

Long-term Follow-up of Individual Development Accounts: *Evidence from the ADD Experiment*

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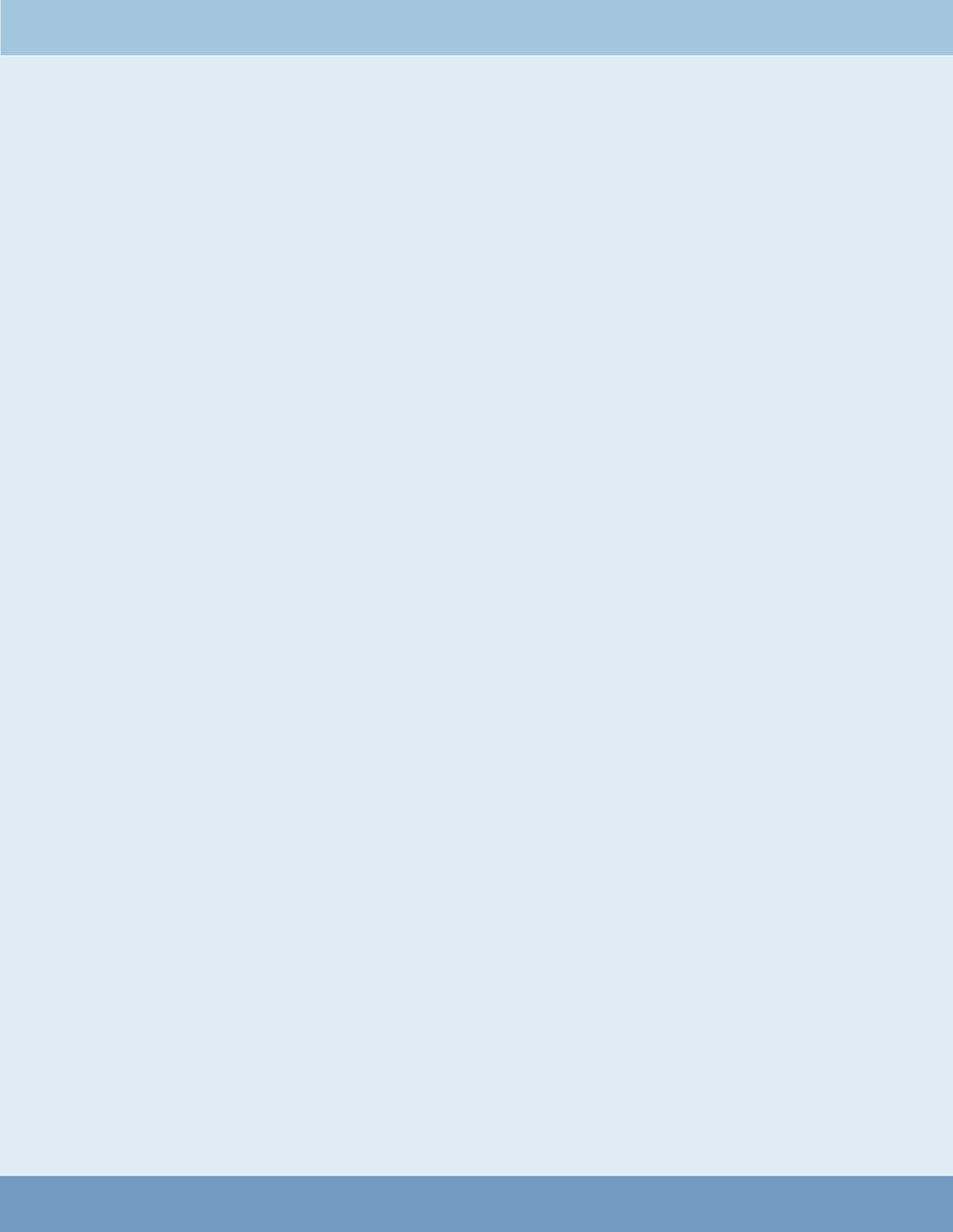
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Preface and Acknowledgements

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Previous research from the American Dream Demonstration has provided invaluable evidence for program and policy development. We hope that this research will continue to guide policy makers in their efforts to help low- and moderate-income families build assets and improve their lives. Questions on the long-term impacts of IDAs are complex and future evaluation and research will continue to build a foundation of knowledge for evidence-based policy making.



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Executive Summary

This report presents findings from the fourth wave of the American Dream Demonstration (ADD) experimental study of Individual Development Accounts (IDAs). The ADD was a set of 14 privately funded local IDA programs initiated in the late 1990s. It was the first large-scale test of IDAs in the United States and used a variety of research methods in order to learn about IDAs. One of these programs, in Tulsa, Oklahoma, was implemented as a random assignment experiment.

The ADD experiment, which ran from 1998 to 2003, was the first experimental study of IDAs. In total, 1,103 low-income participants were surveyed at baseline and randomly assigned to either the treatment or control group. Treatment group members received access to an IDA as well as financial education and case management. The IDA provided matched withdrawals at a 2:1 rate for home purchase and a 1:1 rate for home repair, small business investment, post-secondary education, or retirement savings. Participants who made the maximum matchable deposits throughout the 3 years of the program could accumulate \$6,750 (plus interest) for a home purchase or \$4,500 (plus interest) for the other qualified uses.

Participants were surveyed at baseline (1998 and 1999), again about 18 months later (2000 and 2001), and then again in a follow-up survey in 2003, about 48 months after random assignment. Many interesting findings emerged from these three waves of data collection, primarily that the program had a positive, statistically significant impact on homeownership rates at Wave 3 (Grinstein-Weiss et al., 2008; Mills, Gale, Patterson, Engelhardt, Eriksen, & Apostolov, 2008). In addition, evidence gathered from extended personal interviews with 84 experiment participants (59 treatment, 25 control) suggests positive psychological, cognitive, behavioral, and economic effects (Sherraden, McBride, Hanson, & Johnson, 2005; Sherraden & McBride, 2010).

While this research provided rigorous evidence of the short-term impact of IDAs, there was no evidence on the long-term effects of IDA participation. Effects of asset building on individuals may not be immediate. To the

extent that asset building produces changes in behavior or attitudes, the effects may take time to manifest. Measuring long-term performance is important in understanding the true impacts of participation in an IDA program.

The purpose of the fourth wave of data collection (ADD4) was to assess the impact of short-term IDAs 10 years after random assignment (6 years after the program ended). To accomplish this, an additional survey was conducted among the individuals who participated in the Tulsa, Oklahoma, randomized IDA experiment. Combining the new survey data with earlier surveys of the same individuals made possible rigorous statistical analysis of the effects of IDAs on program participants 6 years after the program ended.

Data collection ran from August 2008 to March 2009 and reached 80.1 percent of the baseline sample (855 individuals, excluding deceased participants and those who had emigrated from the United States). The ADD4 study was designed to address two primary questions:

1. *What are the long-term effects of access to an IDA program on targeted asset building and overall wealth among low-income families?*
2. *What are the long-term psychological and health effects of access to an IDA program?*

Analyses of the data use bivariate and difference-in-differences estimates and also employ regression analyses, controlling for selected baseline characteristics. For most continuous outcomes, ordinary least squares (OLS) regression is used. Logistic or probit regressions were used for dichotomous outcomes, and Poisson regressions were used for count outcomes. In all analyses, an alpha of 0.100 was accepted as the threshold for identifying significant differences and effects. Unless otherwise indicated, 2-tail tests of significance are used throughout the report.

In this report, we detail basic investigation into the effect of treatment assignment on key outcomes. Each outcome will be explored and reported in more detail in future

publications. Below we briefly highlight the central results of the ADD4 study on the five allowable uses and net worth.

Homeownership

Both the treatment and control groups experienced large increases in homeownership between Wave 1 and Wave 4 and there is no observed significant effect of treatment on the level of homeownership among the full sample. However, among participants with an above-median income at baseline (about \$15,480 per year), treatment significantly increased both homeownership rates and duration of homeownership.

Home maintenance and repair

For the full sample, there was no impact of the treatment on home repairs, the dollar amount spent on repairs, or housing price appreciation. Treatment group members did report however, that the estimated cost of unmade repairs was significantly lower compared with the control group. Among baseline homeowners, treatment group members experienced a significantly higher rate of housing price appreciation and were less likely to report foregoing needed repairs.

Education

There is a significant positive impact on education enrollment among the treatment group at Wave 4. No significant impacts are found on increase in level of education or degree completion among the full sample. Among those who reported high school education or less at baseline, there is a significant positive effect on the likelihood of gaining “some college” among treatment group members compared to the control group. Further, we find that men experience a larger effect of treatment on education outcomes than women. Specifically, we find that men may benefit more from the IDA program in terms of educational enrollment and attainment compared with women.

Business ownership

No significant effect of treatment is found on business ownership or equity.

Retirement

There were very high rates of increased retirement saving among both the treatment and control group members; however, no significant effect of treatment is found on retirement savings.

Net worth

No significant effect of treatment is found on overall net worth. There is a marginally significant but economically small effect found on liquid assets: assignment to treatment is associated with \$79 more in liquid assets relative to assignment to control.

Summary

In examining the five allowable uses, the study finds some impacts of IDAs on education, especially for males, and on home maintenance and repair 10-years after the program. However, we find no impact on homeownership, businesses, and retirement savings in the follow-up study. The positive findings for education and home maintenance and repair may suggest that IDAs are best suited to support asset purchases that can be accomplished incrementally over a period of time. Targeting IDAs for education and home maintenance and repair may be more effective than applying them to “all-or-nothing” purchases like a house.

These findings may suggest two implications for the field of asset-building for low and moderate-income households. First, the findings imply that program benefits may have to be greater or that programs may need to have longer savings periods in order to result in lasting impacts on wealth and asset accumulation. Second, long-term impacts of a three-year program may be a lot to expect. The findings raise a broader question of whether a short-term program that provides modest benefits to program participants can outweigh the many other factors that influence ones’ social and economic outcomes. Finally, the results highlight the importance of using experimental design in generating evidence-based policy.



Chapter 1

Background on Asset Building and IDAs

Saving and Asset Building: An Overview

What is the best way to help low-income people improve their long-term economic prospects? Public policies in the United States have historically focused on a combination of income maintenance, consumption support, and work incentives to help families meet daily needs. While these policies help families manage in the short-term, they may not increase long-term financial stability. In recent years, an additional approach has aimed to complement traditional policies by helping low-income households save and accumulate wealth with the goal of increasing their longer-term economic prospects.

These programs, such as matched savings and tax-time savings programs, provide subsidies to encourage low-income individuals to save for the purchase of specific assets, such as a home, or for general asset-building needs, such as an emergency fund or clearing debt, and have become a policy option implemented by governments in countries around the world.

These programs provide one policy tool to help address growing wealth inequality in the United States. A large number of studies have highlighted that wealth in the United States is unequally distributed and highly concentrated (Keister & Moller, 2000; Kopczuk & Saez, 2004; Wolff, 2010). Furthermore, wealth inequality has increased over the past few decades. In 2009, the net worth of the wealthiest 1% of American households was 225 times larger than that of the median American household, the highest ratio on record (Allegretto, 2011). Racial disparities in assets and wealth are also extreme: the latest data indicate a striking inequality of median net worth of whites compared to African Americans (20:1) and Hispanics (18:1) (Oliver & Shapiro, 2006; Kochhar, Fry, & Taylor, 2011).

Beyond the goal of encouraging wealth accumulation and addressing growing inequities, several research findings may drive policy support for saving by low-income people.

1. The United States already has many public policies that encourage asset accumulation via saving
2. incentives, mortgage interest tax deductions, and other means. However, lower-income households often have little or no access to such savings structures, and these benefits primarily accrue to people in the top half of the income distribution (Sherraden, 1991; Howard, 1993; Seidman, 2001). A report from the Annie E. Casey Foundation and Corporation for Economic Development (CFED) on the federal asset-building budget finds that the bottom 60% of taxpayers receive only 4% of federal asset-building tax expenditures (Woo, Rademacher, & Meirer, 2010).
3. Compared to income-transfer approaches to poverty reduction, asset-development approaches may have greater potential to foster financial stability (McKernan & Sherraden, 2008; Moser & Dani, 2008).
4. While the acquisition of major non-financial assets (e.g., a house) can transform a household's standard of living, the up-front financial cost may be out of reach for low-income people (Shapiro, 2004).
5. The process of accumulating assets may, in itself, alter people's outlooks and choices. The asset-effect, as it is sometimes called, is hypothesized to make a person more future-oriented, to increase the sense of personal efficacy, and to enhance some positive behaviors and attitudes (Sherraden, 1991).
6. People need savings to weather temporary setbacks such as a spell of unemployment or an unexpected expense.

As a response to the current asset-building policy structure that favors higher-income households, Individual Development Accounts (IDAs) were proposed as a way to include everyone in asset building (Sherraden, 1991). IDAs were proposed as universal, progressive savings plans, beginning as early as birth, with the aim of making asset-building policy life-long and fully inclusive of the population (Sherraden, 1991). Instead, bowing to practical realities and the challenge of creating a full-scale and inclusive policy, IDAs were implemented throughout the United States during a demonstration period as

short-term subsidized savings programs targeted to lower-income adults.

There has been limited short-term analysis of the effectiveness and efficiency of IDA programs, and no long-term analysis. Given the growing interest in asset-building strategies, policy-makers need to know if these programs have an impact over the long run and whether they are cost-effective. This report presents findings from the fourth wave of the American Dream Demonstration (ADD) experimental study of IDAs, which was designed to help answer these questions.

The purpose of the fourth wave of data collection (ADD4) was to assess the 10-year impact of a short-term (3-year) IDA program. To accomplish this, we conducted a fourth survey of individuals who participated in the Tulsa, Oklahoma, randomized IDA experiment that ran from 1998 to 2002 as a part of the ADD. Combining the new survey data with earlier surveys of the same individuals enabled us to conduct rigorous statistical analysis of the effects of IDA eligibility on the subsidized assets and on wealth, earnings, health, and psychological outcomes of IDA participants ten years after the program began. In addition, the ADD4 study includes a cost-benefit analysis of the Tulsa IDA program, which is presented in a separate report.

The Need for Asset-Building Policies

The overall perspective guiding this work is that poverty and well-being, while typically measured as income levels, are not determined solely by income. Accumulated savings and other assets also matter (Oliver and Shapiro, 2006; Shapiro, 2001; Sherraden, 1991).

Millions of households in the United States have accumulated little or no savings and have few assets. Many more families are “asset poor” than “income poor.” While the official (income-based) poverty rate in 2006 was 12.3%, the asset poverty rate was almost 26%¹ (U.S. Census Bureau, 2011; CFED, 2009). Families with children are even more likely to be asset-poor: 31% of families

with children live in asset poverty. When only liquid assets are considered, this number rises to 52% (Aratani & Chau, 2010). In other words, over half of U.S. families could not support themselves at the poverty-level for three months if they lost their income.

Examination of economic disparities in the United States indicates that different social groups experience different extents and magnitudes of income and asset inequalities. As noted in the prologue, the U.S. faces growing asset inequality by income and by race. Among households with children, minority households and female-headed households are more likely to live in asset poverty (Wiedrich, Crawford, & Tivol, 2010).

These patterns have not arisen randomly, nor do they result solely from individuals “making choices” in the market. Historically, asset inequality has been influenced by officially—or quasi-officially—sanctioned institutions including land confiscation, slavery, Jim Crow laws, residential discrimination, targeting of FHA mortgages to white homeowners, targeting of USDA programs in the South to white farmers, unequal educational opportunity, red-lining, and predatory lending. These and other institutional arrangements have generated wealth inequalities over a long period of time (Oliver and Shapiro, 2006).

Today, the non-poor benefit from institutional structures that encourage asset building, including auto-enrollment in savings programs, default savings choices and targets, automatic deposits, and, sometimes, large public subsidies (Beverly & Sherraden, 1999; Beverly et al., 2008; Choi, Laibson, & Madrian, 2004; Madrian & Shea, 2001; Sherraden & Barr, 2005). In this regard, the United States has created policies that build assets of the non-poor (e.g., 401(k) plans) that include both paternalistic structures and large public subsidies through tax benefits. The poor have little or no access to such savings structures and subsidies. Thus, current public policy exacerbates asset inequality (Dynarski, 2004; Howard, 1997; Seidman, 2001; Sherraden, 1991; Woo et al., 2010).

1. Asset poverty here is defined as net worth below three months of poverty-level income.

As an asset-building policy targeted to lower-income households, IDAs have provided subsidized saving opportunities to low-income families. It is important to note that, for most IDA participants in the ADD, saving is not automatic as in many 401(k) accounts—most IDA participants must take action to save each month. Low-income families do save in IDA accounts, though not surprisingly saving remains very difficult (Schreiner & Sherraden, 2007; Sherraden & McBride, 2010).

Growth of IDAs

IDAs have proven to be popular and have garnered bi-partisan support in the United States. Over the last decade, over 1,000 IDA programs with more than 85,000 account holders have been created (CFED, 2011). Community-based IDA initiatives have received support from foundations, financial institutions, other corporate sponsors, private donors, and from local, state, and federal government.

Federal funding was allocated to support IDA programs with the enactment of the Assets for Independence Act (AFIA) in 1998. The Assets for Independence Program (AFI) is now the largest funding source of IDAs in the United States, with AFI-sponsored IDA programs in 49 states and the District of Columbia. From 1998 to 2009, the program provided \$180 million in competitive grant funds to community-based organizations to support nearly 600 IDA projects. AFI programs have provided more than 72,000 low-income participants with access to IDAs, resulting in more than 29,000 asset purchases, such as houses, post-secondary education, and micro-enterprise.

