



RESEARCH REPORT

# From Savings to Ownership

## Third-Year Impacts from the Assets for Independence Program Randomized Evaluation

### Final Report

*November 2019 (updated August 2020)*

*OPRE Report #2019-106*

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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, last funded in fiscal year 2016, is a federally supported IDA demonstration grant program authorized under the Assets for Independence Act of 1998. This evaluation of the AFI program uses a randomized controlled trial, the gold standard for measuring program effectiveness.

This evaluation shows that among the full sample of study participants, AFI did not increase homeownership, business ownership, or postsecondary education or training three years after study enrollment. But exploratory subgroup analyses provide suggestive evidence that AFI participation had effects on certain subgroups. Specifically, we find suggestive evidence that AFI increased homeownership among renters at study enrollment and increased business ownership among non-business owners at study enrollment. Results from the subgroup analyses suggest the following:

- A 4.7 percentage-point (52 percent) increase in the homeownership rate among participants who rented at study enrollment
- A 5.1 percentage-point (53 percent) increase in the business ownership rate among participants who were not business owners at study enrollment

Beyond asset ownership, we assess the program's medium-term effects on participants' savings, material hardship, use of alternative financial services, and personal outlook, among other outcomes. We also find evidence that AFI affects several secondary outcomes:

- A 25 percent reduction in the number of hardships (0.6 hardships) related to utilities, housing, or health, which suggests that AFI participation improves financial stability and thereby reduces material hardship.
  - » Examining the specific types of hardship, AFI reduced the number of medical hardships experienced (i.e., could not afford to see a doctor, dentist, or purchase a prescription drug when needed) by 41 percent (0.5 hardships) and reduced the likelihood of experiencing a medical hardship by 29 percent (10 percentage points).
- A 47 percent (3 percentage-point) decline in the use of alternative (nonbank) check-cashing services, which suggests that AFI participation helps people enter and remain in the financial mainstream.
  - » AFI shifted people's time preferences so they were more willing to pay a cost today rather than a higher cost later. Specifically, the share of people willing to accept an interest rate of 25

percent or more on a major purchase (e.g., refrigerator) declined 35 percent (5 percentage points). Because high-interest-rate products can cause financial difficulties later, shifting behavior to be more future oriented can lead to long-term improvements in financial well-being.

Although AFI did not result in greater asset ownership among the full sample, these third-year findings—that AFI participation results in greater asset ownership among renters and non-business owners, less material hardship, lower use of alternative (nonbank) check-cashing services, and more future-oriented time preferences—combined with the first-year findings that AFI increased savings, suggest that savings and wealth-building opportunities can promote economic well-being and personal responsibility. This evaluation provides results that can inform the next stage of incentivized savings programs that benefit low-income earners. Findings from this evaluation suggest that by encouraging low-income families to build assets, AFI eased economic hardship and increased asset ownership while providing a foundation for long-term upward mobility.

# Executive Summary

This report presents medium-term (third-year) findings from a randomized evaluation of the Assets for Independence (AFI) program, a former federally supported individual development account (IDA) grant program authorized under the Assets for Independence Act of 1998. AFI IDAs are matched savings accounts where personal deposits are matched only when used for allowable asset purchases, which include a first-time home purchase, business capitalization, and postsecondary education or training. AFI used IDAs, which are commonly coupled with financial education, to help low-income households achieve greater economic 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. Federal fiscal year 2016 marked the final year of five-year grant awards under the demonstration, as no funds were appropriated for the AFI program for 2017. Organizations operating AFI projects with 2016 (or earlier) grants continued the projects for the funded project period.

This evaluation provides findings that can inform the next stage of incentivized savings programs to benefit low-income earners. This study examines evidence from the demonstration to assess the AFI program's effects on participants, addressing the following research questions through an experimental design:

- What are the impacts of AFI program participation on the intermediate outcomes it is designed to address (e.g., homeownership, business ownership, postsecondary education or training)?
- How do the impacts of AFI program participation vary over time and by participant characteristics?<sup>1</sup>
- What are the impacts of AFI program participation on other outcome domains that the program might influence (e.g., material hardship, alternative financial product use, economic well-being)?

We randomly assigned study participants at two AFI project sites to a treatment group and a control group, allowing us to attribute differences in outcomes between the two groups to the AFI program.<sup>2</sup> This is the first large-scale, multisite study to evaluate the AFI program using a randomized design. Our early impact report found that AFI increased participants' savings one year into the program.

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<sup>1</sup> A related synthesis brief discusses findings over time, comparing the first- and third-year impact estimates.

<sup>2</sup> "AFI project" refers to IDA programs operated by AFI grantees, funded by the federal AFI program.



Among all study participants, AFI did not statistically significantly increase homeownership, business ownership, or postsecondary education or training three years after study enrollment. However, exploratory subgroup analyses provide suggestive evidence that AFI increased homeownership among renters at study enrollment and business ownership among non-business owners at study enrollment.

## Study Design and Implementation

The two participating AFI evaluation sites were in Albuquerque and Los Angeles: Prosperity Works in Albuquerque—with its partner at Central New Mexico Community College (CNM), the student support department known as CNM Connect—and RISE Financial Pathways in Los Angeles. CNM served AFI participants in a student center that offers academic and financial coaching and connects students to other college and community resources. RISE Financial Pathways is 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, having large enough AFI grants to serve the sample size required by the evaluation, experience operating AFI projects, mainstream characteristics regarding asset types offered, and past experience working with research studies.

At both sites, AFI participants had financial education requirements and could receive match funds on savings up to \$1,000. In Albuquerque, participants could receive a \$4 match for every \$1 saved. In Los Angeles, participants could receive \$2.50 for every \$1 saved. The AFI project holds match funds separately from personal IDA savings and pays the match directly toward the asset purchase. Both sites had a minimum savings period of six months, and the Los Angeles site had a maximum savings period of 24 months. Participants used their savings, plus the AFI project savings match, to purchase an asset. The federal grant funded half the match, and half had to be provided by the grantee from nonfederal sources.

Both AFI sites 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 (Mills et al. 2016). Over 19 months (January 2013 to July 2014), 807 people enrolled in the study (299 in Albuquerque and 508 in Los Angeles). Among the 807 study participants, 407 were assigned to the treatment group and 400 were assigned to the control group. Most treatment group members—91 percent in Albuquerque and 71 percent in Los Angeles—opened an IDA and made at least one deposit (Mills et al. 2016).

This third-year report draws primarily on a baseline survey completed at study enrollment and a follow-up survey conducted roughly three years after study enrollment (between April 2016 and September 2017). Completion of the baseline survey was required before random assignment, so the baseline survey response rate is 100 percent. The third-year follow-up survey achieved a 77 percent response rate, yielding an analysis sample of 621 people.

The Los Angeles site faced difficulties securing the nonfederal funds required by the AFI program. These difficulties meant that AFI participants in Los Angeles who tried to make a matched withdrawal after May 2015 did not receive the promised match for an asset purchase. This is consistent with survey data that show that Los Angeles participants were about half as likely as Albuquerque participants to make a matched withdrawal. Specifically, according to survey data only 23 percent of treatment group members in Los Angeles made a matched withdrawal by the third year, compared with 51 percent in Albuquerque. Also, among AFI participants in Los Angeles who made an *unmatched* withdrawal, 30 percent reported doing so because match funds were unavailable. The fact that matched withdrawals for asset purchases were not available to many Los Angeles participants likely dampens our third-year impact estimates.

## Estimated Third-Year Program Impacts on Participants

We use the experimental design to estimate medium-term (third-year) 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 six figures below.<sup>3</sup>

### Primary Outcome: AFI Increased Assets

Among all study participants, we find no statistically significant effect of AFI on homeownership, business ownership, or postsecondary education or training. However, exploratory subgroup analyses provide suggestive evidence that AFI participation increased homeownership among the subgroup of *renters* (at study enrollment) and increased business ownership among the subgroup of *non-business owners* (at study enrollment) (figure ES.1). Results from the subgroup analyses suggest the following:

- AFI participation led to a 4.7 percentage-point (52 percent) increase in the homeownership rate at the third-year follow-up among participants who rented at study enrollment.
- AFI participation led to a 5.1 percentage-point (53 percent) increase in the business ownership rate at the third-year follow-up among participants who did not own businesses at study enrollment.

Though we do not find an increase in asset ownership among all study participants, these increases in home and business ownership among renters and non-business owners, respectively, suggest that AFI

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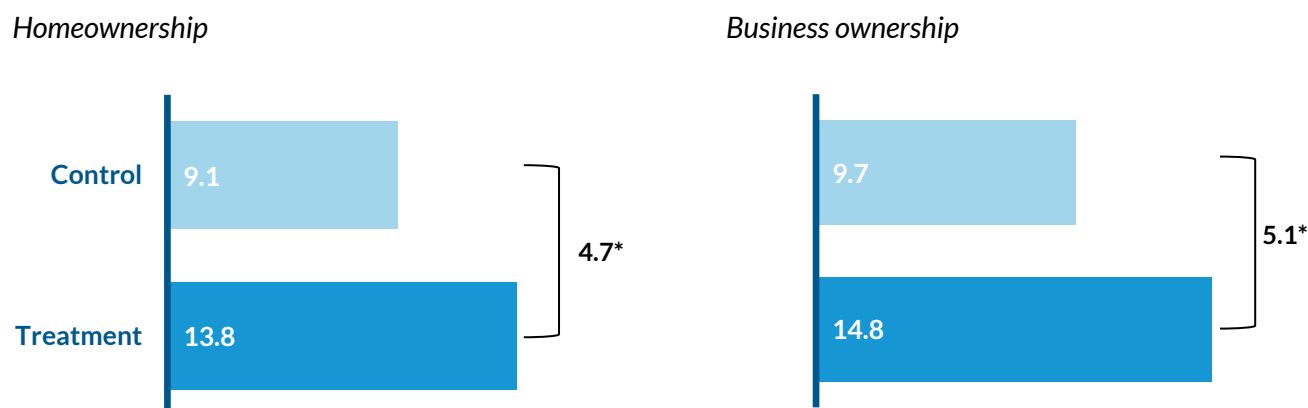
<sup>3</sup> We highlight impact estimates that are statistically significant at the 1 percent, 5 percent, and 10 percent levels, but we interpret significance at the 10 percent level as providing only suggestive evidence of AFI impacts.

increased assets. Our conceptual framework considers asset ownership a primary hypothesized effect and outcome.

