



Building Savings for Success

Early Impacts from the Assets for Independence Program Randomized Evaluation

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Overview

Individual development accounts (IDAs) help low-income families save by matching their personal savings for specific investments, such as a first home, business capitalization, or higher education and training. The Assets for Independence (AFI) program is a federally supported IDA grant program authorized under the Assets for Independence Act of 1998. Our evaluation at two sites—Albuquerque and Los Angeles—shows that AFI is increasing low-income participants' savings one year into the program.

This is the first evaluation of the AFI program to use a randomized controlled trial, the gold standard for measuring program effectiveness. We assess the program's early (first-year) effects on participants' savings, asset ownership, and economic well-being. Results show two beneficial primary effects:

- A 7 percentage point (9 percent) increase in the share of participants with liquid assets.
- A \$657 median increase and \$799 mean increase in liquid assets. Because we look at all liquid assets—including savings, checking, money market, and retirement accounts plus stocks and bonds—our results indicate that participants are not simply shifting savings from other types of accounts into their IDAs, but instead are creating new savings.

We also find evidence that AFI affects several secondary outcomes:

- A 34 percent reduction in hardships related to utilities, housing, or health, equivalent to one less hardship experienced.
- A 39 percent (4 percentage point) decline in the use of alternative (nonbank) check-cashing services, suggesting that AFI participation helps people enter the financial mainstream.
- A 10 percent increase in participants' confidence in their ability to meet normal monthly living expenses.

These major first-year impact findings—that AFI participation results in more savings, less material hardship, and improved perceptions of one's financial situation—provide empirical evidence that AFI promotes economic well-being.

While the vast majority of federal asset-building subsidies (such as the mortgage interest deduction) disproportionately benefit high-income earners, the AFI program is one of few federal efforts that actively encourages saving among low-income families. By encouraging low-income families to save, AFI can improve their short-term stability while providing a foundation for longer-term upward mobility.

Executive Summary

This report presents early (first-year) findings from a randomized evaluation of the Assets for Independence (AFI) program, a federally supported individual development account (IDA) grant program authorized under the Assets for Independence Act of 1998. IDAs are savings accounts that match personal deposits when used for specific assets. Under AFI, allowable assets are a first-time home purchase, a business start-up or expansion, and postsecondary education or training. AFI uses IDAs—commonly coupled with financial education—to help low-income households achieve greater self-sufficiency. The AFI program is administered by the Office of Community Services, within the Administration for Children and Families at the US Department of Health and Human Services.

This study addresses the following research question through an experimental design:

- What is the early impact of the AFI program on participant outcomes such as savings, asset ownership, and material hardship?

We randomly assigned study participants at two AFI project sites to a treatment group and a control group.¹ This experimental design allows us to attribute the differences in outcomes between the two groups to the AFI program. This study is the first to evaluate the AFI program using a randomized design.

Evaluation Design

The two participating AFI evaluation sites are located in Albuquerque and Los Angeles. The Albuquerque site operates at Central New Mexico Community College (CNM), which serves AFI participants in a student center that offers academic and financial coaching and connects students to other college and community resources. CNM is a partner of Prosperity Works, an Albuquerque-based AFI grantee. The Los Angeles site is RISE Financial Pathways, a nonprofit community-based organization in South Central Los Angeles focused on local economic development.

Both sites met the selection criteria to participate in the study. Both had sufficient grant capacity to reach the necessary project scale for the evaluation, familiarity with field research through prior studies, experience operating an AFI-funded project, and project features within the range of variation among AFI grantees nationwide. Under the project design at both sites, AFI participants could receive match funds on savings up to \$1,000. In Albuquerque, participants could receive a \$4 match for every \$1 saved. In Los

¹ AFI project refers to IDA programs operated by AFI grantees, funded by the federal AFI program.

Angeles, participants could receive \$2.50 for every \$1 saved. Both sites had a minimum savings period of 6 months, and the Los Angeles site had a maximum of 24 months.

Between January 2013 and July 2014, 807 people enrolled in the study (299 in Albuquerque and 508 in Los Angeles). Each site first determined applicant eligibility. Applicants then provided their consent to participate in the study, completed the baseline survey, and underwent random assignment (407 to the treatment group and 400 to the control group).

To answer the study's research question, the research team assembled data from multiple sources: the baseline survey completed at study enrollment, on-site qualitative interviews conducted in October and November 2014, site-provided project data on services received by treatment group members during their first 12 months of AFI participation, and a follow-up survey conducted roughly 12 months after study enrollment (between April 2014 and September 2015). This follow-up survey achieved a response rate of 78 percent, yielding an analysis sample of 628 people.

Services Provided to Treatment Group Members

Both AFI sites successfully launched the evaluation in 2013, delivering initial services to treatment group members with fidelity to site-specific project designs and to the randomized evaluation design. The Albuquerque site achieved higher participation rates in AFI services than the Los Angeles site during the first year after study enrollment.

- **Most participants opened accounts.** AFI project data show that 91 percent of treatment group members in Albuquerque and 71 percent in Los Angeles opened an IDA and made at least one deposit. In Albuquerque, all accounts were opened within six months of study enrollment, compared with 89 percent in Los Angeles.
- **Most participants completed financial education.** In Albuquerque, 83 percent of treatment group members participated in the required financial education within their first year. In Los Angeles, 87 percent participated in required financial education courses. These shares could rise over time; participants with longer savings periods may not have prioritized completing financial education within one year after enrollment.
- **More matched withdrawals in Albuquerque, few unmatched withdrawals at both sites.** The share of treatment group account holders who made matched withdrawals during their first year enrolled in the study was 43 percent in Albuquerque and 12 percent in Los Angeles. The share who made

unmatched withdrawals in the first year was 5 percent in Albuquerque and 2 percent in Los Angeles.

Estimated Early Program Impacts on Participants

We use the experimental design to estimate early AFI program impacts. We focus on regression-adjusted impacts, which control for measurable differences between the treatment and control groups at study enrollment. For each outcome, the estimated impact is the regression-adjusted difference between the first-year outcome for the treatment group and the corresponding outcome for the control group, shown in the five figures below. By basing the analysis on data collected from two randomly assigned groups, we estimate the causal effect of AFI services.

Primary Outcome: AFI Increased Savings, Measured by Liquid Assets

The study's primary first-year finding is that AFI participation increased household savings, as measured by liquid assets held by the participant and his or her spouse or partner. This measure includes all liquid assets, not just a participant's IDA balance. By capturing all liquid assets, this finding indicates that AFI participants are creating new savings; they are not simply shifting liquid assets from other types of accounts (e.g., savings accounts) into an IDA. This finding provides strong evidence that AFI achieves its primary goal of increasing savings.

- **AFI participation led to a 7 percentage point (9 percent) increase in the share of participants with liquid assets at the first-year follow-up** (figure ES.1). Liquid assets include amounts held in savings accounts (including the IDA), checking accounts, money market accounts, stocks, bonds, and retirement accounts.
- **The increase in liquid assets at the first-year follow-up is substantial.** AFI increased participants' liquid assets by an average of \$799 (about \$67 per month), or by \$657 (about \$55 per month) for a person with median liquid assets at the first-year follow-up.

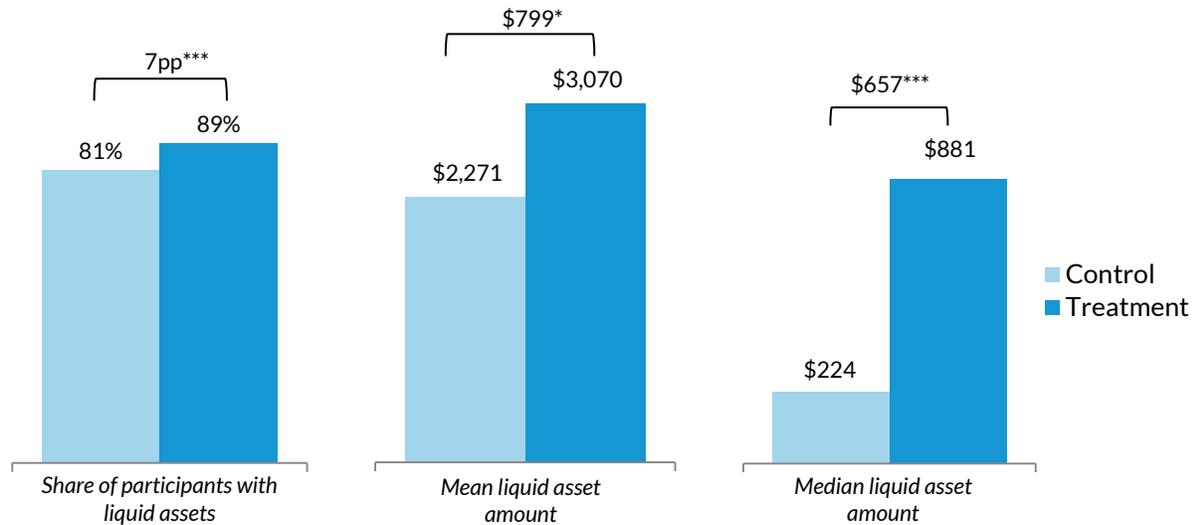
These findings suggest that AFI connects participants to the financial mainstream, enabling them to accumulate savings when they would not otherwise. Participants appear to attain their higher dollar holdings in liquid assets through a change in saving behavior, not simply through shifting existing liquid assets into their IDAs. Within the first year of participation, the combination of financial incentives and financial education increases savings, which is the first step toward using match funds for investment in allowable asset purchases.

The first-year evidence does not indicate an increase in asset ownership related to a home, a business, or postsecondary education and training. We did not expect such program effects to emerge within the first year, when participants have typically not yet completed their savings periods.

FIGURE ES.1

AFI Increases Savings

The impact of AFI on savings, as measured by liquid assets



Source: AFI first-year follow-up and baseline surveys.

Notes: pp = percentage point. Liquid assets are measured at the first-year follow-up survey (roughly 12 months after study enrollment). We present regression-adjusted impact estimates, means, and medians. The difference between the means may not equal the impact estimate due to rounding. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 622 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

* $p < 0.10$ *** $p < 0.01$

Secondary Outcomes

Secondary outcomes of AFI participation include reduced material hardship, decreased use of nonbank check-cashing services, and improved perception of financial security.

REDUCTIONS IN MATERIAL HARDSHIP

These first-year findings indicate that AFI reduces material hardship for three of nine hardship measures, which capture hardship in the 6 to 12 months after study enrollment.

- AFI participation led to a 34 percent (one hardship) reduction in the number of total hardships experienced (i.e., the number of times participants could not pay for housing, utilities, or needed

medical care) and a 38 percent (0.4 hardships) reduction in the number of utility hardships (i.e., the number of times participants were unable to pay bills or had services shut off; figure ES.2).

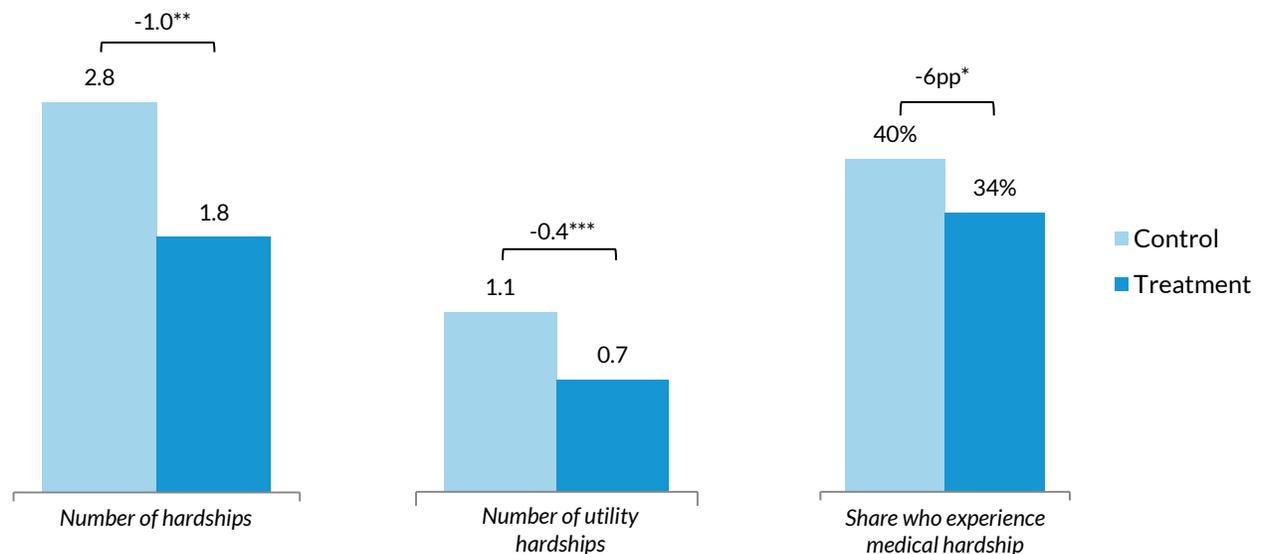
- AFI participation led to a 6 percentage point (16 percent) decline in the share of participants experiencing a medical hardship, measured by the inability to afford a doctor, a dentist, or a prescription.

Our finding of reduced hardship suggests that for AFI participants, increased saving does not need to entail greater hardship.

FIGURE ES.2

AFI Reduces Material Hardship

The impact of AFI on material hardship



Source: AFI first-year follow-up and baseline surveys.

Notes: pp = percentage point. Material hardship is measured in the 6 months before the first-year follow-up survey (roughly 6 to 12 months after study enrollment). We present regression-adjusted impact estimates and means. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 622 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

* $p < 0.10$ ** $p < 0.05$

We initially hypothesized that the AFI program would reduce material hardship through unmatched hardship withdrawals—account holders making emergency withdrawals from IDA savings instead of achieving their asset purchase goals. Given the low incidence of unmatched withdrawals at both sites, we consider other explanations for these effects. AFI participants may have set aside non-IDA savings and later used a portion of these savings to avert hardship. Another possibility is that AFI participants supplemented family resources with means-tested program benefits.

MAINTAINANCE OF MEANS-TESTED BENEFIT RECEIPT

Evidence indicates that AFI helps participants maintain means-tested benefits—such as cash assistance (e.g., Temporary Assistance for Needy Families), Supplemental Nutrition Assistance Program and other food-related assistance, housing assistance, energy assistance, Medicaid, or a child care subsidy.

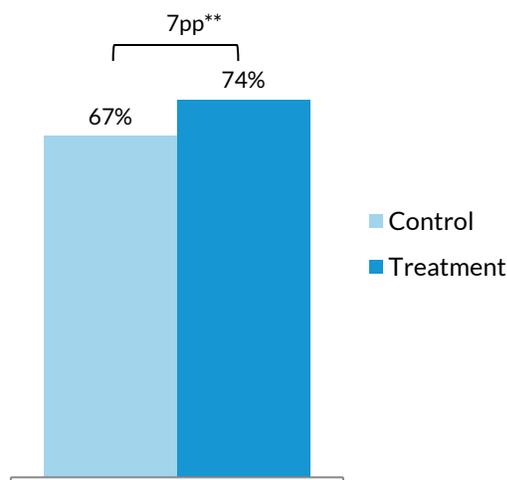
- Across the two sites, the share of study participants who received at least one form of means-tested benefits in the month before completing the first-year follow-up survey was 7 percentage points (10 percent) higher among AFI participants versus nonparticipants (figure ES.3).

In separate analyses by site, only the Los Angeles site exhibited a statistically significant higher likelihood of benefit receipt. Interestingly, a descriptive analysis of the Los Angeles baseline and one-year follow-up survey data suggests that the AFI project is helping keep AFI participants connected to benefits while they work toward their saving goals, not increasing benefit receipt. Benefit receipt among AFI participants was roughly flat over time (i.e., between the baseline and follow-up surveys), while benefit receipt fell for nonparticipants. AFI staff may help recipients cope with the procedural requirements for retaining their benefits, for example. Viewing this result in conjunction with our evidence that AFI reduces material hardship suggests that these benefits may help AFI participants avoid hardship as they work to save for their long-term investments.

FIGURE ES.3

AFI Helps Maintain Means-Tested Benefit Receipt

The impact of AFI on means-tested benefit receipt



Source: AFI first-year follow-up and baseline surveys.

Notes: pp = percentage point. Means-tested benefit receipt is measured in the month before the first-year follow-up survey (roughly 11 months after study enrollment). We present regression-adjusted impact estimates and means. The maximum sample consists of 622 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

** $p < .05$

Additionally, an element of the AFI legislation may have allowed AFI participants to maintain benefit eligibility and, thus, continued receipt. Specifically, the AFI legislation ensures that IDA savings do not reduce means-tested eligibility or benefits.

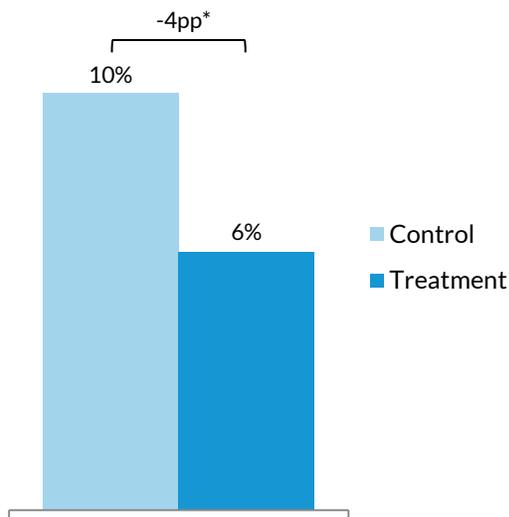
REDUCED NONBANK CHECK-CASHING USE

The first-year results provide evidence that AFI participation reduces the use of alternative (nonbank) check-cashing services, possibly because the IDA connects participants with a bank. AFI led to a 4 percentage point (39 percent) reduction in the share of participants who used nonbank check-cashing services. Six percent of treatment group members used nonbank check-cashing services in the year after study enrollment, compared with 10 percent of control group members (figure ES.4). AFI also reduced the frequency that people used nonbank check-cashing services by an average of 0.1 points (the scale ranges from 1 to 5). Although a modest decline in absolute terms, this represents a 41 percent decline compared with the control group.

FIGURE ES.4

AFI Reduces the Use of Nonbank Check-Cashing Services

The impact of AFI on the use of nonbank check-cashing services



Source: AFI first-year follow-up and baseline surveys.

Notes: pp = percentage point. Use of nonbank check-cashing services is measured in the 12 months before the first-year follow-up survey (roughly the year after study enrollment). We present regression-adjusted impact estimates and means. The maximum sample consists of 622 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

* $p < 0.10$

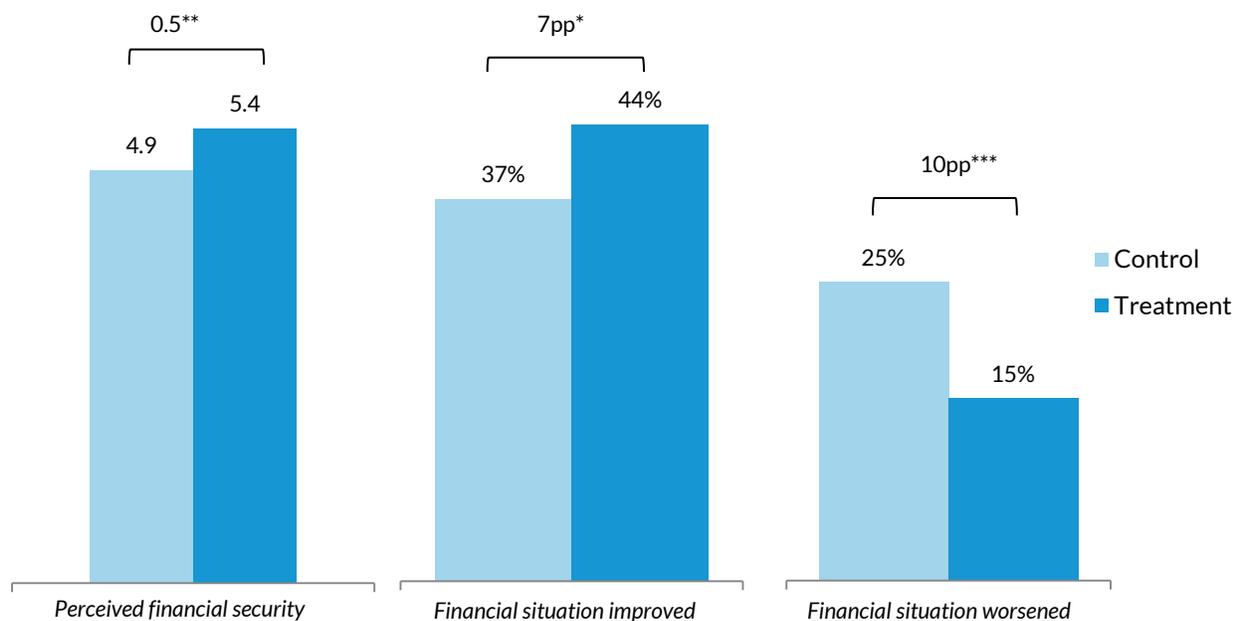
IMPROVED PERCEIVED FINANCIAL SECURITY

The results also provide evidence that AFI improves participants' sense of personal financial security (figure ES.5).

- AFI participants are more confident than nonparticipants that they can meet their monthly living expenses (0.5 points on a scale from 1 to 10, 10 percent).
- AFI participants are more likely than nonparticipants to report their financial situation has improved (by 7 percentage points, 19 percent) and less likely to say it has worsened (by 10 percentage points, 38 percent).

FIGURE ES.5

AFI Improves Perceived Financial Security *The impact of AFI on perceived financial security*



Source: AFI first-year follow-up and baseline surveys.

Notes: pp = percentage point. Perceived financial security is measured at the first-year follow-up survey (roughly 12 months after study enrollment). We present regression-adjusted impact estimates and means. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 622 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

The improved financial security findings, combined with the evidence on increased liquid assets and reduced material hardship, suggests that AFI participants can save and still meet their basic living needs without risking hardship or causing undue financial strain. Reduced financial worry is important in the

context of behavioral-economics research on scarcity. Financial stress and worry tend to sap cognitive resources when people can least afford to make poor choices. Reduced worry may also enhance parenting and benefit children.

The first-year impacts show no significant program effects on the following secondary outcomes hypothesized: use of alternative financial service credit products (e.g., payday loans), employment status, health status, perceived ability to make ends meet, self-esteem, and present-oriented time preference. We also find no significant effect on credit scores, estimated from data available for Albuquerque.

Emerging Insights and Potential Implications

The major first-year impact findings—that AFI participation resulted in higher savings, reduced material hardship, higher benefit receipt, and improved perceptions of one’s financial situation—provide strong empirical support that AFI promotes the economic well-being of participants. Additionally, the study’s finding that AFI participation reduced the use of nonbank check-cashing services suggests that AFI may help people enter the financial mainstream. These findings suggest three emerging insights and potential implications:

- **IDAs allow low-income people to save without reducing benefits.** Disregarding savings in IDAs when determining means-tested program eligibility and benefits may allow low-income families to save and avoid hardship. The AFI legislation stipulates that there be no reduction in benefits from saving in an AFI IDA (see SEC. 415. No Reduction in Benefits).² This is consistent with our evaluation’s finding that low-income AFI participants saved, did not lose benefits, and avoided hardship.
- **The AFI program can help integrate saving and financial education into existing programs.** As an ACF-administered program, AFI should be considered in the context of ACF’s strategic goals, not solely in terms of asset building. AFI grantees nationwide commonly integrate their AFI projects into other programmatic activities that serve low-income households by providing benefits, social services, loans, and other assistance. AFI participation and the savings it encourages may be a mechanism for meeting other ACF goals, such as promoting health, economic, and social well-being. Keeping eligible low-income people connected to benefits and reducing hardship can help ACF meet those goals.