Proposals to expand IDAs were a staple of the federal budget during both the Clinton and the George W. Bush administrations. More recently, the Obama administration has promoted savings in general through proposals such as the Saver's Bonus, which would provide a tax credit to match low-income individuals' savings. Thus, promoting asset-building for low-income households continues to generate interest at the federal level.

Asset building has also received increasing attention in other countries. Versions of IDA projects are being implemented in Australia, Canada, Hong Kong, Korea, Mozambique, Peru, Taiwan, and Uganda.

There has also been interest—in the United States and other countries—in Children's Development Accounts. These accounts aim to encourage the lifelong habit of saving by promoting saving during childhood (Cramer, O'Brien, & Boshara, 2007).

Research on IDAs

Several studies on the efficacy and impact of IDAs conducted over the past two decades have provided insights into the savings and asset-building behaviors of low-income households. The Canadian *learn\$ave* study and the American Dream Demonstration's experimental study are the only randomized controlled trials of IDAs to date.

The Canadian *learn\$ave* study is the largest experimental demonstration of matched savings accounts. This experiment tested the use of IDAs to support adult education and micro-enterprise development among nearly 5,000 individuals in ten locations across Canada (about 3,500 in the experimental component). The *learn\$ave* experiment also tested the impact of additional services including financial education and intensive case management.

The longitudinal research, conducted from 2001 to 2008, includes four waves of data collection with post-participation follow-up. Compared with the control group, treatment members demonstrated increased enrollment in training and education programs. There was no significant effect on net worth or total savings but *learn\$ave* did appear to affect the overall composition of financial assets and have a positive impact on financial

goal setting, ongoing saving activities, and budgeting. Treatment group members had higher average bank account balances and lower retirement savings than control group members. Results suggest that the additional provision of financial education and case management resulted in a higher likelihood of saving, of qualifying for matched credits, and of saving the maximum matchable amount. Though these additional services had little impact on withdrawal of matching funds, they did significantly increase educational outcomes (Leckie, Hui, Tattrie, Robson, & Voyer, 2010). Research from non-experimental studies of IDAs has yielded additional findings. These studies include studies using quasi-experimental designs as well as studies with no comparison group that draw data from participant surveys and/or account monitoring. While these findings do not come from randomized controlled trials and may differ from experimental findings, they can still provide some insight into savings contributions and participant experiences in IDAs.

Non-experimental research has identified several factors associated with a greater likelihood of contributing to an IDA. Analysis of account monitoring data from the 14 ADD projects shows that use of direct deposit, higher match rates, and higher match caps are associated with increased likelihood of contributing to an IDA. Of these, only higher match caps are also associated with higher monthly deposits. Participants with higher levels of education and working students were more likely than other participants to make account contributions. Financial education is correlated with increased contributions in several studies. Research from the ADD finds that every hour of financial education, up to 8 hours total is helpful; additional hours beyond this point may have a negative effect on saving. Conversely, debt may be a barrier to saving: participants with debt are less likely to make account contributions and make lower average monthly deposits in the IDA (Schreiner, Clancy, & Sherraden, 2002; Schreiner & Sherraden, 2007).

Evidence from non-experimental research also suggests that IDAs encourage the purchase of assets among

participants. Using a comparison group drawn from the 2001 Survey of Income and Program Participation, the first AFI Evaluation estimates that AFI IDA participation increases the rates of homeownership, business ownership, and enrollment in postsecondary education (Mills, Lam, DeMarco, Rodger, & Kaul, 2008). No significant differences between participants and nonparticipants were found on savings, home equity, or consumer debt. There is also evidence that IDAs may improve mortgage loan terms and protect low-income households from foreclosure. A report by CFED and The Urban Institute compared IDA homebuyers with other low-income homebuyers purchasing homes in the same communities between 1999 and 2007. The study finds that IDA home purchasers were much less likely to have high-interest mortgage terms and two to three times less likely to experience foreclosure (Rademacher, Wiedrich, McKernan, Ratcliffe, & Gallagher, 2010).

The American Dream Demonstration and the Tulsa Experiment

The American Dream Demonstration (ADD) is a set of 14 privately funded local IDA programs initiated in the late 1990s. The ADD is the first large-scale test of IDAs in the United States and used a variety of research methods to learn about IDAs (Schreiner et al., 2002).

The IDA program in Tulsa, Oklahoma, was administered by Community Action Project of Tulsa County (CAPTC) and was the only ADD program that was implemented as a random assignment experiment. CFED proposed and organized the ADD intervention. The ADD research program was conceived and initiated by the Center for Social Development (CSD) at Washington University in St. Louis. For the ADD experiment, the CSD organized the selection of the site and the survey firm, and drafted the initial survey instrument. Abt Associates was selected to conduct random assignment, data collection and initial analysis.

The ADD Experiment

Selection into the Program

Recruitment of participants for the experiment took place over a 15-month period from October 1998 to December 1999. CAPTC reached out to clients who received other services, such as tax preparation assistance and homeownership preparation classes, to participate in the ADD IDA program. The program was also advertised in local media, and flyers were mailed to former clients and distributed at other local social service agencies. Those who indicated an interest in the program were encouraged to fill out an application, documenting their eligibility.

To be eligible for the program, participants had to be employed (confirmed with pay stubs) and have a prior year's income below 150% of the federal poverty line (verified using the 1997 or 1998 income tax return adjusted gross income; about \$25,000 for a family of

four). Applicants who appeared to meet the criteria were invited for an interview that confirmed the content of their application, explained the program and the random assignment process, and obtained informed consent.

Participants in the ADD experiment were informed of the nature and goals of the IDA program and notified that, regardless of whether they were assigned to the treatment or control group, they would not be able to use other matched savings programs at CAPTC, nor could they receive any financial assistance for homeownership from CAPTC for the four years of the study period.

As a result, during the experimental period through 2003, treatment group members had access to the CAPTC IDA, while both control and treatment group members could access a set of other subsidy options at CAPTC that were *less* attractive than those available to the typical low-income household. All sample members could use CAPTC services for tax preparation, employment, education, child

The Community Action Project of Tulsa County (CAPTC)

CAPTC is a multi-service community action agency that serves the low-income population of the Tulsa metropolitan area. The organization was founded in 1973 and in 1998 described itself as follows:

"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 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."

Source: Community Action Project of Tulsa County, "The IDA Program of CAPTC – Informational Packet," 1998. As cited in Abt Associates Inc. "Evaluation of the American Dream Demonstration, Final Evaluation Report," 2004.

care, and so on during the experiment period. Control group members could receive homeownership counseling from CAPTC and, if they requested it, they were provided with general financial information and referrals to other agencies in the Tulsa area that provided similar services. At these other agencies, control group members were free to seek any service for which they qualified, including financial assistance for homeownership. After 2003, all participants reverted to being eligible for all CAPTC programs.

Treatment group members had access to financial education, case management, and IDA matched savings accounts held at the Bank of Oklahoma.

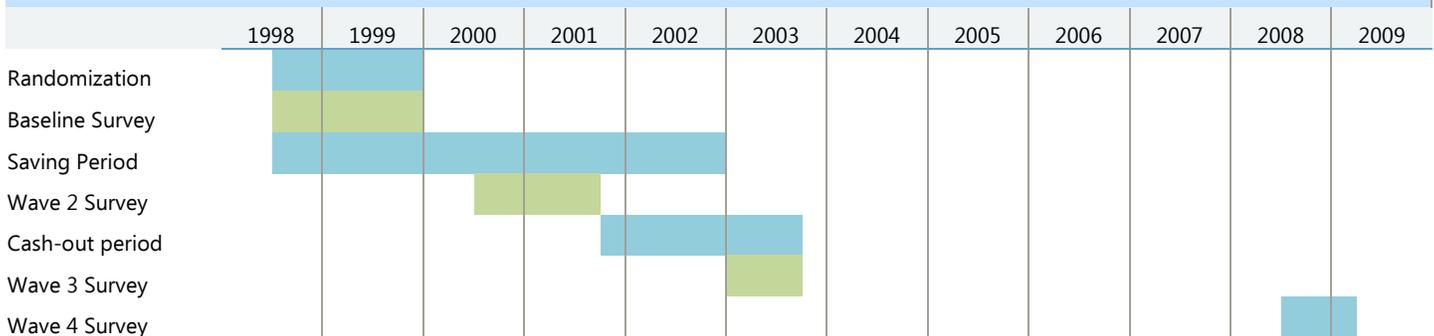
- The account earned an interest rate of 2 to 3%.²
- Participants could receive matches for up to \$750 in deposits each year, with deposits above \$750 in a given year eligible to be matched in subsequent years.³
- Participants could make matchable deposits for 36 months after opening the account.
- Unmatched withdrawals could be made at any time.
- Matched withdrawals could only be made six or more

months after account opening.

- Withdrawals were matched at 2:1 rate for home purchase and 1:1 for home repair, small business investment, post-secondary education, and retirement savings.
- A participant who made the maximum matchable deposit in all three years could accumulate \$6,750 for a home purchase or \$4,500 for other qualified uses.
- At the end of the program, participants could request to put any remaining IDA balance into a Roth IRA with a 1:1 match.

The financial education component included both general money-management training and asset-specific training.⁴ The general financial education consisted of six 2-hour courses on topics such as saving strategies, budgeting, credit repair, and financial planning. The asset-specific classes provided information on a particular asset investment. For example, participants who were saving for a home attended classes that addressed how to shop in the real estate market and how to work with real estate agents and loan officers. Program staff provided program participants with assistance and consultation by phone or in-person, and they sent out monthly postcards urging participants to make deposits in their accounts.

Figure 1.1
ADD Experiment Timeline



2. There were no fees to open or withdraw from the account unless the respondent made more than three withdrawals in one year, which induced a \$3 fee. They could also use direct deposit to transfer money automatically into the IDA.

3. However, individuals who contributed less than \$750 in a year were not allowed the following year to make “catch-up” deposits retrospectively.

4. Participants were required to attend a minimum of four hours of financial education before they were allowed to open the account and to accrue 12 hours of general financial education, as well as some asset-specific training, before making a matched withdrawal.

A total of 1,147 applicants were found to meet the eligibility requirements and were referred for baseline interview and random assignment.

The Baseline Sample

Baseline interviews were conducted by telephone using a computer assisted telephone interviewing (CATI) system. Because the baseline interviews preceded random assignment, the characteristics measured there can be assumed to be strictly exogenous to subsequent

Table 1.1
Baseline Sample (N=1,103)

	Mean/ percentage	Standard deviation
Total household income (monthly)	1,422	744
Age	35.8	10.3
Female	78.4	
Bank account ownership	83.7	
Married	27.8	
Homeowner	21.7	
Children in home	77.2	
Welfare recipient	27.3	
Race		
Caucasian	44.3	
African American	42.5	
Other	13.2	
Education		
HS degree or less	33.8	
Some college	41.1	
College graduate	25.1	
Assets		
Less than 1 month of assets	23.6	
1-2 months of assets	10.8	
2-3 months of assets	8.7	
3+ months of assets	43.2	
Missing on assets	13.7	
Debts		
Less than 1 month of debts	21.2	
1-2 months of debts	7.2	
2-3 months of debts	5.4	
3+ months of debts	47.6	
Missing on debts	18.6	

treatment. A total of 1,103 baseline interviews were completed, usually two weeks after the application interview. A plurality of applicants was recruited near the end of the recruitment window. Respondents interviewed during the last three months of the recruitment period (October-December 1999) comprise 30% of the total sample.

At baseline, the respondents were predominantly female (78%), had children (77%), and were not married (28% married). Forty-four percent of baseline respondents were white and 43% were African-American. A plurality had attended some college (41%) and 84% had either a checking or a savings account. The average age of respondents was 35 years. At baseline, 22% of respondents owned their residence. The mean monthly household income was \$1,422 (median \$1,320) while about a quarter of respondents held assets worth less than one month of the sample average income and nearly half had liabilities exceeding three months sample average income.

Random Assignment

Within one week of their baseline interview, Abt Associates randomly assigned those with completed interviews to the treatment or control group and CAPTC notified respondents of their assignment. At the outset of the sampling, the assignment ratio was five treatments to six control group members because of concerns about differential attrition. About half-way through the recruitment period, the assignment ratio was adjusted to one treatment to one control. In total, 537 were assigned to the treatment group and 566 were assigned to the control group.

Waves 2 and 3

The Wave 2 survey was conducted between May 2000 and August 2001, about 18 months after baseline interview and random assignment. An interview with respondents was first attempted by telephone. If telephone attempts were unsuccessful, a field interviewer attempted to arrange an in-person interview at the respondent's residence. The response rate for Wave 2 was 84.6%. The Wave 3 survey followed the same process

Table 1.2
Program Use Among Treatment Group Respondents
(N=537)

	Mean/ proportion
Account use	
Average monthly net deposit (\$)	18
Average gross deposits (\$)	1,549
Deposit frequency	0.50
Unmatched withdrawals	
Took any unmatched withdrawal	0.70
Value of unmatched withdrawals (\$)	552
Matched withdrawals	
Received any match	0.39
Value of matched withdrawals (\$)	574
Value of match funds received (\$)	721
Proportion who made matched withdrawals for	
Home purchase	0.13
Home repair	0.14
Education	0.07
Retirement	0.13
Business	0.03
Took match for intended savings goal*	0.27
Took match for reason other than original reason for saving*	0.26
Matched withdrawals for multiple purposes	0.10

* n=472 who opened IDA accounts

between January and September 2003, about 48 months after random assignment, with a 76.2% response rate. The average interval between the baseline and Wave 3 interviews was 1,449 days for treatment cases and 1,456 days for controls; the difference is not statistically significant. Interviews were conducted using computer-assisted telephone and personal interviewing methods.

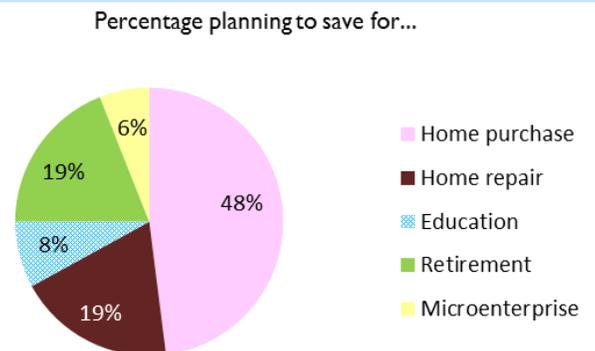
Program Use

Table 1.2 describes program use among those assigned to the treatment group (n=537). Of those assigned to treatment, 87.9% opened an IDA account (n=472). In keeping with the intent-to-treat approach used in this report, reported account-use figures include all those assigned to treatment, independent of whether they

opened an IDA account, unless otherwise noted. Overall, at account opening, the most popular savings goal among account openers, as shown in Figure 1.2, was home purchase (48% of treatment group members), followed by home repair (19%) and retirement savings (19%). Less than 10% intended to save for post-secondary education or for microenterprise. On average, those in the treatment group made deposits in about half of the months they had access to their account. Monthly net deposits averaged \$18 per month, with an average of \$1,549 in gross deposits during the program period.

About 70% of participants took an unmatched withdrawal at some point. About 40% made a matched withdrawal, receiving on average \$721 in matching funds. Consistent

Figure 1.2
Reason for Saving Among Treatment Group Respondents



with reasons for saving, matched withdrawals were made in roughly equal proportions for home purchase (13%), home repair (14%), and retirement (13%). Fewer participants made withdrawals for education (7%) and small business (3%). Twenty-seven percent of the sample made withdrawals for their originally stated savings purpose, while 10% made matched withdrawals for multiple purposes, and a total of 26% made withdrawals for purposes other than their originally stated intention.

Table 1.3***Program Use by Reasons for Saving Among Treatment Group Members Present at Wave 4 with Opened Accounts (N=368)***

Reason for saving	Share of treatment group	Average gross deposits (\$)	Average monthly net deposit (\$)	Deposit frequency	Probability of any matched withdrawal
Home purchase	0.46	1,402	9	0.46	0.18
Home repair	0.21	2,278	39	0.73	0.79
Education	0.08	2,330	29	0.66	0.71
Retirement Saving	0.2	2,384	32	0.69	0.71
Small business	0.06	1,526	21	0.49	0.48
Total sample	1	1,855	22	0.58	0.47

Table 1.3 describes program use by baseline-reported reason for saving for those assigned to treatment who opened accounts and responded to the 10-year follow-up survey. More than 70% of participants saving for home repair, education, or retirement took a matched withdrawal. Only about half of those saving for small business made a matched withdrawal, while savers for home purchase had the lowest likelihood of making a matched withdrawal (18%). Based on all metrics, participants saving for home repair saved on average \$39 per month and \$2,278 total. Deposit frequency and probability of withdrawal were also highest among those saving for home repair.

Although home purchase was the most popular reason for saving in the program, these savers had the lowest program-use outcomes, depositing only \$9.30 per month and accumulating at the mean \$1,400 over the course of the program. With the 2:1 match rate, this would result in \$4,200 to put toward a down payment on a home.

Findings from Waves 1—3

The effects of the experiment on homeownership and wealth through 2003 were presented in three recent articles (Grinstein-Weiss, et al., 2008; Mills, Gale, Patterson, Engelhardt, Eriksen, & Apostolov, 2008; Han, Grinstein-Weiss, & Sherraden, 2009). The program had a positive, statistically significant impact on homeownership rates after five years. Among households who rented at baseline, homeownership rates between 1998

and 2003 rose by 7 to 11 percentage points for treatment group members relative to control group members. The program's impacts on net worth and on qualified asset building uses were not consistent.