FIGURE ES.1

### AFI Increased Homeownership among Renters and Business Ownership among Non-Business Owners

*The impact of AFI on homeownership and business ownership*



**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Home and business ownership is measured at the third-year follow-up survey (roughly 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and third-year follow-up surveys and did not have missing data for key variables.

\*  $p < 0.1$ .

## Secondary Outcomes

Among the secondary outcomes, we find that AFI reduced material hardship and the use of nonbank check-cashing services. We also find some evidence that AFI improved participants' perceived financial security and shifted their time preferences to be more future oriented (i.e., more willing to pay today rather than accept a higher cost in the future).

### REDUCTIONS IN MATERIAL HARDSHIP

These third-year findings indicate that AFI reduced material hardship for three of nine hardship measures, which capture hardship 30 to 36 months after study enrollment.

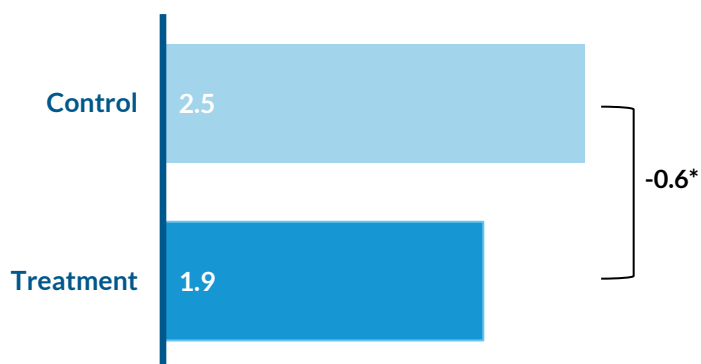
- AFI participation led to a 25 percent (0.6 hardships) reduction in the number of total hardships experienced—that is, the number of times participants could not pay for housing, utilities, or needed medical care (figure ES.2).

- AFI participation led to a 41 percent (0.5 hardships) reduction in the number of medical hardships (i.e., could not afford to see a doctor or dentist or purchase prescription drugs) experienced and a 29 percent (10.3 percentage-point) reduction in the likelihood of experiencing a medical hardship (figure ES.3).

FIGURE ES.2

### AFI Reduced Number of Hardships Experienced

*The impact of AFI on number of hardships*



**Source:** AFI third-year follow-up and baseline surveys.

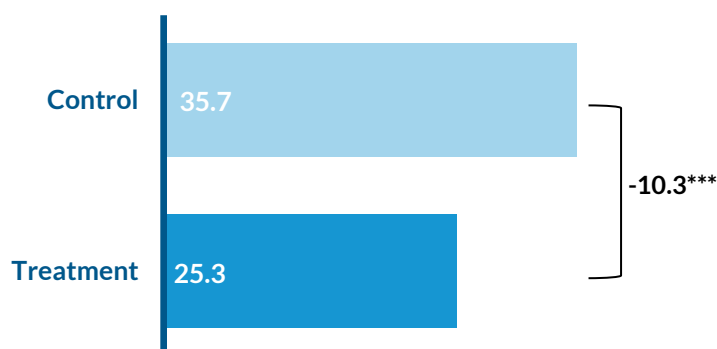
**Notes:** Material hardship is measured in the 6 months before the third-year follow-up survey (30 to 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

\*  $p < 0.1$ .

FIGURE ES.3

### AFI Reduced Share with Medical Hardship

*The impact of AFI on share with medical hardship*



**Source:** AFI third-year follow-up and baseline surveys.

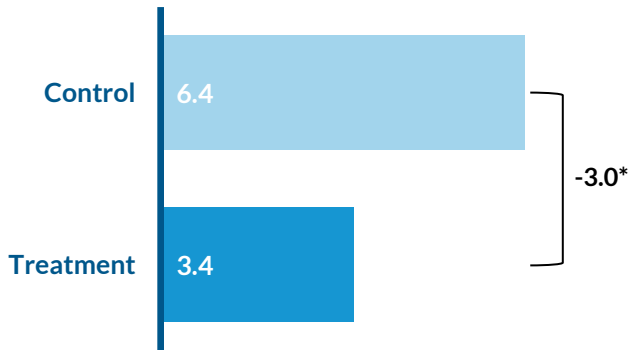
**Notes:** Medical hardship is measured in the 6 months before the third-year follow-up survey (30 to 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

\*\*\*  $p < 0.01$ .

REDUCED NONBANK CHECK-CASHING USE

The third-year results provide evidence that AFI participation reduced the use of alternative (nonbank) check-cashing services, possibly because of greater familiarity with banking options. AFI led to a 47 percent (3 percentage-point) decline in the use of alternative (nonbank) check-cashing services, suggesting that AFI participation helps people enter and remain in the traditional banking system (figure ES.4).

FIGURE ES.4  
**AFI Reduced Use of Alternative Financial Services**  
*The impact of AFI on the use of nonbank check-cashing services*



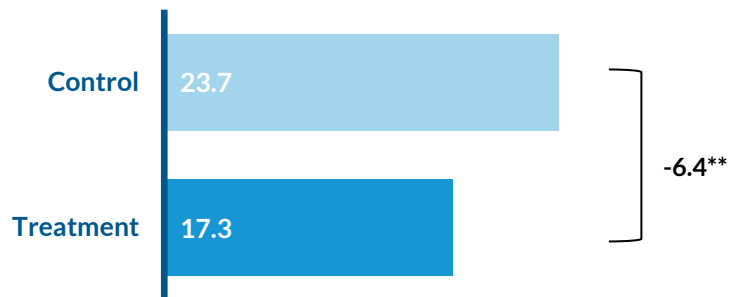
**Source:** AFI third-year follow-up and baseline surveys.  
**Notes:** Use of nonbank check-cashing service is measured in the 12 months before the third-year follow-up survey (24 to 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.  
\*  $p < 0.1$ .

BETTER FINANCIAL WELL-BEING

AFI exhibited a positive impact on one of the four financial well-being outcomes. Specifically, we find that AFI led to a 27 percent (6.4 percentage-point) decline in the share of participants who reported that their financial situation worsened in the past year (figure ES.5). But we find no evidence that AFI increased the share of participants whose financial situation improved in the past year, nor that AFI increased participants' ability to make ends meet or improved their perceived financial security.

FIGURE ES.5

**AFI Participants Were Less Likely to Report Being Worse Off Financially**  
*The impact of AFI on perceived financial well-being*



**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Perceived financial security is measured in the 12 months before the third-year follow-up survey (24 to 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

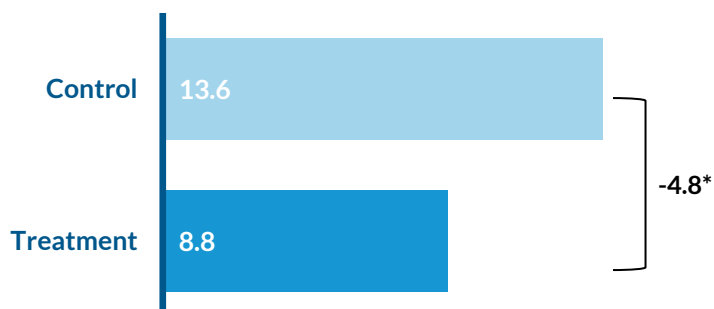
\*\*  $p < 0.05$ .

**SHIFTED TIME PREFERENCES TOWARD BEING FUTURE ORIENTED**

AFI shifted people's time preferences so that they are more willing to pay a given cost today rather than a higher cost later (i.e., they become more future oriented). Figure ES.6 shows that AFI led to a 35 percent (4.8 percentage-point) decrease in the share of people willing to accept an interest rate of 25 percent or more on a major purchase (e.g., refrigerator). Shifting behavior in this way can improve AFI participants' long-term financial well-being, as high-interest-rate products can lead to financial difficulties later.

FIGURE ES.6

**AFI Reduced the Interest Rate Participants Were Willing to Pay for a Major Purchase**  
*The impact of AFI on willingness to accept a 25 percent interest rate on a major purchase*



**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Willingness to accept an interest rate of 25 percent or more is measured at the third-year follow-up survey (roughly 36 months after study enrollment). We present regression-adjusted impact estimates. Sample sizes for specific outcomes may vary because of missing values. The maximum sample consists of 621 respondents who completed the baseline and follow-up surveys and did not have missing data for key variables.

\*  $p < 0.1$ .