² See section 415 in Community Opportunities, Accountability, and Training and Educational Services Act of 1998, 42 USC 604 (1998), <http://www.acf.hhs.gov/ocs/resource/afi-legislation-0#SEC415NOREDUCTIONINBENEFITS>.

- **Matched savings programs such as IDAs provide a vehicle for public policies to encourage low-income families to save for their short-term stability as a foundation for longer-term upward mobility.** The AFI program is one of the few federal efforts that encourage low-income people to save. Most federal asset-building subsidies, such as the home mortgage interest deduction, disproportionately benefit high-income families who are more likely to shift savings in response to incentives rather than create new savings (Steuerle et al. 2014). In addition, asset limits for benefit programs create a disincentive to save (Ratcliffe et al. 2016). Michael Sherraden’s original proposal for IDAs in 1991 was for universal, progressive, lifelong accounts (Sherraden 1991). The AFI program provides a small dose of this vision and provides insights relevant to other public policy efforts to improve the financial health, security, and well-being of low-income earners and their households.

Chapter 1: Introduction and Background

To increase economic self-sufficiency and stability, the United States and other countries have experimented with expanding asset-building policies and programs to low-income families. Much of this expansion has taken the form of matched savings accounts, which provide families a financial incentive to save. Individual development accounts (IDAs), first proposed in 1991, were among the first of these accounts (Sherraden 1991). IDAs are special-purpose, matched savings accounts for low-income households that match personal deposits when used for specific investments such as a home purchase, a business, or postsecondary education. Since then, the field has developed and tested other matched savings programs, including children's savings accounts and financial matches at tax time (e.g., SaveUSA).

The Assets for Independence (AFI) program, a demonstration program authorized by the Assets for Independence Act (1998), is the largest funding source for IDAs in the United States. Yet, the \$19 million program amounts to less than one-hundredth of 1 percent of the \$384 billion in federal asset-building subsidies made through the tax code (Steuerle et al. 2014). The AFI program is one of the few federal subsidies that provides incentives for saving for low-income families. Over 70 percent of other federal asset-building subsidies, such as mortgage interest deductions and retirement savings incentives, go to high-income tax filers.

AFI is a discretionary grant program administered by the Office of Community Services (OCS), within the Administration for Children and Families (ACF) at the US Department of Health and Human Services (HHS). In AFI-funded IDA projects, participants save toward an allowable asset purchase—a first-time home purchase, business capitalization, or postsecondary education or training. Once participants reach their savings goals, they use their savings plus a savings match provided by the AFI project to purchase an asset. Half the match is funded by the federal grant, and half must be raised by the grantee from nonfederal sources. Besides matching funds, AFI projects must help participants obtain skills and information for economic self-sufficiency through asset purchases. Assets held in AFI accounts are disregarded in determining federal means-tested program eligibility and benefits.

Despite a large literature on IDAs (Harris et al. 2014; Zielewski et al. 2009), the effects of participating in AFI-funded IDAs have not been evaluated using a randomized controlled trial. This report presents early findings (approximately one year after study enrollment) from a randomized evaluation undertaken at AFI project sites in Albuquerque and Los Angeles.

This evaluation assesses the early impact of participation in AFI-funded IDA projects on the savings, asset ownership, and economic well-being of low-income individuals and families. The primary first-year hypothesized effects relate to savings and associated secondary economic well-being effects. We expect the program's impacts on asset purchases to occur after the first year (i.e., not before most participants reach the end of their savings periods). The research question is as follows:

- What is the early impact of AFI program participation on outcomes such as savings, asset ownership, and material hardship?

Findings from this evaluation and from an ongoing longer-term evaluation, which will measure impacts at three years, will provide important contributions to the asset-building field.

IDAs and the AFI Program

Through fiscal year 2014 (FY 2014)—the last year covered by the most recent AFI Report to Congress (OCS 2016)—AFI has provided approximately \$214 million in grant funds to support 846 regular AFI projects nationwide,³ 23 special state projects in Indiana and Pennsylvania, and 13 organizations involved in the Native Asset Building Initiative. Each AFI grantee designs its own project to meet community needs within basic federal restrictions regarding participant eligibility and allowed asset purchases. Thus, AFI projects can differ in several aspects, including nonfederal funding sources, whether one or several agencies operate the project, characteristics of accounts offered (e.g., match rate, maximum matchable savings amount, minimum deposits), whether they allow all three asset types (first-home purchase, business capitalization, and postsecondary education or training), amounts of financial education, and type and level of case management and other support services provided. Projects must meet the following federal guidelines:

- A match of between \$1 and \$8 for every \$1 in participant savings.
- No more than \$4,000 in match funds paid to each participant (and no more than \$8,000 per household), with at least 50 percent of the match funded by the grantee through nonfederal sources.
- Matched withdrawals no sooner than six months after the first deposit.
- Emergency unmatched withdrawals allowed only to cover medical expenses, rent or mortgage payments, or necessary living expenses following loss of employment.

³ *AFI project* refers to IDA programs operated by AFI grantees and funded by the federal AFI program. A total of 438 grantees have operated the 846 AFI projects funded through fiscal year 2014.

- Help for participants in obtaining the skills and information needed to purchase assets (e.g., financial education, financial coaching, credit-building services, credit/debt counseling, assistance with tax credits and tax preparation, and asset-specific training).
- Eligibility requirements for participants: household is eligible for Temporary Assistance for Needy Families, or has adjusted gross income less than or equal to 200 percent of the federal poverty level or the federal earned income tax credit limit and has net worth not exceeding \$10,000, excluding house and one vehicle.

During the nearly two decades since AFI's enactment, the operating environment for IDA projects has evolved. The population in poverty has become more racially and ethnically diverse, and increasingly more Hispanic.⁴ Fewer traditional community-action agencies and more educational or training organizations, such as community colleges, have sought and received AFI funding. Projects are also seeking to operate at greater scale.⁵

Research on IDAs and Other Matched Savings Programs

Much of the IDA literature comes from the American Dream Demonstration (ADD), implemented from 1999 to 2003 in one experimental site (Tulsa, Oklahoma) and a set of nonexperimental sites. The literature also includes nonexperimental analyses of the AFI program (McKernan et al. 2011; Mills, Lam, et al. 2008).

IDA programs demonstrate that low-income families save when provided financial incentives and financial education (Mills, Lam, et al. 2008; Schreiner and Sherraden 2007a; Stegman and Faris 2005). However, the first-phase nonexperimental AFI evaluation finds no significant effect of AFI participation on liquid (financial) assets (Mills, Lam, et al. 2008), and the few studies that examine net worth do not find a significant relationship between net worth and IDA program participation (Mills, Gale, et al. 2008; Mills, Lam, et al. 2008; Schreiner and Sherraden 2007b).

The literature finds that participating in an IDA program increases the likelihood a person starts or expands a business (Mills, Lam, et al. 2008; Moore et al. 2001), pursues postsecondary education (Mills, Lam, et al. 2008), or buys a home (Grinstein-Weiss et al. 2008; Mills, Gale, et al. 2008; Mills, Lam, et al. 2008).

⁴ Among persons below the poverty level, the Hispanic share increased from 23.4 percent in 1998 to 28.1 percent in 2015. See US Census Bureau, Historical Poverty Tables: People and Families—1959 to 2015 (<http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html>), Table 14: Distribution of the Poor by Race and Hispanic Origin: 1966 to 2015.

⁵ Tables shown in the AFI annual reports to Congress for 2009 and 2014 illustrate these trends (OCS 2010; OCS 2016).

However, these program effects typically take more than 12 months to materialize and may fade after 10 years. One study based on the experimental evaluation of the Tulsa ADD site finds that after 18 months, IDA program participation significantly increased only debt repair and had no sample-wide effects on liquid assets, total assets, total liabilities, or net worth (Mills et al. 2004). Others find that IDA program participation had no effect on homeownership rates among renters at 18 months, but these rates increased at 48 months (Grinstein-Weiss et al. 2008; Mills, Gale, et al. 2008).

A 10-year follow-up of the Tulsa sample finds that while most IDA participants had positive homeownership outcomes, the control group caught up with participants, so there are no long-term, statistically significant differences in the homeownership rate (Grinstein-Weiss et al. 2014). One potential explanation for this phenomenon is that control group members could access the treatment after four years.

The IDA program's effect on homeownership rates is still important. Foreclosure rates for IDA homebuyers were one-half to one-third the rates of other low-income homebuyers in the same communities, suggesting that low-income AFI participants fared better in the foreclosure crisis than other low-income homebuyers (McKernan et al. 2011).

Few IDA studies have examined program effects beyond savings that we might expect in the short term. No known studies examine whether IDAs reduce material hardship in the short term. The Tulsa evaluation only examines material hardship 10 years after participation and finds no effect. Other studies find no effect of IDA program participation on perceived financial situation or perceived ability to make ends meet at 18 months (Grinstein-Weiss et al. 2012; Mills, Gale, et al. 2008).

Some studies have examined how IDA design features influence participant outcomes, but most studies do not use an experimental design. Rather, they rely on nonexperimental methods that use comparison groups identified through nonrandomized procedures and that exploit the variation in project design features to estimate the relationship between features and outcomes. Higher match caps, for example, are associated with greater savings (Cramer 2007; Han and Sherraden 2009; Schreiner and Sherraden 2007a), possibly because participants want to take advantage of the added financial benefits and because they use the cap as a savings goal. Higher match rates are associated with increased program participation, but the relationship between match rates and savings is uncertain (Curley, Ssewamala, and Sherraden 2009; Schreiner and Sherraden 2007a). In one study, raising the maximum match rate is associated with increased net worth (McKernan, Ratcliffe, and Nam 2007).

The SEED random assignment evaluation in Oklahoma reports mixed findings regarding children's savings accounts (Nam et al. 2013). Only 16 percent of treatment group families opened a privately held children's savings account that would receive the financial match, compared with 1 percent of the control group (Nam et al. 2013). After 18 months, the treatment group had higher participant-owned savings, but

the difference between the two groups was a modest \$34. Average participant-owned savings was \$47 for the treatment group and \$13 for the control group. The authors suggest that universal children's savings accounts can be implemented and that automatic features matter, but the effects on private college savings appear limited.

Other matched savings initiatives, such as SaveUSA, include matched saving at tax time. Evaluations of SaveUSA find that most program members opened an account and received a savings match (Azurdia et al. 2014). In the short term, after 18 months, SaveUSA program participants increased liquid nonretirement savings by \$512 on average and saw no significant increase in average total (nonretirement and retirement) liquid assets. Participants did increase liquid assets by \$255 at the median, however (Azurdia et al. 2014). In the longer term, after 42 months, SaveUSA program participants increased total liquid savings by \$522 on average (Azurdia and Freedman 2016). The SaveUSA evaluation finds no effects of participation on liquid net worth (total liquid assets minus nonhousing debts) or incidence of financial hardship at either 18 or 42 months (Azurdia and Freedman 2016; Azurdia et al. 2014).⁶

Conceptual Framework

The conceptual framework that underlies IDAs was first articulated by Sherraden (1991) and was developed further through the ADD and other asset-building research (Lerman and McKernan 2008; McKernan and Sherraden 2008). Here, we describe the conceptual framework for this evaluation.⁷ This framework motivates our choice of outcomes examined and our basic analytic approach for estimating AFI program impacts.

⁶ The high levels of hardship incidence (63 percent) found for both the treatment and control groups in the SaveUSA 18-month evaluation (Azurdia et al. 2014) prompted us to move beyond incidence (whether a person experiences hardship) to intensity (how many times a person experiences hardship) in this AFI evaluation, as described in chapter 3.

⁷ This conceptual framework does not necessarily apply to non-AFI IDA programs, which may differ in allowable assets and other ways.

All AFI projects have three central elements:

- **Individual development account.** The personal savings account into which the participant makes deposits and from which the participant withdraws funds for authorized asset purchases or allowed emergency expenses.
- **Potential match funds.** The offer of match dollars (at a specified rate, for a maximum savings amount) paid to the participant when deposits are withdrawn for allowable asset purchases (first-home purchase, business capitalization, and postsecondary education or training).
- **Assistance in obtaining the skills and information necessary to make asset purchases.** This usually consists of financial education (i.e., instruction in basic financial management), financial coaching, asset-specific training, and other supportive services.

The primary causal relationships between these elements and participant outcomes are discussed below.

Conceptually, all three central elements promote participant savings. Financial education, training, and coaching provide useful information about budgeting, credit building and repair, and how to pursue asset-specific strategies (homebuying, business planning, and career-focused educational advancement). The IDA is the financial tool by which clients can act on their desire to save. Potential match funds provide incentives to participate and to save by multiplying personal deposits when used for allowable asset purchases.

Participants deposit savings into their IDAs and can then make matched withdrawals and unmatched emergency withdrawals. The matched withdrawals must result in authorized asset purchases. Emergency withdrawals can cover medical expenses, rent or mortgage payments, or living expenses following loss of employment. Unmatched withdrawals that clients spend on emergency household needs do not jeopardize participation if the account is later replenished.

We group the hypothesized effects of AFI participation into two primary domains and nine secondary domains. The primary hypothesized effects are increases in savings (early, in approximately the first year after project enrollment) and increases in asset ownership associated with allowable AFI asset purchases: first-home purchase, business capitalization,⁸ and postsecondary education or training (in the medium and longer term). These are the primary target outcomes against which the AFI program's success should be evaluated (savings for this early evaluation, table 1.1).

⁸ AFI allows purchases associated with first-home purchase and business capitalization. This evaluation measures homeownership and business ownership.

TABLE 1.1

Primary Hypothesized Effects of AFI Program Participation

Outcome measure	Early	Medium term	Longer term
Savings			
Has liquid assets	+	-	+/-
Liquid asset amount	+	+/-	+/-
Asset ownership			
First-home purchase	None	+	+
Business capitalization	None	+	+
Postsecondary education	None	+	+

We also hypothesize AFI participation to affect secondary outcomes that in the first year include reductions in material hardship and use of alternative financial services (AFS) (e.g., nonbank check-cashing services, payday loans), increases in employment and credit scores, improvements in personal outlook, and a shift toward a more future-oriented financial perspective. In the medium and longer terms, we hypothesize AFI participation will affect the following additional secondary outcomes: net worth, earnings and income, and community involvement. These secondary outcomes are hypothesized to occur through the pathways described below.

By encouraging participants to save, we hypothesize AFI program participation will increase savings (measured by liquid assets) and have a positive effect on a participant's personal outlook, measured by perceived financial security, increased self-esteem, and reduced financial stress. Savings can be used for emergencies, thereby reducing participants' short-term material hardship and reliance on AFS. We also hypothesize AFI program participation will connect participants to the financial mainstream by providing a savings account, thereby reducing use of another alternative financial service, check-cashing stores.

We hypothesize that the financial education and coaching that may be offered to AFI participants can improve participant credit scores in the short term (primarily through actions to repair credit) and shift participant time preferences away from immediate consumption purchases and toward future investment purchases.

For some secondary outcomes, the direction of the early impact is uncertain, depending on the strength of opposing effects (table 1.2). AFI participation could increase public benefit receipt in the short run if project staff encourage participants to apply for means-tested benefits they may be eligible to receive. It could increase employment and earnings by providing an incentive to earn more and thus have more savings that qualify for match funds. More emergency savings could also reduce employment barriers, such as a vehicle breakdown. It is also possible, however, that earnings could drop in the short run if enough

participants enter postsecondary education. AFI program participants must earn income,⁹ so we do not expect employment to decrease, even in the short term.

TABLE 1.2

Secondary Hypothesized Effects of AFI Program Participation

Outcome measure	Early	Medium term	Longer term
Net worth			
Net worth (i.e., assets minus debts)	None	None	+
Material hardship			
Food hardship	-	-	-
Housing hardship	-	-	-
Utilities hardship	-	-	-
Medical hardship	-	-	-
Alternative financial services			
Use AFS credit	-	-	-
Use nonbank check cashing	-	-	-
Means-tested benefit receipt			
Receive benefits	+/-	-	-
Employment, earnings, and income			
Employed	+	+	+
Monthly household earnings	+/-	+	+
Monthly household income	+/-	+	+
Credit score			
Vantage 2.0 score	+	+	+
Change in Vantage 2.0 score	+	+	+
Personal outlook			
Ability to make ends meet	+	+	+
Perceived financial security	+	+	+
Better off financially	+	+	+
Worse off financially	-	-	-
Good health	+	+	+
Self-esteem	+	+	+
Community involvement			
Community involvement (e.g., volunteer)	None	+	+
Time preference			
Present-oriented time preference	-	-	-

In the short term, AFI participation could increase net worth through additional savings (liquid assets). However, the program’s effect could decrease net worth if, for example, a participant takes out a student loan to meet college costs beyond those he or she expects to cover with a matched withdrawal from an IDA. We expect no significant effect on net worth in the first year after enrollment. Most allowable asset purchases—first-home purchase, business capitalization, or postsecondary education or training—are likely to take more than one year. Participants need time to reach their savings goals and become ready to purchase assets. Further, program rules stipulate that participants wait at least six months after opening their IDA to make a matched withdrawal. Associated secondary impacts, such as community involvement, would be expected to emerge following asset purchase.

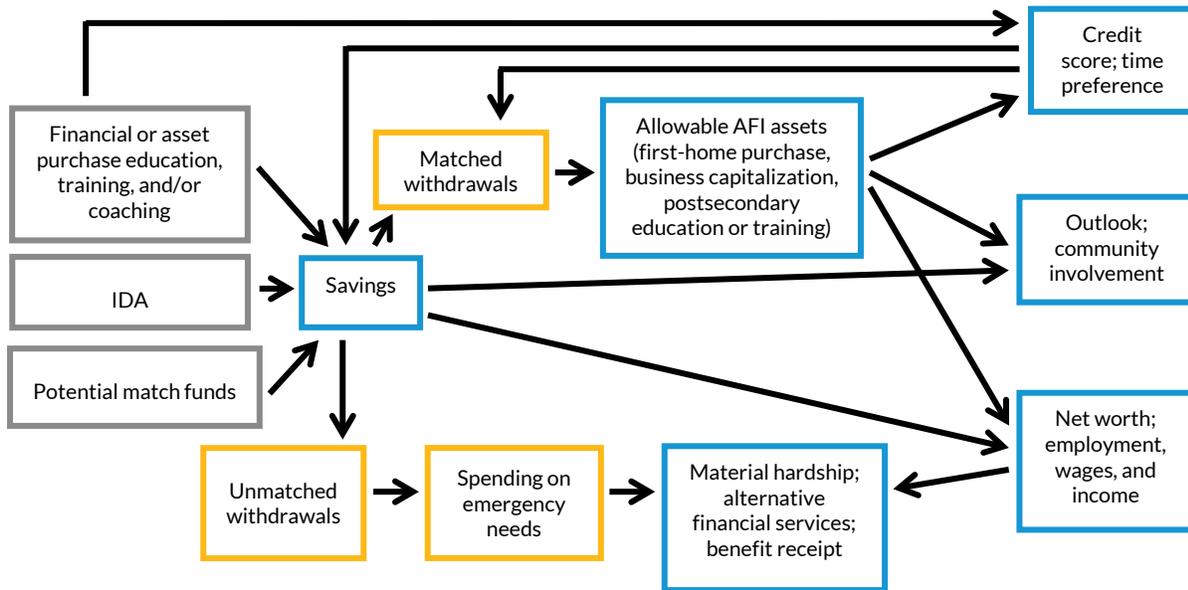
⁹ The Albuquerque site allowed parental earned income in lieu of participant earned income.

In the medium term (approximately three years after program enrollment), AFI participants should have completed their allowable asset purchases, so we hypothesize participation to increase homeownership, business ownership, and postsecondary education or training. The effect on savings (liquid assets) in the medium term is ambiguous. Savings could decrease if savings before enrollment are used to make and maintain allowable asset purchases; savings could remain unchanged after enrollment if all funds saved toward the asset purchase are used; and savings could increase if participants form a regular savings habit and continue to save beyond their allowable asset purchases and their maintenance. Net worth could increase because of the new assets or decrease because of the transaction costs and loans associated with the asset purchases. Given these offsetting effects, we expect no significant effect on net worth in the medium term.

In the longer term, we hypothesize the asset purchases to increase net worth. Through improved financial stability, we hypothesize that AFI IDAs will reduce material hardship. Likewise, we hypothesize participant credit scores to improve through credit repair, financial coaching, and timely repayment of a home mortgage, business loan, or student loan. We further hypothesize that in the long run, by having promoted the account holder's investments in human capital and business capital, AFI participation will increase employment and income and reduce public benefit receipt.

Some secondary effects identified in this conceptual framework and summarized in figure 1.1 are ones for which little evidence exists—material hardship, credit scores, and AFS use. Testing for these effects is an important contribution of this evaluation. If AFI shows early impacts on material hardship, unmatched withdrawals or deferred deposits may play a role. Similarly, because credit reports and credit scores can affect families in multiple ways—they can influence access to rental housing, mortgage loans, insurance coverage, and employment opportunities—it is important to understand if AFI can improve credit scores over time.

FIGURE 1.1
 Pathways for Participant Impacts



Chapter 2: Study Sites, Study Enrollment, and Participant Characteristics

This chapter presents a brief overview of the evaluation’s site selection process, a description of the study sites, and a description of study participant characteristics.

Study Sites

Site Selection

We selected study sites according to six criteria under two major categories: whether the site could implement a random assignment evaluation and whether its project features were typical for AFI projects nationwide. The six criteria were (1) previous AFI experience, (2) financial and organizational capacity to recruit the targeted sample size, (3) consistent project enrollment procedures across locations, (4) limited IDA-like alternatives for the control group, (5) interest in participation in evaluation and willingness to implement the experimental design, and (6) project features similar to other mainstream AFI projects.

We used OCS’s AFI grantee data from FY 2010 and FY 2011 to determine which AFI projects met the first two criteria. This resulted in 28 grantees among all 353 AFI grantees. We refined this list by comparing candidates on the other four criteria. We also conducted a more general qualitative scan of grantees, which included input from AFI Resource Center staff.¹⁰ We then conducted phone calls with 19 remaining grantees, which led to four finalist sites.

Among these four, two were deemed qualified sites and remained interested in participating in the evaluation: RISE Financial Pathways in Los Angeles and Prosperity Works in Albuquerque, with its partner at Central New Mexico Community College (CNM), the student resource center known as CNM Connect.¹¹ Both sites had large enough AFI grants to serve the sample size required by the evaluation, experience operating IDA projects, mainstream characteristics regarding asset types offered, and past experience working with research studies. They both had small intake staffs, few intake locations, and a single model of

¹⁰ The AFI Resource Center is ACF’s technical assistance resource for AFI grantees.

¹¹ CNM Connect was recently renamed Connect Services.

the project, which simplified evaluation implementation. See appendix A for further information on site selection.