Using ADD Waves 1–3 data, Engelhardt, Eriksen, Gale, & Mills (2010) estimate impacts of homeownership for low-income households on a wide variety of social outcomes, including political involvement, neighborhood involvement, and assistance given to others. They find zero or negative effects on measures of political involvement. Results for other social outcomes were not statistically significant.

Evidence gathered from extended personal interviews with 84 experiment participants (59 treatment, 25 control) suggest positive psychological, cognitive, behavioral, and economic effects. In addition, some IDA participants with children reported feeling reassured that their savings would benefit their children by paying for their children's education, improving their living environment, or generally providing for their children's future (Sherraden, McBride, Hanson, & Johnson, 2005; Sherraden & McBride, 2010).



Chapter 2

American Dream Demonstration Wave 4

The Need for Long-Term Analysis

The research discussed in the previous section focuses on outcomes over the first 4 years of the ADD program and can be described as short-term impacts. Participants had up to 3 years to save in their IDAs, and then they had another 6 months to use their funds for matched purposes. The ADD program ended at Wave 3. However, post-participation analysis is important for understanding the longer-term impacts after the IDA program ended.

There is considerable uncertainty surrounding the long-term effects because, prior to ADD4, there was no experimental study on the long-term impacts of IDAs and, indeed, very little long-term experimental evidence regarding saving policies in general. Analysis of other (non-saving) policies has shown that long-term effects can be stronger or weaker than short-term effects.⁵

Impacts of asset building on individuals may not be immediate. To the extent that asset building produces changes in behavior or attitudes, the effects may take time to manifest. Indeed, the difference between dynamic impacts that take place over time and static impacts that are measured at one point in time is one of the key differences in underlying philosophy between the asset-building approach and the conventional, welfare-based approach to social policy. For example, saving for a down payment may require more than three years, especially for low-income households. People might initially use the IDA to invest in education, in which case their homeownership rates and financial wealth may not be affected until much later. Starting a business may yield higher or lower returns during the start-up period relative to a longer period of time. As a result, measuring long-term performance is important in understanding the true impacts of participation in an IDA program.

The incentives built into the Tulsa IDA experiment suggest one reason why the long-term effects may be smaller than the short-term effects. Specifically, treatment group members had incentives to purchase homes before the end of 2003 (in order to receive a 2:1 match) while control group members had incentives to delay home

purchases until 2004 (when they would become eligible once again for a variety of CAPTC home-buyer assistance programs). On the other hand, financial education and the impact of the very act of saving and owning wealth (as posited by Sherraden 1991) might spur members of the treatment group to maintain or increase gains after the program ended in 2003.

ADD4 Research Questions and Hypotheses

Propelled by the need for evidence of the long-term impacts of IDAs, we designed and implemented the ADD4 study. Our investigation was guided by two overarching research questions:⁶

1. What are the long-term effects of access to a 3-year IDA program on targeted asset building and overall wealth among low-income families?

Specifically, we address the following questions by comparing changes in a variety of outcome variables between treatment and control members over a ten-year period.

- Does net worth increase?
- Do rates of homeownership rise (among those who did not own homes at baseline)?
- Do rates of business ownership rise and does the value of business equity rise?
- Does educational attainment increase?
- Does the likelihood of having a retirement savings account rise and do retirement saving balances rise?
- Among those who owned homes at baseline, is the likelihood of undertaking home improvement greater?

2. What are the long-term psychological and health effects of access to a 3-year IDA program?

We test whether in the Wave 4 survey, relative to control group members, treatment group members:

- Score higher on a measure of future orientation?
- Express higher life satisfaction?
- Score lower on a measure of depression?
- Report better health outcomes?
- Report less alcohol and tobacco use?

5. See Almond & Currie (2010) for a discussion and review of long-term impacts of early childhood interventions and Chetty et al. (2010) for a recent contribution to that literature.

6. The ADD4 study also includes a cost-benefit analysis of the Tulsa IDA program, presented in a separate report and giving particular attention to capturing both the economic impacts and the social-psychological effects of the program.

Three Waves of Previously Collected Data

As discussed in Chapter 1, the earlier data in the Tulsa IDA experiment were collected in three waves from 1998 to 2003. Because three waves of data had already been collected from this group, it is important to explain the basic design of earlier surveys before discussing the ADD Wave 4 survey.

The surveys covered a wide range of questions relating to individuals' employment, assets, debt, family structure, education, and related issues. Besides basic demographic information, the questions also focused on financial stability and financial knowledge. Several questions inquired about the participants' expectations for and behavior toward their children. In addition, the surveys included a few questions about life satisfaction and future orientation.

In the studies mentioned earlier, looking at the outcomes from Waves 1 to 3, the data from the surveys were supplemented with administrative data from the Management Information System for Individual Development Accounts (MIS IDA). MIS IDA tracked program characteristics, participant characteristics (both sociodemographic and financial), and saving transactions of IDA participants (beyond net savings amount). MIS IDA electronically imported account information from financial institutions, and thus provides highly accurate data on all IDA account transactions of all ADD participants.

This may be the best available dataset on savings patterns among low-income families (Schreiner et al., 2002). CSD designed and created MIS IDA as a research tool for the ADD, collected the MIS IDA data, and merged these data with the survey data. Merging this data with the ADD Wave 4 data is important for various analyses such as examining saving performance during the program and outcomes 6 years after program completion.

The ADD Wave 4 Survey⁷

The ADD fourth wave of data collection started in August 2008, about 10 years after random assignment, and about 6 years after the IDA program ended. The potential sample for Wave 4 was 1,068 respondents. No differential efforts were used to track down treatment versus control group members, nor were any information sets used if they predominantly identified only treatment or control group members. We imposed these constraints to ensure that we did not collect a sample of study participants that was biased with respect to the treatment. Further, interviews were conducted at an even pace for both the treatment and control groups, which was important to avoid bias due to the economic downturn that developed and worsened during the period of data collection.

Data collection lasted about 8 months and ended in March 2009. The interviews were primarily conducted in-person for participants living in greater Tulsa; the 17% of respondents who lived elsewhere were interviewed by telephone.

After much consideration, we changed the primary survey method from chiefly telephone interviews (Waves 1 to 3) to primarily personal interviews at Wave 4. This was done for several reasons. First, research suggests that response rates tend to be higher for personal interviews than they are for telephone interviews. Second, in-person respondents give more attention to interviewers, which typically yields more complete data. The presence of the interviewer allows for response to non-verbal cues and allows the interviewer to address respondents' uncertainty about answers, and generally reduces item non-response. Third, questions related to income, debts, and property ownership, which comprise a significant portion of the Wave 4 survey, are among the most sensitive survey topics. Interviewing respondents face-to-face is likely to make respondents feel more comfortable and forthcoming with financial data (Biemer, Groves, Lyberg, Mathiowetz, & Sudman, 1991). In addition, a field

7. Some text in this section and in Chapter 3 also appear in manuscripts detailing the ADD4 findings on particular outcomes.

interviewer may be better equipped to clarify confusing items or terms related to financial questions. Finally, given the expected length for survey completion (60 minutes), an in-person interviewer is better equipped to deal with respondent fatigue or lagging motivation as the interview proceeds.

Wave 4 questions retained the format and content of questions employed in the earlier surveys. Unlike other waves, however, the Wave 4 survey also asked retrospective questions about homeownership history in addition to current economic, financial, demographic, community, and health status. Respondents were asked to report on their homeownership history starting in 1998: What was their status at that time? When did they buy a house? When did they sell it? When did they buy another house? When did they sell that house, etc.? Using this information, we constructed a homeownership history for each respondent from 1998 to 2009.



Chapter 3

Methodology

Wave 4 Response Rate and Attrition

At Wave 4, 10 years after random assignment and 6 years after the end of the intervention, 855 participants were located and surveyed (80.1% of living members of the baseline sample). The Wave 4 sample included 146 respondents who were not interviewed at Wave 3 and 48 respondents who had not been interviewed since baseline. Response rates for each wave are shown in Table 3.1.

Table 3.1
Sample Size by Treatment Status and Survey Wave

	Wave 1		Wave 2		Wave 3		Wave 4	
	n	%	n	%	n	%	n	%
Control	566	100	472	83.4	428	75.6	448	81.5
Treatment	537	100	461	85.8	412	76.7	407	78.6
Full sample	1103	100	933	84.6	840	76.2	855	80.1

As shown on Table 3.2, when those who responded at Wave 4 are compared on baseline interview characteristics to those who did not, we find that Wave 4 respondents were more likely to be female, white, unmarried, and a homeowner. Wave 4 respondents were also more likely to have been in the top assets category (assets equaling at least 3 months of sample mean income worth) and less likely to have been in the lowest assets category at baseline. Respondents and attriters were statistically the same with respect to bank account ownership, children, welfare receipt, education, debt levels, age, and monthly household income.

Panel Imbalance

The American Dream Demonstration is one of two IDA research projects that used a randomized controlled research design, which makes the study a true experimental test of the impact of IDA program participation, by enabling a comparison between two similar groups whose only difference is their treatment group assignment. Random assignment, however, may

Table 3.2
Baseline Characteristics of Attriters and Wave 4 Respondents

	Attrite	Respond	Diff	p
	Mean/ prptn	Mean/ prptn	T-C	
Total household income (monthly)	1,418	1,422	-4.19	0.94
Female	0.73	0.8	-0.07	0.02
Age	35.56	35.9	-0.34	0.65
Bank account ownership	0.81	0.85	-0.04	0.19
Married	0.32	0.27	0.06	0.08
Homeowner	0.15	0.24	-0.09	0.00
Children in home	0.78	0.77	0.01	0.78
Welfare recipient	0.31	0.26	0.04	0.17
Race				
Caucasian	0.38	0.46	-0.08	0.02
African American	0.47	0.41	0.06	0.11
Education				
HS degree or less	0.38	0.33	0.05	0.16
Some college	0.4	0.41	-0.01	0.78
College graduate	0.22	0.26	-0.04	0.22
Assets				
Less than 1 month	0.29	0.22	0.07	0.03
1-2 months of assets	0.1	0.11	0	0.86
2-3 months of assets	0.09	0.09	0	0.92
3+months of assets	0.38	0.45	-0.06	0.07
Missing on assets	0.14	0.14	0	0.99
Debts				
Less than 1 month	0.24	0.2	0.04	0.19
1-2 months of debts	0.06	0.07	-0.01	0.62
2-3 months of debts	0.04	0.06	-0.02	0.15
3+months of debts	0.47	0.48	-0.01	0.77
Missing on debts	0.19	0.18	0	0.87
N	248	855		

Note: The asset and debt categories refer to the value of respondents' assets and debts relative to the sample mean monthly income.

not result in completely equivalent treatment groups. In addition, due to differential attrition, treatment groups which are equivalent at baseline may become unbalanced in later waves. For these reasons, it is necessary to measure important variables at assignment, and to compare groups on these measures to verify that random assignment fully controlled for differences between the two groups. In this section we assess the extent of panel imbalance in the ADD study sample. We compare groups among respondents present at baseline, Wave 3 and Wave 4 (see Table 3.3).

Based on measures taken at baseline ($n=1,103$ for most measures), randomization resulted in only a few significant or marginally significant differences between treatment and control groups. There is some evidence that the control group was better off financially at baseline. The control group had significantly more assets at baseline than the treatment group ($p<0.05$), and was more likely to have assets equivalent to 3 or more months of income ($p<0.05$). The control group was also significantly more likely to own a home at baseline ($p<0.05$). In contrast, the treatment group was more likely to say that their financial situation had worsened during the previous year ($p<0.05$). On the other hand, the control group had slightly higher scores on a scale measure of financial strain ($p<0.10$).

As the study progressed, the composition of the sample shifted at each data collection wave due to attrition. At Wave 3 ($n=840$ for most measures), the treatment group was still more likely to have had a baseline monthly income above \$3,000 ($p<0.10$). The control group was also still more likely to have had assets at baseline worth more than 3 months of income ($p<0.10$), although the difference between the two groups with regard to absolute value of baseline assets was no longer significant. The treatment group as it appeared at Wave 3 was also more likely to have a checking or savings account at baseline ($p <0.05$). At Wave 3, there was no longer a difference between the two groups with regard to home ownership, although the treatment group was more likely to have owned property at baseline ($p<0.05$). Conflicting differences regarding baseline financial

hardship remain in the study sample at Wave 3: the treatment group was more likely to report a worsening financial situation at baseline ($p<0.05$), while the control group scored higher on a scale measure of financial strain ($p<0.10$).

At Wave 4, the study survey recovered 146 respondents who had been missing at Wave 3. Surprisingly, the study sample composition at Wave 4 ($n=855$) is more balanced with regard to baseline characteristics than it is at either random assignment or Wave 3. However, there is still some evidence that the control group was at a greater financial advantage at baseline; they were more likely to have assets equivalent to 3 or more months of income ($p<0.05$), while the treatment group was more likely to have fewer assets at baseline ($p<0.05$).

Item Nonresponse

Among the 855 Wave 4 respondents, not every respondent was asked every question and not every respondent gave valid answers to every question they were asked. The former, caused by skip patterns in the survey instrument and, most often, the non-applicability of the question to a given respondent (e.g. those who never owned a home are not asked about home repair and improvement), does not compromise inference. When this arises in outcome variables, we analyze those cases with valid, non-missing data.

The second type of missing data, caused by the respondent being unwilling or unable to provide a response, can affect the analysis and interpretation of study results. In survey research, it is often the case that these missing values are related to the true but unreported value of the variable being measured. That is to say, those with item nonresponse may systematically differ from respondents.

Across the baseline and Wave 4 data, item nonresponse rates are most often under 5% of the question-eligible survey sample for each survey item. On sensitive variables such as income, nonresponse rates are between 5% and 10%. For the baseline characteristics used as covariance

Table 3.3
Panel Imbalance on Baseline Characteristics Among Respondents from Different Waves

	Baseline sample				Wave-3 sample				Wave-4 sample			
	Treat	Cont	Diff	p	Treat	Cont	Diff	p	Treat	Cont	Diff	p
	mean/ prptn	mean/ prptn	T-C	2-tail	mean/ prptn	mean/ prptn	T-C	2-tail	mean/ prptn	mean/ prptn	T-C	2-tail
Female	0.78	0.79	-0.01	0.755	0.79	0.81	-0.02	0.479	0.79	0.81	-0.02	0.656
Married	0.28	0.28	0.00	0.950	0.28	0.24	0.04	0.204	0.27	0.26	0.01	0.884
Banked	0.86	0.82	0.04	0.121	0.89	0.83	0.06	0.014	0.86	0.83	0.03	0.139
Age												
Less than 25	0.15	0.12	0.03	0.135	0.13	0.12	0.01	0.522	0.15	0.13	0.02	0.289
25-35	0.35	0.37	-0.02	0.328	0.33	0.36	-0.03	0.418	0.34	0.37	-0.03	0.455
35-45	0.33	0.29	0.04	0.171	0.35	0.30	0.05	0.113	0.32	0.29	0.03	0.481
45-55	0.13	0.16	-0.03	0.093	0.14	0.17	-0.03	0.202	0.14	0.16	-0.02	0.451
55+	0.04	0.05	-0.01	0.709	0.05	0.05	0.00	0.618	0.05	0.05	0.00	0.770
Income												
\$1,000-\$2,000	0.71	0.72	-0.01	0.625	0.73	0.72	0.01	0.782	0.71	0.72	-0.01	0.897
\$2,000-\$3,000	0.18	0.16	0.02	0.635	0.18	0.18	0.00	0.918	0.18	0.17	0.01	0.583
\$3,000+	0.05	0.03	0.02	0.091	0.06	0.03	0.03	0.069	0.05	0.03	0.02	0.120
Income Missing	0.04	0.04	0.00	0.865	0.02	0.04	-0.02	0.204	0.03	0.04	-0.01	0.468
Race												
White	0.43	0.45	-0.02	0.496	0.45	0.49	-0.04	0.227	0.44	0.47	-0.03	0.364
Black	0.43	0.41	0.02	0.455	0.43	0.39	0.04	0.245	0.43	0.39	0.04	0.243
Other	0.14	0.14	0.00	0.928	0.12	0.12	0.00	0.922	0.13	0.14	-0.01	0.717
Assets												
Less than 1 month	0.25	0.22	0.03	0.362	0.21	0.22	-0.01	0.832	0.22	0.22	0.00	0.996
1-2 months	0.12	0.10	0.02	0.430	0.11	0.11	0.00	0.761	0.12	0.10	0.02	0.298
2-3 months	0.09	0.09	0.00	0.875	0.09	0.08	0.01	0.770	0.09	0.08	0.01	0.850
3+ months	0.40	0.46	-0.06	0.036	0.42	0.48	-0.06	0.073	0.41	0.48	-0.07	0.041
Assets Missing	0.15	0.12	0.03	0.190	0.17	0.11	0.06	0.020	0.16	0.12	0.04	0.064
Liabilities												
Less than 1 month	0.21	0.22	-0.01	0.777	0.20	0.20	0.00	0.878	0.20	0.21	-0.01	0.756
1-2 months	0.07	0.08	-0.01	0.565	0.07	0.07	0.00	0.903	0.07	0.07	0.00	0.998
2-3 months	0.05	0.06	-0.01	0.394	0.05	0.06	-0.01	0.359	0.06	0.06	0.00	0.834
3+ months	0.47	0.48	-0.01	0.579	0.49	0.51	-0.02	0.628	0.47	0.48	-0.01	0.816
Liabilities Missing	0.21	0.16	0.05	0.059	0.19	0.16	0.03	0.151	0.19	0.18	0.01	0.623
Unsubsidized Housing	0.74	0.75	-0.01	0.537	0.76	0.76	0.00	0.880	0.75	0.75	0.00	0.959
Health Insurance	0.59	0.57	0.02	0.512	0.59	0.58	0.01	0.688	0.59	0.58	0.01	0.587
Own computer	0.28	0.25	0.03	0.308	0.31	0.27	0.04	0.206	0.29	0.27	0.02	0.425
Own dishwasher	0.22	0.22	0.00	0.958	0.23	0.24	-0.01	0.919	0.23	0.23	0.00	0.965
Own washer	0.53	0.50	0.03	0.304	0.57	0.56	0.01	0.625	0.55	0.52	0.03	0.411
Own dryer	0.52	0.51	0.01	0.727	0.56	0.56	0.00	0.886	0.53	0.52	0.01	0.857
Own refrigerator	0.48	0.51	-0.03	0.427	0.52	0.56	-0.04	0.170	0.50	0.54	-0.04	0.368
Own freezer	0.15	0.15	0.00	0.842	0.17	0.16	0.01	0.600	0.17	0.15	0.02	0.427
Own air conditioning	0.18	0.19	-0.01	0.409	0.20	0.21	-0.01	0.748	0.19	0.20	-0.01	0.789
Own sewing machine	0.22	0.25	-0.03	0.151	0.26	0.29	-0.03	0.259	0.23	0.27	-0.04	0.192
Own car	0.81	0.83	-0.02	0.300	0.84	0.84	0.00	0.965	0.84	0.85	-0.01	0.755
Own home	0.19	0.24	-0.05	0.037	0.23	0.24	-0.01	0.568	0.21	0.26	-0.05	0.106
Own property	0.04	0.03	0.01	0.416	0.05	0.02	0.03	0.043	0.05	0.03	0.02	0.248