These third-year findings—that AFI participation resulted in greater asset ownership among renters and non-business owners, less material hardship, lower use of alternative (nonbank) check-cashing services, improved financial well-being, and more future-oriented time preferences—combined with first-year findings that AFI increased savings provide empirical evidence that savings and wealth-building opportunities promote economic well-being and personal responsibility.

## Emerging Insights and Potential Implications

The third-year findings—that AFI participation resulted in increased homeownership among renters, increased business ownership among non-business owners, and reduced economic hardship—suggest that AFI increased asset ownership among participants who started the program without these key assets, thereby promoting economic security for individuals who needed the greatest support. These preliminary positive effects may warrant further examination. Additionally, the study’s finding that AFI participation reduced the use of nonbank check-cashing services suggests that AFI may have helped participants increase financial capability and obtain or use more stable or secure forms of banking. Further, the findings show that AFI reduced material hardship and improved financial well-being, which can have positive spillover effects into other domains, such as improved mental health, as individuals become more financially secure. These findings suggest the following emerging insights and potential implications:

- **Incentivized savings programs such as IDAs can help low-income people build assets, particularly among those who have yet to make an asset purchase.** The AFI program was one of few federal

efforts that encouraged low-income people to save and build assets. Savings penalties (asset limits) for benefit programs create a disincentive to save (Ratcliffe et al. 2016). Michael Sherraden's original IDA proposal in 1991 included universal, progressive, lifelong accounts (Sherraden 1991). The AFI program provided a small demonstration of this vision and can offer insights relevant to other public policy efforts to improve the financial health, security, and well-being of low-income earners and their households.

- **Incentivized savings programs can help connect low-income people to mainstream financial services.** By connecting people to a bank or credit union—so they cash checks at a bank rather than a nonbank check-cashing outlet—incentivized savings programs help low-income people stay out of the alternative financial services market. Because alternative financial services products tend to be more expensive, diversion can have long-term benefits.
- **Difficulty securing nonfederal funds can be an impediment to scale and evaluation.** The Los Angeles site could not provide the nonfederal match funds necessary for participation in the AFI program, and consequently, 30 percent of participants who made an unmatched withdrawal did so because match funds were not available. Difficulty in raising nonfederal funds was an impediment to project scale-up in many other AFI settings (DeMarco, Mills, and Ciurea 2008), limiting the number of potential sites for a large-sample program evaluation.
- **Limited allowable postsecondary education uses is a program limitation.** We do not find any effects of AFI participation on education or training outcomes, even among those without a college degree at study enrollment. The requirement that AFI IDA dollars for postsecondary education be used only for tuition, fees, and books and supplies bought directly from an eligible educational institution is one possible explanation. Community colleges are already free or nearly free to many low-income students eligible for Pell grants. Future incentivized savings programs that include a postsecondary education focus could consider accommodating additional expenses not paid directly to an eligible educational institution, such as internet, computers, transportation and parking, standardized tests, and living expenses (e.g., rent, groceries) to improve education and training success.
- **Incentivized savings programs present an opportunity for integrated service delivery, connecting savings and financial education with existing program services to maximize the program's reach.** Some AFI grantees—such as the Albuquerque site, which operated at a community college—integrate their AFI projects into other programmatic activities that serve low-income households (Mills et al. 2019). Integrating incentivized savings programs into existing programs (college, safety net, employment) may increase participants' financial capability and may help programs achieve faster and longer-lasting results that will save program money in the future.



- **There is still space for future research.** Disentangling the relative roles of the IDA account, promise of a savings match, financial education or coaching, and other support services would be a fruitful area for future research. Particularly, understanding the mechanisms through which AFI participation reduced economic hardship could inform future program design. In addition, future research could investigate the tradeoffs between expanding AFI to more participants (lower service level) or providing more resources and services to a limited number of participants (higher service level). Large scale and sample sizes would likely be needed for such evaluations. Finally, future asset-building research should build in subgroup analyses that account for differences in study participants' asset holdings at study enrollment.

# Authors' Note

This report was updated August 4, 2020. In the overview and executive summary, we updated the description of our findings to clarify that our exploratory analysis *suggests* that, in the medium term, AFI increased homeownership among renters and increased business ownership among non-business owners. We also added a sentence noting that the study identified preliminary positive effects that may warrant further examination.

# 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 where personal deposits are matched when used for specific investments such as a home purchase, business capitalization, or postsecondary education or training. 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 of 1998, was the largest funding source for IDAs in the United States. Congress last funded the AFI program in fiscal year (FY) 2016. AFI grants were fully funded for five years at the time of award, so grants made in FY 2016 and before continue to operate through the completion of their project period. No new grants have been awarded since FY 2016. 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), the AFI discretionary grant program is one of the few federal subsidies providing incentives for low-income families to save.

In AFI-funded IDA projects, participants saved toward an allowable asset purchase: a first-time home, business capitalization, or postsecondary education or training. Once participants reached their savings goals, they used their savings plus match funds provided by the AFI project to purchase an asset. Half the match funds come from the federal grant, and half must be provided by the grantee from nonfederal sources. Besides matching funds, AFI projects assisted participants in obtaining skills, information, and other support to increase economic independence and general well-being through asset purchases. Assets held in AFI accounts were disregarded in determining federal means-tested program eligibility and benefits, thus avoiding savings penalties common in programs targeting low-income families.

Despite a large literature on IDAs (Harris et al. 2014; McKernan and Sherraden 2008; Zielewski et al. 2009), the medium-term effects of participating in AFI-funded IDAs have not been evaluated using a randomized controlled trial. This report presents medium-term findings (approximately three years after study enrollment) from a randomized evaluation undertaken at AFI project sites in Albuquerque, New

Mexico, and Los Angeles, California. It builds on the early impact findings (approximately one year after study enrollment) presented by Mills and colleagues (2016).

This report presents the third-year impact of participation in AFI-funded IDA projects on the asset ownership and economic well-being of low-income adults and their families. The primary medium-term hypothesized effects relate to asset purchases and associated secondary economic well-being effects. The research questions are as follows:

- What are the impacts of AFI program participation on the intermediate outcomes it is designed to address (e.g., homeownership, business ownership, postsecondary education or training)?
- How do the impacts of AFI program participation vary by participant characteristics?<sup>4</sup>
- What are the impacts of AFI program participation on other outcome domains that the program might influence (e.g., material hardship, alternative financial product use, economic well-being)?

The 807 study members enrolled in the study between January 2013 and July 2014, with the analyses capturing program impacts roughly three years after enrollment.

The remainder of this chapter describes IDAs and the AFI program, presents the conceptual framework, and discusses AFI first-year evaluation and other IDA research findings.

## IDAs and the AFI Program

Through fiscal year 2016, AFI has provided \$267 million in grant funds to support 978 AFI grants nationwide.<sup>5</sup> Each AFI grantee designed its project to meet community needs within basic federal restrictions regarding participant eligibility and allowed asset purchases. Thus, AFI projects differed by nonfederal funding sources, whether one or several agencies operated the project, characteristics of accounts offered (e.g., match rate, maximum matchable savings amount, minimum deposits), whether projects allowed 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 were required to meet the following federal guidelines:

- A match of between \$1 and \$8 for every \$1 in participant savings;

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<sup>4</sup> A related synthesis brief discusses findings over time, comparing the first- and third-year impact estimates.

<sup>5</sup> Fiscal year 2016 is the last year covered by the most recent AFI Report to Congress (OCS 2018). “AFI project” refers to IDA programs operated by AFI grantees and funded by the federal AFI program. In fiscal year 2016, 207 grantees operated the 335 active AFI grants.

- No more than \$4,000 in match funds paid to each participant (and no more than \$8,000 to a household), with at least 50 percent of the match funded by the grantee's 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;
- 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); and
- 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 primary residence and one vehicle.

During the nearly two decades since AFI's enactment, the operating environment for IDA projects evolved. The population in poverty became more racially and ethnically diverse and increasingly more Hispanic.<sup>6</sup> Fewer traditional community-action agencies and more educational or training organizations, such as community colleges, sought and received AFI funding. Projects also sought to operate at greater scale.<sup>7</sup>

## Conceptual Framework

The conceptual framework that underlies IDAs was first articulated by Sherraden (1991) and was developed further through other asset-building research, including the first-year follow-up AFI program evaluation report (Lerman and McKernan 2008; McKernan and Sherraden 2008; Mills et al. 2016).<sup>8</sup> Below, we describe the framework that motivates our choice of outcome variables and our basic analytic approach for estimating AFI program effects.

<sup>6</sup> Among people below the federal poverty level, the Hispanic share increased from 23.4 percent in 1998 to 27.4 percent in 2016. See US Census Bureau, Historical Poverty Tables: People and Families—1959 to 2016 (<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 2016.

<sup>7</sup> Tables in the AFI annual reports to Congress for 2009 and 2016 illustrate these trends (OCS 2010, 2018).

<sup>8</sup> 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 directly toward the asset when deposits are withdrawn for allowable asset purchases (first-home purchase, business capitalization, and postsecondary education or training). The potential match provides an incentive to accumulate assets by saving a portion of earned income.
- **Assistance in obtaining the skills, information, and other support to enable asset purchases.** This usually consists of financial education (i.e., instruction in basic financial management), financial coaching, asset-specific training, and other supportive services. It also includes the exclusion of AFI account balances from countable assets in determining a participant's eligibility for other federal means-tested benefits.