Key Site Features

The Albuquerque and Los Angeles study sites shared some key project features, but differed in other respects (table 2.1). Both sites provided matching funds for all three types of asset purchases allowed in AFI: first-home purchase, business capitalization, and postsecondary education or training. Both sites had a maximum matchable savings amount of \$1,000, and both allowed participants to make partial matched withdrawals of less than \$1,000. The Los Angeles project required participants to reach the \$1,000 savings target before making any partial matched withdrawals. The sites differed in their match rates: 4:1 in Albuquerque and 2.5:1 in Los Angeles. Both sites had a minimum savings period of 6 months, and the Los Angeles site had a maximum of 24 months. In Albuquerque, participants could save longer than 24 months if needed.¹² The study sites were typical of AFI projects nationwide in most respects, according to information on 805 AFI grants made through FY 2014 (OCS 2016).

The approach to financial education differed between the two sites. The Albuquerque site required a one-semester community college financial education course (or self-paced online equivalent), while the Los Angeles site required on-site classroom sessions taught by AFI project staff. In Albuquerque, two project partners provided asset-specific training: Homestart (an external organization) for homeownership training and the Small Business Development Center of CNM for small business training. In Los Angeles, RISE itself provided small business training, and participants were referred out for homeownership counseling.

¹² Participants who were not ready to make asset purchases by October 2015—the end of the grant under which the Albuquerque site enrolled its participants—could request an extension, and their matches would be funded by a second AFI grant that Prosperity Works won during the evaluation. For early study enrollees, this could result in a savings period longer than 24 months. However, staff encouraged participants—including late enrollees who had been saving closer to 12 months—to make asset purchases by October 2015.

TABLE 2.1

Major AFI Project Features

	Albuquerque	Los Angeles	AFI Act rules/ AFI projects nationwide ^a
Site	Prosperity Works (grantee) and CNM Connect of Central New Mexico Community College (partner)	RISE Financial Pathways, formerly Community Financial Resource Center (grantee)	—
Geographic area for sample recruitment	Metropolitan Albuquerque, NM, and surrounding vicinity	Los Angeles County, CA	—
Allowable asset types	First-time home purchase, business capitalization, and postsecondary education or training	First-time home purchase, business capitalization, and postsecondary education or training	First-time home purchase, business capitalization, and postsecondary education or training 72 percent of projects allowed all three
Maximum savings amount eligible for match	\$1,000	\$1,000 (required savings amount before any matched withdrawal)	Mean: \$1,545 AFI Act limits the amount of funds from one AFI grant to \$2,000 per individual and to \$4,000 per household
Match rate	4:1	2.5:1	1:1 to 8:1 Mode: 2:1, followed by 3:1 or 4:1
Minimum savings period	6 months	6 months	At least 6 months from initial deposit until a matched withdrawal is allowed
Maximum savings period	24 months ^b	24 months	No restriction other than the end of the grant
Financial education	One-semester CNM credit course (21 classroom hours) or self-paced online option	Multisession on-site classes offered on weekday evenings or Saturdays (10 hours)	No requirement for participants Mean: 11.3 hours
Homeownership training provider	Homestart	Several external agencies	—
Small business training provider	Small Business Development Center at CNM	RISE staff	—
Postsecondary education training provider	CNM Connect academic coaches (worked to develop an education plan)	RISE staff (reviewed information on financial aid)	—

^aThis column lists AFI Act requirements, where applicable, and provides characteristics of 805 AFI projects nationwide through fiscal year 2014 from the most recent AFI Report to Congress (OCS 2016).

^bStaff encouraged participants to save for no more than 24 months (or less if they enrolled later in the enrollment period) to complete saving by the end of the AFI grant supporting this evaluation (October 2015). Participants who needed more time could take it because Prosperity Works won another AFI grant during the evaluation, which could fund these participants' matches.

SITE PROFILE: PROSPERITY WORKS AND CNM CONNECT (ALBUQUERQUE)

Prosperity Works was founded in Albuquerque in 1999 and received its first AFI grant in 2004. Prosperity Works does not offer AFI IDAs directly but operates the New Mexico Assets Consortium, a partnership with other organizations that provide the IDAs. Under this evaluation, CNM Connect at Central New Mexico Community College, an AFI partner of Prosperity Works since 2006, provided the AFI IDA.

CNM Connect is a one-stop student resource center with six locations across CNM's campuses available to all of CNM's 40,000 students. It provides financial and academic coaching, help accessing public benefits, and referrals for legal counseling, and is a free tax-preparation site. CNM Connect receives nonfederal match funds from the CNM Foundation. Staff at Prosperity Works administer and monitor the AFI accounts, monitor savers' credit scores, and provide credit-building information.

Before the current AFI evaluation, CNM Connect served approximately 15 IDA participants at a time. Little outreach was necessary to reach that number. CNM Connect's "achievement coaches" recruited IDA participants among students who came in for financial coaching, and more students were interested than the organization could serve.

For this evaluation, four of CNM Connect's achievement coaches served AFI participants directly. Staff from both Prosperity Works and CNM Connect, along with an external marketing consultant, helped recruit and enroll participants.

SITE PROFILE: RISE FINANCIAL PATHWAYS (LOS ANGELES)

RISE Financial Pathways (RISE) is a small nonprofit community development financial institution in South Central Los Angeles that focuses on small business development. It was founded in 1993 as the Community Financial Resource Center and changed its name in 2013, partway through the evaluation. RISE served study participants through its first AFI grant. It had previously been a partner of two different AFI grantees. Besides AFI, RISE offers business training and small business loan programs. RISE previously offered two accelerated savings programs, and it was an enrollment site for a matched savings program for children's education, but the funding for these ended during the evaluation.

The AFI project was operated primarily by two staff members: the program director and one supporting staff member. Two other staff members contributed to the project and three contractors assisted with recruitment during the last six months of study enrollment. Both full-time staff members left RISE before enrollment was complete.

Study Enrollment and Random Assignment

Study Eligibility

Study participants in both sites were required to meet the AFI program’s eligibility requirements: an applicant’s household must (1) be eligible for Temporary Assistance for Needy Families or (2) have adjusted gross income equal to or less than 200 percent of the federal poverty level or within the income limits of the federal earned income tax credit, and have net worth—excluding the primary residence and one vehicle—that does not exceed \$10,000. To enroll in AFI, participants needed to have earned income (or parental earned income in Albuquerque) for their deposits. For the evaluation, participants needed to be at least 18 years old.

Each site had additional requirements. In Albuquerque, participants initially needed to be enrolled in at least six credit hours at CNM. Late in the enrollment period, the credit-hour requirement was relaxed. Students enrolled in the GED or English as a Second Language programs and students registered for fall 2014 classes but not yet enrolled were also permitted to apply. In Los Angeles, participants had to reside in Los Angeles County.

To ensure random assignment resulted in comparable treatment and control groups, we asked the sites to adopt study-specific rules. For example, we established a rule for handling situations in which two members of the same household applied to the program at different times. The first applicant was to be included in the study and randomly assigned. The second applicant was to be assigned the same group status as the first (treatment or control) but not included in the study. This ensured that a control group member would not live in the same household as a treatment group member and thereby potentially benefit from the program as well.

Recruitment and Study Enrollment

The Albuquerque and Los Angeles sites each used many recruitment methods. These included recruitment events, referrals from other organizations, flyers, social media postings, e-mails, and public outreach. The Albuquerque site relied mostly on events and the recruitment of participants by already-enrolled study participants, while the Los Angeles site relied on referrals and mass public outreach.

Enrollment into AFI and the study required several steps: submission of the completed project application, determination of eligibility, and completion of the study’s online baseline survey, at the end of which people were randomly assigned to the treatment group (which could receive AFI and non-AFI

services) or the control group (which could receive non-AFI services only) through a computer algorithm. At both sites, enrollment into AFI and the study frequently occurred immediately following an orientation session describing the project and study. In Albuquerque, enrollment occurred in the student center on the main CNM campus, with a few participants enrolled at satellite campuses. In Los Angeles, most enrollment occurred at RISE; a few referral partners also hosted enrollment opportunities.

As part of enrollment, study participants signed an informed consent form before taking the baseline survey and gave their consent again as part of the survey. The informed consent specified that each study participant had a 50-50 chance of being allowed to enter AFI and that participants assigned to the control group could not apply again for AFI for up to three years. It also explained that control group members could access all site-offered non-AFI services for which they qualified.

Study Enrollment by Site

Both the Albuquerque and Los Angeles sites began study enrollment in January 2013 (though in Albuquerque the process was put on hold until May after one participant was enrolled in January). The enrollment period was initially expected to last 15 months, ending in March 2014, but was later extended by three and a half months to July 2014. Recruitment was a challenge for both sites; this extension provided more time to reach target enrollment numbers for the evaluation.¹³ Enrollment proved especially difficult at the Albuquerque site, where outreach could not extend beyond CNM students. Technical complications delayed the start-up of intensive recruitment in Albuquerque until August 2013.

The original targets for sample enrollment were set at 600 for Albuquerque and 500 for Los Angeles. With the extended enrollment period, the Albuquerque site enrolled 299 people, just below half its original target. The Los Angeles site exceeded its enrollment target, with 508 people entering its study sample. In total, 807 participants were enrolled into the study.

The numbers of cases enrolled into the study overall and by time period are shown in table 2.2. In both sites, more than twice as many participants entered the study in the final seven months of the enrollment period (January to July 2014) than entered in all of 2013.¹⁴ Staff in Albuquerque reported in interviews that

¹³ Because enrolled cases were randomly assigned between the treatment and control groups at a 1-to-1 ratio, each site's enrollment target was twice the number of funded slots under the site's AFI program.

¹⁴ The sites accelerated recruitment later in the recruitment period for three reasons: (1) In Albuquerque, delays in establishing a contract between the site and the evaluator and in finalizing arrangements to pull credit scores led to initial delays in recruitment; (2) both sites brought on more staff to help with recruitment and enrollment, as well as expanded the variety of recruitment methods later in the enrollment period; and (3) both sites streamlined their program application forms in fall 2013.

late-enrolled participants appeared less ready to save and perhaps less likely to succeed in the project, while most staff in Los Angeles did not notice a difference in enrollees.

TABLE 2.2

Study Enrollment by Time Period and Site

Time period	Sites combined	Albuquerque	Los Angeles
January–December 2013	255	97	158
January–April 2014	324	121	203
May–July 2014	228	81	147
Total	807	299	508

Source: Authors’ calculations of AFI baseline survey data.

Study Participant Demographic Characteristics

Study participant characteristics and how they compare with other AFI project participants nationwide speak to the evaluation’s external validity (the extent to which the evaluation population is representative of the nationwide AFI population). This section uses baseline survey data and OCS’s AFI grantee data from FY 2013 and 2014 to assess the evaluation’s external validity. Chapter 3 describes these sources.

Study participants were predominantly under age 40, female, nonwhite, and unmarried, and had some college education at the time of study enrollment (table 2.3). About half the study participants had annual household incomes below \$15,000 and nearly all were employed (92 percent).¹⁵

Study participants are similar to other AFI participants who enrolled in FY 2014 (October 2013–September 2014). Nationwide, participants were also predominantly under age 40, female, nonwhite, unmarried, employed, and had some college at the time of enrollment. These similarities, as well as the sites’ similarities to AFI projects nationwide in terms of project features, strengthen the evaluation’s external validity.

There were a few differences between study participants and other AFI participants. While both study participants and AFI participants nationwide were predominantly nonwhite, a larger share of study participants was Hispanic and a smaller share was white. While both were predominantly under age 40 and had some college, fewer study participants were under age 40, fewer had children, and more had completed some college, compared with the AFI participants enrolled nationwide. See appendix B for more detail on

¹⁵ In Albuquerque, some study participants may not have been employed because the eligibility rules allowed earned income from the individual or from the individual’s parent. In both sites, some individuals may have reported earned income in their applications and been accepted to the study, but may not have reported employment in the baseline survey.

these differences. Given US demographic trends, the more ethnically diverse study population is likely to reflect the future AFI-eligible client population (more diverse racially and ethnically) (Martin et al. 2015).

When we look at study participant characteristics across the two sites, the share of female participants was similar (roughly 70 percent), but participants differed on most other demographic characteristics (table 2.3). This reflects in part the differences in the two organizational settings: a large public community college (the Albuquerque site) and a nonprofit community development financial institution (the Los Angeles site). Study participants in Albuquerque were younger, more likely to be Hispanic and white, and far less likely to be black than study participants in Los Angeles. Study participants in Albuquerque were also more than twice as likely to have some college education but no degree compared with participants in Los Angeles, and they were more likely to have income below \$15,000.

TABLE 2.3

Selected Characteristics of AFI Study Participants and Other AFI Participants Enrolled in FY 2014

Demographic explanatory variables	Sites combined	Albuquerque	Los Angeles	Other AFI participants enrolled in FY 2014
Age (%)				
0–29	34.5	51.5	24.6	44.8
30–39	26.5	23.2	28.3	31.1
40–49	21.7	14.8	25.8	16.2
50+	17.3	10.4	21.3	7.9
Gender (%)				
Female	69.9	64.5	73.0	72.7
Male	30.1	35.5	27.0	27.3
Race/ethnicity (%)				
White	14.8	30.5	5.2	31.6
Black	29.0	8.9	41.3	33.3
Hispanic	44.9	50.0	41.7	23.2
Other	11.3	10.6	11.7	11.9
Language spoken at home (%)				
Speaks only English at home	56.5	65.1	51.4	
Speaks a language other than English at home	43.5	34.9	48.6	
Education level (%)				
Up to high school diploma/GED	18.2	10.0	23.1	37.3
Some college, but no degree	43.4	61.5	32.7	30.0
License, certificate, or associate's degree	24.0	21.7	25.3	13.9
Bachelor's degree or higher	14.4	6.7	18.9	18.8
Marital status (%)				
Married	18.7	16.4	20.0	22.5
Not married	81.3	83.6	80.0	77.5
Number of adults, including participant (%)				
1	46.4	44.0	47.8	55.5
2	32.0	36.9	29.1	33.6
3+	21.6	19.1	23.1	10.9
Number of children ages 0–17 (%)				
0	49.4	54.5	46.2	33.7
1	22.6	23.6	22.0	21.8
2	14.7	14.1	15.1	21.6
3+	13.3	7.7	16.7	22.9
Employment (%)				
Employed	91.8	89.6	93.1	86.5
Not employed	8.2	10.4	6.9	13.5
Annual household income (%)				
Less than \$15,000	50.6	59.3	45.5	+
\$15,000–\$24,999	24.5	21.4	26.4	+
\$25,000 plus	24.8	19.3	28.1	+

Sources: Authors' tabulations of the AFI baseline survey (columns 1–3) and the Office of Community Services' (OCS) AFI grantee data from fiscal year 2014 (column 4).

+ We cannot compare income because income measures in the OCS data and baseline survey data differ.

Chapter 3: Data Sources and Variable Definitions

This chapter describes the data sources and defines variables used in the evaluation.

Data Sources

To answer our key research question, we use baseline and first-year follow-up survey data from both sites and aggregated credit scores provided by project staff at the Albuquerque site. To provide context for our impact findings, we use project data to examine treatment group members' participation in the project. We also incorporate findings from qualitative research conducted during visits to both sites. We describe each data source below.

Baseline Survey

A total of 807 participants (299 in Albuquerque and 508 in Los Angeles) completed the baseline survey between January 2013 and July 2014. Eligible project applicants at each site completed the self-administered baseline survey on the web immediately before random assignment, with staff helping them navigate the website as needed. Because only those who completed the baseline survey were randomly assigned, the baseline survey response rate among sample members is 100 percent. The survey took 30 to 40 minutes to complete and contained questions on demographics, savings, assets, material hardship, use of alternative financial services, public benefit receipt, employment, earnings, income, net worth, and personal outlook.

First-Year Follow-Up Survey

The principal data source for measuring early impacts among study participants is the first-year follow-up survey, which participants completed between April 2014 and September 2015. Originally designed to be conducted by telephone, the survey evolved to include a self-administered web option and an in-person field interview option. The follow-up survey covered the same topics as the baseline survey, except that demographic questions were excluded and questions about IDA services received were added. We also added questions that captured how often respondents experienced material hardship.

RESPONSE RATES BY SITE, GROUP, AND MODE

For both sites combined, the first-year follow-up survey response rate was nearly 78 percent, with a higher rate in Albuquerque (83 percent) than Los Angeles (75 percent; table 3.1). The 628 sample members who completed at least the first seven of nine survey sections—all but the sections on material hardship and social engagement—constitute the analysis sample. Most people completed the survey within 30 days of their one-year anniversary of study enrollment.¹⁶ Fifty-seven percent of survey respondents completed their surveys by phone, nearly 40 percent completed on the web, and 3 percent completed with a field interviewer. Those who responded to the survey differed slightly from those who did not, but the differences were not systematic. See appendix C for further detail on these differences.

TABLE 3.1

First-Year Follow-Up Survey Response Rates, by Group and Site

Group	Sites combined		Albuquerque		Los Angeles	
	Sample	% completed	Sample	% completed	Sample	% completed
Treatment	407	82.3	151	86.1	256	80.1
Control	400	73.3	148	80.4	252	69.0
Total sample	807	77.8	299	83.3	508	74.6

Credit Scores

Credit scores are available for the Albuquerque site only. Albuquerque project staff recorded each sample member's Vantage 2.0 credit score at baseline and 12 months after study enrollment. Under Fair Credit Reporting Act restrictions that regulate the credit score use, the Albuquerque site could not make individual scores available for the evaluation. Instead, the Albuquerque site provided the mean and standard deviation of the baseline credit score, the follow-up credit score (at month 12), and the change in score (over the intervening 12 months) separately for the treatment and control groups.

Project Data

We use AFI project data from the study sites to examine patterns of engagement for the treatment group. Both study sites provided case-level data extracted from internal records on participation and engagement for treatment group members. These data cover the 12 months following the participant's study enrollment. The data items pertain to the timing of AFI IDA opening and timing of financial education. The Albuquerque site also provided data from internal records on the timing and amount of deposits and withdrawals.

¹⁶ Fourteen percent of survey respondents completed the survey 31 to 60 days before the one-year anniversary of random assignment, 59 percent within 30 days of the anniversary, 9 percent 31 to 60 days after, and 19 percent more than 60 days after the anniversary, with a maximum of 393 days after the anniversary.

Qualitative Interviews and Document Review

As part of the evaluation's implementation study, we visited each evaluation site in 2014. We conducted semi-structured interviews with site staff and treatment group participants and observed a credit information workshop and demonstration of the online financial education curriculum in Albuquerque and a financial education class in Los Angeles. We also reviewed site-specific materials available online and those obtained from project staff before and during each visit, as well as notes from site monitoring calls that occurred during enrollment and follow-up data collection. Interviews addressed organization and AFI project features, recruitment and enrollment, project services, and perceived project effects.

AFI Grantee Data from FY 2013 and 2014

Chapter 2 compared characteristics of study participants from the baseline data with other AFI participants nationwide. Characteristics of AFI account holders nationwide come from AFI grantee data provided to us by OCS, which collects data from AFI grantees on the status of their projects at the end of each federal fiscal year. We used FY 2013 and FY 2014 data to determine the characteristics of people who opened accounts in FY 2014 by subtracting cumulative figures from the FY 2013 data set from cumulative figures from the FY 2014 data. We focus on FY 2014 (October 2013–September 2014) because 85 percent of the study sample was enrolled during that year. See appendix B for more detail on this data source.

Variable Definitions

The key outcome and explanatory variables for the evaluation come from the baseline and first-year follow-up survey data. Table 3.2 lists the primary outcome variables, and table 3.3 lists the secondary outcomes grouped by the 11 domains described in the conceptual framework. Appendix table D.2 lists the demographic and study participation explanatory variables that we incorporate into our analyses.¹⁷

TABLE 3.2

Primary Outcome Variables

Variable	Definition
Savings as measured by liquid assets (participant and spouse/partner)	
Any liquid assets	Has any financial assets (i.e., liquid assets are positive), including amount in savings and checking accounts, certificates of deposit, money-market accounts, stocks, bonds, retirement accounts, and other savings.
Liquid asset amount	Dollar value of liquid assets (see above).
Natural log of liquid assets	Natural logarithm of the dollar value of liquid assets.
Asset ownership (participant or participant and spouse/partner)	
Homeownership	Participant owns home he or she lives in.
Business ownership	Participant or spouse/partner owns business or farm, or participant is self-employed.
Education	Participant’s level of postsecondary education increased between baseline and first-year follow-up between the following categories: high school diploma/GED or less; some college credit but less than one year; one or more years of college but no degree; license or certificate; associate’s degree; bachelor’s degree or greater.

Note: Liquid asset amount (and its respective natural log) was top coded at the 98th percentile of the first-year follow-up survey, which corresponds to \$35,500.

¹⁷ Appendix D also summarizes outcome variable means at baseline, in table D.1.

TABLE 3.3

Secondary Outcome Variables

Variable	Definition
Net worth (participant and spouse/partner)	
Net worth	Dollar value of all assets minus all liabilities. Assets are the sum of financial assets (individual development accounts, bank accounts, stocks, bonds, 401(k)s/individual retirement accounts, and other savings) and nonfinancial assets (value of homes, real estate, vehicles, and businesses). Liabilities include unsecured debt (e.g., credit card balances, education loans, payday loans) and secured debt (mortgages, other real estate debt, vehicle debt, and business debt).
Material hardship in last six months (household)	
Food hardship	Sometimes or often did not have enough to eat.
Housing hardship	Couldn't pay rent or was foreclosed on or evicted.
Housing hardship	Number of times couldn't pay rent.
Utilities hardship	Couldn't make utility payment, or had gas, electricity, or phone turned off.
Utilities hardship	Number of times couldn't make utility payment, or had gas, electricity, or phone turned off.
Medical hardship	Couldn't afford doctor, dentist, or prescription when needed.
Medical hardship	Number of times couldn't afford doctor, dentist, or prescription when needed.
Any hardship	Experienced any of the above hardships.
Number of hardships	Number of times experienced any of the above hardships.
Alternative financial services (AFS) in last 12 months (participant and spouse/partner)	
Use AFS credit	Used a payday loan, auto loan, or pawnshop.
AFS credit	Number of times used a payday loan, auto loan, or pawnshop.
Use nonbank check cashing	Used nonbank check-cashing services.
Nonbank check cashing	Number of times used nonbank check-cashing services.
Means-tested benefit receipt (household)	
Receive benefits	Received any of the following public benefits in the past month: Special Supplemental Nutrition Program for Women, Infants, and Children; Supplemental Nutrition Assistance Program, free/reduced price school lunch, Supplemental Security Income, Temporary Assistance for Needy Families, housing subsidy, energy subsidy, child care subsidy, or Medicaid.
Employment (participant) and earnings and income (household)	
Employed	Participant working for pay at time of survey.
Monthly earnings	Total household earnings (before taxes) in the last month.
Natural log of monthly earnings	Natural logarithm of total household earnings (before taxes) in the last month.
Monthly income	Total household income in the last month, including job earnings, Supplemental Security Income, public assistance or welfare, unemployment insurance, worker's compensation, child support, and income or benefits from family and friends outside the household.
Natural log of monthly income	Natural logarithm of total household income in the last month.