Table 3.3 (continued)
Panel Imbalance on Baseline Characteristics Among Respondents from Different Waves

	Baseline sample				Wave-3 sample				Wave-4 sample			
	Treat	Cont	Diff	p	Treat	Cont	Diff	p	Treat	Cont	Diff	p
	mean/ prptn	mean/ prptn	T-C	2-tail	mean/ prpt	mean/ prptn	T-C	2-tail	mean/ prptn	mean/ prptn	T-C	2-tail
IRA account	0.09	0.07	0.02	0.207	0.10	0.08	0.02	0.207	0.09	0.08	0.01	0.358
Satisfied with general health (y/n)	0.85	0.87	-0.02	0.431	0.87	0.86	0.01	0.562	0.86	0.86	0.00	0.973
Satisfied with financial situation (y/n)	0.63	0.60	0.03	0.274	0.63	0.58	0.05	0.164	0.63	0.60	0.03	0.315
Financial situation worse (y/n)	0.19	0.13	0.06	0.012	0.18	0.13	0.05	0.020	0.19	0.13	0.06	0.021
No of other adults in HH	0.51	0.53	-0.02	0.611	0.49	0.51	-0.02	0.655	0.47	0.52	-0.05	0.308
No of children in HH	1.74	1.65	0.09	0.277	1.75	1.62	0.13	0.149	1.72	1.62	0.10	0.250
Total assets	14378	18881	-4503	0.014	16677	18729	-2053	0.33	16126	19386	-3260	0.128
Total liabilities	12631	14334	-1702	0.179	13589	14753	-1164	0.42	12995	14690	-1695	0.245
Ownership scale	2.57	2.58	-0.01	0.943	2.82	2.85	-0.03	0.845	2.70	2.70	0.00	0.992
Financial strain scale	0.55	0.58	-0.03	0.076	0.55	0.58	-0.03	0.078	0.56	0.57	-0.01	0.516
Giving help in community scale	0.56	0.54	0.02	0.330	0.56	0.54	0.02	0.263	0.56	0.54	0.02	0.172
Getting help from community scale	0.35	0.35	0.00	0.854	0.36	0.36	0.00	0.652	0.36	0.36	0.00	0.955
Community involvement scale	0.40	0.40	0.00	0.817	0.41	0.41	0.00	0.852	0.39	0.40	-0.01	0.546

control variables in the models discussed in this report, 65 respondents (7.6%) are missing on at least one variable and are thus excluded in models that use listwise deletion.

The notable exception to the low rates of item non-response is found in the variables that sum to net worth. Net worth is composed of 33 individual asset and debt measures and 44% of respondents are missing at least one of these, and are thus missing on the net worth variable. This high rate of nonresponse is driven by two measures: car value and non-housing property value, which were initially omitted from the Wave 4 survey and—after a supplemental survey to cover these questions—still are missing for 27% and 21% of respondents, respectively. For analyses of the wealth, assets, and debt outcomes, we supplemented listwise deleted data with imputed data as discussed below.

Analysis Plan and Methods

The ADD4 data were collected to estimate the long-term effect of assignment to eligibility for the CAPTC IDA program on various financial and nonfinancial outcomes. The outcomes of interest are those related to the behaviors subsidized by the IDA program (e.g. saving, home purchase, etc.) as well as the potential impacts of those behaviors on assets, health, and other social and economic variables. In this report, we detail basic investigation into the effect of treatment assignment on these outcomes. Each outcome will be explored and reported in more detail in future publications.

The ADD experiment randomly assigned study participants to the treatment and control groups, thus there should be no systematic difference between these groups. In principle then, the long-term impact of the Tulsa IDA program could be estimated as the simple difference between treatment and control on each outcome. In the results section below, we include

estimates of these differences for outcomes of interest. Similarly, in the program evaluation literature, difference in differences (DiD) analysis is often used when pre- and post-test measures are available on outcomes, as they are for many ADD outcomes. In experimental data, DiD may account for baseline differences in composition between groups, though not when baseline imbalance interacts with the treatment effect, as is shown and discussed below.

To supplement bivariate and DiD estimates and to improve the precision of the estimate of the treatment effect, we also employ regression analyses to examine the relationship between treatment assignment and the outcome. In these analyses, we control for selected baseline characteristics in the regression analysis. For most continuous outcomes, ordinary least squares (OLS) regression is used. Logistic or probit regressions were used for dichotomous outcomes, and Poisson regressions were used for count outcomes. In all analyses, an alpha of 0.100 was accepted as the threshold for identifying significant differences and impacts. Unless otherwise indicated, 2-tail tests of significance are used throughout the report. All regressions take the form:

$$W4 = a + bT + cW1 + dX + e$$

where $W4$ is the outcome variable at time 4, a is a coefficient on a constant, here taken to be one, bT is the treatment condition and its coefficient, $cW1$ is the Wave 1 value for the outcome variable and its coefficient, when available, dX is a vector of control variables,⁸ measured at baseline, and e is the error term.

Corrective steps were taken when influential, outlying cases biased point estimates of the average treatment effect. These outlying cases harm the precision of the point estimate of the treatment effect and inflate standard errors. For some outcomes, we used robust regressions, wherein outlying observations are

8. Regression models control for age, income, sex, education, bank account ownership, race, marital status, interview cohort, total assets, total debt, number of adults in the household, presence of children in the household, receipt of housing subsidy, health insurance, business ownership, non-housing property ownership, presence of retirement savings, car ownership, welfare receipt, ownership of big-ticket household goods, financial strain, community integration and involvement, health, and financial satisfaction, all measured at baseline. These covariates capture the main demographic and economic conditions at baseline that may influence the trajectory of respondents with respect to the outcomes.

down-weighted. For other outcomes, particularly those with a threshold connected to the common understanding of the mechanism or phenomenon, we used winsorizing. In winsorizing, extreme high and low values are recoded to a threshold value. Thus, the direction and valence of the case is maintained, but its leverage is reduced. When outcome variables were winsorized, sensitivity analyses with different threshold criteria were performed.

In some instances, in spite of random assignment, baseline sample imbalance existed between the treatment and control groups, which threatened the reliability of inference. For outcomes with evidence of this problem, in addition to covariance control in regression, we also fit models with propensity score weights. Propensity score weights account for unequal allocation to treatment, conditional on observed characteristics. After weighting, the treatment and control groups are equivalent on observed covariates. However, while propensity score weighting can control for observed differences, there could still be unmeasured differences in level of economic functioning that we are unable to control for.

For some of the outcomes, there were strong theoretical reasons to suspect that treatment may differentially affect specific sub-groups in the population, defined by exogenous baseline characteristics. When this was the case, the interaction of the characteristic with treatment was investigated using either sub-group analysis or the inclusion of interaction terms in regression models. Sub-group analyses were also performed and reported when a specific baseline population was thought to experience the effect of treatment in a different way from the sample at large, with respect to an outcome under investigation.

The specific analytic approaches used for each outcome reported below are reported when the outcome is discussed. Each analysis uses the available analytic sample, using listwise deletion to remove cases with item-missing data. The exception is the analysis of net worth, assets and debts. Because of a higher-than-usual percentage of item-missing data, five imputates were created using multiple imputation through chained equations and used for the analysis.

Limitations

Internal Validity (Crossovers and Contamination)

The internal validity of the experiment depends on how well it was implemented. We discuss two countervailing concerns: crossovers and other services. Each issue applies only to the period through 2003, rather than the entire study period through 2009.

For the first issue, a formal definition of a *crossover* is a control group member who, during the 1998 to 2003 period, received some part of the treatment—that is, opened an IDA or attended financial-education classes reserved for treatment. Crossovers could also be defined more expansively as control group members who, during the experimental period, received access to CAPTC’s homebuyer-assistance programs (other than the IDA) or who were able to open an IDA at some other non-CAPTC location.

Orr (1999) developed an intent-to-treat estimate adjusted for crossovers, ITT_o , that is calculated as $ITT_o = ITT/(1-c)$ where ITT is the intent-to-treat estimate, c is the proportion of the control group represented by crossovers, and where it is assumed that all treatment group members participate in the treatment.⁹ This

9. In the IDA experiment, crossovers are probably not a representative sample of controls; they are probably more highly motivated to save and so would have done better than the typical control even in the absence of crossover. As a result, dropping crossovers from the sample would undermine the balance between treatments and controls that is the purpose and chief benefit of random assignment.

10. The adjusted effect, $ITT_o = p(TOT) + (1-p)0 - c(TOT) - (1-c)0$. Collecting terms and noting that $ITT = TOT/p$ yields the equation in the text. The formula in the text collapses to the formula given by Orr when $p=1$. Both formulas are actually upper bounds on the adjustment for crossovers, since they assume that each crossover household received the full treatment. This assumption seems like an overstatement both because even those controls who opened an IDA are unlikely to have received all of the financial education and case management that treatment group members did and because (as discussed in the text below) more than half of those respondents we are counting as crossovers did not open an IDA.

adjustment alters the magnitude of the estimated treatment effect, but does not alter its statistical significance. We generalize this formula to allow for less than 100% participation by members of the treatment group ($p < 1$) in IDAs, in which case the resulting adjustment is $ITT_o = ITT * p / (p - c)$.¹⁰

The data show 21 control group members who reported participating in an IDA program during the experimental period and an additional 27 who reported participating in CAPTC's down payment assistance program, which was off-limits to both control and treatment group members under the experiment protocol. Even if all 48 members were considered crossovers, c is small ($0.107 = 48/448$), and the adjusted impact estimates are only slightly larger than the ITT estimates.¹¹

Table 3.4
Use of CAPTC Services During the Experimental Period

	N	Treat	Cont	Diff	p
Social programs	807	0.12	0.09	0.04	0.10
Workforce programs	807	0.03	0.02	0.01	0.39
Medical services	806	0.12	0.13	-0.01	0.83
Youth programs	806	0.12	0.09	0.04	0.08
Small business programs	807	0.07	0.01	0.06	0.00
Home buying programs	806	0.23	0.07	0.17	0.00
Education services	807	0.03	0.03	0.01	0.68
Tax preparation services	807	0.46	0.38	0.08	0.02

Note: The sample for this table includes Wave 4 respondents who were also in either Wave 2 or Wave 3.

A second issue works in the opposite direction from the crossover effect. As shown in Table 3.4, treatments were generally more likely than controls to use permitted non-IDA social services at CAPTC—especially tax-preparation services. In addition, although 27 control group members used home buying assistance services for which they were not eligible, 90 treatment group members used such services. It is not clear whether this is an outcome of the IDA program, part of the IDA treatment itself, or merely represents treatment group members misreporting permitted IDA-related home-buyer education as being part of another CAPTC program. The main point, though, is that treatment and control groups received different sets of benefits from CAPTC.

External Validity (Self-Selection and Motivation)

Efforts to generalize the results for the Tulsa IDA experiment should account for five considerations. The first is the condition of housing markets in the United States during the study period. Between 1998 and 2007 it was relatively easy to buy a home in the U.S. During that time, favorable demographics, strong economic conditions, innovations in mortgage markets—particularly sub-prime lending—and public policies and programs supporting homeownership all worked to increase the homeownership rate in aggregate and among low-income households in particular (Bostic & Lee, 2008; Herbert & Belsky, 2008). The general condition of United States housing markets during this period certainly contributed to the large increase in homeownership rates we find in both the treatment and control groups. In a housing market where obtaining loans is more difficult, IDA program participation may have a stronger impact on home purchase.

11. As an example of the magnitude of the effect, a 2 percentage point ITT effect would imply a 2.27 percentage point adjusted effect when $c = .107$ and $p(\text{IDA participation}) = .90$.

12. The median home value in Tulsa County (adjusted to 2008 dollars) was \$99,332 in 1990, \$111,481 in 2001, and \$124,607 in 2007 (Ard, O & Puckett, D., 2002; American Community Survey 2007). In 2009, the median home price to income ratio for Tulsa County was 2.8, compared to 6.2 for the nation (National Association of Realtors 2009).

13. Other evidence that may be indicative of the availability of homebuyer assistance programs in Tulsa is the fact that about 90% of both treatment and control group members with mortgages held fixed-rate mortgages, during a period of heavy sub-prime lending when mortgages increasingly featured adjustable rates.

A second issue is the housing market in Tulsa. Housing costs in the Tulsa area were substantially below national averages during the experiment, making homeownership even more affordable for low-income people.¹²

A third issue is the availability of other local home-ownership assistance. Tulsa seems to have had several affordable-housing programs during the study period, which offered financial assistance. For example, Housing Partners of Tulsa offered down-payment and closing-cost assistance equal to 5% of the purchase price upon completion of a home buyer education program (Tulsa Housing Authority 2008). No matched savings were required to receive those funds.¹³ IDA programs in areas that do not have other effective and competing home-buyer assistance programs may have stronger impacts.

A fourth issue has to do with program design. The Tulsa IDA program was among the first programs in the country when it started in 1998. Based on field experience, many current IDA programs are structured differently in terms of match rates, maximum available matches, duration, qualified uses of the funds, and so on. For example, most of the IDA programs today, funded

through the federal AFI program, offer savings period of up to 5 years rather than the 3-year period of the Tulsa program (U.S. Department of Health and Human Services, 2010). Alternative program designs may result in different program impacts.

Fifth, although the sample in Tulsa may well be a representative subsample of the population most interested in IDAs, it was not a representative sample of all qualified households. Mills, Gale, et al. (2008) find substantial differences between Tulsa IDA respondents and IDA-eligible samples drawn from the 1998 Survey of Consumer Finances and from 2000 Census data for the greater Tulsa area. Study participants were more educated, and are more likely to be single, female, and black than the comparison samples of IDA-eligible households. The impact of IDA program participation on a more representative sample of eligible participants may vary from those reported here, although our subgroup analysis suggests that, other than income, there were no statistically significant differences within subgroups.

To provide additional evidence on this, we drew a sample from the 1999 Panel Survey of Income Dynamics (PSID)

Table 3.5
Change in Homeownership Rates: IDA Control Group Sample Versus IDA-Eligible PSID Sample

	Tulsa IDA control group	IDA-eligible PSID sample	Difference	p
Whole Sample				
Homeownership in Wave 1 (1999)	0.24	0.3	-0.06	0.04
Homeownership in Wave 4 (2007)	0.53	0.43	0.1	0.00
Difference	0.29	0.14	0.16	0.00
Owners in Wave 1 (1999)				
Homeownership in Wave 1/1999	1	1	0	-
Homeownership in Wave 4/2007	0.79	0.84	-0.05	0.28
Difference	-0.21	-0.16	-0.05	0.28
Renters in Wave 1 (1999)				
Homeownership in Wave 1/1999	0	0	0	-
Homeownership in Wave 4/2007	0.45	0.26	0.19	0.00
Difference	0.45	0.26	0.19	0.00

14. One potential concern with this comparison is that even after selecting for IDA eligibility in 1999, the PSID sample was substantially different from the ADD sample on demographic and financial characteristics. In sensitivity analysis, we reweighted the samples using propensity score radius matching and the basic finding did not change.

based on the eligibility rules for the Tulsa IDA. The time elapsed between the 1999 and 2007 waves of the PSID is roughly comparable to the period between the Wave 1 and Wave 4 surveys described above. Table 3.5 shows substantial differences in the increase in homeownership between the PSID sample and the Tulsa control group. In the PSID sample, the homeownership rate rose by 14 percentage points, from 30% in 1999 to 43% in 2007. In contrast, among Tulsa control group members, the homeownership rate rose by 29 percentage points, from 24% in 1998-99 to 53% by 2009. Among renters in the initial period, the increase in homeownership rates was 19 percentage points higher in the Tulsa control group than in the PSID subsample. All of these differences are highly significant.¹⁴ These results may suggest that controls in the CAPTC experiment either were more motivated to purchase homes or faced more favorable housing market and housing assistance conditions than the general US population with similar observed characteristics. This also demonstrates the importance of using a randomized evaluation to study the impacts of IDAs, rather than drawing on a nonrandomized sample of observationally equivalent households that did not self-select into an IDA experiment.