Conceptually, all three central elements promote participant savings. Financial education, training, and coaching provide useful information about budgeting, credit building and repair, and pursuing asset-specific strategies (home buying, business planning, and educational advancement). The IDA is the financial tool by which clients can act on their desire to save. Potential match funds provide incentives 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. Withdrawals for authorized asset purchases are matched with funds held separately and paid directly toward the asset purchase. Emergency withdrawals can cover medical expenses, rent or mortgage payments, or living expenses following loss of employment.

We group the hypothesized effects of AFI participation into primary and secondary outcomes within 11 domains. We group into primary and secondary outcomes because the pathways for the secondary outcomes are largely through the primary outcomes.

The primary hypothesized early effect (i.e., one year after project enrollment) is increased savings, and the primary hypothesized medium-term effect (i.e., three years after project enrollment) is increased asset ownership associated with allowable AFI asset purchases: first-home purchase, business capitalization, and postsecondary education or training.

TABLE 1.1

**Primary Hypothesized Effects of AFI Program Participation**

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

In the medium term, we expect enough time to have passed for AFI participants to save, receive the match, and complete their allowable asset purchases, so we hypothesize participation to increase homeownership (among renters), business ownership (among non-business owners), and postsecondary education or training.<sup>9</sup> The medium-term effect on savings (liquid assets) is ambiguous. Savings could decrease if savings before enrollment are used to make and maintain allowable asset purchases; savings could remain unchanged if only savings after enrollment are used to make and maintain the asset purchase; and savings could increase if participants form a regular savings habit and continue to save beyond their allowable asset purchases and maintenance.

We hypothesize AFI participation to affect secondary outcomes in the third year. We identify nine secondary domains (table 1.2). Net worth could increase because of the new assets or decrease because of the transaction costs (e.g., closing costs associated with a home purchase) and loans associated with the asset purchases (e.g., student loans). Given these offsetting effects, we expect no significant medium-term effect of AFI on net worth.

Through improved financial stability, we hypothesize that AFI IDAs will reduce material hardship and improve personal financial outlook in the medium term. AFI connects participants to the financial mainstream by providing a savings account and is hypothesized to reduce the use of alternative financial services, such as nonbank check-cashing stores. 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.

<sup>9</sup> AFI allowed purchases associated with first-home purchase and business capitalization. This evaluation measures homeownership and business ownership, which are proxies for what AFI allowed.

TABLE 1.2

**Secondary Hypothesized Effects of AFI Program Participation**

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

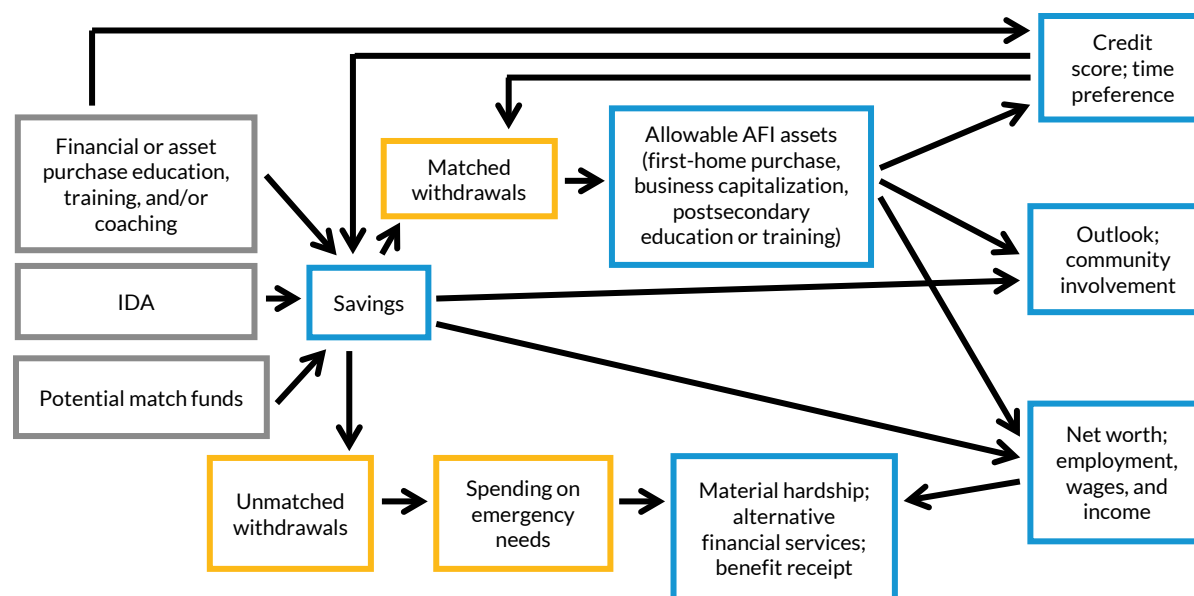
We further hypothesize that, by having promoted the account holder's investments in human capital and business capital, AFI participation will increase employment and earnings and reduce public benefit receipt. But earnings and income could drop if enough participants are in school or if participants' earnings fall as they grow a business. Though there are offsetting effects, we expect the increase to dominate.



Secondary impacts associated with asset purchase (e.g., increased community involvement) are expected to be small in the medium term given the short asset ownership period captured in this third-year evaluation. Participants may only have owned their AFI IDA-purchased assets for less than a year, because it can take two years (or longer) to save.<sup>10</sup> Finally, by saving and building assets, we hypothesize that AFI will shift time preferences so that participants become less present oriented and more future oriented.

Figure 1.1 presents the pathways through which AFI is hypothesized to affect participant outcomes.

**FIGURE 1.1**  
**Pathways for Participant Impacts**



## AFI First-Year Evaluation Findings

We summarize the AFI early (first-year) findings, reported by Mills and colleagues (2016), to contextualize this report's third-year findings.

<sup>10</sup> The maximum time participants could have owned IDA-purchased assets at the third-year follow-up was two and a half years. The AFI statute requires that participants wait at least six months after opening their IDA to make a matched withdrawal. But most participants in this study owned their assets less than two years. The first-year AFI program evaluation finds that 75 percent of treatment group members who opened an IDA had not made a matched withdrawal one year after study enrollment (Mills et al. 2016).

Both AFI sites 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. First-year impact results show two beneficial early 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 (not including match funds)

Because Mills and colleagues (2016) examined all liquid assets—including savings, checking, money market, and retirement accounts plus stocks and bonds—the results indicate that participants did not simply shift savings from other types of accounts into their IDAs but instead created new savings.

First-year impact results also find evidence that AFI affects several secondary outcomes:

- A 34 percent reduction in utilities, housing, or health hardships, equivalent to one less hardship
- 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. We explain how these results fit into the IDA savings literature below.

## Research on IDA Savings Programs

This study builds on previous research evaluating both AFI IDAs and other IDAs. The AFI IDA literature includes early (first-year) experimental evidence from Mills and colleagues (2016) and nonexperimental analyses (McKernan et al. 2011; Mills, Lam, et al. 2008). Much of the earlier IDA literature comes from the American Dream Demonstration, implemented from 1999 to 2003 in one experimental site (Tulsa, Oklahoma) and several nonexperimental sites. A growing literature looks to IDAs both in the US and other developed countries to increase higher education (Azzolini et al. 2018).

IDA programs demonstrate that low-income families save when provided a savings account, financial incentives, and financial education (Mills, Lam, et al. 2008; Mills et al. 2016; Schreiner and Sherraden 2007a; Stegman and Faris 2005). As mentioned, the first-year experimental AFI evaluation finds that after one year AFI led to a \$657 median increase in savings before matching funds (Mills et al. 2016).<sup>11</sup> But the few studies

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<sup>11</sup> The earlier nonexperimental AFI evaluation finds no significant effect of AFI participation on liquid assets (Mills, Lam, et al. 2008).

that examine net worth do not find that IDA program participation increases net worth (Mills, Gale, et al. 2008; Mills, Lam, et al. 2008; Schreiner and Sherraden 2007b).

While the AFI first-year experimental evaluation finds reduced material hardship and increased confidence in participants' ability to meet normal monthly living expenses (Mills et al. 2016), earlier research does not find such links. The experimental American Dream Demonstration evaluation examines material hardship only 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).

Consistent with the AFI first-year evaluation finding of no effect on asset ownership or net worth, the experimental American Dream Demonstration evaluation finds no sample-wide effects on asset holdings or net worth at 18 months (Mills et al. 2004). Although the American Dream Demonstration evaluation had no effect on homeownership at 18 months, the demonstration increased homeownership among renters at the four-year follow-up (Grinstein-Weiss et al. 2008; Mills, Gale, et al. 2008). The American Dream Demonstration four-year follow-up did not, however, find increases in business ownership (overall or among non-business owners) or educational achievement (Grinstein-Weiss et al. 2012).

The experimental American Dream Demonstration 10-year follow-up finds that though most IDA participants had positive homeownership outcomes, the control group caught up with the treatment group, so there were no long-term, statistically significant differences in the homeownership rate between the treatment group (IDA participants) and control group (nonparticipants) (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 nevertheless important. A nonexperimental study shows that 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 (McKernan et al. 2011).

The American Dream Demonstration 10-year follow-up does find a significant positive impact on education enrollment (Grinstein-Weiss et al. 2012). Specifically, they find that treatment group members with a baseline level of high school or less are likely to gain "some college" compared with the control group.

# Chapter 2. Study Sites, Research Design, and Participant Characteristics

This chapter presents a brief overview of the study sites, with key site features and program eligibility, study enrollment and random assignment, and participant demographic characteristics.