Continued on next page

TABLE 3.3

Secondary Outcome Variables (continued)

Variable	Definition
Credit score (participant)^a	
Vantage 2.0 score	Group mean value of Vantage 2.0 credit score in the 12th month after study enrollment, for participants who are scoreable in that month (501-990).
Change in Vantage 2.0 score	Group mean value of change in Vantage 2.0 credit score between month of random assignment and 12th month after study enrollment, for participants who are scoreable in both months.
Personal outlook (participant)	
Ability to make ends meet	Feels it is very easy, easy, or neither hard nor easy to make ends meet, rather than hard or very hard to make ends meet.
Perceived financial security	Perceived financial security as measured by absence of worry about their ability to meet monthly living expenses. Ranges from 1 to 10: 1 indicates worry all the time, 10 indicates never worry.
Better off financially	Financial situation improved in the last 12 months, rather than worsened or stayed the same.
Worse off financially	Financial situation worsened in the last 12 months, rather than improved or stayed the same.
Good health	Health is generally excellent, very good, or good, rather than fair or poor.
Self-esteem	Rating of self-esteem based on Rosenberg scale (0-30).
Community involvement in last 12 months (participant)	
Community involvement	Worked on a neighborhood project, volunteered, attended a community event, or helped raise money for a community organization.
Time preference (participant)	
Present-oriented time preference	Interest rate willing to accept to defer payment on a major purchase (0-1).

Notes: Monthly earnings, annual income (and their respective natural logs), and net worth were top coded at the 98th percentile of the first-year follow-up survey values, which corresponds to \$10,000, \$71,000, and \$214,000, respectively. Frequency of utilities hardship and frequency of medical hardship are each the sum of three individual component variables, each of which was top coded at 6. Frequency of housing hardship is based on one component, top coded at 6. Frequency of composite hardship is the sum of these three outcomes, with a maximum possible value of 42. Because of the survey question design, the frequency of check-cashing use variable takes values 0, 1, 3, or 5, where 3 indicates 2 to 4 uses, and 5 indicates 5 or more. The frequency of AFS credit use variable is the sum of three variables with this pattern (payday loan, auto title loan, pawnshop), which results in a maximum possible value of 15, indicating 15 or more uses.

^aCredit score is available for Albuquerque participants only.

Chapter 4: Project Implementation

This chapter describes delivery of AFI services based on information gathered through the implementation study and data from the study sites. We also discuss services delivered to the study's control group members.

Delivery of AFI Project Services

Overview

This section focuses on the contextual factors and developments at each site that influenced study enrollees and the delivery of project services to treatment group members. The information presented here draws on interviews with staff and participants during visits to the sites in 2014 and calls with site staff throughout the evaluation.

In Albuquerque, Prosperity Works' partner, CNM Connect, implemented the AFI project. CNM Connect's achievement coaches provided AFI IDA services and worked with students on academic success planning, financial goals, study skills, accessing community and college resources (including public benefits), and other services to support retention and graduation. Prosperity Works' partnership with CNM Connect was advantageous because the community college setting provided a ready pool of AFI applicants, and existing staff at CNM Connect could assist with AFI recruitment and service provision. It also presented unique enrollment challenges. For example, the academic calendar meant that recruitment could not occur continuously; enrollment slowed or stopped during school breaks, exams, and registration, when achievement coaches were busy or when potential participants weren't on campus.

RISE Financial Pathways in Los Angeles describes itself as providing commercial lending opportunities—including microloans, peer lending, and small business loans—along with business training, asset development programs, and banking programs. RISE had a small staff with high turnover that made serving a large influx of new project participants difficult. The site also experienced operating budget shortfalls. Executive and project staff at RISE said that an insufficient budget prevented the organization from recruiting and serving participants as effectively as they otherwise could have.

Partnering and financing arrangements also presented contrasting challenges and advantages for each site. In Albuquerque, Prosperity Works had a long-standing partnership with Wells Fargo Bank that made opening and monitoring IDAs seamless. In Los Angeles, the partnership with Union Bank was new for RISE

and presented problems for both project participants and RISE. In Albuquerque, CNM Connect secured all required nonfederal match funds from the CNM Foundation. In Los Angeles, staff faced difficulties raising the nonfederal funds. These difficulties constrained RISE participants' ability to make matched withdrawals, starting in May 2015.¹⁸

Project Services

FINANCIAL EDUCATION

Both sites required participants to complete a financial education course before making a matched withdrawal. Savers and staff reported benefits from financial education, with particularly positive comments from savers in Los Angeles.

"I was able to say, you know what, I have made a mess in my financial past, but that doesn't define who I am, and where I want to go... that was the biggest thing I got from the classes."
AFI project participant

In Albuquerque, financial education for most participants was a three-credit, semester-long CNM course. Rarely did staff permit someone to complete financial education elsewhere. CNM offered the course both in person and online to all CNM students. CNM faculty developed the curriculum and did not design it solely to serve AFI participants. The course covered topics in personal finance, education financing, and saving and borrowing. The in-person course involved 21 hours of class time; CNM staff said successful students spent 7 to 10 hours per week on the class, including homework time.

Participants had to pay for the course as they would any other class at CNM, though Prosperity Works covered the cost for students facing financial barriers. Starting in fall 2014, the course became eligible to be covered by financial aid, which helped more participants meet the cost.

A Prosperity Works staff member said that participants deeply appreciated the course, and another cited it as one reason for Prosperity Works' historically high college graduation rate among AFI participants.

¹⁸ Because this problem did not arise until May 2015, it likely only affects first-year outcomes for participants who enrolled late in the enrollment period and had short savings periods.

One Albuquerque participant reported that financial education made her more aware of scams and identity theft, and another said it helped him make better financial choices.

The Los Angeles site required participants to complete a free RISE course offered only to AFI participants. Participants could complete the 10-hour course either in five two-hour sessions on weekday evenings or two five-hour Saturday sessions. Early on, RISE required participants to complete financial education within 45 days of enrolling. Because of the number of participants increased under this AFI grant, RISE could not serve everyone as quickly and subsequently permitted participants to take the course any time before making a matched withdrawal. Staff viewed this as not ideal, as participants did not learn important information soon enough.

RISE used its own financial education curriculum, which addressed credit, consumer awareness, financial statements, predatory practices, and saving and investing. During the class that researchers observed, the instructor delivered information informally and interactively—students played learning games and the instructor shared savings tips and reinforced the lesson points by sharing experiences from her own life.

Staff in Los Angeles said that the interactive financial education class is helpful because participants can talk with someone, often for the first time, about their finances. They said participants were glad they attended financial education. The interviewed participants confirmed this: two participants said the classes helped them absorb principles of financial management. One participant who found the course empowering said it allowed her to acknowledge past financial mistakes but not let them define her future. Although participants were ultimately glad they received financial education, RISE staff said initial resistance was common. RISE staff reported that some participants left the project because they did not wish to attend all the classes.

HOMEOWNERSHIP TRAINING

Albuquerque participants saving for a home purchase attended training at Homestart, a nonprofit organization that offers classes and one-on-one counseling, helps people find homes and acquire mortgages, and can provide down payment assistance and act as a mortgage lender. There was no cost to the classes or counseling. The classes addressed important steps for homeowners, such as paying the mortgage and participating in neighborhood associations. In addition to the classes, home savers met with a home purchase adviser to establish a home purchase plan. The Homestart adviser had to sign off on the plan for the participant to purchase his or her home.

RISE referred Los Angeles participants saving for a home to external agencies to complete their homeownership training. RISE gave participants a list of providers approved by the City of Los Angeles Housing Authority and advised the participants to attend as many training sessions as they could. Some

referral organizations charged a fee for training (\$20 to \$70), but three did not; participants generally chose the free trainings, according to RISE staff. The training covered credit, shopping for a mortgage, selecting a real estate agent, and keeping a home. Home savers received a certificate upon completion, which they submitted to RISE.

SMALL BUSINESS TRAINING

In Albuquerque, the New Mexico Small Business Development Center (SBDC) provided training for small business savers. SBDC provides business consulting and training for people starting or already running a business. The training included a two-hour orientation that covered the basics of starting and running a small business and topic-specific workshops covering tax and revenue, bookkeeping, QuickBooks, and other topics. CNM did not require AFI participants to take the training classes, but did require them to work with SBDC counselors to develop a business plan before making a matched withdrawal. SBDC staff said AFI participants usually completed about six hours of counseling sessions. There was no cost for individual counseling or training, with the exception of an optional QuickBooks training.

In Los Angeles, RISE staff provided small business training—open only to AFI participants—at the organization’s offices, free of charge. The training lasted 10 hours and covered such topics as business missions, elevator pitches, financial projections and statements, cash flow, and how to create a business plan. RISE also required business savers to write a business plan, often involving several drafts. A staff member said that the business ideas had to come from the participants themselves: “I’d say, ‘How are you going to make the business grow?’ And it really has to come from them...if you give them a business idea...they don’t know how to develop it.” Staff said some participants left the project because they saw the business plan AFI required as too much work. Some became frustrated with the lengthy process of revisions.

“With this program, it wasn’t just the thousand dollars [that helped me]... it was more the structure, the mind-set, and setting up the business plan.”
AFI project participant

POSTSECONDARY EDUCATION TRAINING

Neither site offered classroom-based assistance in planning postsecondary education. CNM required education-focused savers to work with their achievement coaches to develop an education plan before making a matched withdrawal. In Los Angeles, RISE staff reviewed financial aid information with savers

individually. Before the evaluation, RISE offered a seminar for education savers to help them identify career paths, but staff reported that participants did not need this.

OTHER PROJECT SERVICES

In addition to asset-specific training, the projects offered AFI participants advice on building and repairing credit, as well as general support related to the AFI IDA or to other issues, including referrals to other agencies and benefits.

Credit-building support. In Albuquerque, Prosperity Works pulled participants' credit reports and scores at study enrollment and one year later, and participants could discuss their credit with Prosperity Works staff. Prosperity Works offered individual or small-group credit workshops to all study participants on the CNM campus. In the workshop researchers observed, staff provided handouts and led an interactive session, with students commenting and asking questions throughout.

In Los Angeles, RISE required participants to work with staff to improve their credit scores and offered participants individual meetings to go over their credit reports. RISE staff asked participants to bring in credit reports, but many participants pulled the report at RISE with the help of a staff member.

"[We want participants] to know the importance of credit and how that's going to be the thing that moves them on to the next level.. whether they get a student loan, whether they buy a house, or get a car so they can transport themselves to work."

AFI project staff

Coaching and case management services. In Albuquerque, CNM achievement coaches guided participants through the project. They also connected students to resources such as financial aid, scholarships, and public benefits, and provided tutoring and mentoring. While coaching is available to all CNM students, staff said only some students take advantage of the services, and AFI offered a pathway to do so. The participants we spoke with said they had not visited an achievement coach before joining AFI, and one reported being more likely to use CNM Connect services and to read its e-mails since joining the project.

CNM achievement coaches used a coaching model, in which participants helped determine their course of action and the services and level of support they received, to engage with AFI participants and with students generally. According to CNM staff, this model treats participants as "creative, resourceful, and

whole.” Coaches do not tell people what to do or give advice, because the participant is seen as the expert in his or her own life.

“[Our coaching model] assumes these are whole and complete people who have lacked opportunity, and the coach’s job is not to tell you what to do but to reconnect you with your original vision and goal and problem-solve with you.”

AFI project staff

Albuquerque staff said participants themselves influenced how much support they received from coaches. Some participants met with their coaches monthly, showing proof of each deposit, while others met less frequently. Staff at both CNM and Prosperity Works reported that CNM coaches were overburdened and may not have provided AFI participants the support they needed. Coaches said that they could not support savers well because they were so focused on recruitment and non-AFI duties. In contrast, participants reported varying levels of engagement, but none indicated that the engagement was insufficient. They considered the individual support helpful, describing their coach as great to work with, honest, kind, and special. They said one particular coach was like “a friend who worked at the school,” and that the personal connection with her made all the difference.

As in Albuquerque, the Los Angeles project staff offered support in completing the project and provided information on outside resources, such as a list of mortgage lenders and real estate agents for home savers. In contrast to the coaching model in Albuquerque, interaction between staff and participants in Los Angeles was based on a more typical case management model, in which staff were more responsible for determining the clients’ needs and guiding them toward resources. Lack of individual time spent with staff was a problem noted by both staff and participants. RISE’s IDA manager relayed that she had less engagement with clients than when RISE was a partner under previous AFI grants and it was not as much as clients needed or wanted—she only contacted those who put in more effort. She said many participants needed very specific instructions on next steps or they would not move forward. In addition to individual case management, staff reported sending participants e-mail blasts with information on free training and events, along with monthly reminders to make a deposit. Reminders were sent by mail to those without an e-mail address.

Saving and Bank Services

OPENING IDAS

Processes for opening IDAs differed between the two sites. In Albuquerque, Prosperity Works had an ongoing arrangement with Wells Fargo that allowed it to automatically establish accounts for participants once they were accepted into the project. Wells Fargo did not deny an account to any participant. Prosperity Works did not consider a person enrolled in the project until he or she made a deposit. After Wells Fargo opened the account, participants picked up deposit slips from CNM Connect, and the bank sent Prosperity Works the participant's account number and monthly statements thereafter.

In Los Angeles, clients and staff faced many difficulties opening IDAs. Participants opened accounts themselves through a reportedly lengthy and cumbersome process, and an opening deposit of at least \$50 was required. Union Bank experienced delays setting up RISE's master account. Participants could not open accounts until August 2013, so the study's earlier enrollees could not begin saving in their IDAs for up to seven months. Further, some people were denied accounts at Union Bank.¹⁹ When participants opened accounts, RISE was not automatically able to view them through the online master account, nor did the bank mail statements to RISE. Bank staff had to enable RISE to see each account online, which either took a long time or did not occur. RISE was not aware of all accounts that had been opened and could not monitor all account activity.²⁰

SAVINGS AGREEMENTS

AFI participants at both sites filled out participant savings agreements, which included a target savings amount, a monthly savings amount, a period for saving, and the asset type they planned to purchase. In both sites, participants could change these original plans. In Los Angeles, all participants set a savings target of \$1,000, the amount the site required for participants to receive a match. The Albuquerque site did not require a \$1,000 goal; CNM coaches helped savers set appropriate savings goals.

¹⁹ According to bank staff and RISE staff, participants who had been reported for suspected fraud or had a "retail indicator" for writing bad checks could not open accounts. In addition, RISE staff said participants who owed money on taxes or had bank levies could not open accounts. RISE staff reported that 10 participants who appeared in ChexSystems—a check verification service and consumer credit reporting agency—were not permitted to open IDAs. RISE allowed participants to open accounts at other banks but had to rely on the participant to provide copies of statements in order to monitor the account.

²⁰ In 2015, the RISE online master account was deleted, and staff had no access to accounts. The bank restored online access in February 2016 for active accounts.

DEPOSITS

Both sites expected AFI participants to make deposits each month, though they permitted participants to make lower deposits than planned or skip a monthly deposit. When this occurred, participants would make up for it with a larger deposit later, extend the savings period, or (in Albuquerque only) reduce the savings goal. Participants could make deposits in person or through direct deposit, though staff at both sites said few used direct deposit. According to project rules in both sites, participants could be asked to leave the project if they missed three consecutive months of deposits, but in practice, this did not occur.

The Los Angeles site expected participants to contact RISE and explain why they were going to miss a deposit, and RISE would excuse it. Staff said participants usually had good reasons for missing a deposit, such as paying rent to avoid losing their housing. If participants had an emergency situation (e.g., job loss, medical needs), they could take a leave of absence for six months and not make any deposits. Staff said participants liked keeping money in their IDAs because they knew they could not withdraw it except for emergencies.

In Albuquerque, Prosperity Works monitored savers' accounts and notified coaches of savers who had missed deposits for three months. In interviews, Albuquerque participants said they met their monthly savings targets most or all of the time. Participants said the IDA was different than their past saving efforts because they had a specific dollar amount as a goal, which was a new and motivating challenge.

BANK SERVICES

The banks in both sites provided regular statements to savers. The bank in Albuquerque sent savers monthly statements; Prosperity Works staff also sent savers quarterly statements produced from their internal data system, which showed saved amounts and potential match amounts. The bank did not charge participants fees associated with their IDAs.

In Los Angeles, Union Bank sent savers monthly statements as long as they did not miss a monthly deposit; once a saver missed a deposit, the bank only sent quarterly statements. Many savers received only quarterly statements. The bank mistakenly charged some participants fees for their IDAs but later reimbursed them.

Withdrawals and Asset Purchases

MATCHED WITHDRAWALS AND ASSET PURCHASES

In Albuquerque, when participants were ready to purchase an asset, they met with their CNM achievement coach, who documented that they had completed the required steps. The coach sent a packet to Prosperity Works staff, who initiated the withdrawal of the saver's funds with the bank. Match funds were issued

directly by Prosperity Works. The withdrawn funds were paid directly to the mortgage company for a homeownership saver, the business account for a business saver, or the college, training institution, or college bookstore for an education saver. In general, Albuquerque savers said the withdrawal process was quick and straightforward.

In Los Angeles, when participants were ready to purchase an asset, RISE wrote them a check drawing on their IDA savings and the match funds that RISE contributed.

Both sites permitted partial withdrawals (of less than the full savings goal and match amount). In Albuquerque, participants could make partial withdrawals before they reached their savings goal, but in Los Angeles participants first had to reach their \$1,000 savings goal. Partial withdrawals were especially common in Albuquerque, because most participants were education savers, for whom partial withdrawals were common. Because in-state tuition at CNM was just \$600 per term, it was not possible to spend the full savings and match amount at once. In addition, staff said education savers often did not use the full match available because of the low tuition.

UNMATCHED WITHDRAWALS

Both sites permitted unmatched emergency withdrawals to cover rent or mortgages, medical bills, or other living expenses, with approval from project staff. Albuquerque staff described these withdrawals positively, noting that having emergency withdrawals available was a relief to participants, as they could meet their immediate needs. Albuquerque staff also noted that some participants made unmatched withdrawals because they no longer had a purpose for the match funds (e.g., they received a scholarship to cover tuition). Others withdrew funds and returned to saving once they could.

In Los Angeles, staff described unmatched withdrawals less favorably, explaining that savers who made emergency withdrawals were less likely to complete the project, as the need for emergency withdrawals reflected a precarious financial situation that would make completing the project more difficult. Some participants in Los Angeles may have made unmatched withdrawals because they did not wish to or could not wait for match funds to become available once RISE addressed its funding shortfall.

“There are going to be some families [who] need access to money very quickly just to cover basic needs. [IDAs] are good for specific asset goals, but they’re not the panacea. . they’re just one tool in the toolbox, in a very small toolbox that these people have access to.”

AFI project staff

Overarching Challenges

The large scale of the evaluated AFI projects and changes over time in the type of participants and the project intervention could affect participants' experiences, project outcomes, and evaluation results. Below, we further describe these issues.

SCALE OF PROJECT

Expanding the AFI projects to serve more participants was a challenge for both sites and had unintended consequences. The sites were accustomed to serving fewer participants at a time—about 15 at CNM Connect and 80 at RISE. Under the evaluation, each site enrolled about 150 to 250 AFI project participants and an equal number for the control group in a short time. This required new recruitment strategies and much more time and resources devoted to recruitment.

Both sites noted that because they could not meet enrollment targets by enrolling only the most project-ready participants, some participants were enrolled who staff believed were not as likely to succeed. Although this concerned project staff, all of the enrolled participants met the AFI eligibility criteria. Further, this broader population of AFI participants could lead to greater external validity of the evaluation results.

Albuquerque staff noted that in the past, CNM achievement coaches could select the most saving-ready students among those already receiving services from CNM Connect to participate in the AFI project. Under the study, coaches could not limit enrollment to students already involved with CNM Connect. In addition, CNM and Prosperity Works staff reported that those who enrolled later in the study tended to be less saving ready; in their view, early study enrollees' greater motivation and capability of completing the project application indicated a greater likelihood of project success.

In Los Angeles, RISE had to recruit from the general public, not just from the City of Los Angeles's Family Development Networks as it had in the past. In the opinion of one RISE staff member, about 25 percent of applicants were not ready to save. RISE did not turn these people away as they normally would, however, because of the need to enroll 500 sample members. RISE staff also reported not implementing additional enrollment requirements (e.g., a minimum income for home savers and business ownership of at least one year for business savers) that they thought would lead to more success.

Staff in both sites also noted that service provided to participants decreased because of the effort needed to recruit enough participants. Prosperity Works staff reported that the focus on enrollment prevented CNM coaches from providing participants with enough early savings plan counseling.

A RISE staff member stated there were too few staff and too many participants, so the level of service was much lower than in the past. Another staff member reported that RISE moved some communications

online to deal with the expansion, although there were limits to how much can be moved online because human touch was an important element of the project.

“We do as much as we can online, but you’re still dealing with people. . and at some point there’s this human touch that becomes important, meaning I’m not going to sit here and hold your hand but I’m going to sit here and talk to you.”

AFI project staff

CHANGES OVER TIME IN THE PROJECT INTERVENTION

In both sites, the intervention changed over time. The slower pace of enrollment in the study’s first year meant that staff in both sites could offer more individual attention to earlier enrollees.

In Los Angeles, there were other changes. With few exceptions, only enrollees who finished saving before May 2015 could make a matched withdrawal. Moreover, beginning in April 2015, RISE staff departures made it difficult for participants to get in touch with staff about withdrawals or other concerns. Another factor that changed the intervention was that participants who enrolled before August 2013 could not open accounts promptly, because of a delay in the bank’s setup of RISE’s master account. Financial education also differed by enrollment time: RISE required early enrollees to complete financial education within 45 days of enrolling, but later abandoned this requirement and participants simply had to complete the class before making a matched withdrawal. This delay in financial education for some participants may have affected their savings habits.

Project Services Received by the Treatment and Control Groups

In a randomized controlled trial, the percentage of treatment group members receiving project services should be as high as possible. Likewise, the percentage of control group members receiving project services should be as low as possible. To gauge the desired between-group differential in services, we monitored each site’s AFI project implementation, and we tracked their fidelity to random assignment. Below we describe first-year AFI project participation among those assigned to the treatment group (i.e., take-up). Participation included opening an IDA, taking financial education classes, and making deposits into and

withdrawals from the IDA. Then, we provide information on the extent of unintended project take-up among those in the control group, sometimes referred to as crossover.²¹

Treatment Group Participation

Key elements of AFI participation by the treatment group are presented below. These findings are based on the first-year follow-up survey, which included questions about the respondent's opening and use of an IDA since the date of random assignment, and the data collected and maintained by the study sites.²²

- **Over three-quarters (78 percent) of treatment group members opened an IDA** within 12 months of random assignment, according to project data.²³ The rate of account opening was higher in Albuquerque (91 percent) than Los Angeles (71 percent). This may have occurred because opening accounts was simpler in Albuquerque. The difficulty or delay in opening accounts for Los Angeles participants with a “debt indicator” or a problematic ChexSystems indicator in their credit history may have also been a factor.
 - » All IDAs in Albuquerque were opened by the end of the sixth month after study enrollment, but in Los Angeles, 11 percent of those opened were opened after six months or more had elapsed. For these participants, higher monthly deposits would be required for them to reach their savings goals in the month they originally planned.
- **Eighty-five percent of treatment group members participated in financial education**—83 percent in Albuquerque (the CNM course or another course) and 87 percent in Los Angeles (at RISE), among all assigned to the treatment group. These shares could rise over time; participants with longer savings periods may not have prioritized completing financial education within one year after enrollment.

²¹ The combination of a high program take-up rate for the treatment group and a low take-up rate for the control group provides a strong foundation for the program impact estimates presented later in this report. Our treatment-on-treated program effects adjust for the combination of treatment group nonparticipation and control group crossover.