Measurement Error

A universal concern in survey-based research is the potential deviation of given responses from the true value. Misunderstanding of the question, data entry errors, recall errors, and biases such as social desirability bias can all introduce errors in measurement.

Furthermore, due to self-reporting, the data may not be a precise measure. The CAPI and CATI systems used in data collection included automatic range check prompts and follow-up verification by interviewers. Though the instruments and interview modes of ADD took steps to minimize measurement error, it could still persist in the data. There is no reason to believe, however, that measurement error would correlate with treatment assignment. Nevertheless, measurement error, even when random, has the effect of creating noise and damping effects that might exist.

Minimal Detectable Effects

The small sample size reduces power and makes it challenging to find statistically significant differences, even when effect sizes are meaningful. To illustrate this challenge, we calculate the minimum detectable effects for major impacts, including net worth, homeownership, education, and business outcomes, presented in Table 3.6.

Table 3.6
Minimal Detectable Effects for Major Outcomes

	Control proportion/ mean (SD)	Point estimate	Power	MDE (power=.80, alpha=.10)
Homeownership	0.52	.03	.19	.09
Duration of homeownership	6.43 (2.95)	.18	.23	.52
Appreciation rate	3,154 (7,018)	1,280	.50	1,921
Winsorized appreciation rate	2,933 (5,294)	735	.35	1,447
Rate of return per year of ownership	.12 (23)	26	1.0	6.3
Expense amount	6,350 (9,577)	-532	.15	-2,400
Winsorized expense amount	4,026 (4,066)	-325	.20	-1,026
Amount needed	11,691 (11,700)	-1,796	.27	-4,341
Winsorized amount needed	8,566 (5,061)	-2,091	.87	-1,866
Any repairs	0.68	0.00	n/a	.12
Any forgone repairs	0.47	-0.03	.12	-.13
Increase in education	0.34	0.04	.22	.11
New some college	0.28	0.11	.55	.15
New college degree	0.21	-0.01	.08	.09
Enroll in class	0.46	0.06	.50	.09
New degree	0.31	0.04	.31	.09
Business equity	4,501 (43,102)	-169	.10	7,749
Winsorized business equity	681 (2,328)	-53	.12	419
Business ownership	0.14	-0.02	.18	.07
Any dedicated retirement savings	0.47	0.02	.12	.10
Mean value of retirement savings	5,545 (15,026)	-1,315	.31	2,855
Winsorized value of retirement savings	5,658 (9,086)	-346	.14	1,717
Untrimmed net worth	31,057 (106,816)	-1,874	.11	18,193
Net worth, robust regression	31,057 (106,816)	2,889	.13	18,193
Untrimmed total assets	93,260 (161,643)	-1,630	.10	27,340
Total assets, robust regression	93,260 (161,643)	2,362	.11	27,340
Untrimmed total debts	62,203 (97,477)	-22	.10	-16,703
Total debts, robust regression	62,203 (97,477)	1,557	.11	-16,703
Untrimmed liquid assets	3,870 (12,859)	-753	.22	2,180
Liquid assets, robust regression	3,870 (12,859)	791	.23	2,180
Untrimmed short-term debt	8,251 (26,859)	-2,132	.32	-4,551
Short-term debt, robust regression	8,251 (26,859)	-7	.10	-4,551





Chapter 4

Findings from the ADD4 Study

In this chapter, we present an overview of the central results of the Tulsa ADD experiment. These are preliminary results and are not the final findings on any outcome. These outcomes will be explored in more detail in future work.

For these analyses we use a consistent methodological approach. We present bivariate and regression results and briefly discuss the findings.

Homeownership

Homeownership was the most popular intended use of the IDA in the CAPTC IDA program. Saving for homeownership also received a higher match rate (2:1) than the other qualified program uses (1:1).

Below, we evaluate the effect of treatment assignment on homeownership using a variety of measures. First, we examine whether treatment increased the rate of homeownership at Wave 4. Second, we examine the effect of treatment on duration of homeownership between 1998 and 2008. Finally, we examine the effect of treatment on homeownership by subgroups.

Tables 4.1 and 4.2 present the estimates of the effect of treatment assignment on homeownership rates at Wave 4. The effect is measured using DiD, regression, and, because at baseline treatment group members were less likely to own their home, regressions weighted with propensity scores. Both the treatment and control groups experienced large increases in homeownership between Wave 1 and Wave 4. As presented in Table 4.1, the homeownership rate at Wave 4 was 31 percentage points higher than at Wave 1 for treatment group respondents while the Wave 4 homeownership rate was 26 percentage points higher than at Wave 1 for the control group. Though the DiD analysis suggests a slight difference for the full sample, regression analyses show that this is a result of the baseline sample composition (see Table 4.2). There was no observed significant effect of treatment on the level of homeownership at Wave 4.

Table 4.1
Difference in Differences Analysis of Homeownership Rates

	Treatment proportion	Control proportion	Difference T-C	p
Homeownership among full sample (n=852)				
Wave 1	0.21	0.26	-0.05	0.11
Wave 4	0.53	0.52	0.01	0.80
Wave 4-Wave 1	0.31	0.26	0.06	0.15
Homeownership among baseline owners (n=201)				
Wave 1	1	1	0	
Wave 4	0.79	0.77	0.02	0.78
Wave 4-Wave 1	0.79	0.77	0.02	0.78
Homeownership among baseline renters (n=651)				
Wave 1	0	0	0	
Wave 4	0.45	0.43	0.03	0.49
Wave 4-Wave 1	0.45	0.43	0.03	0.49

There are substantial programmatic and theoretical reasons, though, to suspect that the effect of treatment may not be equivalent across all subgroups in the sample. In particular, those who rented at baseline faced a very different set of incentives and opportunities in the IDA program than did those who owned at baseline. Moreover, respondents with higher incomes at baseline may have been more able to accumulate the lump sum needed for a down payment and closing costs. Thus, we examine these groups separately and compare the treatment effect between the subgroups.

Table 4.2
Regression Analysis of Homeownership Rate

Treatment effect on homeownership				
	N	b	S.E.	p
Full sample	823	0.03	0.03	0.39
Propensity score weighted	823	0.03	0.03	0.38
Propensity score matched	650	0.00	0.04	0.91

Analysis of Interaction Between Treatment and Income

In Table 4.3, we see that treatment had no impact on the homeownership rates of baseline renters or baseline owners, however there was a positive, significant impact of treatment on homeownership among those with an above-median income at baseline (about \$15,480 per year). For the above-median income group, the treatment raised homeownership rates by about 10.6 percentage points at Wave 4 ($p < 0.05$), statistically significant relative to those in the control group with above-median income. We tested a number of other subgroup interactions. Among the other factors tested, none interacted significantly with treatment and are not presented here. It is possible that the significant interaction between baseline income and treatment reported here results from multiple comparisons and random chance, rather than from a real effect.

Duration of Homeownership

The impact on homeownership levels observed at Wave 3 suggests that treatment might have increased the duration of homeownership between 1998 and 2009 for those in the treatment group, relative to those in the control group.

Figure 4.1 shows the pattern of homeownership for the treatment and control groups using information that integrates retrospective and prospective data to estimate homeownership in each year. Figure 4.1 shows trends in

Figure 4.1
Estimated Homeownership Rate, 1998/99—2008/09

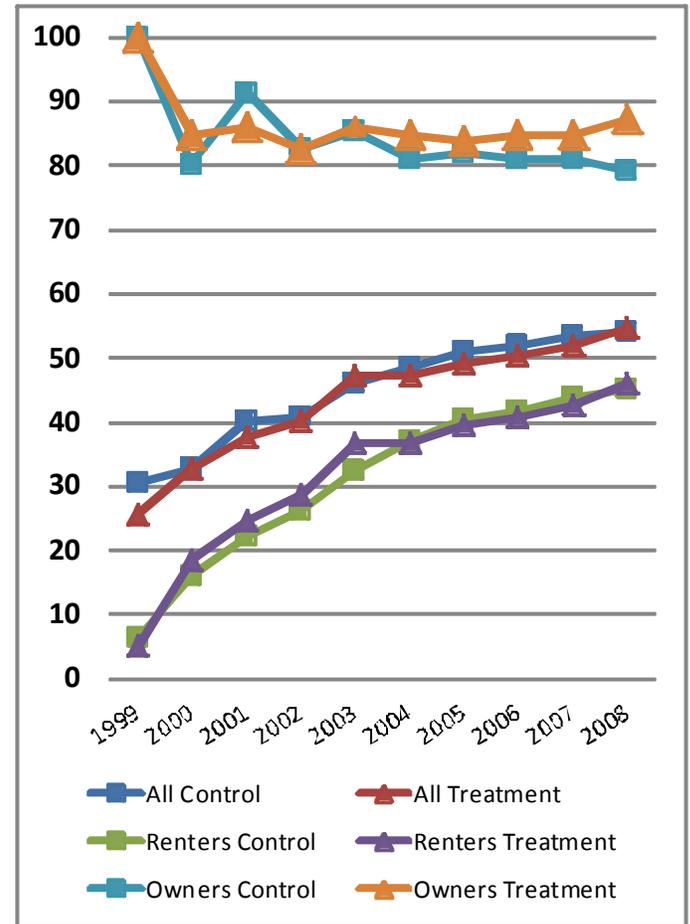


Table 4.3
Subgroup Analysis of Homeownership Rates at Wave 4

	Baseline owner			Baseline renter		
	N	dF/dx [chi-sq]	p	N	dF/dx [chi-sq]	p
Treatment effect	197	-0.01	0.86	626	0.03	0.44
Subsample comparison test		[0.22]	0.83			
	Median income and above			Below median income		
	N	dF/dx [chi-sq]	p	N	dF/dx [chi-sq]	p
Treatment effect	413	0.11	0.04	413	-0.05	0.31
Subsample comparison test		[5.76]	0.02			

homeownership for baseline owners, all respondents, and baseline renters. Among baseline renters, we observe a higher rate of ownership among treatment group members in 2003, consistent with the use of incentivized funds at the end of the program. Figure 4.1 shows, though, that the homeownership rate of the control group grew steadily throughout the period and that the homeownership rate for those in the treatment group was the same as that observed in the control group by 2004.

We explore this dynamic in more detail by examining the estimated duration of homeownership between 1998 and 2009, defined as the number of years in that period in which the respondent owned a home.

Control group members averaged 4.5 years of homeownership between 1999 and 2009 whereas treatment group members averaged 4.4 years of homeownership. The difference between the two groups is not statistically significant. Moreover, the aggregate comparison is biased by the higher rates of baseline homeownership in the control group. As before, the bias is resolved by examining trends for baseline owners and baseline renters separately and by the use of regression

Table 4.4
Regression Analysis of Duration of Homeownership

Treatment effect on duration of homeownership				
	N	b	S.E.	p
Full sample	823	0.19	0.23	0.42
Propensity score weighted	823	0.18	0.23	0.43
Propensity score matched	650	0.08	0.25	0.72

analysis, which controls for initial baseline status.

Table 4.4 presents regression analysis of the impacts of the IDA program on the duration of homeownership. The estimated treatment effects are in the range of about 0.1 to 0.2 years, but none of the effects are statistically significantly different from zero.

Table 4.5 presents the impacts of IDAs on the duration of homeownership for the same subsamples as in Table 4.3. There was no effect of treatment on the duration of homeownership among baseline owners or among baseline renters. As with the analysis of the homeownership rate at Wave 4 presented above, IDA treatment affected duration of homeownership for

Table 4.5
Subgroup Analysis of Duration of Ownership at Wave 4

	Baseline owner			Baseline renter		
	N	dF/dx [chi-sq]	p	N	dF/dx [chi-sq]	p
Treatment Effect	197	0.35	0.41	626	0.20	0.47
Subsample Comparison Test		[0.51]	0.33			
	Median income and above			Below median income		
	N	dF/dx [chi-sq]	p	N	dF/dx [chi-sq]	p
Treatment Effect	413	0.87	0.01	413	-0.299	0.39
Subsample Comparison Test		[1.17]	0.02			

15. Where indicated, spending on home repair was winsorized at \$10,000.
 16. Where indicated, the cost of unmade repairs was winsorized at \$15,000.
 17. Where indicated, rate of home appreciation was winsorized at \$25,000/year and -\$25,000/year.

higher-income respondents relative to lower-income respondents. The duration of homeownership for treatment group members earning above the sample median income was 0.87 years longer than for control group members earning above the sample median income, a statistically significant difference ($p < .05$).

Home Repair and Improvement

Treatment group members could use IDA funds at a match rate of 1:1 to pay for improvements to home and property that they owned. We test the effect of treatment assignment on home repair outcomes by looking at a set of related outcomes. We examine whether the respondents engaged in home repair or improvement, the amount they reported spending on those efforts,¹⁵ whether a repair was needed but unmade, and the estimated cost of those unmade repairs.¹⁶

Because investment in improvement and repair should affect the value of an owned home, we also examine the rate of home appreciation during this period.¹⁷ The rate of home appreciation was calculated as the difference in self-reported home value or sale price between the beginning of the observation period (baseline home

value or purchase price of a bought house) and the end of the observation period (Wave 4 home value or selling price of a sold house), divided by the number of years in the home during observation.

As shown in Table 4.6, at Wave 4, about 68% of both treatment and control group members report having engaged in home improvement or repair costing more than \$500 since their baseline interview. The two groups also report statistically equivalent amounts spent on home repair and are equally likely to indicate that they have forgone a repair that they could not afford. However, the estimated cost of those unmade repairs differs significantly between the two groups. Adjusting for outlying values, the treatment group reports about \$1,500 dollars less in unmade repairs than the control group.

We also observe a statistically significant difference in the rate of appreciation between treatment and control group members. Treatment group members gain \$964 more in home value per year of ownership than do control group members.

Table 4.6
Bivariate analysis of home repair

	N	Treatment	Control	Difference	p
		mean/prptn	mean/prptn	T-C	
Appreciation rate	367	4,829	3,057	1,772	0.08
Winsorized appreciation rate	367	3,817	2,853	964	0.09
Rate of return per year of ownership	367	25	13	12	0.36
Expense amount	440	5,938	6,557	-619	0.57
Winsorized expense amount	440	6,659	6,990	-332	0.25
Amount needed	200	9,234	11,627	-2,392	0.12
Winsorized amount needed	200	7,517	9,058	-1,541	0.03
Any repairs	443	0.68	0.69	-0.011	0.81
Any forgone repairs	443	0.48	0.47	0.01	0.77

Table 4.7
Regression Analysis of Home Repair

Treatment effect on...	Full sample				Baseline owners			
	N	b	S.E.	p	N	b	S.E.	p
Appreciation rate	330	1,203	1,175	0.30	138	2,102	958	0.02
Winsorized appreciation rate	330	686	631	0.14	138	1,815	852	0.02
Rate of return per year of ownership	330	26	15	0.09	138	80	49	0.11
Expense amount	394	-539	1,113	0.69	142	-1,002	1,694	0.72
Winsorized expense amount	765	-184	303	0.73	183	-380	690	0.71
Amount needed	181	-1,775	1,842	0.17	73	-1,051	5,118	0.42
Winsorized amount needed	191	-2,077	802	0.01	75	-558	2072	0.39
Any repairs	395	0.02	0.24	0.53	125	-0.45	0.64	0.76
Any forgone repairs	395	-0.13	0.23	0.29	137	-1.33	0.51	0.01

These outcomes were explored further using regression analysis. As shown in Table 4.7, the regression analyses largely confirm the pattern of results observed in the descriptive statistics. There is no difference between the treatment conditions in terms of the presence of repairs, or the amount spent on repairs. As above, treatment group members in the full sample, though no more likely to report unmade repairs, reported that the cost of those unmade repairs was significantly lower. However, whereas we observed a difference in appreciation rate in the bivariate analysis, the regression analysis finds no significant difference in appreciation for the full sample.

Because baseline owners may have been more likely than baseline renters to invest in home repair, we also analyzed that subsample as part of the regression analysis. When examining home repair outcomes among those who owned their home at baseline, several interesting findings emerge. Among baseline owners, those in the treatment group were significantly less likely to report needed repairs they could not afford. Baseline owners in the treatment group also enjoyed a significantly higher rate of appreciation than did members of the control group.

Table 4.8
Bivariate Analysis of Education Outcomes

	N	Treat	Cont	Diff	p
		prptn	prptn	T-C	
Baseline					
Less than HS	824	0.07	0.07	0.01	0.70
HS degree	824	0.26	0.25	0.01	0.86
Some college	824	0.41	0.42	-0.01	0.70
College grad or more	824	0.26	0.26	0	0.98
Outcome					
Less than HS	824	0.08	0.07	0.01	0.69
HS degree	824	0.20	0.20	0	0.93
Some college	824	0.37	0.33	0.03	0.32
College grad or more	824	0.35	0.39	-0.04	0.26
Enrolled in new course	824	0.52	0.45	0.06	0.06
New degree from course	824	0.35	0.30	0.04	0.18
Increase in degree level	707	0.39	0.35	0.04	0.33
New HS degree	824	0.24	0.29	-0.04	0.70
New some college	824	0.35	0.28	0.07	0.23
New college grad	824	0.20	0.21	-0.01	0.76
Enrolled in job training	824	0.24	0.26	-0.02	0.43
Completed job training	824	0.22	0.25	-0.03	0.34

Education

As shown in Table 4.8, at baseline, the treatment and comparison groups are well-matched with respect to education level. A plurality of participants (a bit more than 40% in both groups) report that they have some college education, but not a college degree. About one-quarter of participants in both groups report having only a high school diploma, while another quarter report that they have a college degree. Only a small percentage (7% in each group) report that they did not complete high school.

