Lower bound	Upper bound		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

Exhibit D.1: Treatment Effects—Point Estimates, Confidence Intervals, and Minimum Detectable Effects *(Continued)*

Outcome	Control mean	Estimated treatment effect				Minimum detectable effect[b]	As % of control mean	
		Point estimate[a]	Standard error of estimate	95 percent confidence interval			Upper bound of conf. int.	Minimum detectable effect
				Lower bound	Upper bound			
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).