²² We obtained project data from the sites on the timing of AFI IDA opening and financial education. The Albuquerque site also provided data on the timing and amount of deposits and withdrawals. We present estimates from the project data for items for which they are available for both sites, otherwise we present survey data. We use project data where possible for two reasons: (1) the project data cover all treatment group members except for two Los Angeles participants (405 total), and (2) the monthly project data allow us to tabulate information consistently at the 12th month after random assignment.

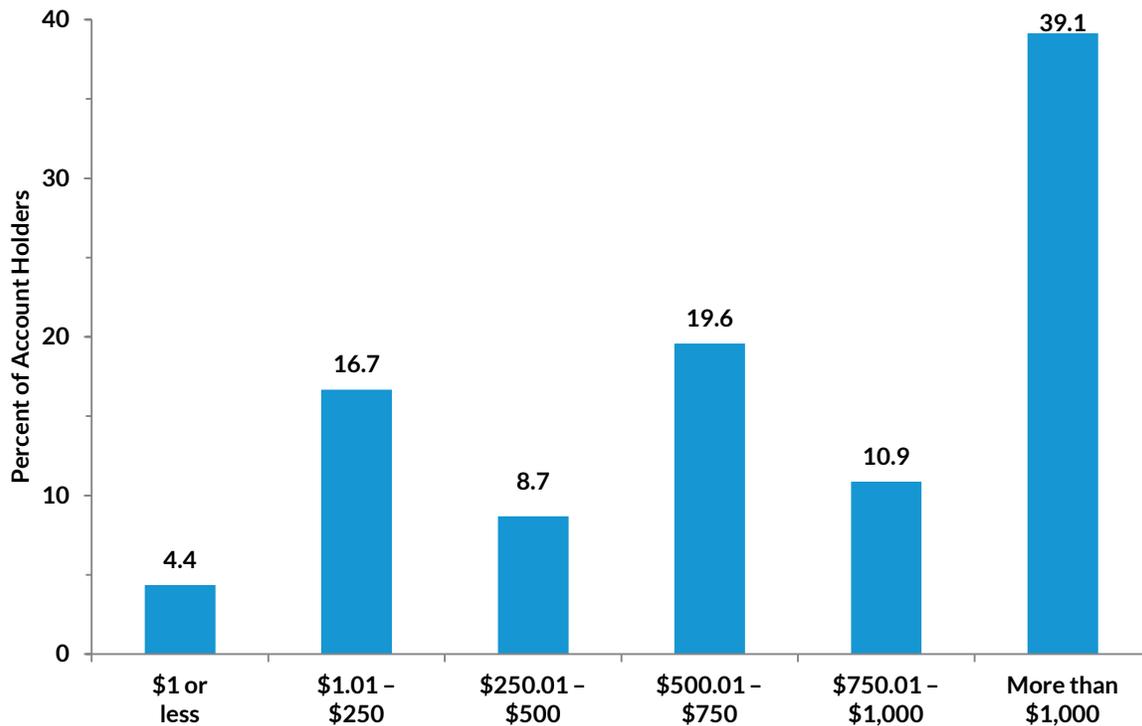
²³ In Albuquerque, an account is considered open if the site established the account for the participant and the participant made at least one deposit. In Los Angeles, account openings were undertaken by the participants and required an initial deposit.

- **Among treatment group members who opened an IDA, one-quarter (25 percent) made at least one matched withdrawal** within a year after study enrollment, according to the combined-sample survey data. More Albuquerque participants (43 percent) than Los Angeles participants (12 percent) made a matched withdrawal.
- **The low rate of matched withdrawals in both sites is expected** because many participants established savings periods of longer than 12 months, many in Los Angeles could not immediately open their IDAs, and the projects expected a time lag between reaching a savings goal and making a matched withdrawal. The low rate is also consistent with this study’s conceptual framework, in which early impacts are more likely to result from financial education and unmatched withdrawals than from matched withdrawals and asset purchases.
- AFI allows unmatched withdrawals for emergencies (i.e., to cover medical expenses, make rent or mortgage payments, or cover other necessary living expenses). According to survey data, **only 3 percent of treatment group account holders made any unmatched withdrawals** during the first year after study enrollment. Unmatched withdrawals were slightly more common in Albuquerque (5 percent) than in Los Angeles (2 percent). Unmatched withdrawals were made to make up for lost income, cover basic living expenses, or pay for car or house repairs.
- **For the 138 account holders in Albuquerque, mean cumulative IDA savings was \$691 and median savings was \$762**, according to project data. Such data were not available for Los Angeles.²⁴ These calculations are based on the sum of the year-ending IDA balance plus any matched withdrawals (participant savings portion only) made during the year. The distribution of cumulative IDA savings is shown in figure 4.1.

²⁴ There were delays accessing IDA information through the bank’s online banking system. By the time RISE could view this information, some IDAs had already been closed (and had no available online information) or had only partial information (as the online system only retained the most recent 18 months of data).

FIGURE 4.1

Share of Albuquerque Account Holders by Cumulative AFI IDA Savings Amount



Source: Authors' tabulations of project data provided by site staff.

Note: Estimates represent AFI IDA balances one year after random assignment, plus any matched withdrawals (participant savings portion only) made during the year.

Services Received by Control Group

INTENDED NON-IDA SERVICES

At each site, control group members had access to some services. As members of the general student population at CNM, control group members in Albuquerque could access all CNM Connect services (except the IDA), such as coaching and referrals to other resources. The control group could take the same financial education course at CNM as the treatment group, and they could access services provided by the SBDC and Homestart. Prosperity Works staff could pull the credit scores for control group members annually and offered credit workshops on the CNM campus that control group members could attend. Prosperity Works staff reported the control group members seemed more eager than treatment group members to attend the credit workshops, as they wanted to get something out of their study enrollment.

RISE offered fewer non-IDA services. They gave control group members a list of resources upon enrollment that included homebuyer workshops and referrals to agencies that assist small businesses. RISE

did not provide referrals for those interested in education, but staff gave them information on scholarships. RISE also told control group members about RISE's Capital Partners program, which provides low-interest loans for small business owners.

PREVENTING AFI PROJECT REENROLLMENT AND CROSSOVER

As part of informed consent, staff instructed study participants assigned to the control group that they could not reapply to the site's AFI project for three years. Both sites took steps to prevent control group members from reapplying and entering the project. According to survey data, few control group members opened an IDA at the sites: none in Albuquerque and three in Los Angeles (1 percent overall). Prosperity Works also monitored entry into its other partners' projects (and one other Albuquerque-based AFI project) to make sure control group members were not enrolling. (For most of the enrollment period in Albuquerque, there were no other active local IDA programs serving adults into which the control group might enroll.) In Los Angeles, staff said they could not monitor whether control group members enrolled in other local IDA programs.

Chapter 5: Analytic Approach

This chapter presents our analytic approach for obtaining AFI impact estimates. We also discuss statistical power and the implications of the available sample size for our results.

AFI Program Impact Estimates

The analyses are designed to estimate the early impact of AFI participation on such outcomes as savings, net worth, and material hardship. The basic method for estimating the impacts is to model the differences in mean outcomes between the treatment and control groups (intent-to-treat analysis). By basing the analysis on data collected from two randomly assigned groups, we estimate the causal effect of AFI. We thereby identify the effect of offering AFI services to all interested participants, including those in the treatment group who do not complete the program and those who never access any of its services. We focus on regression-adjusted impacts, to control for measured baseline differences between the treatment and control groups.²⁵ One exception is our analysis of credit scores. We received aggregate (group-level) credit score data only for Albuquerque, so we present difference-in-means estimates for that site only.

The intent-to-treat (ITT) impacts are generally obtained from regression models that control for the participants' sites and other individual- and household-level demographic and economic characteristics, including age, gender, race/ethnicity, educational attainment, marital status, English proficiency, household size, and annual household income at study enrollment.²⁶ Each model also includes the value of the outcome variable from the baseline survey to capture any differences in outcome values at study enrollment. The models also control for two study participation variables—the mode of the follow-up survey (phone, web, or in person) and enrollment cohort (whether the person entered the study in months 1 to 12, 13 to 16, or 17 to 19 of the study enrollment period). Appendix F presents details of the regression models.

For each outcome, regression models are estimated with data from the two sites separately and combined. We also test for differences between the program impacts in the two sites.

²⁵ This regression approach improves the precision of the impact estimates. In addition, even with random assignment, there are a few statistically significant differences in the baseline characteristics of treatment and control group members (see appendix E). The regression models control for these differences.

²⁶ One exception is the quantile regression models, which we estimate for liquid assets. The quantile regressions only include the baseline value of the dependent variable. We exclude the other baseline demographic and economic characteristics from these models because including them does little to improve or substantially reduces the precision of the impact estimates, and none of the coefficients on the baseline characteristics are statistically significantly different from zero at the 10 percent level.

When examining several outcomes, some impact estimates may achieve statistical significance by chance. Our analysis also tests for statistical significance taking account that some domains include multiple outcomes. We adjust the statistical significance in the six domains that have multiple outcomes using a procedure developed by Benjamini and Hochberg (1995). In chapter 6, we note outcomes that lose statistical significance (i.e., are no longer statistically significant at the 10 percent level) when we adjust for multiple outcomes.

In addition to this ITT analysis, we report effects for the subpopulation of people who participated in AFI—that is, who made at least one deposit into the AFI IDA—when assigned to the treatment. This treatment-on-treated (TOT) analysis considers that not all people assigned to the treatment group participated in AFI services. While the ITT analysis provides unbiased estimates of offering the AFI program, we carry out the TOT analysis because program operators and policymakers are frequently interested in knowing the impact on participants who receive the treatment. Under this evaluation, the treatment group's nonparticipation rate produces TOT estimates that are 16 percent larger than the ITT impact estimates. The levels of statistical significance are the same in the TOT and ITT results. Appendix F presents our approach for obtaining the TOT estimates and a discussion of the TOT results.

Statistical Power: Minimum Detectable Effects

In interpreting the estimated effects, we acknowledge the limitations of the available sample size. The adequacy of a sample is normally expressed as the minimum size of an effect that can be detected with 80 percent probability and 10 percent confidence level: the minimum detectable effect (MDE). We have calculated the MDEs for two illustrative first-year outcomes: one binary measure and one continuous measure (appendix F). The calculated MDEs, expressed as a percentage of the corresponding control group mean, are in the 20 to 25 percent range for combined-site estimates. The analysis sample is sufficient to detect program effects normally considered of moderate size or larger.

Chapter 6: First-Year AFI Program Impacts

In this chapter, we present the estimated first-year impacts of AFI participation in the two primary domains (savings as measured by liquid assets, asset ownership) and nine secondary domains (net worth, means-tested benefit receipt, material hardship, use of alternative financial services, earnings and income, credit score, personal outlook, community involvement, and time preference). For the two sites combined, we present the regression-adjusted impacts and the associated levels of statistical significance for each outcome except credit score, along with the regression-adjusted sample means for the treatment and control groups. We also present the regression-adjusted program impacts for the Albuquerque and Los Angeles sites separately.²⁷ For credit scores, we only received aggregated treatment and control group scores for the Albuquerque site, so we present difference-in-means estimates for Albuquerque only. We highlight impact estimates that are statistically significant at the 1 percent, 5 percent, and 10 percent levels, but we interpret significance at the 10 percent level as providing only suggestive evidence of AFI impacts.²⁸

Overview

AFI had several beneficial effects on participants roughly one year after study enrollment. Among the primary outcomes, AFI led to a 7 percentage point (9 percent) increase in the share of participants with liquid assets and an increase in the liquid asset amount. We estimate that AFI increased mean liquid assets by \$799 and median liquid assets by \$657. By capturing all liquid assets, the findings indicate that AFI is creating new savings; participants are not simply shifting liquid assets from one type of account (e.g., savings account) into an IDA. Consistent with our conceptual model (which hypothesizes increased asset ownership only in the longer term), AFI did not increase participants' early asset ownership—homeownership, business ownership, or postsecondary education or training.

Among the secondary outcomes, AFI reduced material hardship and the use of nonbank check-cashing services. Using a composite measure of material hardship (housing, utility, and medical hardship), we estimate that AFI reduced the number of hardships by 34 percent in the 6 to 12 months after study enrollment. We further estimate that AFI reduced the likelihood participants used a nonbank check-cashing

²⁷ The regression-adjusted sample means for the Albuquerque and Los Angeles sites are presented in appendix G.

²⁸ At the 10 percent level, there is less than a 10 percent chance that AFI has no effect on the outcome in question and we still find a statistically significant impact.

outlet by 41 percent in the year after study enrollment. We also find some evidence that AFI improved participants' perceived financial security. We describe the early impacts of AFI within each of the 11 domains below.

Primary AFI Outcomes

Savings as Measured by Liquid Assets

AFI produced early increases in the share of participants who had any liquid assets and the amount of their liquid assets, where liquid assets includes amounts in savings and checking accounts, money market accounts, stocks, bonds, and retirement accounts, among other savings. We find positive impacts of AFI on liquid assets across multiple measures, providing strong evidence that AFI achieves its primary goal of increasing savings.

AFI led to a 7 percentage point (9 percent) increase in the share of participants with any liquid assets one year after study enrollment (table 6.1). With this increase, the vast majority—nearly 90 percent—of treatment group members had some liquid assets roughly one year after study enrollment. The program impacts are similar in the two sites—7 percentage points in Albuquerque and 9 percentage points in Los Angeles—with over 85 percent of treatment group members in both sites having at least some liquid assets at the first-year follow-up.

The increase in liquid assets at the first-year follow-up is substantial, particularly given the \$1,000 match cap in the two sites. AFI increased participants' liquid assets by an average of \$799, an increase of 35 percent over control group members. This increase occurred over roughly one year and corresponds with a monthly increase in savings of \$67. AFI increased the median (50th percentile) level of liquid assets by \$657 (\$55 per month).

We also examine the impact of AFI at the 75th and 25th percentiles of liquid assets. The effect of AFI at the 75th percentile of liquid assets is \$1,089, or \$91 per month. The estimated effect of \$173 at the 25th percentile is not significantly different from zero.

TABLE 6.1

Impacts of AFI Program on Liquid Assets and Asset Ownership at First-Year Follow-Up

Primary outcomes	Sites Combined			P-value	Albuquerque	Los Angeles
	Treatment ^a	Control ^a	Impact ^b		Impact ^b	Impact ^b
Liquid assets (participant and spouse/partner)						
Has liquid assets (%)	88.6	81.1	7.4 ***	0.008	7.1 *	8.7 **
Liquid asset amount (\$)	3,070	2,271	799 *	0.091	872	594
Natural log of liquid assets (\$)	6.3	5.3	1.0 ***	0.000	1.0 ***	0.9 ***
Liquid assets (\$)						
25th percentile	177	4	173	0.324	120	214
50th percentile	881	224	657 ***	0.005	667 **	685 **
75th percentile	2,834	1,745	1,089 ***	0.008	1,433 **	820
Asset ownership (participant or participant and spouse/partner)						
Homeownership (%)	10.4	10.4	0.0	0.984	2.5	-1.1
Business ownership (%)	20.4	24.0	-3.6	0.210	-3.8	-4.9
Education (%)	23.0	22.7	0.3	0.924	3.1	-0.9

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means and quantile values at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). These models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race and ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). Quantile regressions for liquid assets are estimated at the 25th, 50th, and 75th percentiles of the liquid asset distribution and only control for the baseline value of liquid assets (see appendix F for more detail). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

An alternate specification, where the outcome variable is measured as the natural log of liquid assets, provides additional support that AFI increases liquid assets. We estimate this model because some people have very high liquid asset amounts, which disproportionately affect the estimated impact.²⁹ A key feature of the natural log model is that it gives less importance to high liquid asset values compared with the model that examines the level (i.e., dollar value) of liquid assets. Results from this model suggest that AFI nearly tripled participants' liquid assets on average (a 180 percent increase) at the one-year follow-up.³⁰

These findings differ from the short-term (18-month) ADD findings, which found no effect of the ADD IDA on liquid assets (Mills et al. 2004). Beyond the difference in follow-up period (12 versus 18 months), ADD focused on a lower-income population (below 150 percent of the federal poverty level).

²⁹ The mean value of liquid assets is \$2,700, but values go as high as \$35,000. The natural log model is often used for dollar-valued outcomes that have this distributional feature, such as income, earnings, and wealth. Because the natural log of zero is not defined, we add \$1 to each person's liquid assets and then take the natural log (i.e., natural log[liquid assets+1]).

³⁰ To translate the impact estimate (i.e., coefficient) from the natural log regression model (β_1) to a percentage change, the calculation is $(100 * (e^{\beta_1} - 1))$. For the cross-site impact estimate of 1.03, the percentage change is 180 percent.

Our separate analyses by site find significant impacts of AFI on liquid assets, but not for all the measures. Results from the natural log model suggest that AFI increased participants' liquid assets by an average of 181 percent in Albuquerque and 155 percent in Los Angeles.³¹ We also find that AFI increased the median level of liquid assets by \$667 in Albuquerque and \$685 in Los Angeles. When examining the level of liquid assets, the program effects are not significantly different from zero in either Albuquerque or Los Angeles, although the estimated magnitudes of the savings increases are large (\$872 and \$594, respectively). These impact findings are consistent with AFI project staff interviews in the two sites. Staff in both sites reported that AFI helped participants develop the habit of saving.

"They have such limited resources that [saving is] not anything they would have [gone] outside of the box to do on their own. But the match has inspired them to do that because it grows so much faster...and it's something you can actually use at the end that's substantial, whereas if they just put \$20 aside to start with...it would most likely never work out...we've heard them say 'I've tried to save, but I just can't.'"

AFI project staff

These findings suggest that the financial incentives designed to encourage AFI participants to save do just that. Participants are not simply shifting liquid assets from other types of accounts (e.g., savings accounts) into the AFI IDA. Total liquid savings increased, not just AFI IDA savings. Additionally, AFI is helping the typical AFI participant (at the median) save, not just better-off participants whose savings are more likely to be reflected in the mean.

Asset Ownership

The first-year results provide no evidence that AFI increases participants' early asset ownership—homeownership, business ownership, or postsecondary education. No significant program effect emerges for the combined sample or for either site separately (table 6.1). This finding is consistent with the hypotheses from our conceptual model. While asset ownership is a primary goal of AFI, we expect AFI to affect asset purchases only in the longer term, particularly because the participants have 24 months or

³¹ The impact estimates of 1.04 in Albuquerque and 0.94 in Los Angeles translate into increases of 181 percent and 155 percent, respectively.

longer to save for their purchases. Our finding is consistent with findings from the short-term (18-month) ADD, which found that ADD IDAs had no effect on homeownership, business start-up, or postsecondary education after 18 months (Mills et al. 2004).

Secondary AFI Outcomes

Net Worth

The effect of AFI on net worth is expected to take more than one year to emerge, particularly as it relates to the return on investments in homeownership, business ownership, and postsecondary education. However, AFI could affect net worth in the first year; a participant's net worth could increase because of additional savings or decrease if the participant takes out a student loan to meet college costs beyond those from the matched withdrawal. Our results suggest that AFI did not affect participants' net worth in the first year. We find no evidence that AFI affected net worth for the combined sample or for either site separately (table 6.2)

Material Hardship

We examine whether AFI reduces material hardship, measured by whether participants experienced reductions in four types of hardship—food, housing, utilities, and medical care. We focus on hardships experienced approximately 6 to 12 months after study enrollment (i.e., in the 6 months before the first-year follow-up survey) because we hypothesize that AFI can reduce material hardship via unmatched hardship withdrawals, which can only happen after participants have built up some savings. AFI can also reduce material hardship if project staff help participants connect or stay connected with public benefits.

For each type of hardship, we examine whether study participants experienced the hardship in the six-month period. We also create a composite hardship measure that captures whether they experienced any of the four hardships. To gauge the degree of hardship, we also examine the number of times participants experienced a housing, utility, or medical hardship, along with a second composite measure that captures the number of times participants experienced any of these hardships.³² In total, we examine nine material hardship measures.

³² We did not ask survey respondents to provide information on the number of times they experienced a food-related hardship.

TABLE 6.2

Impacts of AFI Program on Net Worth, Material Hardship, Benefit Receipt, and Alternative Financial Service Use at First-Year Follow-Up

Secondary outcomes	Sites Combined			P-value	Albuquerque	Los Angeles
	Treatment ^a	Control ^a	Impact ^b		Impact ^b	Impact ^b
Net worth (participant and spouse/partner)						
Net worth (\$)	-25,511	-10,950	-14,561	0.255	-12,585	-22,831
Material hardship (household)						
Any hardship (%)	52.4	55.8	-3.5	0.369	7.7	-10.5 **
Number of hardships (#)	1.8	2.8	-1.0 **	0.016	-0.5	-1.4 **
Food hardship (%)	18.2	19.0	-0.8	0.795	2.3	-3.4
Housing hardship (%)	15.6	16.7	-1.2	0.684	2.3	-3.5
Housing hardship (#)	0.4	0.4	-0.1	0.338	0.0	-0.2 *
Utilities hardship (%)	28.7	29.0	-0.3	0.932	2.5	-2.4
Utilities hardship (#)	0.7	1.1	-0.4 ***	0.010	-0.3	-0.6 ***
Medical hardship (%)	33.7	40.1	-6.4 *	0.079	1.5	-12.6 ***
Medical hardship (#)	1.1	1.5	-0.4	0.108	-0.1	-0.7 **
Means-tested benefit receipt (household)						
Receive benefits (%)	73.7	66.9	6.8 **	0.036	3.9	8.1 *
Alternative financial service use (participant and spouse/partner)						
Use AFS credit (%)	14.3	15.7	-1.4	0.576	-4.7	0.2
AFS credit (#)	0.5	0.5	0.1	0.615	0.1	0.0
Use nonbank check cashing (%)	5.8	9.5	-3.7 *	0.069	1.1	-7.4 **
Nonbank check cashing (#)	0.2	0.3	-0.1 *	0.069	0.0	-0.2 *

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race and ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). There are statistically significant differences between the Albuquerque and Los Angeles impact estimates for any hardship (%), has medical hardship (%), and use nonbank check cashing (%).

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

The results provide some evidence that AFI reduces material hardship. For the combined sample, AFI reduced material hardship for three of the nine hardship measures.³³ Focusing first on the two composite measures, we find a significant reduction in material hardship using the intensity measure only. Specifically, AFI reduced the average number of hardships by 1 (34 percent; table 6.2). For specific types of hardship, AFI reduced the average number of utility hardships by 0.4 (38 percent). AFI also reduced the likelihood of experiencing a medical hardship by 6 percentage points (16 percent).³⁴

³³ With nine material hardship outcome measures, we expect roughly one to be statistically significantly different from zero (at the 10 percent level) by chance.

³⁴ After adjusting for multiple comparisons, the estimated impact on the likelihood of experiencing a medical hardship is no longer statistically significantly different from zero.