## Study Sites

### Site Selection

We selected study sites according to six criteria, reflecting 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 the evaluation and willingness to implement the experimental design, and (6) project features similar to other mainstream AFI projects.

Based on AFI grantee data, including a qualitative scan of all grantees and phone calls with a subset of grantees, two sites were deemed qualified and 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 support department known as CNM Connect.<sup>12</sup> Both sites had large enough AFI grants to serve the sample size required by the evaluation, had experience operating IDA projects, had mainstream characteristics regarding asset types offered, and had experience working with research studies. They both had few intake locations and a single model of the project, which simplified evaluation implementation.

### Site Profiles

Prosperity Works was founded in Albuquerque in 2000 and received its first AFI grant in 2006. Prosperity Works did not offer AFI IDAs directly but operated the New Mexico Assets Consortium, a partnership with other organizations that provided the IDAs. This can be a model delivery system for a rural state that relies

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<sup>12</sup> CNM Connect was recently renamed Connect Services.

on the known and respected entities in communities statewide. Under this evaluation, Prosperity Works' program partner was CNM Connect at Central New Mexico Community College.

CNM Connect is a one-stop student resource center with six locations across CNM's campuses available to all 40,000 CNM 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 received nonfederal match funds raised by the CNM Foundation. Staff at Prosperity Works administered and monitored the AFI accounts, monitored savers' credit scores, and provided credit-building information.

RISE Financial Pathways (RISE)—a small nonprofit community development financial institution in South Central Los Angeles—focused 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, although it had previously been a partner of two different AFI grantees. Besides AFI, RISE offered business training and small business loan programs. RISE previously offered two accelerated savings programs and was an enrollment site for a matched savings program for children's education, but the funding for these ended during the evaluation.

RISE faced obstacles late in its implementation. Many participants could not obtain matched funds because of the site's failure to provide the nonfederal match. With few exceptions, only enrollees who finished saving before May 2015 could make a matched withdrawal.<sup>13</sup> Moreover, beginning in April 2015, RISE staff departures made it difficult for participants to get in touch with staff about withdrawals or other concerns.

## Key Site Features

The Albuquerque and Los Angeles sites shared some key project features but differed in other respects (table 2.1). Both sites allowed all three types of AFI asset purchases: first-home purchase, business capitalization, and postsecondary education or training. Both sites had a maximum matchable savings amount of \$1,000, and both allowed AFI participants to make partial matched withdrawals of less than \$1,000. But the Los Angeles project required participants to reach the \$1,000 savings target before making any partial matched withdrawals; the Albuquerque site did not require this. 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. The Los Angeles site had a maximum savings period of 24 months, but in Albuquerque, participants could

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<sup>13</sup> With people enrolling into the study between January 2013 and July 2014, May 2015 is 28 to 10 months after enrollment. Most participants did not reach RISE's maximum 24-month savings period by May 2015.

save longer than 24 months.<sup>14</sup> 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). How the study sites compare with other AFI projects nationwide at the time of study enrollment (2013–14) speaks to the evaluation’s external validity (how much the evaluation represents AFI nationwide).

**TABLE 2.1**  
**Major AFI Project Features**

	<b>Albuquerque</b>	<b>Los Angeles</b>
<b>Site</b>	Prosperity Works (grantee) and CNM Connect of Central New Mexico Community College (partner)	RISE Financial Pathways, formerly Community Financial Resource Center (grantee)
<b>Geographic area for sample recruitment</b>	Metropolitan Albuquerque, NM, and surrounding vicinity	Los Angeles County, CA
<b>Allowable asset types</b>	First-time home purchase, business capitalization, and postsecondary education or training	First-time home purchase, business capitalization, and postsecondary education or training
<b>Maximum savings amount eligible for match</b>	\$1,000	\$1,000 (required savings amount before any matched withdrawal)
<b>Match rate</b>	4:1	2.5:1
<b>Minimum savings period</b>	6 months	6 months
<b>Maximum savings period</b>	24 months <sup>a</sup>	24 months
<b>Financial education</b>	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)
<b>Homeownership training provider</b>	Homestart	Several external agencies
<b>Small business training provider</b>	Small Business Development Center at CNM	RISE staff
<b>Postsecondary education training provider</b>	CNM Connect academic coaches (worked to develop an education plan)	RISE staff (reviewed information on financial aid)

<sup>a</sup> Staff 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.

The approach to financial education differed between the two sites. The Albuquerque site required a one-semester community college financial education course, and 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

<sup>14</sup> 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 was awarded during the evaluation. For early study enrollees, this could result in a savings period longer than 24 months. But staff encouraged participants—including late enrollees who had been saving closer to 12 months—to make asset purchases by October 2015.

Development Center of CNM for small business training. In Los Angeles, RISE provided small business training, and participants were referred out for homeownership counseling.

## 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 their primary residence and one vehicle—not exceeding \$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 age 18.

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 general education diploma or English as a second language programs and students registered for fall 2014 classes but that had not yet started were also permitted to apply. In Los Angeles, participants had to reside in Los Angeles County.

### Study Enrollment

Enrollment into the study was combined with enrollment into AFI. AFI applicants had to submit the completed project application, be determined eligible, and complete the study's online baseline survey. At the end of the baseline survey, eligible applicants were randomly assigned (through a computer algorithm) to the treatment group (which could receive AFI and non-AFI services) or the control group (which could receive non-AFI services, though the reality was more nuanced).<sup>15</sup> Each study participant had a 50-50 chance of being allowed to enter AFI.

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, but control group members could access all site-offered, non-AFI services for which they qualified. Both worked to prevent control group members from

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<sup>15</sup> 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.

reapplying and entering the project. Prosperity Works monitored entry into its partners' projects (and one other Albuquerque-based AFI project) to ensure control group members did not enroll in an IDA at another location. 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. Per survey data, 6.8 percent of control group members opened an IDA by the third-year follow-up. We describe services received by the control group in chapter 4.

Between January 2013 and July 2014, 807 people (299 in Albuquerque and 508 in Los Angeles) enrolled in the study (table 2.2). Among these participants, 407 were assigned to the treatment group, and 400 were assigned to the control group. 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.

**TABLE 2.2**  
**Study Enrollment by Period and Site**

Period	Sites combined	Albuquerque	Los Angeles
January–December 2013	255	97	158
January–April 2014	324	121	203
May–July 2014	228	81	147
<b>Total</b>	<b>807</b>	<b>299</b>	<b>508</b>

**Source:** Authors' calculations of AFI baseline survey data.

Differences in characteristics between the treatment and control groups at the time of random assignment and differential sample attrition can make the two groups unequal at the third-year follow-up. We test whether there are statistically significant treatment-control differences in baseline characteristics within the third-year analysis sample (appendix A). Treatment group members had higher baseline net worth than control group members but had higher rates of food hardship. Compared with control group members, treatment group members were worse off in terms of personal outlook, were more likely to be married, and had more adults in their households at baseline. To account for these differences, the regression models include covariates for baseline demographic and economic characteristics, as well as the baseline value of the dependent variable.



# Study Participant Demographic Characteristics

The majority (61 percent) of third-year follow-up survey respondents were under age 40 at study enrollment. Respondents were also predominantly female, nonwhite, and unmarried at the time of study enrollment, and about half had annual household incomes below \$15,000 (table 2.3).<sup>16</sup>

Across the two sites, the share of female participants is similar (roughly 70 percent), but participants differ on most other demographic characteristics. This partially reflects the differences in the two organizational and geographic settings: a large public community college (Albuquerque) and a nonprofit community development financial institution (Los Angeles). Consistent with local census data, study participants in Albuquerque are less likely to be black than study participants in Los Angeles.<sup>17</sup> Consistent with organizational settings, study participants in Albuquerque (community college setting) are also more likely to have some college but no degree compared with participants in Los Angeles (nonprofit setting), and Albuquerque participants are more likely to be younger and have income below \$15,000.

How the characteristics of study participants who completed the third-year survey (i.e., the analysis sample) compare with other AFI project participants nationwide speaks to the evaluation's external validity (how much the evaluation population represents the nationwide AFI population). These study participants (enrolled 2013–14) are similar to other AFI participants who enrolled in FY 2014 (table 2.3). Nationwide, AFI participants were predominantly younger than 40, female, nonwhite, and unmarried at enrollment.<sup>18</sup> These similarities, as well as the sites' similarities to AFI projects nationwide in terms of project features, strengthen the evaluation's external validity. But there are differences: a larger share of study participants was Hispanic (not surprising, given geographical locations), and study participants were more likely to have completed some college (a result of the Albuquerque site's community college setting), compared with the AFI participants enrolled nationwide.

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<sup>16</sup> Participants who did not respond to the survey tend to be more disadvantaged. See appendix B for details.

<sup>17</sup> Data from the 2013–17 American Community Survey five-year estimates.

<sup>18</sup> Source: Office of Community Services' (OCS) AFI grantee data from fiscal year 2014.