At Wave 4, the distribution of educational achievement is changed slightly, due to a greater proportion of respondents reporting higher levels of education. The proportion of respondents without a high school degree is essentially unchanged, while the proportions with a high school degree or with some college are lower at Wave 4 than at Wave 1. The individuals who exited these categories seem to have moved into the college graduate category, which is the only category for which the proportion is higher at Wave 4 than at baseline. The treatment and control groups did not differ significantly on educational attainment at any level.

In addition to comparing the distribution of education level at Wave 4, we explore several other outcomes. The first is whether respondents enrolled in an education program at any point since baseline. A larger proportion of treatment group members than control group members enrolled in such a program, and this difference was significant. In addition, we compare treatment groups on their receipt of a degree from an education program since baseline. About 30% of both groups reported receiving a degree, and there was no statistically significant difference between the two groups.

In addition to measuring whether respondents received a degree, we assessed whether they reported an increased educational level on the categorical measure of education. By this measure, around 35% of the control group and 39% of the treatment group increased their education. The treatment and control groups did not differ

significantly from one another on this outcome. In order to better understand the experience of those participants who increased their education, we created a series of variables to indicate whether a respondent had achieved a high school diploma, some college, or a college degree

Table 4.9
Propensity Score Weighted Regression Analysis of Education Outcomes

Treatment effect on...	N	dF/dx	S.E.	p
Increase in education	548	0.04	0.04	0.37
New some college	267	0.11	0.06	0.09
New college degree	609	-0.01	0.02	0.69
Enroll in class	824	0.06	0.04	0.09
New degree	824	0.04	0.03	0.23

since baseline. Each of these variables is only created for those respondents who had a lower level of education at baseline, reducing the sample size for the analyses of these variables. It is possible for a respondent to be coded as having achieved more than one type of additional education. For example, if an individual had a high school diploma at baseline, but was able to earn a college degree over the course of the study period, he would be coded as having newly earned both 'some college' and a college degree. The proportion of treatment and control groups achieving each kind of new education are roughly the same, except for a higher proportion of treatment group members achieving some college. Bivariate analyses of these variables show no difference between the two treatment groups.

To further explore the potential relationship between treatment and educational outcomes, we conducted marginal effects probit regression analyses predicting outcomes from treatment assignment while controlling for a variety of covariates.

Table 4.9 shows the treatment effect as the marginal difference between treatment and control. It is interpreted as the difference between the proportion of treatment

group and control group members achieving the outcome.

We observe a small but significant effect of treatment on the likelihood that a respondent enrolled in an education program ($p < 0.10$). However, there is no significant impact on the likelihood of earning a degree, or on the likelihood of increasing education level. When examining the estimated effect of treatment on the likelihood of gaining certain levels of education, we find that there is a marginally significant impact on the likelihood of gaining 'some college' during the study period, but not on the likelihood of earning a college degree. Due to small sample size, the few respondents who earned a high school degree during the study period are not analyzed. Finally, with regard to the job training outcomes, there is

no evidence of a positive treatment effect.

There is reason to believe that treatment may have differential impacts on certain subsamples of respondents. To explore this possibility, we examined the treatment effect separately for subsamples based on gender, income, and whether the respondent was banked at baseline. We did so for three major outcomes: enrollment in an educational program, receipt of a degree, and increased education. Marginal effects probit models were used to estimate treatment effects. Table 4.10 presents results of these analyses. For each subsample we present the estimated treatment effect.

Subsample on Gender

With regard to subsamples based on gender, men

Table 4.10
Subgroup Analysis of Education Outcomes

	Enrolled in school				Acquired degree or certificate from school				Increased education level			
	Female (n=659)		Male (n=152)		Female (n=659)		Male (n=145)		Female (n=435)		Male (n=110)	
	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p
Treatment effect	0.06	0.16	0.20	0.04	0.03	0.46	0.14	0.05	-0.03	0.55	0.43	0.01
Subsample comparison test	[2.08]	0.15			[4.57]	0.03			[18.20]	0.00		
	R < median income (n=400)		R > median income (n=424)		R < median income (n=400)		R > median income (n=424)		R < median income (n=284)		R > median income (n=264)	
	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p
Treatment effect	0.06	0.27	0.02	0.76	0.08	0.10	-0.02	0.64	0.09	0.16	0.01	0.89
Subsample comparison test	[0.34]	0.56			[2.38]	0.12			[0.86]	0.35		
	Banked (n=697)		Not banked (n=126)		Banked (n=697)		Not banked (n=113)		Banked (n=449)		Not banked (n=82)	
	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p	dF/dx [chi-sq]	p	dF/dx	p
Treatment effect	0.06	0.19	0.20	0.18	0.03	0.20	0.22	0.02	0.06	0.23	0.02	0.90
Subsample comparison test	[1.14]	0.29			[4.67]	0.03			[0.05]	0.83		

experience a larger effect of treatment on education outcomes than women. For the likelihood of enrollment in school, the treatment effect for men ($dF/Dx=0.20$, $p<0.05$) is much larger than that for women ($dF/Dx=0.06$, $p>0.10$). Nevertheless, the post-test of the difference between the two treatment effects was not significant. In the case of the receipt of degree outcome, the treatment effect once again differs by gender, showing a large and significant impact for men ($dF/Dx=0.14$, $p<0.10$), and a smaller impact for women ($dF/Dx=0.03$, $p=0.46$). In this case, the treatment effect for women is not statistically significant, and the comparison between the two treatment effects revealed that they are significantly different from one another ($p<0.05$). In our examination of the increased education outcome, the pattern is repeated: men experience a large and significant impact ($dF/Dx=0.43$, $p<0.01$), while the treatment effect for women is non-significant ($dF/Dx= -0.03$, $p=0.55$), and these treatment effects are significantly different ($p<0.01$).

Subsample on Income

A similar set of subsample analyses were conducted for groups divided by income level at baseline. Specifically, we compared treatment effects on those who earned the median income or more at baseline, and those who made less. For the enrollment outcome, the treatment effect is not significant for either group. For the degree outcome, there is no effect of treatment on those with below-median income or those with higher incomes. A similar pattern was seen with regard to the increased education outcome. There is no significant effect of treatment on higher-income respondents or on those with below-median incomes. The treatment effect comparison tests for both the degree and increased education outcomes were non-significant.

Subsample on Banked and Unbanked

Finally, we analyzed subsamples composed of those who were banked and unbanked at baseline. For the enrollment outcome, there is not a significant impact on either

Table 4.11
Bivariate Analysis of Business Ownership

	Treatment		Control	Difference	p
	N	mean/proportion	mean/proportion	T-C	
Business equity	845	7,365	4,204	3,161	0.32
Winsorized business equity	845	741	664	76	0.64
Number of part-time employees	854	0.04	0.08	-0.04	0.16
Number of full-time employees	854	0.07	0.08	-0.01	0.72
Age of the business	120	10.19	9.75	0.43	0.80
Business ownership	855	0.13	0.13	0.00	0.89

Table 4.12
Regression Analysis of Business Ownership

Treatment effect on...	Full sample				Wave 4 business owners			
	N	b	S.E.	p	N	b	S.E.	p
Business equity	760	-169	2,735	0.95	97	6,094	24,729	0.81
Winsorized business equity	760	-53	169	0.75	97	830	1,082	0.45
Business ownership	760	-0.13	0.13	0.40				

*Where noted, business equity is winsorized at \$10,000.

the banked ($dF/Dx=0.06$, $p>0.10$) or unbanked ($dF/Dx=0.20$, $p>0.10$). Although the treatment effect for the unbanked is much larger than that for the banked respondents, the post-test comparing treatment effects was not significant. An even more pronounced difference was visible with regard to the degree attainment outcome. Banked respondents did not experience a significant treatment effect, while unbanked respondents had a large and significant treatment effect ($dF/Dx=0.22$, $p<0.10$). Post-testing indicated that the difference between the two treatment effects was significant. For the increased education outcome, however, the pattern is not repeated. There is no significant treatment effect on either the banked or unbanked subsamples, and the difference between the two was also not significant.

Business Ownership and Equity

At baseline, about 7% of the sample reported owning a business. While this proportion substantially increased

Table 4.13
Bivariate Analysis of Retirement Savings

	N	Treat mean/ prptn	Cont mean/ prptn	Diff T-C	p
Any dedicated retirement savings	853	0.44	0.42	0.03	0.48
Mean value of retirement savings	785	4,836	5,500	-664	0.56
Winsorized mean value of retirement savings	785	3,559	3,795	-236	0.66

between baseline and Wave 4 (13% of respondents own a business at Wave 4), there was no significant effect of treatment on business ownership at Wave 4.

In Table 4.11, we see that about 13% of each group owned a business at Wave 4. We also find that while the treatment group had \$3,161 more in business equity,

Table 4.14
Regressions for Retirement Savings

Treatment effect on...	N	b	S.E.	p
Any dedicated retirement savings	687	0.11	0.17	0.52
Mean value of retirement savings	687	-1,315	1,131	0.25
Winsorized mean value of retirement savings	687	-346	504	0.49

after adjusting for outliers¹⁸ and looking at the full sample, there was no difference in business equity between treatment and control group members. The regression analyses presented in Table 4.12 confirm the findings from the descriptive analyses; control and treatment group members do not differ with respect to business.

Retirement Savings

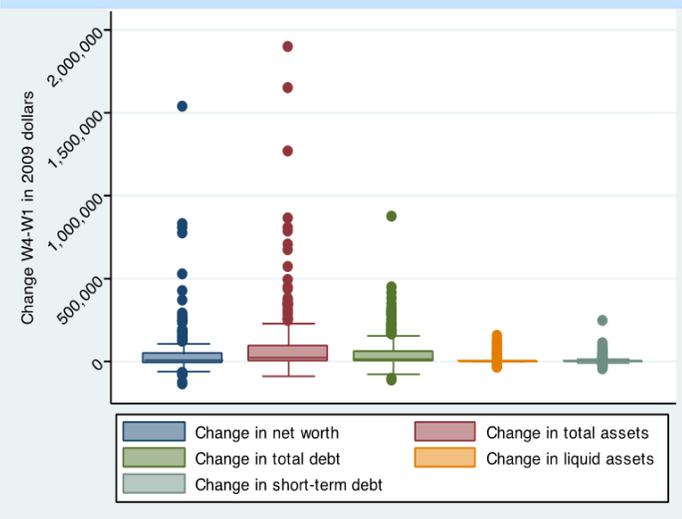
One qualified use of IDA savings was to roll the funds over into an IRA account. For participants who used their savings for contributing to their retirement account, which was the third most common savings goal among IDA account holders, participants were given a 1:1 match. To assess the impact of IDA participation on retirement savings 6 years after program completion, we compared treatment and control group members on both the presence and value of retirement savings.¹⁹

As shown in Table 4.13, bivariate comparisons did not reveal statistically significant differences between the two groups with regard to retirement savings outcomes. Slightly more than 40% of both groups reported having dedicated retirement savings such as IRAs and 401(k)s. The value of these savings was roughly \$5,000 for both groups, although this amount was closer to \$4,000 once we adjusted for extreme values.

These results were further explored using regression analysis and controlling for relevant covariates (see Table

19. Where noted, the value of retirement savings is winsorized at \$25,000.

Figure 4.2
Distribution of Selected Wealth Variables, Change from 1998/99 to 2008/09



4.14). Regression analyses did not detect significant differences between groups. It is important to note, however, that these analyses are based on self-reported data about dedicated retirement accounts. Thus, the data do not reflect savings that may be intended for use during retirement but which are saved in other ways, for instance, long-term retirement savings that may be held in a general savings account. Furthermore, due to self-reporting, the data may not be a precise measure of the actual value of retirement accounts.

Wealth, Assets, and Liabilities

One of the long-term impacts of interest is the impact of Tulsa IDAs on wealth accumulation. The study of net worth is frequently hampered by methodological challenges. Our analysis attempts to address some of these issues including item-missing data, outliers, and heteroskedasticity. In this section, we present findings on five wealth outcomes: net worth, total assets, total debt, liquid assets, and short-term debt. Because value can be shuffled among these, our key outcome in this analysis is net worth.

Figure 4.2 shows box plots of each outcome for all respondents. Box plots show the dispersion of the variables and are useful in the identification of outliers. For each variable, shown here as change in value between Wave 1 and Wave 4, the box plot shows five distributional characteristics. The box itself represents the location of the 25th percentile (bottom of the box) and the 75th percentile (the top of the box). The line inside the box is the median. The braces beyond the box extend 1.5 times the interquartile range (distance between the 25th and 75th percentile in each direction) and data outside of the braces are indicated by dots. As in all other wealth data, our wealth outcomes are characterized by fairly compact interquartile ranges, low medians, skewed distributions, and large numbers of extreme outliers.

Because of the large number of outliers, concerns arise about the influence of outlying cases on our estimates of the treatment effect. In the analyses below, we attempt to reduce the influence of outlying cases through the use of symmetrical trimming and robust regression. We also show findings with multiple approaches to item-missing data. As discussed above, net worth measures are comprised of variables gauging the level of assets and debt (33 in total). A large portion of Wave 4 respondents are missing information on at least one of these 33 measures. Consequently, data are imputed using multiple imputations through chained equations. The creation and analysis of implicates allows us to incorporate into our analyses the characteristics of those who are dropped by listwise deletion. Data are imputed for each item and aggregated variables are regenerated in each implicate.

Table 4.15 shows the results of the regressions, using listwise deletion. The results above demonstrate the challenges of inference on a variable with the characteristics of our wealth measures. First, in this version, because we trim on extreme changes from Wave 1 to Wave 4 in the outcome variable, different cases are trimmed in each analysis. In addition, results are inconsistent between different trim levels and between trimmed and robust regressions. Significance level,

Table 4.15
Regression Analysis of Wealth Outcomes, Unimputed Data

	Untrimmed		2.5 % on W4- W1 extremes		5% on W4-W1 extremes		Robust regressions	
	b/se	p	b/se	p	b/se	p	b/se	p
Net worth								
Treatment	-9209 [12,569]	0.46	-6,423 [5,400]	0.24	-1,316 [4,341]	0.76	-3,670 [3,863]	0.34
N	348		330		312		348	
Total assets								
Treatment	2,268 [16,122]	0.89	5,898 [7,291]	0.42	7,493 [6,246]	0.23	7,674 [5,781]	0.19
N	447		423		401		447	
Total debts								
Treatment	-2,145 [7,128]	0.76	-71 [4,170]	0.99	598 [3,740]	0.87	573 [3,619]	0.87
N	657		623		591		657	
Liquid assets								
Treatment	-724 [842]	0.39	-234 [265]	0.38	-223 [187]	0.23	98 [52]	0.06
N	748		710		672		748	
Short-term debt								
Treatment	-748 [1,426]	0.60	1,103 [543]	0.04	489 [443]	0.27	175 [318]	0.58
N	745		706		669		745	

Table 4.16
Regression Analysis of Wealth Outcomes, Imputed Data

	Untrimmed		2.5 % on W4- W1 extremes		5% on W4-W1 extremes		Robust regressions	
	b/se	p	b/se	p	b/se	p	b/se	p
Net worth								
Treatment	-1,874 [7,310]	0.80	2,439 [3,478]	0.49	3,819 [2,904]	0.19	2,889 [2,747]	0.30
N	855		810		763		855	
Total assets								
Treatment	-1,630 [10,231]	0.87	2,565 [5,460]	0.64	-800 [4,600]	0.86	2,362 [4,546]	0.60
N	855		808		765		855	
Total debts								
Treatment	-22 [6,185]	0.99	909 [3,866]	0.81	1,849 [3,434]	0.59	1,557 [3,365]	0.64
N	855		809		765		855	
Liquid assets								
Treatment	-753 [765]	0.33	-259 [245]	0.30	-212 [172]	0.22	79 [47]	0.10
N	855		810		766		855	
Short-term debt								
Treatment	-2,132 [1,384]	0.12	369 [362]	0.31	-75 [251]	0.77	-7 [64]	0.92
N	855		810		764		855	

magnitude of impact, and even sign direction of the impact all change between analyses. Even after trimming, large magnitudes of impact are insignificant because of the size of the standard errors. This leads to results that are hard to interpret. The top line result is large and negative. Given this we expect, at the mean, for the point estimates of the effect on total debt to be larger than the

Table 4.17
Bivariate Analysis of Psychological Outcomes

	N	Treatment mean	Control mean	Diff T-C	p
CES-D 10	817	7.41	6.69	0.722	0.10
Zimbardo	817	1.19	1.18	0.004	0.87
Stress	817	23.51	23.25	0.260	0.63

Table 4.18
Regression Analysis of Psychological Outcomes

Treatment effect on...	N	b	S.E.	p
CES-D 10	817	0.62	0.44	0.16
Zimbardo	817	0.00	0.03	0.17
Stress	817	0.22	0.54	0.68

effect on total assets. Instead, we find the opposite. We suspect that missing data contribute substantially to this finding. Notice that the sample size changes between analyses. This is because different respondents are missing on different outcome variables. We address this here through the use of imputation.

Looking at the impacts on the indicators of wealth in Table 4.16, the results are mixed. After adjusting for outliers, there is a moderate but non-significant effect of treatment on net worth (in robust regressions, \$2,889, $p=0.30$). We observe a similarly large impact on total assets and total debts, with assignment to treatment

substantially but non-significantly increasing both. There is a marginally significant but economically small effect of treatment on liquid assets. Assignment to treatment is associated with \$79 more in liquid assets, relative to the control group.