Very different patterns emerge across the two sites. In Albuquerque, we find no statistically significant declines in material hardship. In Los Angeles, however, AFI significantly reduced material hardship for six of the nine outcome measures. Focusing on the composite measures, the AFI project in Los Angeles reduced the likelihood of experiencing any hardship by 11 percentage points (17 percent) and reduced the average number of hardships experienced by 1.4 (44 percent). We are better able to discern program impacts when we examine the more refined hardship measures that capture intensity (i.e., number of hardship occurrences) than when we measure whether any hardship occurred. The results provide evidence that AFI reduced the number of housing hardships by 0.2 (45 percent), the number of utilities hardships by 0.6 (48 percent), and the number of medical hardships by 0.7 (41 percent). They also provide evidence that AFI reduced the likelihood of medical hardship by 13 percentage points (29 percent).³⁵

Means-Tested Public Benefit Receipt

In collecting information on household income in the month before the first-year follow-up survey, we obtained information on study participants' receipt of means-tested benefits, including cash assistance (e.g., Temporary Assistance for Needy Families, Supplemental Security Income, Supplemental Nutrition Assistance Program and other food-related assistance, housing assistance, and Medicaid). AFI could reduce benefit receipt by providing an incentive for participants to earn more so they can save more and qualify for more matched funds. Or, AFI could increase benefit receipt if AFI participants use public benefits to help them make ends meet while they save for a longer-term investment or work toward a postsecondary degree.

Evidence indicates that AFI helped participants maintain means-tested benefit receipt. Across the two sites, the share of study participants who received at least one form of means-tested benefits in the month before completing the first-year follow-up survey was 7 percentage points (10 percent) higher among AFI participants versus nonparticipants (table 6.2). With this increase, 74 percent of treatment group members lived in a household that received benefits roughly one year after random assignment. These results, in conjunction with our evidence that AFI reduces material hardship, suggest that mean-tested benefits help AFI participants avoid material hardship as they work to save for their long-term investments.

In separate analyses by site, we find a statistically significant difference in benefit receipt in Los Angeles, with treatment group members having higher levels of benefit receipt (by 8 percentage points, or 12 percent) than control group members. Interestingly, a descriptive analysis of the baseline and one-year

³⁵ After adjusting for multiple outcomes, the estimated impact on one of the hardship outcomes in Los Angeles—number of housing hardships—is no longer significantly different from zero.

follow-up survey data in Los Angeles suggests that the higher level of benefit receipt among the treatment group (i.e., AFI participants) versus control group results from treatment group members staying connected to benefits, not from an increase in benefit receipt among the treatment group. Benefit receipt among Los Angeles treatment group members was relatively flat over time (i.e., between the baseline and follow-up surveys), while benefit receipt fell for control group members. It appears that AFI helps keep people connected with public benefits while they work toward their saving goals. Public benefits may be an important element in helping AFI participants avoid material hardship as they save for their asset purchases.

No significant difference emerged in the level of benefit receipt between the treatment and control group members in Albuquerque. A descriptive look at the survey data shows that benefit receipt increased slightly over time for both the treatment and control groups in Albuquerque. The AFI project in Albuquerque was delivered by CNM Connect, an on-campus student resource center offering students multiple services, including help accessing publicly funded benefits, so the control group members in Albuquerque could have been connected with benefits via this resource.

Use of Alternative Financial Services

We examine whether AFI led to reductions in the use of AFS credit products (i.e., payday, auto title, and pawnshop loans) and alternative nonbank check-cashing services. Similar to our analysis of material hardship, we examine whether study participants used these services and the number of times they used such services, separately for use of AFS credit and AFS check-cashing services.³⁶ We measure use in the past year, which approximately covers the year after study enrollment.

For lower-income people, alternative products and services are generally easier to access than traditional financial services (e.g., from a bank or credit union), but they can be more expensive. Alternative credit products, such as payday loans, can also trap consumers in a cycle of debt. AFI could reduce the use of AFS products by connecting participants to mainstream financial services, so they cash checks at a bank rather than a nonbank check-cashing outlet,³⁷ and by providing lessons in financial education classes on how to maximize savings and avoid costly financial products. AFI could also reduce participants' need for credit because they can fall back on their IDA savings if a financial emergency arises. Offsetting this are findings

³⁶ Intensity of AFS use in the past year was collected in a categorical variable: 0, 1, 2 to 4, or 5 or more times. See table 3.3 for details.

³⁷ In the Albuquerque site all the accounts are owned by Prosperity Works as a custodian (at Wells Fargo). If an AFI IDA participant went into the bank without his or her account number to cash a check, the teller could not identify the participant as a customer.

that suggest that some people take out high-cost loans (e.g., payday loans) even when they have savings because they don't want to draw down their emergency funds (Levy and Bianchi 2013).

The results provide suggestive evidence that AFI led to reduced use of nonbank check-cashing services but no evidence that AFI reduced the use of high-cost AFS credit products. AFI led to a 4 percentage point (39 percent) reduction in the share of participants who used nonbank check-cashing services (table 6.2). With this decline, 6 percent of treatment group members used nonbank check-cashing services in the year after study enrollment, compared with 10 percent of control group members. For our measure of check cashing that captures the intensity of use (from 1 to 5), AFI reduced the use of nonbank check-cashing services by an average of 0.1 points. Although a modest decline in absolute terms, it represents a 41 percent decline compared with the control group.³⁸

For each site individually, AFI had no impact on AFS credit product use, but Los Angeles showed a statistically significant decline in the use of nonbank check-cashing services. We estimate that the AFI project in Los Angeles led to a 7 percentage point (57 percent) reduction in the share of participants who used check-cashing services and reduced the intensity of use (scale from 1 to 5) by an average of 0.2 points (44 percent).³⁹

An examination of study participants' baseline data, which were collected when they applied for their AFI IDAs, shows that a higher share of Los Angeles participants reported using nonbank check-cashing services (10 percent) than did Albuquerque participants (5 percent). Thus, AFI could have a bigger impact on the check-cashing behavior in Los Angeles than in Albuquerque. The more frequent use of nonbank services in Los Angeles could be because Los Angeles had fewer banked participants (proxied by having liquid assets) versus Albuquerque. At baseline, 17 percent of Los Angeles participants and 12 percent of Albuquerque participants had no liquid assets.

Employment, Earnings, and Income

AFI is not an employment intervention, so we do not expect the program to directly affect participants' employment or earnings. However, AFI could have secondary effects on these outcomes through AFI's matched savings component. AFI could increase earnings by incentivizing participants to earn more so they have more savings to qualify for match funds. Our analyses provide no evidence that the AFI program

³⁸ None of these of treatment effects are significant at the 10 percent level after adjusting for multiple comparisons.

³⁹ After adjusting for multiple outcomes in the Los Angeles analysis, AFI led to a significant decline in the likelihood of using nonbank check-cashing services but not in the number of times participants used these services.

increased employment or earnings at the first-year follow-up (table 6.3).⁴⁰ We find no program effects for the combined sample or for either site.

However, the results provide some suggestive evidence at the first-year follow-up that the AFI program increased household income. AFI exhibited no significant impacts on the level (or dollar value) of income but significantly increased household income in models that examine the natural log of monthly household income. Specifically, we estimate that AFI increased participants' monthly household incomes by 48 percent. While large in magnitude, this impact estimate is statistically significant only at the 10 percent level.

TABLE 6.3

Impacts of AFI Program on Employment, Earnings, Income, and Credit Score at First-Year Follow-Up

Secondary Outcomes	Sites Combined				Albuquerque	Los Angeles
	Treatment ^a	Control ^a	Impact ^b	P-value	Impact ^b	Impact ^b
Employment (participant) and earnings and income (household)						
Employed (%)	76.3	76.7	-0.4	0.913	1.8	-0.3
Monthly earnings (\$)	1,507	1,611	-104	0.521	53	-221
Natural log of monthly earnings (\$)	5.7	5.4	0.3	0.259	0.7	0.2
Monthly income (\$)	1,901	1,769	132	0.471	70	225
Natural log of monthly income (\$)	6.7	6.3	0.4 *	0.086	0.6	0.5 *
Credit score (participant)^c						
Vantage 2.0 score (501-990)	—	—	—	—	3.0	—
Change in Vantage 2.0 score (#)	—	—	—	—	5.5	—

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using a probit model for the binary outcome variable (for which we report the marginal effects). All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race and ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

^c Credit score is the Vantage 2.0 scores for all randomly assigned participants in Albuquerque and is not regression adjusted.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Looking separately at each site, we find evidence that AFI increased participant incomes in Los Angeles (by 64 percent) but not in Albuquerque. This result is consistent with the increase in public benefit receipt in Los Angeles only.

⁴⁰ Participants' employment status is measured at the time of the first-year follow-up survey, and household earnings are measured in the month before the follow-up survey.

Credit Score

AFI could improve credit scores in the short run via actions taken as a result of financial education or other financial counseling provided by AFI staff. We expect increases in credit scores in the longer run.

Participation in AFI could improve credit scores through timely payment on a loan taken out in conjunction with buying a home, capitalizing a small business, or advancing one's postsecondary education (such loans are not required).

With the limitation of aggregated credit scores from the Albuquerque site only, there is no evidence that AFI increased participants' credit scores in the first year after study enrollment (table 6.3). The treatment-control differential is in the expected positive direction (i.e., higher credit scores for the treatment versus the control group), but the differentials are not statistically significant and the magnitudes are very small (an increase of less than 6 points on a score that ranges between 501 and 990).

Personal Outlook

In the first year, we hypothesize that AFI can improve participants' personal outlook, measured by financial well-being, self-esteem, and physical health, via increased savings. Results from these analyses provide evidence that AFI improves participants' sense of financial security, but not their physical health or self-esteem.

AFI exhibits positive impacts on three of the four financial well-being outcomes (table 6.4). Based on a question that asks respondents how easy or hard it is to make ends meet, we estimate no statistically significant increase in the ability of treatment group members to make ends meet. However, using a 10-point scale that measures respondents' level of worry over their ability to meet normal monthly living expenses (where 1 indicates that they worry all the time and 10 indicates that they never worry) shows that AFI increased the score of treatment group members by 0.5, which is 10 percent higher than the score of control group members.

This is a promising finding because the increase in liquid savings among AFI participants could have led AFI participants to report more worry over meeting monthly living expenses. The higher level of public benefit receipt among AFI participants could be helping people offset some of the strain from making monthly deposits into their AFI IDAs.

AFI also improved participants' perceptions of how their financial situation changed in the past year. AFI led to a 7 percentage point (19 percent) increase in the share of participants who reported that their financial situation has improved and a 10 percentage point (38 percent) decline in the share of participants

who reported that their financial situation has worsened. The increase in liquid assets is likely contributing to this improved sense of financial security.⁴¹

TABLE 6.4

Impacts of AFI Program on Personal Outlook, Community Involvement, and Time Preference at First-Year Follow-Up

Secondary outcomes	Sites Combined			P-value	Albuquerque	Los Angeles
	Treatment ^a	Control ^a	Impact ^b		Impact ^b	Impact ^b
Personal outlook (participant)						
Ability to make ends meet (%)	53.0	47.3	5.7	0.121	4.3	5.0
Perceived financial security (1–10)	5.4	4.9	0.5 **	0.041	0.3	0.7 **
Better off financially (%)	44.1	36.9	7.2 *	0.064	7.0	9.1 *
Worse off financially (%)	15.4	25.0	-9.6 ***	0.002	-17.1 ***	-6.1
Good health (%)	75.8	75.5	0.4	0.912	-2.5	2.1
Self-esteem (0–30)	23.1	23.2	-0.1	0.817	-0.2	0.0
Community involvement (participant)						
Community involvement (%)	57.3	58.1	-0.7	0.845	-4.6	1.9
Time preference (participant)						
Present-oriented time preference (0–1)	0.1	0.1	0.0	0.134	0.0	0.0

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race and ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

There are differences across the two sites. In Los Angeles, we estimate that AFI increased participants' perceived financial security and the share of participants who reported that their financial situation has improved.⁴² In Albuquerque, AFI decreased the share of participants who reported that their financial situation has worsened. No other evidence indicates improvements in Los Angeles or Albuquerque.

Interviews with AFI project staff and participants reinforce the finding that AFI participants feel better off financially. A staff member at the Albuquerque site said participants feel more in control of their finances. Participants in both sites reported having learned to make better financial choices. A participant in

⁴¹ After adjusting for multiple comparisons, only the decline in the share reporting that their financial situation has worsened is statistically significant.

⁴² These results are not significant at the 10 percent level after adjusting for multiple outcomes.

Los Angeles said the AFI project allowed her to own up to poor past financial choices without feeling shame or letting them define her future path. An Albuquerque participant reported gaining a sense of security about paying for college. Another, in a written testimonial completed after project completion, remarked that she could “build [her] credit and keep [herself] out of trouble financially.”

“I did feel financially helpless when I entered [the project] and now I feel like my life truly changed. . . I look forward to no longer living paycheck to paycheck and realizing personal dreams!”

AFI project participant written testimonial

While AFI led to improvements in financial security, the results provide no evidence that this translated into improvements in physical health or self-esteem. Beyond being statistically insignificant, the magnitudes of the estimated impacts are very small. We hypothesize that improvements in physical health would result from lower financial stress. However, our measure of physical health—health is good, very good, or excellent—may not be sufficiently detailed to capture changes in physical health resulting from changes in stress levels.

Community Involvement and Future-Oriented Time Preference

We hypothesize that AFI can shift participants’ preferences away from immediate consumption purchases to future investment purchases within the first year of entering the program. We do not, however, hypothesize that AFI will increase participants’ community involvement in the first year, as we expect greater community involvement to emerge only after asset purchases, such as from setting down roots via homeownership. We find no evidence that AFI shifts participants toward a more future-oriented financial perspective or increases community involvement in the first year, for the combined sample or for either site separately.

Chapter 7: Conclusions and Implications

This chapter draws conclusions from the key early impact evaluation findings, interprets the results in light of earlier IDA research, and suggests potential implications for AFI policy and practice.

Emerging Insights on the Role of AFI

The major first-year impact findings—that AFI participation resulted in higher levels of savings (liquid assets), increased receipt of means-tested benefits, fewer material hardship occurrences, and improved perceptions of one’s financial situation—provide strong empirical support that the AFI program promotes participants’ economic well-being. Additionally, the study’s finding that AFI participation reduced AFS check-cashing use suggests that AFI may help people enter the financial mainstream. This evidence provides new insights about AFI beyond the findings of earlier research.

“A young woman who is close to aging out of the foster system...comes back to pick up her bank passbook in order to make her first deposit. She had a small paycheck. ‘Can you help me with something,’ she asked ... She asked if I can teach her (a) how to make a bank deposit, and (b) what to do with the check. Yup—I got a little choked; gave her a hug and we entered a banking 101 conversation.”

AFI project staff

The significant early effect on liquid assets—found at both study sites and in the combined sample—had not been found in the early (18-month) impact study conducted at the Tulsa, Oklahoma, large-scale experimental site under the American Dream Demonstration (Mills et al. 2004). Our finding suggests that AFI IDA deposits represent new savings by the account holder’s household, rather than liquid assets shifted into the IDA from other sources. IDAs are meant to cause this change in household economic behavior. If AFI IDA balances instead represented asset shifting, one could argue that the program was equivalent to a

direct subsidy or grant to people investing in their own homes, businesses, or postsecondary educations, and could be delivered more cost-effectively than through an IDA program.

The increase in means-tested benefit receipt—significant in the combined sample and the Los Angeles site—is another early impact not found in the Tulsa ADD evaluation, where “household receipt of public assistance” was a measured outcome. Nor was this effect found in the national AFI nonexperimental evaluation, where “receipt of means-tested benefits” was a third-year outcome measure (Mills, Lam, et al. 2008). This new finding sheds light on the potentially important role of the AFI program in an extended public-private service delivery network, connecting low-income individuals and families to benefits they qualify to receive and helping recipients cope with the procedural requirements for retaining their benefits. The presence of this impact in Los Angeles suggests that local community-based organizations (who already partner with public agencies and nonprofit social service providers and who tend to adopt a case management approach with their clients) may be well positioned to serve this connector role.

The reduction in material hardship—again, significant in the combined sample and for Los Angeles—is a major new insight in IDA research. Neither the randomized impact study at the Tulsa ADD site nor the national AFI nonexperimental impact evaluation addressed this outcome domain. A contrary finding had emerged from the in-depth qualitative research conducted under ADD: some IDA participants had forgone medical services and dental care to achieve savings goals and use their match funds. Our finding of reduced hardship suggests that for AFI participants, increased savings need not cause reductions in consumption spending that put households at greater risk of hardship.

Given that the material hardship reductions estimated here were more pronounced in Los Angeles, means-tested benefit receipt may be the mechanism through which Los Angeles participants were better able to avert hardship. If instead AFI IDA deposits had been the source of funds to meet emergency spending needs, we would have expected a higher share (than the survey-estimated 2 percent) of treatment group members making unmatched withdrawals in their first year. Other possible explanations are that reduced material hardship may come via financial education and coaching (e.g., help with budgeting or credit repair) or from the use of funds that participants had intended to deposit into their AFI IDAs (resulting in below-target deposits in some months). Whatever the cause of the decline in material hardship, the finding suggests that AFI participants achieved greater financial stability.

This study’s findings related to improved perceptions of one’s financial situation also contribute new understanding of IDAs. Among several related outcome measures, significant effects emerged in the first year at both sites and in the combined sample. Perhaps most noteworthy is the effect on perceived financial security, based on a survey question about how often people worry about being able to meet monthly living expenses. The significant reduction in financial worry found in the combined sample and in Los Angeles is important in the context of behavioral economics research on economic scarcity and cognitive depletion

(Mullainathan and Shafir 2013). Financial stress and worry tend to sap the cognitive resources of families during periods of urgent economic need, when families can least afford to make poor choices. Reduced worry is also important in the context of toxic stress and its impact on children. According to the American Academy of Pediatrics, toxic stress in early childhood can lead to permanent changes in the structure and function of the brain; these brain alterations can “create a weak foundation for later learning, behavior, and health” (Shonkoff et al. 2012, e236). Our finding that AFI participants are less burdened by financial worry becomes even more salient when viewed in these contexts. The finding also suggests that the AFI program may help improve participants’ financial health and well-being.⁴³

Potential Implications for Policy and Practice

The evaluation’s findings that AFI increased savings suggest that the program achieved a major short-term goal. The findings further suggest that reductions in material hardship happen because AFI project participation helps eligible participants save for emergencies and connect or stay connected to public benefits. These findings highlight three implications. One is that the AFI program can help integrate service delivery systems. A second is that disregarding savings from AFI IDAs when determining benefit eligibility may enable low-income families to save and avoid hardship. A third is that understanding how participation reduced hardship could be important for program design.

Integrated Service Delivery

As an ACF HHS-administered program, AFI should be considered in the context of ACF’s broad strategic goals, not solely in terms of asset building. AFI grantees nationwide commonly integrate their AFI projects into other programmatic activities that serve low-income households by providing benefits, social services, loans, and other assistance. This has further implications for appropriate staff-client interaction (high-versus low-touch programming). Low-touch programs may be more successful when they are integrated with other social services or when they connect participants with these services.

AFI participation and the savings it encourages may connect low-income people with benefits and reduce hardship, meeting ACF goals beyond asset building, such as promoting the economic, health, and

⁴³ The Center for Financial Services Innovation defines financial health as smooth and effective management of one’s daily financial life, resilience during ups and downs, and the capacity to seize opportunities that lead to financial security and mobility (Gutman et al. 2015). The Consumer Financial Protection Bureau defines financial well-being as having financial security and financial freedom of choice, in the present and the future (CFPB 2015).

social well-being of individuals and families.⁴⁴ To create the capacity for new (not just shifted) savings, some households may initially save through a combination of reduced consumption and increased benefit receipt. Over the longer term, the allowable asset purchases and additional income and wealth associated with them should promote self-sufficiency.

AFI IDAs Allow Low-Income People to Save without Reducing Benefits

Disregarding savings from AFI IDAs when determining benefit eligibility, and making this rule clear to AFI participants, may allow low-income individuals to save and avoid hardship. The AFI legislation stipulates that there be no reduction in benefits because of saving in an AFI IDA (see SEC. 415. No Reduction in Benefits).⁴⁵ This is consistent with this evaluation's finding that low-income AFI participants saved, did not reduce benefits, and avoided hardship. Savings outside AFI IDAs, however, generally count toward asset limits, which creates disincentives for low-income people to save. Ratcliffe and colleagues (2016) find that Supplemental Nutrition Assistance Program (SNAP) asset limits (when not relaxed through broad-based categorical eligibility) decrease low-income households' financial security and stability by decreasing savings and participation in traditional financial markets. This increases the likelihood that a low-income person lives in a household without a bank account and increases the fluctuation in benefits that comes with churning off and on the program.

Role of Unrestricted Savings

Understanding how participation reduced hardship could be important for future program design. The evaluation findings do not show what reduced hardship—whether it was increased public benefit receipt, relying on unrestricted or emergency savings, or participants depositing less into their AFI IDAs in some months than their intended monthly savings target to meet current needs. If the latter, a key question is whether it resulted in reduced matched savings or reduced asset purchases for some participants. If not, then unrestricted savings could be an important component of future matched savings program design.

⁴⁴ See ACF's key goals in the 2015–2016 ACF Strategic Plan: <http://www.acf.hhs.gov/about/acf-strategic-plan-2015-2016>.

⁴⁵ See section 415 in Community Opportunities, Accountability, and Training and Educational Services Act of 1998, 42 USC 604 (1998), <http://www.acf.hhs.gov/ocs/resource/afi-legislation-0#SEC415NOREDUCTIONINBENEFITS>.

Concluding Observations

The AFI program is one of the few federal efforts that encourages low-income people to save. Most federal asset-building subsidies disproportionately benefit high-income families who are more likely to shift savings in response to incentives rather than create new savings (Steuerle et al. 2014).⁴⁶ Asset limits in many benefits programs create a disincentive for low-income people to save outside an AFI IDA.

Sherraden's original proposal for IDAs in 1991 was for universal, progressive, lifelong accounts. The AFI program provides a small dose of his vision. The evidence presented here indicates that the program improves the financial health, security, and well-being of low-income earners and their households, promoting their short-term stability as a foundation for longer-term upward mobility.

⁴⁶ For example, homeownership subsidies are primarily delivered through the tax code, benefit high-income people, and tend to encourage the accrual of higher debt rather than increasing homeownership.

Appendix A: Site Selection Process

We selected study sites according to how well they met criteria under two major categories: the ability to implement a random assignment evaluation and the embodiment of “mainstream” features shared by many other AFI projects. The six criteria were as follows:

1. The grantee should be experienced so the evaluation involves a clean test of the AFI project’s program model without reflecting the expected start-up challenges facing newer grantees.
2. The grantee should have the capacity, in terms of financial and organizational resources, to meet the sample size needs of the evaluation within the study’s enrollment period (i.e., recruit at least 300–500 AFI-eligible individuals within 18 months and serve at least 150–250 participants).
3. If the grantee conducts enrollment at multiple locations, the process should be consistent across these locations.
4. IDA-like alternatives for control group members should be limited to ensure a clear differential in services received by the treatment and control groups.
5. The grantee must be interested in participating in the evaluation and willing to implement the experimental design.
6. To the extent possible, the AFI project should have features similar to other mainstream AFI projects on dimensions such as the following:
 - » asset types supported by the project’s IDAs (preferably including first-home purchase, business capitalization, and postsecondary education or training);
 - » match rates (preferably close to 2:1 or 3:1), match cap, and savings period;
 - » required hours of financial education (preferably about 12 hours of general financial education and 5 to 12 hours of asset-specific training); and
 - » availability of a typical range of other financial services, such as financial coaching and credit building advice (may offer crisis management, structured planning exercises, peer support, employment support, or mentoring).