TABLE 2.3

### Selected Baseline Characteristics of Third-Year Survey Respondents and Other AFI Participants Enrolled in FY 2014

Demographic explanatory variables	Albuquerque	Los Angeles	Sites combined	Other AFI participants enrolled in FY 2014
<b>Age (%)</b>				
0–29	51.7	23.6	34.4	44.8
30–39	22.7	28.9	26.5	31.1
40–49	13.9	27.3	22.1	16.2
50+	11.8	20.2	17	7.9
<b>Gender (%)</b>				
Female	65.8	75.6	71.8	72.7
Male	34.2	24.4	28.2	27.3
<b>Race/ethnicity (%)</b>				
White	30.9	5	15.2	31.6
Black	9.3	43.1	29.8	33.3
Hispanic	50.4	42.5	45.7	23.2
Other	9.3	9.4	9.4	11.9
<b>Language spoken at home (%)</b>				
Speaks only English at home	65.8	53.1	58	
Speaks a language other than English at home	34.2	46.9	42	
<b>Education level (%)</b>				
Up to high school diploma/GED	7.5	23.6	17.3	37.3
Some college, but no degree	61.7	30.2	42.5	30
License, certificate, or associate's degree	22.9	26	24.8	13.9
Bachelor's degree or higher	7.9	20.2	15.4	18.8
<b>Marital status (%)</b>				
Married	16.7	19	18.1	22.5
Not married	83.3	81	81.9	77.5
<b>Number of adults, including participant (%)</b>				
1	45.2	48.9	47.5	55.5
2	36.4	28.9	31.8	33.6
3+	18.4	22.2	20.7	10.9
<b>Number of children ages 0–17 (%)</b>				
0	52.7	42.7	46.7	33.7
1	23.8	23.3	23.5	21.8
2	15.5	15.6	15.6	21.6
3+	7.9	18.4	14.2	22.9
<b>Employment (%)</b>				
Employed	83.2	89.2	86.9	86.5
Not employed	16.8	10.8	13.1	13.5
<b>Annual household income (%)</b>				
Less than \$15,000	56	44.4	48.9	*
\$15,000–\$24,999	22.2	25.8	24.4	*
\$25,000 +	21.8	29.9	26.7	*

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

**Notes:** As described in Mills et al. (2016), project staff 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 focused on FY 2014 (October 2013–September 2014) because 85 percent of the study sample was enrolled during that year.

\* 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 we used in the evaluation.

## Data Sources

To answer our key research questions, we use baseline and third-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 also use the Albuquerque site's administrative data to examine treatment group members' participation.<sup>19</sup> We describe each data source below.<sup>20</sup>

### 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 completed the self-administered baseline survey online immediately before random assignment, with staff helping them navigate the website. Because only those who completed the baseline survey were randomly assigned, the baseline survey response rate among sample members was 100 percent. The survey contained questions on demographics, savings, assets, material hardship, use of alternative financial services, public benefit receipt, employment, earnings, income, net worth, and personal outlook.

### Follow-Up Surveys

The principal data source for measuring medium-term impacts among study participants is the third-year follow-up survey, which participants completed between April 2016 and September 2017. We also

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<sup>19</sup> The Los Angeles site could not provide comparable case-level tracking data on a consistent automated basis for its treatment group members.

<sup>20</sup> As part of the first-year evaluation's implementation study, Urban Institute staff visited each evaluation site in 2014 (Mills et al. 2016). They conducted semistructured 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. They 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. Detail on these efforts and the resulting qualitative information can be found in appendix C and Mills et al. (2016).

conducted a one-year follow up survey, which was the principal data source for measuring the early impacts summarized in chapter 1 and presented in Mills et al. (2016). The surveys used mixed-mode data collection that included web, telephone, and field data collection. The sample includes all baseline study participants. The follow-up surveys covered the same topics as the baseline survey, except that demographic questions were excluded and questions about IDA services received were added. Questions that captured how often respondents experienced material hardship were also added.

### THIRD-YEAR FOLLOW-UP RESPONSE RATES

For both sites combined, the third-year follow-up survey response rate was 77 percent (table 3.1), with a higher rate in Albuquerque (80 percent) than Los Angeles (75 percent). The 621 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. Forty-three percent of survey respondents completed their surveys by phone, 44 percent completed surveys online, and 13 percent completed surveys with a field interviewer.

TABLE 3.1

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

	Sites Combined		Albuquerque		Los Angeles	
<i>Group</i>	<i>Sample</i>	<i>% completed</i>	<i>Sample</i>	<i>% completed</i>	<i>Sample</i>	<i>% completed</i>
Treatment	407	80.1	151	80.8	256	79.7
Control	400	73.8	148	79.7	252	70.2
Total sample	807	77.0	299	80.3	508	75.0

### Credit Score Data

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 and 36 months after enrollment. This report uses the 36-month data. Under Fair Credit Reporting Act restrictions that regulate credit score use, the Albuquerque site could not make individual scores available for the evaluation. Instead, Albuquerque provided the mean and standard deviation of the baseline credit score, the follow-up credit score (at 36 months), and the change in score (over the intervening 36 months) separately for the treatment and control groups.

## Administrative Data

Administrative data are available for the Albuquerque site only. The study site provided case-level data extracted from internal records that included treatment group withdrawal amounts for asset purchase. These data cover the 36 months following enrollment.<sup>21</sup>

## Variable Definitions

The key outcome and explanatory variables for the evaluation come from the baseline and third-year follow-up survey data. Table 3.2 lists the primary outcome variables, and table 3.3 lists the secondary outcomes grouped by the domains described in the conceptual framework.

**TABLE 3.2**  
**Primary Outcome Variables**

Variable	Definition
<b>Asset ownership at the time of the survey (participant or participant and spouse/partner)</b>	
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 or training	Participant's level of postsecondary education or training increased between baseline and third-year follow-up. This includes participants acquiring some college education or a license or certificate.
Homeownership, business ownership, or education or training	Participant became homeowner or business owner or increased his or her education or training level from baseline to third-year follow-up.
<b>Savings at the time of the survey as measured by liquid assets (participant and spouse/partner)</b>	
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.

**Note:** Liquid asset amount was top-coded at the 98th percentile of the third-year follow-up survey, which corresponds to \$54,250.

<sup>21</sup> In practice, all withdrawals for asset purchase happened within 30 months after enrollment because of the timing of the Albuquerque site's enrollments and the end of their grant closeout period.

TABLE 3.3

**Secondary Outcome Variables**

Variable	Definition
<b>Net worth at the time of the survey (participant and spouse/partner)</b>	
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).
<b>Material hardship in last six months (household)</b>	
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.
<b>Alternative financial services (AFS) in past 12 months (participant and spouse/partner)</b>	
Use nonbank check cashing	Used nonbank check-cashing services.
Nonbank check cashing	Number of times used nonbank check-cashing services.
Use AFS credit	Used a payday loan, auto loan, or pawnshop.
AFS credit	Number of times used a payday loan, auto loan, or pawnshop.
<b>Means-tested benefit receipt in the prior month (household)</b>	
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.
<b>Employment (participant) and earnings and income (household)</b>	
Employed	Participant working for pay at time of survey.
Monthly earnings	Total household earnings (before taxes) in the past month.
Monthly income	Total household income in the last month, including job earnings, Supplemental Security Income, public assistance or welfare, unemployment insurance, workers' compensation, child support, and income or benefits from family and friends outside the household.

Variable	Definition
<b>Credit score (participant)<sup>a</sup></b>	
Vantage 2.0 score	Group mean value of Vantage 2.0 credit score 36 months after study enrollment, for participants 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 36 months after study enrollment, for participants scoreable in both months.
<b>Personal outlook (participant)</b>	
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 at the time of the survey).
Perceived financial security	Measured at the time of the survey 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 past 12 months, rather than worsened or stayed the same.
Worse off financially	Financial situation worsened in the past 12 months, rather than improved or stayed the same.
Good health	Health is generally excellent, very good, or good, rather than fair or poor at the time of the survey.
Self-esteem	Rating of self-esteem based on Rosenberg scale (0–30) at the time of the survey.
<b>Community involvement in past 12 months (participant)</b>	
Community involvement	Worked on a neighborhood project, volunteered, attended a community event, or helped raise money for a community organization.
<b>Time preference at the time of the survey (participant)</b>	
Present-oriented time preference	Willing to accept an interest rate of 25 percent or greater to deferred payment.

**Notes:** Monthly earnings, monthly income, and net worth were top-coded at the 98th percentile of the third-year follow-up survey values, which corresponds to \$38,000, \$39,000, and \$497,500, 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 two to four uses, and 5 indicates five or more uses. 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.

<sup>a</sup>Credit score is available for Albuquerque participants only.

# Chapter 4. Services Received

AFI services included IDAs with matched withdrawals for asset purchases; unmatched withdrawals, generally for emergencies; financial education; specific asset-related training on homeownership, small business operation, and postsecondary education; credit-building support; and coaching or case management. Detail about each of these services is provided in appendix C. Below, we provide details for study members assigned to the treatment group in each site. We also provide information on the extent of unintended project take-up among those in the control group, sometimes called crossover.

## Treatment Group Participation

Key elements of AFI participation by the treatment group are presented below. These findings are based on the third-year follow-up survey for both sites, third-year follow-up project data for Albuquerque, and data reported in the first-year evaluation report (Mills et al. 2016), which included data collected and maintained by the study sites.

- **Eighty-two percent of treatment group members opened an IDA**, according to the third-year follow-up survey.<sup>22</sup> The rate was higher in Albuquerque (87 percent) than in Los Angeles (79 percent).<sup>23</sup> This may have occurred because opening accounts was simpler in Albuquerque.<sup>24</sup> Difficulty opening accounts for Los Angeles participants may have stemmed in part from their having problematic credit histories (Mills et al. 2016).
- **Seventy-nine percent of treatment group members participated in financial education** since enrollment according to the third-year follow-up survey. This self-reported information is lower than the roughly 85 percent recorded in administrative data at the first-year follow-up.<sup>25</sup> A lower share of treatment group members completed financial education in Albuquerque (69 percent) than in Los Angeles (84 percent). The lower rate in Albuquerque than in Los Angeles may have resulted

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<sup>22</sup> Eight percent of treatment group survey respondents did not respond to the question about whether they opened an IDA. Among treatment group members who responded to the question, 89 percent opened an IDA.