Using the imputed data the results at each level of trim are more internally consistent. It is also worth noting that, while nothing approaches conventional levels of significance, the sign of the treatment effect on net worth is positive at higher levels of trim. Still, given our findings in the full case data and incorporating methods to correct for missing data and distributional problems, there is no evidence of a treatment effect on wealth after 10 years.

Psychological Outcomes

The Wave 4 survey included several standardized scales of psychological outcomes including depressive symptoms, stress, and future orientation. These measures are new and were not included at baseline. At Wave 4, depressive symptoms were measured with the Center for Epidemiological Studies Depression 10-item scale (CES-D-10), stress was measured with questions developed by Cohen, Kamarck, and Mermelstein, and future orientation was measured with the Zimbardo scale (Andresen, Malmgren, Carter & Patrick, 1994; Cohen, Kamarck, & Mermelstein, 1983; Zimbardo & Boyd, 2003).

In the descriptive statistics, we observe no significant differences between the two groups with respect to these outcomes. The groups are markedly similar on the frequency of depressive symptoms, their future orientation, and their level of stress. This pattern of results was confirmed in regression analysis presented in Table 4.18.

Health and Use of Tobacco and Alcohol

The Wave 4 survey instrument included questions about the health of respondents and use of tobacco and alcohol. The health outcomes include body mass index (BMI), self-assessment of health, and measures of

20. Where noted, total monthly household income, 2007 household income, and monthly income from work are winsorized at \$5,000, \$60,000, and \$5,000, respectively.

Table 4.19***Bivariate Analysis of Health and Substance Use Outcomes***

	N	Treatment	Control	Difference	p
		mean/prptn	mean/prptn	T-C	
Body mass index	798	30.52	30.34	0.18	0.75
Health relative to others your age (higher scores indicate poorer health)	798	1.71	1.69	0.02	0.80
Health is poor or fair relative to others my age (dichotomous)	798	0.22	0.19	0.03	0.30
Pain interferes with normal work (higher scores indicate more interference)	798	1.19	1.07	0.11	0.18
Pain interferes with work not at all (dichotomous)	798	0.39	0.40	-0.01	0.82
Pain interferes with work quite a bit or extremely (dichotomous)	798	0.17	0.13	0.04	0.14
Health limits moderate activities a lot	798	0.10	0.10	0.00	0.98
Health limits ability to climb stairs a lot	798	0.12	0.15	-0.03	0.27
Medical expenses in the past year (\$)	798	1,222	1,132	90	0.57
Medical expenses in the past year, winsorized	798	848	878	-30	0.69
Drinking behavior					
Drinks 2-3x per week or more	812	0.10	0.11	-0.01	0.48
Binge drinks monthly or more	812	0.07	0.09	-0.02	0.24
Alcohol screen score (range 0-12)	812	1.41	1.58	-0.17	0.25
Meets alcohol screen threshold for brief intervention	812	0.12	0.13	-0.01	0.67
Smoking behavior					
Smoked in the last 30 days	812	0.33	0.32	0.01	0.58
Number of cigarettes smoked in past week	812	22.33	23.08	-0.75	0.82

Table 4.20***Regression Analysis of Health and Substance Use Outcomes***

	N	b/OR	S.E.	p
Body mass index	798	0.00	0.54	0.99
Health relative to others your age (higher scores indicate poorer health)	798	0.02	0.07	0.78
Health is poor or fair relative to others my age (dichotomous)	798	1.19	0.23	0.36
Pain interferes with normal work (higher scores indicate more interference)	798	0.10	0.08	0.23
Pain interferes with work not at all (dichotomous)	798	0.98	0.15	0.77
Pain interferes with work quite a bit or extremely (dichotomous)	798	1.34	0.29	0.18
Health limits moderate activities a lot	798	0.90	0.24	0.68
Health limits ability to climb stairs a lot	798	0.78	0.18	0.29
Medical expenses in the past year (\$)	798	72.77	158.96	0.65
Medical expenses in the past year, winsorized	798	-25.25	72.66	0.73
Drinking behavior				
Drinks 2-3x per week or more	812	0.94	0.24	0.81
Binge drinks monthly or more	812	0.76	0.23	0.36
Alcohol screen score (range 0-12)	812	-0.12	0.14	0.42
Meets alcohol screen threshold for brief intervention	812	0.99	0.23	0.96
Smoking behavior				
Smoked in the last 30 days	812	1.17	0.19	0.35
Number of cigarettes smoked in past week	812	0.22	3.24	0.95

Table 4.21
Bivariate Analysis of Employment and Income Outcomes

	N	Treatment	Control	Difference	p
		mean/proportion	mean/proportion	T-C	
Total household income (monthly)	786	2,955	3,079	-124	0.49
Winsorized total household income (monthly)	786	2,586	2,678	-92	0.38
2007 household income (annual)	794	35,431	35,485	-53	0.98
Winsorized 2007 household income (annual)	794	30,690	31,659	-970	0.44
Income from work (monthly)	814	2,398	2,573	-175	0.31
Winsorized income from work (monthly)	814	2,144	2,251	-107	0.35
Proportion employed FT/PT/self	855	0.78	0.80	-0.01	0.70
Proportion with 2+ jobs	855	0.14	0.21	-0.06	0.02

Table 4.22
Regression Analysis of Employment and Income Outcomes

Treatment effect on...	N	b	S.E.	p
Total household income (monthly)	713	-180	176	0.31
Winsorized total household income (monthly)	713	-108	100	0.28
2007 household income (annual)	714	-905	2,123	0.67
Winsorized 2007 household income (annual)	714	-1,224	1,207	0.31
Income from work (monthly)	732	-262	164	0.11
Winsorized income from work (monthly)	732	-145	106	0.17
Proportion employed FT/PT/self	768	-0.09	0.20	0.65
Proportion with 2+ jobs	768	-0.06	0.21	0.01

limitations imposed by health. Substance use questions gauged the frequency of respondent's use of alcohol and tobacco products.

As shown in Table 4.19, among respondents in the treatment and control groups, the mean body mass was over 30, the cut-off for obesity used by the NIH. Still the majority of those in both groups consider themselves in good health generally and among those their age. Few respondents in either group report restrictions on activity from poor health. Fewer than 10% of respondents binge drink monthly and few meet the criteria for problem

drinking. About one in three respondents report having used tobacco in the past month. In bivariate analysis, we observe no statistically significant difference between the two groups on health and substance abuse measures.

In regression analysis, we observe no difference between the treatment and control groups on health and substance use outcomes. Similarly we find that those from the treatment group incur major medical expenses at the same level as those in the control group.

Table 4.23
Bivariate Analysis of Economic Hardship

	N	Treatment	Control	Difference	p
		mean/ prptn	mean/ prptn	T-C	
Difficulty Paying Bills in Past Year					
Rent or mortgage	812	0.31	0.29	0.03	0.43
Medical care	812	0.37	0.34	0.03	0.40
Dental care	812	0.44	0.38	0.06	0.09
Prescription medication	812	0.36	0.31	0.05	0.12
Difficulty paying any of the above	812	0.62	0.61	0.01	0.70
Count of types of bills had difficulty paying (of 4)	812	1.49	1.33	0.16	0.09
Change in financial situation since last interview					
Financial situation has worsened since last interview	812	0.32	0.26	0.06	0.08
Financial situation has improved since last interview	812	0.41	0.43	-0.02	0.57
Felt it was hard or very hard to make ends meet	812	0.59	0.57	0.02	0.57
Sometimes or often did not have enough to eat in past 4 months	812	0.12	0.11	0.01	0.63

Table 4.24
Regression Analysis of Economic Hardship

	N	b/OR	S.E.	p
Difficulty Paying Bills in Past Year				
Rent or mortgage	812	1.11	0.19	0.53
Medical care	812	1.08	0.17	0.62
Dental care	812	1.26	0.20	0.13
Prescription medication	812	1.26	0.20	0.16
Difficulty paying any of the above	812	1.01	0.16	0.93
Count of types of bills had difficulty paying (of 4), OLS	812	0.13	0.10	0.16
Count of types of bills had difficulty paying (of 4), Poisson	812	0.09	0.06	0.12
Change in financial situation since last interview				
Financial situation has worsened since last interview	812	1.23	0.20	0.22
Financial situation has improved since last interview	812	0.93	0.14	0.61
Felt it was hard or very hard to make ends meet	812	1.06	0.17	0.70
Sometimes or often did not have enough to eat in past 4 months	812	1.08	0.25	0.76

21. Where noted, amount owed on mortgages is winsorized at \$150,000.

Employment and Wages

At Wave 4, respondents were asked detailed questions about their current employment and earnings. As shown in Table 4.21, there is no significant difference between treatment and control group members at Wave 4 with respect to employment rate or earnings.²⁰ Treatment

group members are, however, less likely to be working multiple jobs at Wave 4. Both groups report, after adjusting for outliers, about \$2,600 per month in total pre-tax household income (in the survey month) from all sources. For the year prior to the survey, the treatment and control groups, on average, report a statistically identical yearly income of about \$31,000. Both the

Table 4.25
Bivariate Analysis of Loan Characteristics and Performance

	Full sample					Baseline renters				
	Treat	Cont	Diff	p	N	Treat	Cont	Diff	p	
	mean/ prptn	mean/ prptn	T-C		N	mean/ prptn	mean/ prptn	T-C		
Amount owed on mortgages	805	39,346	34,183	5,162	0.27	621	40,406	32,425	7,982	0.15
Winsorized amount owed on mortgages	805	33,719	30,766	2,953	0.37	621	33,756	29,247	4,509	0.24
Monthly mortgage payment	315	765	766	-1	0.98	243	798	764	33	0.54
Rate of primary mortgage	272	6.46	6.41	0.05	0.86	211	6.60	6.47	0.14	0.67
Have mortgage	839	0.44	0.44	0	0.99	642	0.40	0.38	0.02	0.69
Is primary mortgage fixed rate	317	0.90	0.91	-0.01	0.72	242	0.91	0.95	-0.04	0.25
Ever refinanced	367	0.30	0.26	0.04	0.40	251	0.27	0.18	0.09	0.11
Ever 30 days late	366	0.35	0.32	0.03	0.61	251	0.36	0.33	0.03	0.63
Ever 90 days late	365	0.14	0.07	0.07	0.04	249	0.15	0.07	0.07	0.06
Ever foreclosed upon	855	0.03	0.03	0	0.58	652	0.03	0.02	0	0.94

Table 4.26
Regression Analysis of Loan Characteristics and Performance

Treatment effect on...	Full sample				Baseline renters			
	N	b	S.E.	p	N	b	S.E.	p
Amount owed on mortgages	727	-387	4,328	0.93	556	1,842	5,164	0.72
Winsorized amount owed on mortgages	727	174	3,269	0.96	556	1,846	3,821	0.63
Monthly mortgage payment	279	-30	54	0.58	213	17	63	0.79
Rate of primary mortgage	240	-0.03	0.32	0.93	184	0.01	0.39	0.98
Have mortgage	755	0.01	0.16	0.97	573	-0.01	0.19	0.98
Is primary mortgage fixed rate	281	0.24	0.58	0.68	201	-0.38	0.81	0.64
Ever refinanced	326	0.16	0.29	0.59	220	0.48	0.46	0.30
Ever 30 days late	325	-0.17	0.29	0.56	220	-0.29	0.39	0.46
Ever 90 days late	307	0.58	0.46	0.21	213	0.97	0.67	0.15

treatment and control group accrue most of their income from work. In addition 14% of treatment group members report working multiple jobs as compared to 21% of control group members.

Regression analysis, shown in Table 4.22, confirms the descriptive results and finds that treatment assignment significantly reduces the odds of holding multiple jobs at Wave 4. Earnings and employment rate, however, were not statistically different between treatment and control members at Wave 4.

Material Hardship

At Wave 4, study participants were asked if they experienced a range of material hardship as well as their perception of their financial situation at the time of their interview. About 6 in 10 respondents in both groups reported being unable to pay at least one bill during the year prior to their Wave 4 interview (see Table 4.23). For both the treatment and control groups, more respondents reported being unable to pay a dental bill than any other. About 30% of respondents in each group reported having missed a rent or mortgage payment. Slightly more than 10% reported experiencing food insecurity in the 4 months prior to their interview.

Still, a plurality of both groups reported that their financial situation had improved since their last ADD interview. In bivariate and regression analyses, no differences between treatment and control group members were observed.

Loan Terms and Performance

Respondents who had mortgage debt at Wave 4 were asked about the characteristics of the mortgage(s) they held at the time of the interview. Below, we report findings on the characteristics of the loan with the largest value held by respondents.²¹ In addition, we report on loan performance characteristics including refinancing, 30-day delinquency, 90-day delinquency and foreclosure. About 44% of both the treatment and control group

owed money on a mortgage at Wave 4. As shown in Table 4.25, in bivariate analyses, there were no significant differences on presence of a mortgage or outstanding mortgage debt between the treatment and control groups. The terms of the primary loans held by members of each group were not statistically different. About 90% of each group had a fixed rate loan and the average interest rate for both groups was about 6.4%.

While there was no difference in the proportion of each group who had ever been 30 days late on mortgage payments, in bivariate analysis, those in the treatment group were significantly more likely to have experienced 90-day delinquency than members of the control group. Because many of the primary loans held by baseline owners were originated before the start of the CAPTC IDA program, we examine the loans held by baseline renters separately. From bivariate analysis of baseline renters, we note that those in the treatment group are more likely to have been 90-days delinquent.

The patterns of association between loan characteristics and performance and treatment assignment seen in bivariate analysis are also seen when the data are examined using regression techniques (see Table 4.26). We find no difference between the treatment and control groups on loan characteristics and loan performance among the full sample or among baseline renters.



Chapter 5

Conclusion

The ADD4 study provides the first empirical evidence from a randomized, longitudinal experiment on the long-term impacts of a short-term IDA program on economic, psychological, and health outcomes among low-income families. The fourth wave of data for the ADD experiment was collected from treatment and control group members about 6 years after program completion and 10 years after random assignment. This follow-up provides policy makers and practitioners the opportunity to examine impacts of an IDA program on asset building, years after the savings program has ended.

Below we present a summary of results on the five key allowable uses of IDAs: homeownership, home maintenance and repair, post-secondary education, business and retirement savings. We also summarize the results for net worth.

Homeownership

The treatment and control groups both experienced substantial and ongoing increases in homeownership rates over the 10-year study period (1998 to 2008). The rates of increase in homeownership for the ADD4 sample are high compared with the homeownership rate for the nationally representative PSID survey sample (Grinstein-Weiss, Sherraden, Gale, Rohe, Schreiner, & Key, 2011). The increased homeownership rate is especially notable given the broader economic crisis gripping the nation in the later years of the study period.

Participation in the Tulsa IDA program, however, did not result in a significantly higher homeownership rate 10 years after the program began. Earlier findings (Grinstein-Weiss et al., 2008; Mills, Gale, et al., 2008) showed a statistically significant programmatic effect on homeownership rates among baseline renters as of 2003. The longer-term findings show that assignment to the IDA program may have accelerated the onset of homeownership for treatment group households, but in the long run, it did not result in a homeownership rate statistically different from the control group. The gap in the homeownership rate between the treatment and control groups narrowed rapidly after the program

ended in 2003. Thus, the IDA program did not result in a significant increase in the homeownership rate 10 years after it began, nor did it increase the duration of homeownership during that time.

For the subgroup of people with above-sample median annual incomes at baseline (about \$15,500 per year), assignment to the treatment group significantly increased the homeownership rate and duration of homeownership. This may indicate that while IDA programs are not effective in promoting homeownership among very-low-income households, they may be effective for households with higher, although still modest, levels of income. It should be noted, however, that subgroup analysis was conducted on 11 dependent variables and only income was significant. Thus, it is possible that this finding is the result of chance. If the income and homeownership result is not due to chance, then it may be that IDA programs should target those participants with somewhat higher incomes for homeownership, which is a major financial and practical undertaking, and steer very-low-income participants toward other assets such as education, which may be less of a financial challenge.

In addition, given the economic climate and changes that occurred in the housing market during the study period, including the expansion of sub-prime lending, it is important to note that the vast majority of treatment and control group members with housing financing received fixed-rate mortgages, with relatively low interest rates.

The lack of statistically significant effect of the IDA program on the full sample of program participants might be due to several factors. First, housing prices in the Tulsa area were relatively affordable during the study period. The median home value in 2001 was about \$111,000, well below the national median. Thus, buying a home in Tulsa was relatively easy compared to many housing markets, making the IDA program less important in buying a home. Second, other Tulsa area homebuyer assistance programs were available for control group members. At least one of those programs provided down-payment

assistance and homeownership counseling without the savings requirement. Also, as presented above, both groups appear to have received good quality loans.

IDA programs may be more effective in assisting low-income households purchasing homes in higher cost housing markets and/or in markets where there are fewer alternative sources of mortgage assistance and homeownership training.

Home Maintenance and Repair

Over two-thirds of homeowners, both treatment and control group members, reported making home repairs over \$500 during the 10-year study period. For the full sample, there was no impact of the treatment on home repairs, on the dollar amount spent on repairs, or on housing price appreciation. However, treatment group members did report that the estimated cost of unmade repairs was significantly lower compared with the control group members.