We reviewed data on AFI grantees through FY 2010 provided to us by OCS, along with information on grants awarded in FY 2011, and determined whether grantees met considerations 1 and 2 above. This resulted in a group of 28 grantees among all active 353 AFI grantees. We further refined this list by comparing candidates on the other dimensions above. At the same time, we conducted a more general qualitative scan of the grantees that incorporated other available information, including input from a member of the AFI Resource Center. We also included grantees that had expressed interest in the evaluation in response to general outreach e-mails and announcements. This process resulted in a set of 19 grantees.

Of the 19 grantees, we ruled out 10 after e-mail or telephone contact, based on concerns over the sites' ability to reach the sample recruitment target with their limited project locations. (This was after adjusting our initial sample size requirements of 900 recruited and at least 450 served, to 500 recruited and 250 served). One remaining candidate reported that it was no longer operating its IDA program, but it recommended that we consider RISE Financial Pathways (then named the Community Financial Resource Center), an additional grantee in Los Angeles. The organization was not identified in our initial scan because it had not held a recent AFI grant, which was one criterion used to assess grantee experience. However, the grantee informed us that the organization had been a partner (to the United Way of Greater Los Angeles) and had the experience needed for consideration as an evaluation site.

We visited four finalist sites. Ultimately, the team chose RISE Financial Pathways in Los Angeles and Prosperity Works in Albuquerque, with its partner CNM Connect at Central New Mexico Community College. These two study sites shared a number of advantages, including grant capacity sufficient to serve the sample needed, significant experience operating IDA programs, mainstream characteristics regarding asset types offered, and past experience in working with research studies. In addition, with RISE being a single-agency grantee and with Prosperity Works' decision to focus the evaluation on one partner, they both had small intake staffs, few intake locations, and one program model, all of which greatly simplified implementation of the evaluation. Prosperity Works also had a large pool of individuals potentially eligible for AFI through the large student population at CNM.

Yet, neither grantee was perfect on all dimensions. Prosperity Works had an atypical match rate (4:1) and its partner, CNM Connect, required an atypical number of hours of financial education (a one-semester college course); RISE offered other non-IDA savings incentives that might be available to control group members. In addition, neither site was operating at the necessary scale. Nonetheless, we did not consider any of these concerns insurmountable, and these two sites were regarded as offering the greatest likelihood of successful study participation.

Appendix B: Comparability of AFI Study Participants to AFI Participants Nationwide

We present characteristics of the study sample in chapter 2. In this appendix, we detail how study participants compare to other AFI project participants nationwide who enrolled in FY 2014 (October 2013–September 2014) based on data sets provided by OCS. Our analysis used data for the derived number of individuals who opened accounts in FY 2014 as calculated by subtracting total participants in the FY 2013 data set from total participants in the FY 2014 data set. We chose FY 2014 because 85 percent of the study sample was enrolled within this period. Study participants are, for the most part, similar to other AFI participants nationwide, which speaks to the evaluation’s external validity.

Data Source

AFI grantees generally collect demographic information from participants when they enroll in the project, so the data should be comparable to the baseline survey data collected from sample members at the time of random assignment. The national OCS data set and the study data set, however, differ in two ways:

- The national data set includes people who open accounts, while the study data set includes all people randomly assigned to the study regardless of whether they open an account. Both data sets are consistent in the sense that participants who have opened accounts remain in the data even after they complete, or are removed from, the project.
- The study data set includes only primary sample members (the one person per household who applied for the project and was then enrolled in the study and randomly assigned); it excludes any secondary account holders in the same household. In contrast, the national data set encompasses all account holders, and it may include multiple account holders in the same household.

Despite these differences, a comparison between the two data sets should give a reasonable sense of whether demographic characteristics of the sample are similar to characteristics of other AFI participants nationwide.

We include almost all grants in our analysis of the OCS data. We exclude the two grantees that were part of the evaluation, along with two types of grants of which the study sites were not expected to be

representative: the special state projects in Indiana and Pennsylvania that operate under different rules from other AFI grantees, as well as several projects regarded as youth only (i.e., identified from the demographic data as serving only individuals under the age of 20). In total, we include 293 grants active in FY 2013 or FY 2014.⁴⁷

Comparability of Participants

Similar to AFI study participants, other AFI project participants nationwide enrolled in FY 2014 were predominantly under age 40, female, nonwhite, unmarried, employed, and did not have a license, certificate, or degree when they enrolled (see table 2.3, column 4). These similarities suggest that evaluation study participants generally reflect AFI project participants nationwide. However, some differences emerge when we compare other demographic characteristics.

A smaller share of AFI participants nationwide is Hispanic or black (56 percent) compared with study participants (74 percent). The share of Hispanic participants in the study is about twice the share of participants nationwide (45 percent versus 23 percent), and the share of white participants is about half the national share (15 percent versus 32 percent). However, 73 percent of AFI participants nationwide were female, very close to the 70 percent of study participants.

AFI participants nationwide are slightly younger than study participants, though more likely to have children (66 percent nationwide versus 51 percent for study participants). A higher share of AFI participants nationwide is under age 40 (76 percent) than in our study sample (61 percent). A similar share of AFI participants nationwide lacks a license, certificate, or degree (68 percent versus 62 percent among study participants), but within that group a larger share of nationwide program participants reports only a high school education (38 percent, versus 18 percent study participants) and a smaller group has some college but no degree (30 percent, versus 43 percent of study participants). This difference is largely driven by study participants in the community college population in the Albuquerque site, where 62 percent had some college but no degree at study enrollment.

Broadly, the demographic profile of study participants is similar to that of AFI participants nationwide. Important differences when generalizing the study findings to the AFI population nationwide are the larger Hispanic share and smaller white share within the study sample, as well as the predominance of the community college student population at the Albuquerque site.

⁴⁷ Statistics for AFI participants nationwide presented here differ from those presented in the AFI Reports to Congress (OCS 2010; OCS 2016) partly because our analysis focuses on AFI participants nationwide who enrolled in FY2014.

Appendix C: Analysis of Nonresponse Bias

This appendix focuses on the overall effect of survey nonresponse by examining baseline outcome and demographic characteristics among those in the full sample of 807 study enrollees, comparing those who responded to the first-year follow-up survey with those who did not respond.

We examined 41 outcomes, demographic characteristics, and study participation variables for the combined sample (table C.1) and found two variables with differences statistically significant at $p < 0.01$, three variables with differences statistically significant at $p < 0.05$, and one variable with a difference statistically significant at $p < 0.10$. These are more differences than would have occurred by chance alone.

The individuals who did not respond to the survey appear heterogeneous. They tend to be less educated than people who responded to the survey, yet they had lower rates of medical hardship and receipt of public benefits. Given their net worth and income were lower than survey respondents, albeit not statistically significant at any conventional level, we would expect them to have higher rates of hardship and public benefit receipt, indicating an underlying mix of individuals. Importantly, survey nonrespondents were more likely to have enrolled in the study later than those who responded. This may reflect less time to locate, contact, and engage these people than with the earlier enrollees.

Examining differences by site yields fewer differences. In Albuquerque, we found two variables where respondents and nonrespondents differed at $p < 0.01$ and one variable where the groups differed at $p < 0.10$. Of those three variables, average credit scores were lower for nonrespondents by more than 36 points, with a very low p value (0.003). This, along with lower (statistically significant) educational attainment for nonrespondents implies a poorer group, but nonrespondents also showed lower rates of medical hardship.

In Los Angeles, we found one variable with $p < 0.01$, four variables with $p < 0.05$, and one variable with $p < 0.1$. The Los Angeles site accounts for most of the combined-sample differences except there was no difference in medical hardship. Both sites had lower educational attainment among nonrespondents.

Although smaller sample sizes might have led us to find fewer differences at the site level than in the combined sample, in general when one site had a difference that the other didn't show, the site without a difference had a p value well out of the range that would indicate statistical significance might be achieved with a larger sample.

TABLE C.1

Selected Baseline Characteristics of Study Participants, by Follow-Up Survey Response Status and Site

Variable	Sites Combined		Albuquerque		Los Angeles	
	Respondent	Nonrespondent	Respondent	Nonrespondent	Respondent	Nonrespondent
Savings (participant and spouse/partner)						
Has liquid assets (%)	85.2	85.8	87.7	79.5	83.2	88.5
Liquid asset amount (\$)	1,716	1,339	1,863	771***	1,606	1,579
Natural log of liquid assets (\$)	0.72	0.71	0.80	0.70	0.67	0.71
Asset ownership (participant or participant and spouse/partner)						
Homeownership (%)	8.5	9.1	12.1	10.0	6.1	8.7
Business ownership (%)	24.6	23.2	20.6	16.0	27.3	26.0
Net worth (participant and spouse/partner)						
Net worth (\$)	-5,501	-6,566	-6,711	-8,350	-4,539	-5,698
Material hardship (household)						
Food hardship (%)	20.0	21.0	28.3	30.0	14.4	17.5
Housing hardship (%)	17.0	17.3	22.4	23.9	13.3	14.8
Utilities hardship (%)	29.2	27.0	33.6	33.3	26.2	24.6
Medical hardship (%)	38.2	26.5***	46.9	27.7***	32.6	26.0
Any hardship (%)	57.0	50.6	66.4	63.4	50.7	46.2
Alternative financial services (participant and spouse/partner)						
Use AFS credit (%)	19.1	19.3	20.5	28.0	18.2	15.9
AFS credit (#)	0.59	0.55	0.64	0.88	0.55	0.42
Use nonbank check cashing (%)	8.1	10.2	5.2	12.0	10.1	9.5
Nonbank check cashing (#)	0.29	0.38	0.20	0.44	0.36	0.35
Means-tested benefit receipt (household)						
Receive benefits (%)	68.9	60.1**	61.9	60.0	73.7	60.2***
Employment (participant) and earnings and income (household)						
Employed (%)	91.7	92.2	89.6	90.0	93.1	93.0
Monthly earnings (\$)	1,197	1,117	976	818	1,354	1,235
Natural log of monthly earnings (\$)	5.85	5.68	5.71	5.54	5.95	5.73
Monthly income (\$)	1,418	1,309	1,165	991	1,594	1,431
Natural log of monthly income (\$)	6.43	6.26	6.25	6.33	6.55	6.23
Credit score (participant)^a						
Vantage 2.0 score (501–990)	–	–	664.93	628.07***	–	–
Personal outlook (participant)						
Ability to make ends meet (%)	52.8	59.1	43.8	56.0	59.0	60.3
Perceived financial security (1–10)	4.77	5.00	4.35	4.60	5.05	5.16
Better off financially (%)	37.2	32.0	26.9	28.0	44.1	33.6**
Worse off financially (%)	21.2	19.4	27.7	24.0	16.8	17.6
Good health (%)	84.0	83.5	84.3	84.0	83.7	83.3
Self-esteem (0–30)	23.96	24.41	23.19	24.46	24.49	24.40

Continued on next page

TABLE C.1

Selected Baseline Characteristics of Study Participants, by Follow-Up Survey Response Status and Site (continued)

Variable	Sites Combined		Albuquerque		Los Angeles	
	Respondent	Nonrespondent	Respondent	Nonrespondent	Respondent	Nonrespondent
Community involvement (participant)						
Community involvement (%)	69.2	64.7	64.6	61.2	72.3	66.1
Time preference (participant)						
Present-oriented time preference (0–1)	0.06	0.06	0.06	0.09	0.06	0.05
Demographic explanatory variables						
Age (%)						
Under 30	35.0	32.0	52.0	51.0	25.0	25.0
30–39	27.0	25.0	22.0	29.0	30.0	24.0
40–49	21.0	24.0	16.0	10.0	25.0	29.0
50+	17.0	19.0	10.0	10.0	21.0	22.0
Female (%)	70.7	67.0	66.7	54.0	73.4	72.1
Race/ethnicity (%)						
White	16.0	12.0	30.0	31.0	6.0	4.0
Hispanic	44.0	48.0	50.0	50.0	40.0	47.0
Black	30.0	26.0	10.0	4.0	43.0	35.0
Other	11.0	14.0	10.0	15.0	11.0	14.0
Speaks only English at home (%)	58.9	48.0**	66.5	58.0	53.9	44.2*
Educational attainment (%)		**		*		**
High school	16.0	25.0	8.0	20.0	22.0	27.0
Some college	43.0	44.0	63.0	52.0	30.0	41.0
License, certificate, or associate's degree	25.0	21.0	21.0	24.0	27.0	20.0
Bachelor's degree or higher	16.0	10.0	7.0	4.0	21.0	12.0
Married (%)	18.1	20.9	16.9	14.0	18.8	23.6
Number of adults in household (1–6)	1.82	2.01*	1.86	1.84	1.80	2.07**
Number of children under age 6 in household (0–5)	0.35	0.33	0.28	0.32	0.39	0.34
Number of children ages 6–17 in household (0–5)	0.66	0.68	0.49	0.44	0.77	0.77
Annual household income (%)						
Less than \$15,000	50.0	53.0	58.0	67.0	45.0	48.0
\$15,000–\$24,999	24.0	26.0	22.0	19.0	26.0	28.0
\$25,000 plus	26.0	21.0	20.0	15.0	30.0	24.0
Study participation variables						
Baseline survey start date (%)		***				**
Jan–Dec 2013	33.0	27.0	33.0	32.0	34.0	25.0
Jan–April 2014	41.0	35.0	43.0	30.0	40.0	37.0
May–July 2014	25.0	38.0	25.0	38.0	26.0	38.0

Source: Authors' tabulations of the AFI baseline survey.

Note: "Respondent" counts only those who completed the AFI first-year follow-up survey, and "nonrespondent" counts only those who did not complete it.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

^a Credit score is the Vantage 2.0 scores reported for all randomly assigned participants in Albuquerque.

Appendix D: Outcome and Explanatory Variables

Outcome Variables at Baseline

What were the characteristics of study participants at enrollment? Most (85 percent) study participants had liquid assets, with a slightly higher percentage with assets in Albuquerque than Los Angeles (table D.1). Average amount of liquid assets was also higher in Albuquerque (\$1,863) than Los Angeles (\$1,606). An estimated 9 percent of participants owned a home; homeownership was twice as common in Albuquerque (12 percent) as in Los Angeles (6 percent). A quarter of participants owned businesses—27 percent in Los Angeles compared with only 21 percent in Albuquerque. Mean net worth was negative in both sites. Across sites the mean net worth was -\$5,501. In Albuquerque, where more participants were students, net worth averaged -\$6,711.

Participants from both sites experienced various types of material hardship to a similar degree. Approximately one-fifth had food hardship (20 percent) and housing hardship (17 percent), while about one-third had utilities hardship (29 percent) and medical hardship (38 percent). Fifty-seven percent had at least one type of hardship. Use of alternative financial services by the participants or their spouses/partners was also quite common. Nearly one-fifth (19 percent) used a payday loan, auto loan, or pawnshop in the 12 months before random assignment. Eight percent had used a check-cashing store in the past 12 months, more commonly in Los Angeles (10 percent) than in Albuquerque (5 percent). More than two-thirds of participants (69 percent) had received means-tested public benefits in the past 12 months (Special Supplemental Nutrition Program for Women, Infants, and Children, SNAP, free/reduced price school lunch, Supplemental Security Income, Temporary Assistance for Needy Families, housing subsidy, energy subsidy, child care subsidy, or Medicaid). Benefit receipt was more common in Los Angeles (73 percent) than Albuquerque (62 percent).

Most participants were employed at baseline (92 percent), with slightly more employed in Los Angeles than in Albuquerque. Monthly household earnings and income averaged \$1,197 and \$1,418, respectively.

TABLE D.1

Baseline Outcomes of First-Year Follow-Up Survey Respondents

Variable	Sites Combined		Albuquerque		Los Angeles	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Savings (participant and spouse/partner)						
Has liquid assets (%)	85.2	35.6	87.7	32.9	83.2	37.4
Liquid asset amount (\$)	1,716	4,629	1,863	4,595	1,606	4,660
Natural log of liquid assets (\$)	0.72	0.45	0.80	0.40	0.67	0.47
Asset ownership (participant or and spouse/partner)^a						
Homeownership (%)	8.5	27.9	12.1	32.7	6.1	23.9
Business ownership (%)	24.6	43.1	20.6	40.5	27.3	44.6
Net worth (participant and spouse/partner)						
Net worth (\$)	-5,501	34,695	-6,711	37,889	-4,539	31,977
Material hardship (household)						
Food hardship (%)	20.0	40.0	28.3	45.2	14.4	35.2
Housing hardship (%)	17.0	37.6	22.4	41.8	13.3	34.0
Utilities hardship (%)	29.2	45.5	33.6	47.3	26.2	44.1
Medical hardship (%)	38.2	48.6	46.9	50.0	32.6	46.9
Any hardship (%)	57.0	49.5	66.4	47.3	50.7	50.1
Alternative financial services (participant and spouse/partner)						
Use AFS credit (%)	19.1	39.4	20.5	40.4	18.2	38.6
AFS credit (#)	0.59	1.50	0.64	1.55	0.55	1.46
Use nonbank check cashing (%)	8.1	27.3	5.2	22.3	10.1	30.1
Nonbank check cashing (#)	0.29	1.06	0.20	0.91	0.36	1.15
Means-tested benefit receipt (household)						
Received benefits (%)	68.9	46.3	61.9	48.7	73.7	44.1
Employment (participant) and earnings and income (household)						
Employed (%)	91.7	27.7	89.6	30.6	93.1	25.4
Monthly earnings (\$)	1,197	1,373	976	1,176	1,354	1,479
Natural log of monthly earnings (\$)	5.85	2.58	5.71	2.46	5.95	2.67
Monthly income (\$)	1,418	1,325	1,165	1,129	1,594	1,421
Natural log of monthly income (\$)	6.43	2.15	6.25	2.07	6.55	2.20
Credit score (participant)^b						
Vantage 2.0 score (501–990)	—	—	664.93	85.44	—	—
Personal outlook (participant)						
Ability to make ends meet (%)	52.8	50.0	43.8	49.7	59.0	49.3
Perceived financial security (1–10)	4.77	2.90	4.35	2.65	5.05	3.02
Better off financially (%)	37.2	48.4	26.9	44.4	44.1	49.7
Worse off financially (%)	21.2	40.9	27.7	44.8	16.8	37.4
Good health (%)	84.0	36.7	84.3	36.4	83.7	37.0
Self-esteem (0–30)	23.96	4.94	23.19	4.85	24.49	4.95
Community involvement (participant)						
Community involvement (%)	69.2	46.2	64.6	47.9	72.3	44.8
Time preference (participant)						
Present-oriented time preference (0–1)	0.06	0.12	0.06	0.12	0.06	0.12

Source: Authors' tabulations of the AFI baseline survey.

^a The education outcome is measured as an increase in educational attainment between the baseline and follow-up surveys (see table 3.2), so there is no baseline value.

^b Credit score is the Vantage 2.0 scores reported for all randomly assigned participants in Albuquerque.

Participants' ratings of their health and self-esteem were high, while their personal outlook related to finances was less positive. Eighty-four percent of participants, overall and in each site, rated their health as excellent, very good, or good. Overall and in each site, participants' rating of self-esteem averaged 24 on the

30-point Rosenberg scale. However, on a 1–10 scale of perceived financial security, the average was only 4.8. Moreover, only 37 percent perceived their financial situation as improving in the past 12 months, and 21 percent perceived it as getting worse. More participants in Los Angeles (44 percent) perceived an improvement compared with Albuquerque (27 percent), and fewer in Los Angeles (17 percent) perceived a worsening compared with Albuquerque (28 percent).

About 70 percent of participants had worked on a neighborhood project, volunteered, attended a community event, or helped raise money for a community organization in the last 12 months, and such activity was more common in Los Angeles than in Albuquerque. The interest rate participants were willing to accept to defer payment on a major purchase was 6 percent in both sites.

Explanatory Variable Measures

Table D.2 lists the demographic and study participation explanatory variables that we incorporate into our analyses.

TABLE D.2

Explanatory Variable Measures

Variable	Definition
Demographic explanatory variables	
Age	Participant's age (dummy variables: less than 30, 30–39, 40–49, 50 and over).
Female	Participant's gender.
Race/ethnicity (multiple dummy variables)	Participant's race and ethnicity (dummy variables: white, black, Hispanic, other).
Speaks only English at home	Participant speaks only English at home.
Educational attainment	Participant's educational attainment (dummy variables: high school diploma/GED or less, some college but no degree, license/certificate/associate degree, bachelor's degree or higher).
Married	Participant is currently married.
Number of adults in household	Number of adults living in household, including participant (1–6).
Number of children under age 6 in household	Number of children under age 6 living in household (0–5).
Number of children ages 6–17 in household	Number of children ages 6–17 living in household (0–5).
Annual household income	Total household income in the last 12 months, including job earnings, child support, money received from family and friends, and public benefits such as welfare, Supplemental Security Income, unemployment insurance, etc. (dummy variables: less than \$15,000, \$15,000–\$24,999, \$25,000 and over).
Study participation explanatory variables	
Treatment group	Participant is in treatment group.
Albuquerque site	Participant's study site is Albuquerque.
Baseline survey start date	Date that participant began baseline survey (dummy variables: January–December 2013, January–April 2014, May–July 2014).
12-month follow-up survey mode	Means of administering 12-month follow-up survey (dummy variables: in person, online, by phone).

Appendix E: Evaluation of Random Assignment

Differences between the treatment and control groups in their characteristics at the time of random assignment (i.e., at baseline) and differential sample attrition can make the two groups unequal at the first-year follow-up. We examine whether there are statistically significant treatment-control differences in baseline characteristics within the analysis sample (i.e., sample members who completed the first-year follow-up survey). We examine the baseline demographic characteristics and the baseline values of the first-year outcome variables. These tests are performed for the combined analysis sample of 628 cases, and separately for the Albuquerque analysis sample of 249 cases and the Los Angeles analysis sample of 379 cases. We use t-tests for univariate variables and chi-squared tests for categorical variables.

Results show five statistically significant treatment-control differences (table E.1). Specifically, of the 38 characteristics tested, one is statistically significant at the 1 percent level, two are statistically significant at the 5 percent level, and two are statistically significant at the 10 percent level. Given the 38 tests, statistical fluctuation is expected to result in significant treatment-control differences for fewer than one attribute at the 1 percent level, two attributes at the 5 percent level, or four attributes at the 10 percent level. While there are only five statistically significant differences, the higher level of statistical significance for three of the differences (i.e., significant at the 1 and 5 percent levels) results in more differences than we would expect at random.

Among participants' demographic and economic characteristics, treatment group members were more likely to be married at baseline and had higher baseline incomes than members of the control group. Two treatment-control baseline differences are in the personal outlook domain. Specifically, treatment group members had slightly lower self-esteem and were less likely to say they were better off than they were 12 months earlier. Treatment group members also had slightly higher net worth than the control group. To account for these differences, the regression models generally include covariates for baseline demographic and economic characteristics, as well as the baseline value of the dependent variable.

We also find statistically significant treatment-control baseline differences in each of the two sites. In Albuquerque, four baseline characteristics have significant treatment-control differences at the 10 or 5 percent level. In Los Angeles, three baseline characteristics have significant treatment-control differences at the 5 or 1 percent level. The site-specific analyses follow the combined-site analyses and include the same baseline characteristics as covariates in the regression models.