<sup>23</sup> Albuquerque administrative data show a slightly higher rate of account opening in Albuquerque: 91 percent.

<sup>24</sup> In Albuquerque, Prosperity Works had an ongoing arrangement with Wells Fargo that allowed it to automatically establish accounts for participants after the participant made a deposit. In Los Angeles, participants had to individually open accounts through a reportedly lengthy and cumbersome process and make an initial deposit (appendix C).

<sup>25</sup> A drawback of survey data is that they are self-reported with long retrospective periods, so they may underestimate the extent to which people receive financial education. Among treatment group members who responded to the third-year follow-up survey question, 82 percent received financial education.



from the 21-hour community college course required of Albuquerque participants versus the 10-hour course required of Los Angeles participants.

- Seventy-one percent of treatment group members made at least one matched withdrawal in Albuquerque, according to administrative data. The share falls to 51 percent in the survey data (table 4.1).<sup>26</sup> Only 23 percent of treatment group members made at least one matched withdrawal in Los Angeles, according to the survey data. The lower rate of matched withdrawals in Los Angeles was expected because many participants could not make matched withdrawals because of the site's failure to provide the required nonfederal funds.<sup>27</sup>

**TABLE 4.1**  
**AFI Treatment Group Withdrawals after Three Years, by Site (percent)**

	Sites combined	Albuquerque	Los Angeles
<b>Made matched withdrawal</b>	<b>33.1</b>	<b>50.8</b>	<b>22.5</b>
Home purchase/home improvement	2.8	2.5	2.9
Business capitalization	7.7	10.7	5.9
Postsecondary education or training	15.6	34.4	4.4
Not specified	7.4	4.1	9.3
<b>Made unmatched withdrawals</b>	<b>27.9</b>	<b>10.7</b>	<b>38.2</b>
Medical expenses	1.2	0.8	1.5
Living expenses			
Make up for lost income	7.4	4.1	9.3
Basic living expenses	9.2	6.6	10.8
Home repairs or buy an appliance	0.9	0.0	1.5
Car repairs	1.5	2.5	1.0
School and child care expenses	4.3	3.3	4.9
Special gifts/luxuries	0.3	0.8	0.0
Match funds not available	19.6	2.5	29.9
Concerns about deposit safety and access	7.4	0.8	11.3
Other	2.1	0.8	2.9

**Source:** Authors' tabulations of the AFI third-year follow-up survey.

**Notes:** Sample size is 326 for the full sample, 122 for Albuquerque, and 204 for Los Angeles (among the treatment group only).

Respondents could select any of the listed reasons, so percentages are not additive. We do not know why 2.5 percent of Albuquerque treatment groups members reported that match funds were not available. Participants might have been told to cash out by a specific date, did not meet the deadline, and perceived this as match funds not being available.

- Eleven percent of treatment group members made at least one unmatched withdrawal in Albuquerque, but 38 percent did so in Los Angeles (table 4.1). AFI allows unmatched withdrawals for emergencies (i.e., to cover medical expenses, make rent or mortgage payments, or cover living

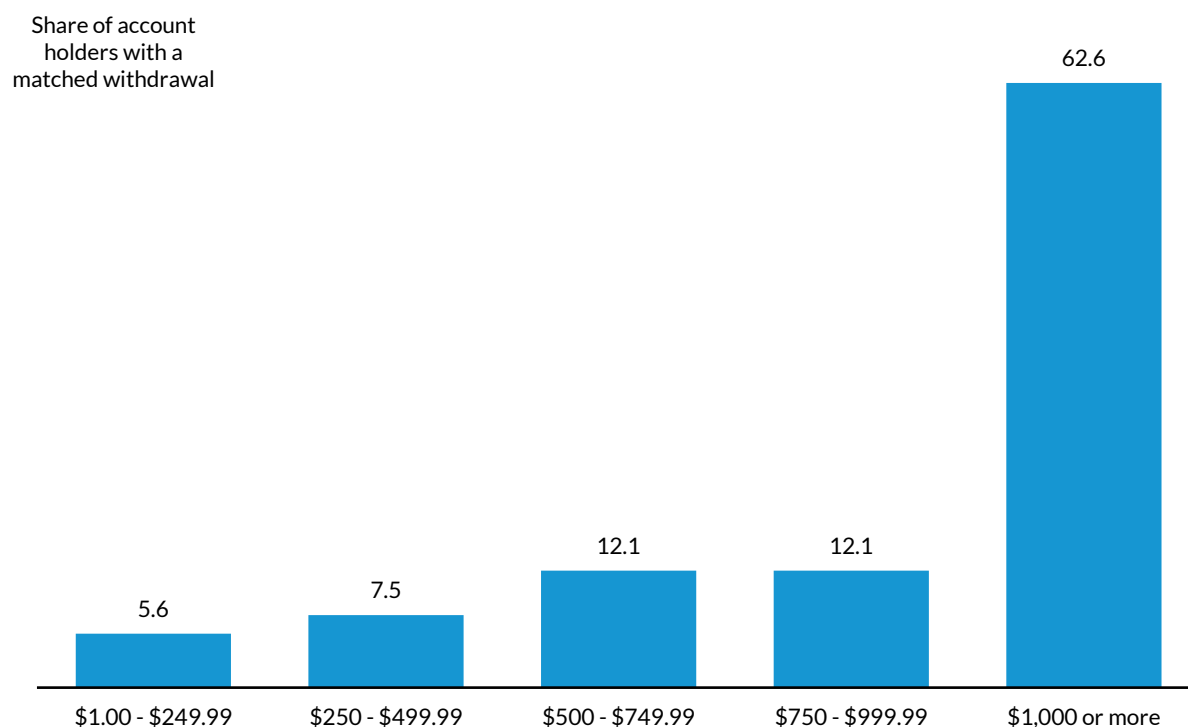
<sup>26</sup> As with the financial education data, this discrepancy may be because of underestimates in self-reporting in the survey data, either because participants did not remember the matched withdrawals or because participants did not think of them in the same terms used in the survey ("a matched withdrawal of savings from your IDA account").

<sup>27</sup> The AFI statute does not allow for the federal funds to be used as a match without corresponding nonfederal funds.

expenses). Among Los Angeles participants who made an unmatched withdrawal, 30 percent made the withdrawal because match funds were not available. Respondents in Los Angeles also took unmatched withdrawals out of concern their funds would be inaccessible if they waited (11.3 percent). Aside from withdrawal issues, across the two sites, the top reasons for taking an unmatched withdrawal were to cover living expenses—basic living expenses (9.2 percent), make up for lost income (7.4 percent), school or child care expenses (4.3 percent), car or house repairs (2.4 percent)—and pay for medical expenses (1.2 percent).

- Among the 71 percent of Albuquerque treatment group members who made at least one matched withdrawal (according to administrative data), 62.6 percent received the maximum \$1,000 match amount (figure 4.1). The mean withdrawal for asset purchases was \$915, and the median was \$1,000 in Albuquerque.<sup>28</sup>

**FIGURE 4.1**  
**AFI Treatment Group Matched Withdrawal Amounts for Asset Purchases in Albuquerque**



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

**Note:** Estimates represent AFI IDA balances three years after random assignment, plus any matched withdrawals (participant savings portion only).

<sup>28</sup> Withdrawals for asset purchases include matched withdrawals and withdrawals of IDA savings beyond the maximum matchable savings amounts by a small number of participants.

## Services Received by the Control Group

Control group members had access to some services also available to the treatment group at the AFI project site. They also could access similar services from other community providers. For example, control group members had access to financial education at both sites. By the third-year follow up survey, 41 percent of control group survey respondents reported receiving financial education since enrollment.

As part 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. Control group participants could take the same financial education course at CNM as the treatment group, and they could access services provided by the Small Business Development Center (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.

RISE offered fewer non-IDA services. It 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 postsecondary 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.

## Unmatched Funds in the Los Angeles Site after 2015

The Los Angeles site faced obstacles during implementation. Many participants could not make matched withdrawals because of RISE's failure to secure sufficient nonfederal funds. With few exceptions, only enrollees who finished saving before May 2015 could make a matched withdrawal. Moreover, beginning in April 2015, staff departures made it difficult for participants to get in touch with staff about withdrawals or other concerns. That matched withdrawals were unavailable to many Los Angeles participants is expected to dampen the third-year impact estimates.

# Chapter 5. Analytic Approach

## AFI Program Impact Estimates

This chapter presents our analytic approach for obtaining third-year AFI impact estimates. The analyses are designed to estimate the third-year impact of AFI participation on such outcomes as asset ownership, net worth, and material hardship. The basic method for estimating impacts is to model the differences in mean outcomes between the treatment and control groups (intent-to-treat, or ITT, analysis). By basing the analysis on data collected from two randomly assigned groups, we estimate AFI's causal effect. We identify the effect of offering AFI services to all interested participants, including the 18 percent of treatment group survey respondents who did not report opening an IDA.<sup>29</sup> We focus on regression-adjusted impacts that control for measured baseline differences between the treatment and control groups.<sup>30</sup> 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.