Moreover, among baseline homeowners, we find that two of the five measured effects of the IDA on home maintenance and repair yielded significant and economically meaningful results. Treatment group homeowners were less likely to report skipping needed home repairs and had a higher rate of housing price appreciation during the program period than control group homeowners. These findings suggest that being assigned to the IDA intervention may have helped those who owned their homes at the start of the IDA program to maintain and improve their homes over the 10-year study period, and to experience a greater increase in housing price appreciation.

The fact that IDA program participants as a whole spent the same amount as control group members on home repairs and yet reported lower costs of forgone repairs suggests that, compared to the control group, they either purchased homes that were in better condition or they achieved more repairs for the same cost by doing home repairs themselves. Both of these possible explanations

could be the result of the homeownership counseling and training courses required of program participants intending to use their matched savings to buy homes.

Education

Among treatment group members present at Wave 4, a small percentage (8.3%) planned to save for education expenses. Among all the matched withdrawals made, 6.9% were put toward education uses. Despite such a small group saving for education, we find a significant impact on education enrollment 10 years after baseline assignment (6 years after program completion). The enrollment results are similar to results from *learn\$ave*, a randomized IDA experiment in Canada (N=3,584), which finds a significant treatment effect on enrollment in community college and university programs six months after the program ended (Leckie et al., 2010). In ADD4, we also find a significant impact on the likelihood of gaining “some college” education among treatment group members compared to the control group.

In addition, the data show positive, but non-significant, effects on degree completion and increase in level of education. There are several possible explanations as to why we do not find a significant impact on these outcomes. First, while IDAs can provide some resources, such as financial capital and information, there are many additional barriers faced by non-traditional students that IDAs are not designed to address (Taniguchi & Kaufman, 2005). Second, effects on educational attainment may take longer to develop than the 6-year time frame between the program end and this study. This may be especially true for non-traditional students who enroll on a part-time basis.

In subgroup analysis, evidence suggests that men may benefit more from the IDA program in terms of educational enrollment and attainment compared with women. Interestingly, the administrative data (MIS IDA) also indicates that males were more likely than females to take a matched withdrawal for education. This is an important finding, given that there is a disturbing trend of declining educational attainment among minority and

lower-income males in the United States (King, 2000; Kim, 2011). Our current data cannot illuminate the channels through which IDAs may have this effect, and this is an important question for future research.

Business Ownership

Among treatment group members present at Wave 4, only 5.7% had planned at baseline to save for business ownership. About half of those saving for business ownership actually made a matched withdrawal. For the full sample, the proportion of business ownership substantially increased between baseline and Wave 4 (7% to 13% of respondents). However, there was no significant effect of treatment on business ownership or equity at Wave 4.

Given the small sample size of people who were saving for business, it may not be surprising that we could not detect an impact. Perhaps a better test of a matched savings program on business ownership would be a randomized control trial on a program that targets matched saving and financial counseling only for microenterprise. Such a program could use a design similar to the *learn\$ave* program, but with a greater focus on business, rather than education.

Retirement

Among treatment group members present at Wave 4, about 19% of the sample had planned to save for retirement. Participants saving for retirement were among the most likely to make a matched withdrawal. Among all the matched withdrawals made, 16.8% were made for retirement savings. However, we observe no statistically significant differences between treatment and control with regard to retirement savings outcomes.

Net Worth

Our findings indicate that there is no detectable treatment effect of the IDA program on wealth 10 years after the program began. This may be partly due to the nature of the data. To provide some context, a difference in net worth of less than \$10,500 probably would not show statistical significance due to the small sample size.

Looking at the effects on the components of wealth, the results are mixed. After adjusting for outliers, there is a substantial but not significant increase in total assets and total debts with assignment to treatment. There is a marginally significant but economically small effect of treatment on liquid assets: assignment to treatment is associated with \$79 more in liquid assets relative to assignment to control.

Despite the mixed results on wealth, the study participants in both groups are doing better relative to national patterns of wealth for low-income households. According to recent research from the Pew Charitable Trusts, lower-income and minority households in the U.S. experienced major declines in wealth in the past 10 years (Kochhar et al., 2011). This loss in wealth is not observed among this sample, suggesting that the participants – both treatment and control group members – were able to maintain their financial wealth better than other low-income families across the country.

Concluding Thoughts

In summary, out of the five allowable uses, we find some long-term impact of IDAs on education, especially for males, and on home maintenance and repair. We do not find a long-term impact on homeownership, businesses, and retirement savings. The positive findings for education and home maintenance and repair may suggest that IDAs are best suited to support asset purchases that can be accomplished incrementally over a period of time. Targeting IDAs for education and home maintenance and repair may be more effective than applying them to “all-or-nothing” purchases like a house. Similarly, these findings may imply that longer savings periods would be beneficial.

There are several possible explanations for the lack of more substantial effects on wealth and assets found 6 years after the experiment ended. First, ADD4 participants were self-selected into the study. The applicants had to take the time and effort to apply for the IDA program; thus, they were more motivated than other potentially eligible persons. That higher level of motivation may have

led members of the control group to find other ways to reach their goals, including participation in other programs. If this is the case, a larger IDA program that includes a less motivated population or is implemented in a location with fewer alternative resources may show different results. Second, our sample size may be too small, and therefore the power too weak to detect an impact. Third, the structure of the Tulsa IDA program, which allowed for five different qualified uses of the matched funds, could make effects even harder to detect. Fourth, noise and errors inherent in income, asset, and liabilities measures make it challenging to study and document changes in wealth. Fifth, in spite of random assignment, some baseline differences were observed between treatment and control group members. In addition to these observed characteristics that were controlled in the analysis, unobserved differences between the groups could still exist and, if present, could affect the impact of IDAs on the observed outcomes.

Finally, long-term efficacy of impacts is a lot to expect from a short-term matched savings program. It is not uncommon to find that impacts of social and economic interventions deteriorate over time, after the treatment group no longer enjoys special conditions compared to the control group. Further, it raises a broader question of whether a short-term program that provides modest benefits to program participants can outweigh the many other factors that influence ones' social and economic outcomes. At the outset of the experiment, there was little way to know the appropriate design or "dose" of IDAs—in program structure, saving incentives, or saving duration. Program benefits may have to be greater or the programs may need to have longer savings periods to result in effects on wealth and assets 6 years after participation ends.

Future Research

The ADD4 research has provided important insights into the long-term effect of short-term IDAs on economic, psychological, and health outcomes among low-income families. The mixed effects of the treatment on program participants indicate a need for additional research on

IDA programs in particular and asset-building efforts in general.

The Tulsa IDA program in this experiment was among the first IDA programs in the country when it started in 1998. At the outset, there was little way to know the appropriate design for an IDA, including program structure, saving incentive, and saving duration. Based on field experience in the intervening years, many current IDA programs are structured differently in terms of match rates, maximum available matches, duration, qualified uses of the matching funds, and so on.

Specifically, most of the IDA programs today, funded through the federal AFI program, offer a saving period of up to 5 years (U.S. Department of Health and Human Services 2010). Therefore, the upcoming evaluation of AFI - funded IDA programs, mandated by AFI's authorizing legislation, should provide new and important evidence on the impact of IDAs. Evaluating the effects of several contemporary AFI-funded IDA programs will help to address some of the challenges in generalizing findings from the Tulsa IDA program to other settings and program designs. It is reasonable to expect that different agencies, regions, and time periods will produce IDA programs with different impacts on participants.

Moreover, regarding program duration, we still lack knowledge of the effects of a long-term or indefinite IDA savings program, structured as a 401(k), for example, without a predetermined savings period. The original proposal for IDAs was for lifelong, progressive accounts (Sherraden, 1991). However, IDAs have been implemented in a demonstration period with short-term savings periods. It seems likely that longer-term saving could be more effective for asset accumulation and that short-term savings periods may be too limited to make a lasting difference. Future research on the question of what might happen with long-term (or life-long) matched savings programs would be valuable to inform economic policy.

Also, because IDAs are made up of a bundle of services, it would be valuable for both policy and research reasons to understand the channels through which IDAs may affect

behavior and well-being. For example, experimental evidence from the Canadian *learn\$ave* program indicates that financial education and case management had a significant impact on saving and education outcomes (Leckie, et al. 2010). Learning more about the mechanisms through which participation in IDAs can lead to positive outcomes will provide an evidence base to better structure matched savings policies and programs for maximum efficacy.

With regard to increasing our understanding of asset building in general, an important follow-up question from ADD4 is how and why participants in the Tulsa IDA experiment—treatment and control group members alike—increased their homeownership rates by more than a random sample of low-income households (as evidenced by the comparison with respondents from the PSID) and had low levels of mortgage delinquency and foreclosure. This is particularly important given that the study period included a time during which the economy in general and housing markets in particular experienced great turbulence.

To date, ADD-based research has made foundational contributions to the field of asset-building and has been instrumental in the development of new policies and programs to promote economic and social mobility among low-income families, including matched savings accounts for adults and children, both in the U.S. and internationally. This 10-year follow-up study is one more contribution to our understanding on the impact of these programs. Future research should build on this work and provide additional evidence to inform the development of future savings and asset-building programs for low-income families.



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Appendix

American Dream Demonstration Wave 4 Advisory Board*

Dalton Conley – Director of the Center for Advanced Social Science Research, Professor of Sociology and Public Policy at NYU; Adjunct Professor of Community Medicine at Mount Sinai School of Medicine, New York

Marion Crain – Deputy Director of the Center on Poverty, Work & Opportunity and the Paul Eaton Professor of Law at UNC Chapel Hill School of Law

Steven Dow – Executive Director, Community Action Project of Tulsa County

Greg Duncan - Edwina S. Tarry Professor, School of Education and Social Policy Faculty Fellow, Institute for Policy Research, Northwestern University Director, Northwestern University of Chicago Joint Center for Poverty Research

Bob Friedman - General Counsel, Founder and Chair of the Board of Directors for the Corporation for Enterprise Development

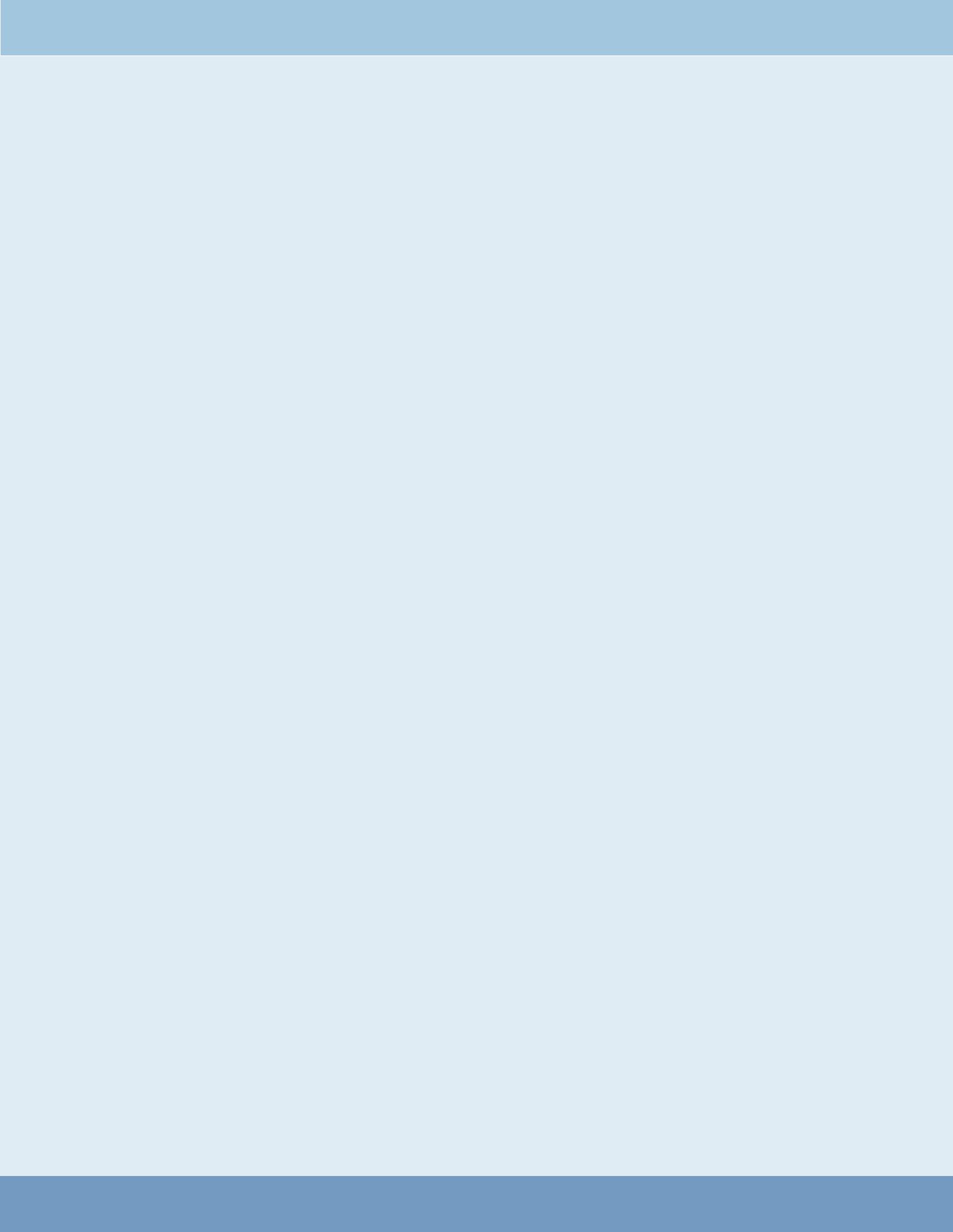
Greg Mills – Principal Associate, Abt Associates

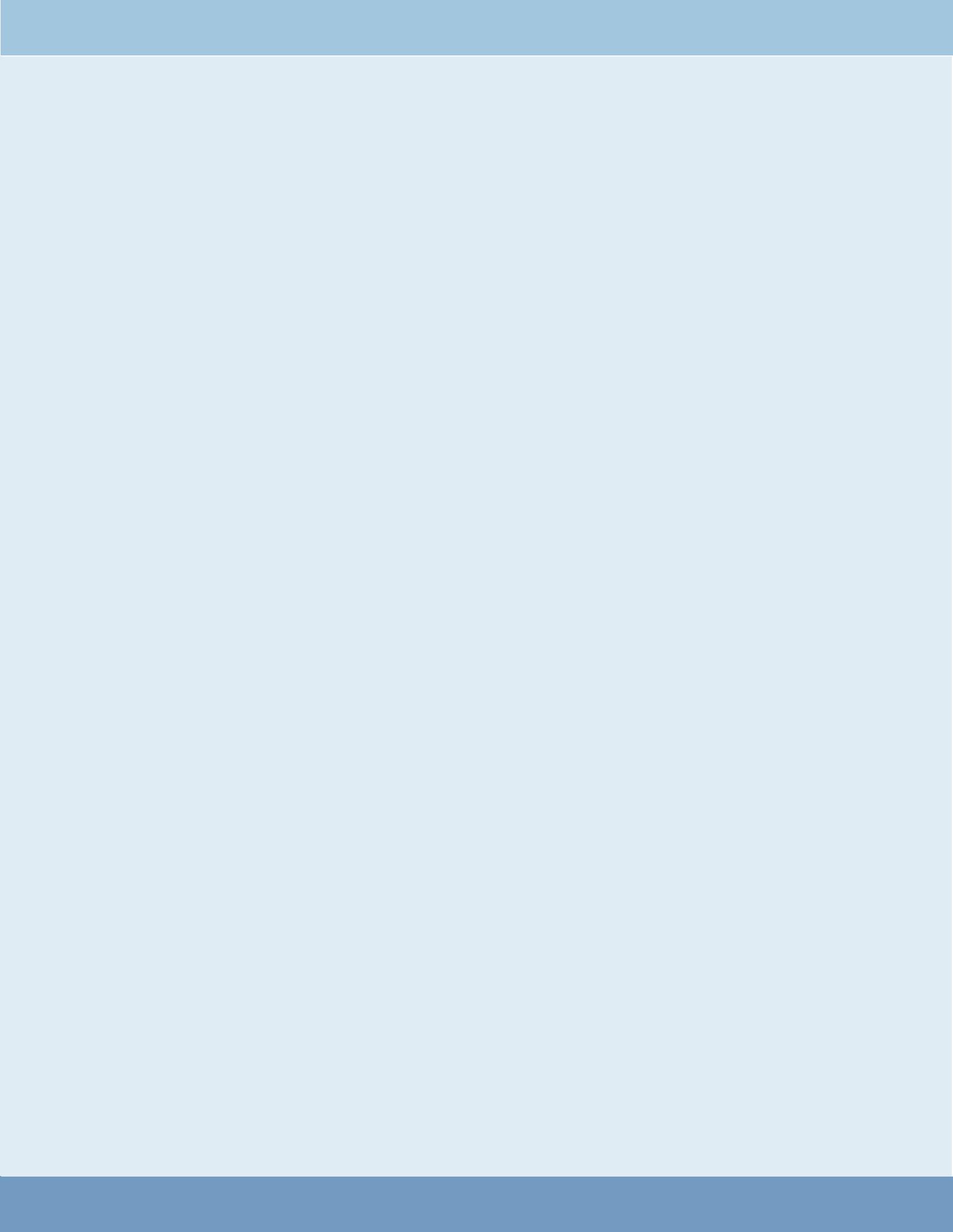
Melvin Oliver - Professor and Dean of Social Sciences, University of Santa Barbara

Robert Plotnick – Professor of Public Affairs at Daniel J. Evans School of Public Affairs, Adjunct Professor of Economics, University of Washington

*Please note: Advisory Board members are listed with the affiliations they held at the beginning of the ADD4 study; these are not their current affiliations in all cases.







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