TABLE E.1

Selected Baseline Characteristics of Follow-Up Survey Respondents, by Group and Site

Variable	Sites Combined		Albuquerque		Los Angeles	
	Treatment	Control	Treatment	Control	Treatment	Control
Savings (participant and spouse/partner)						
Has liquid assets (%)	86.6	83.5	87.8	87.6	85.7	80.4
Liquid assets (\$)	1,777	1,646	1,766	1,977	1,785	1,403
Natural log of liquid assets (\$)	0.73	0.71	0.83	0.77	0.67	0.66
Asset ownership (participant and spouse/partner)						
Homeownership (%)	7.5	9.6	11.5	12.7	4.9	7.5
Business ownership (%)	26.1	22.8	20.8	20.3	29.6	24.6
Net worth (participant and spouse/partner)						
Net worth (\$)	-3,009	-8,466 *	-1,892	-12,280 **	-3,876	-5,345
Material hardship (household)						
Food hardship (%)	22.1	17.6	32.8	23.5	15.3	13.5
Housing hardship (%)	17.4	16.4	26.2	18.3	11.8	15.2
Utilities hardship (%)	28.8	29.7	33.8	33.3	25.4	27.2
Medical hardship (%)	40.2	35.9	51.6	41.6	33.0	32.1
Any hardship (%)	57.6	56.4	67.8	64.8	50.8	50.6
Alternative financial services (participant and spouse/partner)						
Use AFS credit (%)	19.6	18.6	20.9	20.0	18.7	17.6
AFS credit (#)	0.6	0.5	0.7	0.6	0.6	0.5
Use nonbank check cashing (%)	8.9	7.2	4.7	5.9	11.7	8.2
Nonbank check cashing (#)	0.3	0.3	0.2	0.2	0.4	0.3
Means-tested benefit receipt (household)						
Receive benefits (%)	70.7	66.8	63.9	59.6	75.1	72.0
Employment (participant) and earnings and income (household)						
Employed (%)	91.9	91.4	92.3	86.6	91.7	94.7
Monthly earnings (\$)	1,171	1,227	910	1,052	1,353	1,355
Natural log of monthly earnings (\$)	5.90	5.79	5.73	5.69	6.03	5.86
Monthly income (\$)	1,427	1,407	1,157	1,174	1,603	1,584
Natural log of monthly income (\$)	6.56	6.27	6.53	5.95 *	6.58	6.51
Credit score (participant)^a						
Vantage 2.0 score (501-990)	—	—	666.76	663.15	—	—
Personal outlook (participant)						
Ability to make ends meet (%)	50.9	55.0	42.3	45.4	56.6	61.8
Perceived financial security (1-10)	4.8	4.7	4.3	4.4	5.1	4.9
Better off financially (%)	34.1	40.6 *	24.6	29.4	40.3	48.5
Worse off financially (%)	20.5	21.9	26.9	28.6	16.4	17.2
Good health (%)	82.6	85.6	80.8	88.2	83.7	83.7
Self-esteem (0-30)	23.5	24.5 **	22.9	23.5	23.9	25.2 **

Continued on next page

TABLE E.1

Selected Baseline Characteristics of Follow-Up Survey Respondents, by Group and Site (continued)

Variable	Sites Combined		Albuquerque		Los Angeles	
	Treatment	Control	Treatment	Control	Treatment	Control
Community involvement (participant)						
Community involvement (%)	71.7	66.4	70.3	58.5 *	72.6	72.0
Time preference (participant)						
Present-oriented time preference (0-1)	0.1	0.1	0.1	0.1	0.1	0.1
Demographic explanatory variables						
Age (%)						
Under 30	34.0	37.0	53.0	50.0	21.0	28.0
30-39	28.0	25.0	23.0	21.0	32.0	28.0
40-49	21.0	22.0	13.0	19.0	25.0	24.0
50+	17.0	16.0	11.0	10.0	21.0	21.0
Female (%)						
	70.7	70.6	67.7	65.5	72.7	74.1
Race/ethnicity (%)						
White	16.0	15.0	30.0	30.0	7.0	4.0
Hispanic	44.0	44.0	50.0	50.0	41.0	39.0
Black	29.0	30.0	12.0	8.0	41.0	47.0
Other	11.0	11.0	8.0	12.0	12.0	9.0
Speaks only English at home (%)						
	57.7	60.3	69.2	63.6	50.2	58.1
Educational attainment (%)						
High school	17.0	16.0	5.0	11.0	24.0	19.0
Some college	41.0	46.0	64.0	63.0	26.0	34.0
License, certificate, or associate's degree	26.0	23.0	23.0	19.0	28.0	25.0
Bachelor's degree or higher	16.0	15.0	8.0	7.0	21.0	21.0
Married (%)						
	21.3	14.4 **	18.6	15.1	23.0	13.9 **
Number of adults in household (1-6)						
	1.88	1.76	1.92	1.79	1.86	1.73
Number of children under age 6 in household (0-5)						
	0.39	0.31	0.35	0.21 *	0.41	0.37
Number of children ages 6-17 in household (0-5)						
	0.64	0.68	0.43	0.56	0.79	0.76
Annual household income (%)						
Less than \$15,000	45.0	56.0	56.0	60.0	38.0	53.0
\$15,000-\$24,999	24.0	25.0	21.0	22.0	25.0	26.0
\$25,000 plus	31.0	19.0	23.0	17.0	37.0	21.0
Study participation explanatory variables						
Baseline survey start date (%)						
Jan-Dec 2013	34.0	32.0	35.0	30.0	34.0	33.0
Jan-April 2014	40.0	43.0	41.0	45.0	39.0	42.0
May-July 2014	26.0	25.0	25.0	25.0	27.0	25.0

Source: Authors' tabulations of the AFI baseline survey.

^a credit score is the Vantage 2.0 scores reported for all randomly assigned participants in Albuquerque.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Appendix F: Analytic Approach

This appendix presents the estimation method used for both the intent-to-treat (ITT) and the treatment-on-treated (TOT) estimates. We also present the results of the TOT analysis. Finally, we discuss the minimum detectable effects for these analyses.

Estimation of Intent-to-Treat Effects

To obtain the regression-adjusted program effect, we estimate the following model:

$$Y_i = \alpha_0 + \alpha_1 T_i + \gamma_1 S_i + \gamma_2 Y_{ib} + \gamma_3 X_{ib} + \gamma_4 Z_i + \varepsilon_i.$$

In this model, Y_i is the outcome of interest (for individual i), T_i is the treatment group indicator, and the parameter α_1 is the ITT effect. All models also include a site indicator variable (S_i), baseline value of the outcome (Y_{ib}),⁴⁸ a vector of individual-level characteristics measured at baseline (X_{ib}),⁴⁹ and variables that identify the mode of the follow-up survey (phone, web, or in-person) and enrollment cohort (whether the person entered the study in months 1–12, 13–16, or 17–19 of the study enrollment period), which are represented by Z_i .

We estimate the treatment effect of the program using linear least squares when Y_i is a continuous variable and using a probit model when Y_i is dichotomous.⁵⁰ In addition, we estimate quantile treatment effects of the program for liquid assets using quantile regressions.⁵¹ This method provides estimates of AFI's impact at different points of the liquid asset distribution (at the first-year follow-up). Specifically, we estimate the impact of the program at the 25th, 50th, and 75th percentiles of the liquid asset distribution.

⁴⁸ Exceptions are the material hardship intensity models, because the baseline survey did not collect the intensity of material hardship (i.e., number of times the hardship was experienced). In these models, we include an indicator variable from the baseline survey that captures whether the person experienced the hardship.

⁴⁹ We use multiple imputation to impute missing values for six of the individual-level baseline characteristics used in the multivariate models. On average, 11 responses were imputed for each variable.

⁵⁰ We calculate and present the average marginal effect for the probit models.

⁵¹ The quantile regression models only include the treatment group indicator (T_i) and the baseline value of the dependent variable (Y_{ib}). The other covariates (S_i , X_{ib} , and Z_i) are excluded because including them does little to improve or substantially reduces the precision of the impact estimates, and none are statistically significantly different from zero at the 10 percent level.

Estimation of Treatment-on-Treated Effects

Not all individuals assigned to the treatment group fully participated in AFI, defined as making at least one deposit into their AFI IDAs. In particular, some did not open an account. The ITT analysis provides unbiased estimates of offering the AFI program, but program operators and policymakers are frequently also interested in knowing the impact on those who actually participated in the program and received the treatment.

Under the assumption that there are no program effects for nonparticipants in the treatment group, we can rescale the ITT findings to obtain estimates of AFI's impact for those who participated.⁵² We calculate this measure, known as the treatment-on-treated effect, by dividing the difference between the average outcomes of the treatment and control groups as assigned (i.e., the ITT impact) by the proportion of the treatment group who participated in the treatment (Bloom 1984). Thus, the TOT estimate represents the average change in an outcome per treated recipient. The estimate is biased upward as it does not control for the self-selection of individuals who participated in the program, but it provides useful information to program developers to refine their recruiting and retention procedures.

The TOT effect of the program on those who participate is given by:

$$\beta_1 = \frac{E[Y_i|T_i = 1] - E[Y_i|T_i = 0]}{E[P_i|T_i = 1]}$$

where β_1 is the TOT effect and P_i is an indicator of whether individual i has participated in the program. For the TOT calculation, we define being treated as opening an account and making at least one deposit.

Treatment-on-Treated Results

In addition to the ITT estimates presented in chapter 6, we present select TOT estimates, which account for some treatment group members not receiving the treatment. In the case of AFI, we consider participation as making at least one deposit in the AFI IDA. The rate of treatment group nonparticipation is roughly 13 percent overall and approximately 5 percent in Albuquerque and 19 percent in Los Angeles. Using the Bloom adjustment, the overall TOT scales up the ITT effects by 16 percent for the combined sample, 23 percent for Los Angeles, and 6 percent for Albuquerque. With this adjustment, the standard errors are scaled up by the same amount as the ITT estimates, yielding no change in significance levels.

⁵² These calculations do not incorporate “crossovers” (i.e., control group members who received the treatment) because the data suggest that crossovers are almost nonexistent, with only three control group members receiving the treatment.

The TOT estimates suggest that AFI increased the likelihood of having any liquid assets by 9 percentage points in the combined sample, 8 percentage points in Albuquerque, and 11 percentage points in Los Angeles (table F.1). This compares with the ITT estimates of 7 percentage points, 7 percentage points, and 9 percentage points, respectively (table 6.1). For the amount of liquid assets, the TOT estimates suggest that AFI increased average liquid assets by \$924 and median liquid assets by \$759 for those that participated on the program. We present the TOT estimates for outcomes with a statistically significant ITT effect below in table F.1.

TABLE F.1
Treatment-on-Treated Impacts of the AFI Program on Selected Outcomes at the First-Year Follow-Up

Dependent variable	Sites Combined		Albuquerque		Los Angeles	
	Impact ^a	P-value	Impact ^a	P-value	Impact ^a	P-value
Savings (participant and spouse/partner)						
Has liquid assets (%)	8.6 ***	0.008	7.5 *	0.056	10.7 **	0.046
Liquid asset amount (\$)	924 *	0.091	919	0.123	730	0.427
Natural log of liquid assets (\$)	1.2 ***	0.000	1.1 ***	0.003	1.2 ***	0.007
Liquid assets (\$)						
25th percentile	200	0.324	127	0.604	263	0.256
50th percentile	759 ***	0.005	703 **	0.040	842 **	0.030
75th percentile	1,259 ***	0.008	1,511 **	0.025	1,008	0.144
Material hardship (household)						
Utilities hardship (#)	-0.5 ***	0.010	-0.4	0.240	-0.7 ***	0.008
Medical hardship (%)	-7.4 *	0.079	1.6	0.791	-15.5 ***	0.007
Medical hardship (#)	-0.4	0.108	-0.1	0.884	-0.8 **	0.022
Any hardship (%)	-4.0	0.369	8.1	0.174	-12.9 **	0.044
Number of hardships (#)	-1.1 **	0.016	-0.5	0.355	-1.7 **	0.015
Alternative financial services (participant and spouse/partner)						
Use nonbank check cashing (%)	-4.2 *	0.069	1.1	0.692	-9.0 **	0.013
Nonbank check cashing (#)	-0.2 *	0.069	-0.0	0.731	-0.2 *	0.081
Means-tested benefit receipt (household)						
Receive benefits (%)	7.9 **	0.036	4.1	0.440	9.9 *	0.058
Personal outlook (participant)						
Ability to make ends meet (%)	6.6	0.121	4.5	0.455	6.2	0.295
Perceived financial security (1–10)	0.57 **	0.041	0.30	0.440	0.88 **	0.030
Better off financially (%)	8.3 *	0.064	7.4	0.232	11.2 *	0.073
Worse off financially (%)	-11.1 ***	0.002	-18.0 ***	0.001	-7.4	0.112

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque ($N = 248$) and Los Angeles ($N = 374$) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values. Program participation rates for the combined sites, Albuquerque, and for Los Angeles are 86.5, 94.8, and 81.4, respectively.

^a The TOT estimates are obtained using the Bloom adjustment on the ITT estimates presented in the chapter 6 tables, yielding no change in significance levels.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

Statistical Power: Minimum Detectable Effects

As we interpret estimated program impacts presented in chapter 6, it is important to discuss the implications of the available sample size. The power of an impact analysis is its likelihood of detecting a treatment-control difference in an outcome for which there is a true program effect—that is, the probability of correctly rejecting the null hypothesis (no program effect) when it is false. This probability is related most importantly to the size of the available treatment and control group samples, the underlying variation in the outcome measure, and the size of the program effect. The adequacy of a sample is normally expressed as the minimum size of an effect that can be detected with 80 percent probability: the minimum detectable effect (MDE). The larger the sample or the lower the underlying variation of the outcome measure, the smaller is the MDE, indicating a more precise test of the program’s impact. With 80 percent power, there is a 20 percent chance of not detecting a program effect when one indeed exists (false negative). Our calculations also assume a 10 percent significance level for a two-sided test. The significance level is the probability of detecting an effect when none exists—that is, not rejecting the null hypothesis when it is true (false positive).

We have calculated the minimum detectable ITT effects for two illustrative first-year outcomes: one binary measure and one continuous measure (table F.2). The binary outcome has an assumed control mean of 0.5.⁵³ This corresponds approximately to a key first-year survey measure: the percentage who within the past six months experienced material hardship related to housing, utilities, phone service, or health care. Based on findings for the follow-up survey, this composite hardship measure has a control mean of 0.56 (not shown).

For combined-sample impact estimates on the illustrative binary outcome, the MDE is 0.099, which is 20 percent of the control mean of 0.5. For single-site analyses of Albuquerque and Los Angeles, the corresponding MDEs are 0.155 and 0.127, respectively.⁵⁴

For the illustrative continuous outcome, we use assumptions that correspond approximately to the available survey findings for monthly household earnings. We assume a control mean of \$1,500 and a standard deviation of \$2,000. The corresponding combined-sample values from the follow-up survey are roughly \$1,550 and \$2,120, respectively (not shown).

⁵³ A control group value of 0.5 is typically used for such calculations. This is the most conservative assumption, as it yields a larger minimum detectable effect than with any other assumed control group value.

⁵⁴ The indicated MDEs are reasonable approximations. The actual MDEs will be somewhat smaller because we obtain regression-adjusted impacts; controlling for study participants’ baseline characteristics in the regression models improves the precision of the estimated program impacts.

TABLE F.2

Minimum Detectable Effect for Treatment-Control Differences

	Sites Combined	Albuquerque	Los Angeles
Analysis sample			
Treatment	335	130	205
Control	293	119	174
Total	628	249	379
Illustrative binary outcome			
Control mean	0.500	0.500	0.500
Minimum detectable effect ^a	0.099	0.155	0.127
MDE as % of control mean	20%	31%	25%
Illustrative continuous outcome			
Control mean	\$1,500	\$1,500	\$1,500
Control standard deviation	\$2,000	\$2,000	\$2,000
Minimum detectable effect ^a	\$398	\$632	\$513
MDE as % of control mean	27%	42%	34%

Source: Authors' calculations from AFI first-year follow-up survey.

^a Assumes 80 percent power and 10 percent significance (two-tailed test).

Because of the substantial variation in earnings, the minimum detectable effects are relatively large. For the combined sample, the MDE is \$398, or 27 percent of the control mean. For single-site analyses of Albuquerque and Los Angeles, the corresponding MDEs are \$632 and \$513, respectively. Our ITT estimates control for baseline values of the outcomes. This serves to improve the precision of the impact estimates, thus lowering MDEs somewhat.

Stated otherwise, the explanatory power of the multivariate models used in estimating the ITT effects (for binary and continuous outcomes) serves effectively to increase one's sample size. For the combined-sample ITT estimates, if the estimating model explains 20 percent of the variation in the outcome measure, this lowers the indicated MDE by approximately 2 percentage points, from 20 to 18 percent for the binary outcome and from 27 to 25 percent for the continuous outcome.

Appendix G: Impact Estimates by Site

TABLE G.1

Impacts of AFI Program on Liquid Assets and Asset Ownership at the First-Year Follow-Up

Primary outcomes	Albuquerque				Los Angeles			
	Treatment ^a	Control ^a	Impact ^b	P-value	Treatment ^a	Control ^a	Impact ^b	P-value
Liquid assets (participant and spouse/partner)								
Has liquid assets (%)	92.8	85.7	7.1 *	0.056	85.7	77.1	8.7 **	0.046
Liquid asset amount (\$)	3,004	2,132	872	0.123	3,055	2,461	594	0.427
Natural log of liquid assets (\$)	6.6	5.5	1.0 ***	0.003	6.2	5.2	0.9 ***	0.007
Liquid assets (\$)								
25th percentile	132	12	120	0.604	214	1	214	0.256
50th percentile	924	258	667 **	0.040	865	180	685 **	0.030
75th percentile	3,316	1,882	1,433 **	0.025	2,520	1,700	820	0.144
Asset ownership (participant or participant and spouse/partner)								
Homeownership (%)	15.9	13.5	2.5	0.460	6.6	7.7	-1.1	0.645
Business ownership (%)	13.9	17.7	-3.8	0.359	24.3	29.2	-4.9	0.208
Education (%)	34.8	31.6	3.1	0.575	15.8	16.8	-0.9	0.794

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means and quantile values at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). These models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race/ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). Quantile regressions for liquid assets are estimated at the 25th, 50th, and 75th percentiles of the liquid asset distribution and only control for the baseline value of liquid assets (see appendix F for more detail). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE G.2

Impacts of AFI Program on Net Worth, Material Hardship, Benefit Receipt, and Alternative Financial Service Use at the First-Year Follow-Up

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment ^a	Control ^a	Impact ^b	P-value	Treatment ^a	Control ^a	Impact ^b	P-value
Net worth (participant and spouse/partner)								
Net worth (\$)	-25,494	-12,909	-12,585	0.418	-28,326	-5,494	-22,831	0.279
Material hardship (household)								
Any hardship (%)	55.6	48.0	7.7	0.174	50.7	61.1	-10.5 **	0.044
Number of hardships (#)	1.8	2.3	-0.5	0.355	1.7	3.1	-1.4 **	0.015
Food hardship (%)	20.1	17.7	2.3	0.604	16.9	20.3	-3.4	0.394
Housing hardship (%)	20.7	18.5	2.3	0.634	12.0	15.5	-3.5	0.336
Housing hardship (#)	0.4	0.4	0.0	0.708	0.3	0.5	-0.2 *	0.083
Utilities hardship (%)	30.0	27.5	2.5	0.646	27.6	30.0	-2.4	0.585
Utilities hardship (#)	0.8	1.2	-0.3	0.240	0.6	1.2	-0.6 ***	0.008
Medical hardship (%)	36.9	35.4	1.5	0.791	31.5	44.1	-12.6 ***	0.007
Medical hardship (#)	1.2	1.3	-0.1	0.884	1.0	1.7	-0.7 **	0.022
Means-tested benefit receipt (household)								
Receive benefits (%)	70.2	66.2	3.9	0.440	75.8	67.8	8.1 *	0.058
Alternative financial service use (participant and spouse/partner)								
Use AFS credit (%)	11.7	16.4	-4.7	0.270	16.2	16.0	0.2	0.946
AFS credit (#)	0.5	0.4	0.1	0.760	0.5	0.5	0.0	0.820
Use nonbank check cashing (%)	5.4	4.3	1.1	0.692	5.6	12.9	-7.4 **	0.013
Nonbank check cashing (#)	0.1	0.2	0.0	0.731	0.2	0.4	-0.2 *	0.081

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque (N = 248) and Los Angeles (N = 374) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). The models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race/ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (i.e., site location, baseline survey start date, and first-year follow-up survey mode). There are statistically significant differences between the Albuquerque and Los Angeles impact estimates for any hardship (%), has medical hardship (%), and use nonbank check cashing (%).

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE G.3

Impacts of AFI Program on Employment, Earnings, Income, and Credit Score at the First-Year Follow-Up

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment ^a	Control ^a	Impact ^b	P-value	Treatment ^a	Control ^a	Impact ^b	P-value
Employment (participant) and earnings and income (household)								
Employed (%)	72.1	70.3	1.8	0.738	80.5	80.8	-0.3	0.932
Monthly earnings (\$)	1,242	1,190	53	0.809	1,699	1,921	-221	0.352
Natural log of monthly earnings (\$)	5.4	4.8	0.7	0.112	6.0	5.8	0.2	0.616
Monthly income (\$)	1,512	1,442	70	0.791	2,211	1,985	225	0.389
Natural log of monthly income (\$)	6.2	5.7	0.6	0.186	7.1	6.6	0.5 *	0.061
Credit score (participant)^c								
Vantage 2.0 score (501–990)	669.5	666.5	3.0	0.758	—	—	—	—
Change in Vantage 2.0 score (#)	12.0	6.5	5.5	0.419	—	—	—	—

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque ($N = 248$) and Los Angeles ($N = 374$) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b With the exception of credit score, the impact estimates are obtained using ordinary least squares regression models for continuous outcome variables and using a probit model for the binary outcome variable (for which we report the marginal effects). Regression models control for the baseline value of the dependent variable, baseline characteristics (age, sex, race/ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (site location, baseline survey start date, and first-year follow-up survey mode). Credit score impact estimates are not regression adjusted. There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

^c Credit score is the Vantage 2.0 scores for all randomly assigned participants in Albuquerque.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

TABLE G.4

Impacts of AFI Program on Personal Outlook, Community Involvement, and Time Preference at the First-Year Follow-Up

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment ^a	Control ^a	Impact ^b	P-value	Treatment ^a	Control ^a	Impact ^b	P-value
Personal outlook (participant)								
Perceived financial security (1–10)	5.3	5.0	0.3	0.440	5.4	4.7	0.7 **	0.030
Worse off financially (%)	16.3	33.4	-17.1 ***	0.001	13.9	20.0	-6.1	0.112
Self-esteem (0–30)	23.2	23.3	-0.2	0.792	23.1	23.1	0.0	0.994
Community involvement (participant)								
Time preference (participant)								

Source: AFI first-year follow-up and baseline surveys.

Notes: Sample includes respondents from Albuquerque ($N = 248$) and Los Angeles ($N = 374$) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

^a Values in these columns are the regression-adjusted means at the first-year follow-up.

^b The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables and using probit models for binary outcome variables (for which we report the marginal effects). All models control for the baseline value of the dependent variable, baseline characteristics (age, sex, race/ethnicity, English proficiency, educational attainment, marital status, number of adults and children in household, and annual household income), and study participation variables (site location, baseline survey start date, and first-year follow-up survey mode). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

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