The impact estimates are obtained from regression models that control for the participants' sites and individual- and household-level demographic and economic characteristics at study enrollment, including age, gender, race or ethnicity, educational attainment, marital status, English proficiency, household size, and annual household income.<sup>31</sup> Each model also includes the baseline value of the outcome variable 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 (over the phone, online, or in person) and enrollment cohort (whether the participant entered the study in months 1 to 12, months 13 to 16, or months 17 to 19 of the enrollment period). Appendix D presents details of the regression models.

For each outcome, we estimate regression models with data for the two sites combined and each site separately. We also test for differences between the program impacts in the two sites. We highlight impact

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<sup>29</sup> Eight percent of treatment group survey respondents did not respond to the question about whether they opened an IDA. The others reported that they did not open an IDA.

<sup>30</sup> This regression approach improves the precision of the impact estimates. In addition, even with random assignment, some baseline characteristics of treatment and control group members differ significantly (see appendix A). The regression models control for these differences. Appendix E presents unadjusted outcome means by treatment and control group.

<sup>31</sup> One exception is the quantile regression models, which we estimate for liquid assets, net worth, monthly earnings, and monthly income. The quantile regressions include only the baseline value of the dependent variable. We exclude the other baseline demographic and economic characteristics from these models because the results are qualitatively the same (no impact of AFI), and in many cases, including them substantially reduces the precision of the impact estimates.

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.<sup>32</sup>

When examining several outcomes, some impact estimates may achieve statistical significance by chance. Our analysis also tests for statistical significance considering 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 the ITT analysis, we report effects for the subpopulation of AFI participants—that is, who made at least one deposit into an 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 and that some people in the comparison group were able to open an IDA from another source. 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 received the treatment. Following Angrist, Imbens, and Rubin (1996), we estimate an instrumental variable model where in the first stage treatment assignment predicts program participation, and in the second stage, we estimate the effect of program participation on the outcomes of interest. Appendix D presents our approach for obtaining TOT estimates and discusses TOT results.

As we interpret estimated program impacts, we are cognizant of the implications of the available sample size. For this reason, we have calculated the minimum detectable effects for two illustrative third-year outcomes: one binary measure and one continuous measure (appendix D). The calculated minimum detectable effects, expressed as a percentage of the corresponding control group mean, are between 20 percent and 40 percent for combined-site estimates.

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<sup>32</sup> At the 10 percent level of significance, there is a 10 percent probability that AFI results are due to chance.

# Chapter 6. Third-Year AFI Program Impacts

This chapter presents the estimated third-year impacts of AFI participation in the two primary domains (asset ownership and savings) 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.<sup>33</sup> We also present the regression-adjusted program impacts for the Albuquerque and Los Angeles sites separately.<sup>34</sup> For credit scores, we received aggregated treatment and control group scores only for the Albuquerque site, so we present difference-in-means estimates for that site.

Among the full sample of study participants, AFI did not statistically significantly increase homeownership, business ownership, or postsecondary education or training three years after study enrollment. We do, however, find that AFI participation had impacts for select subgroups: AFI increased homeownership among renters at enrollment and increased business ownership among non-business owners at enrollment. Among the secondary outcomes, AFI reduced material hardship and the use of nonbank check-cashing services. We also find that AFI improved participants' perceived financial security and shifted their time preferences to be more future oriented (i.e., more willing to pay today rather than accept a higher cost in the future).

## Primary AFI Outcomes

### Asset Ownership

Among all study participants, we find no statistically significant effect of AFI on asset ownership—homeownership, business ownership, postsecondary education or training, or ownership of any of the three assets (table 6.1).<sup>35</sup> Examining the two sites separately, we also find no statistically significant impacts.

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<sup>33</sup> Appendix E presents the sample means and standard deviations of each outcome for the treatment and control groups separately (appendix table E.1). This appendix also presents the mean and standard deviations for the treatment and control groups combined (appendix table E.2).

<sup>34</sup> The regression-adjusted sample means for the Albuquerque and Los Angeles sites are presented in appendix F.

<sup>35</sup> We interpret an increase education or training as an increase in asset ownership. Education and training are investments in human capital, an intangible asset.

We also analyze the subgroup of people who did not own the specific asset at study enrollment. That is, we analyze homeownership among renters, business ownership among non-business owners, educational and training advancement among those without a college degree, and attainment of any of the three assets among those without a home, business, or college degree at study enrollment. Consistent with our hypothesis that AFI increased medium-term asset ownership, our subgroup analyses find statistically significant effects for two asset types: homeownership and business ownership. Combining sites (table 6.1, middle panel), we find that AFI statistically significantly increased homeownership among renters by 4.7 percentage points (52 percent) and increased business ownership among non-business owners by 5.1 percentage points (53 percent). Looking at the two sites separately, we find that in Albuquerque AFI statistically significantly increased homeownership among renters by 8.7 percentage points (63 percent). We continue to find no evidence that AFI increased postsecondary education or training.

TABLE 6.1

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

	Sites Combined				Albuquerque	Los Angeles
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Impact <sup>b</sup>	Impact <sup>b</sup>
Primary outcomes						
Asset ownership (participant or participant and spouse/partner)						
Full sample (%)						
Homeownership	17.5	14.9	2.7	0.315	6.1	1.3
Business ownership	21.7	17.2	4.5	0.132	7.2	2.3
Education or training	37.1	36.1	1.0	0.773	-2.1	0.8
Homeownership, business ownership, or education or training	55.9	54.0	1.9	0.618	-5.3	4.2
By selected baseline characteristics (%)						
Homeownership–Renter	13.8	9.1	4.7 *	0.078	8.7 *	3.1
Business ownership–Non-business owner	14.8	9.7	5.1 *	0.091	4.4	5.6
Education or training–No college degree	44.1	42.5	1.6	0.689	0.0	0.8
Homeownership, business ownership, or education–No assets	56.1	50.9	5.3	0.28	-3.7	9.5
Liquid assets (participant and spouse/partner)						
Has liquid assets (%)	81.0	77.4	3.6	0.317	1.2	5.0
Liquid asset amount (\$)	5,186	3,991	1,195	0.178	1,469	1,312
Liquid assets (\$)						
25th percentile	279	276	3	0.979	30	31
50th percentile	1,493	1,240	254	0.460	184	79
75th percentile	5,398	3,826	1,572	0.163	1,879	1,460

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and third-year follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for binary outcome variables (for which we report the marginal effects), and quantile regression models. These models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). Quantile regressions for liquid assets are estimated at the 25th, 50th, and 75th percentiles of the liquid asset distribution and may only control for the baseline value of liquid assets. There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

\*  $p < 0.1$ ; no differences are significant at the  $p < 0.05$  or  $p < 0.01$  level.

## Savings as Measured by Liquid Assets

Although all the savings impact estimates are positive at the third-year follow-up, no statistically significant program effect emerges for the combined sample or for either site separately (table 6.1, bottom panel). AFI participants increased their savings but at a slower rate over controls than achieved after one year. At the median, the treatment and control groups had \$1,493 and \$1,240 (respectively) in savings, for a difference of \$254, although this difference is not statistically significant. At the mean, the difference in savings is \$1,195, but again, the difference is not statistically significant. These insignificant program effects are



consistent with our conceptual model, which hypothesizes that AFI increases short-term liquid assets but has an ambiguous effect in the medium term, when liquid asset holdings have been drawn down by AFI participants making matched withdrawals. Indeed, results from our first-year (short-term) follow-up analyses show that AFI increased participants' savings by an average of \$799 after one year and by \$657 at the median. This did not include any match funds.

## Secondary AFI Outcomes

### Net Worth

AFI's effect on net worth is expected to take more than three years to emerge, particularly as it relates to return on investments in homeownership, business ownership, and postsecondary education (e.g., closing costs in the case of homeownership and student loans in the case of postsecondary education). Consistent with this hypothesis, we find no evidence that AFI affected net worth measured at the third-year follow-up for the combined sample or for either site separately (table 6.2). This result mirrors the first-year follow-up evaluation findings.

### Material Hardship

We examine whether AFI reduced medium-term material hardship, as measured by four types of hardship—food, housing, utilities, and medical care—31 to 36 months after study enrollment (i.e., in the 6 months before the third-year follow-up survey). We hypothesize that AFI will reduce medium-term material hardship via improved financial stability.

We examine whether study participants experienced each type of hardship and a composite hardship measure that captures whether participants 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.<sup>36</sup> We examine nine material hardship measures.

Consistent with our conceptual model, the results show that AFI reduced medium-term material hardship. For the combined sample, AFI reduced material hardship for three of the nine hardship measures, including both medical hardship variables.<sup>37</sup> Focusing first on the two composite measures, we find a significant reduction in material hardship using the intensity measure only (table 6.2). Specifically, AFI

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<sup>36</sup> We did not ask respondents to provide information on the number of times they experienced a food-related hardship.

<sup>37</sup> With nine material hardship outcome measures, we expect roughly one to be statistically significantly different from zero (at the 10 percent level) by chance.

reduced the average number of hardships by 0.6 hardships (26 percent). For specific types of hardship, AFI reduced the likelihood of experiencing a medical hardship by 10.3 percentage points (29 percent) and the average number of medical hardships by 0.5 hardships (41 percent).

TABLE 6.2

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

Secondary outcomes	Sites Combined				Albuquerque	Los Angeles
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Impact <sup>b</sup>	Impact <sup>b</sup>
<b>Net worth (participant and spouse/partner)</b>						
Net worth amount (\$)	152	692	-540	0.966	-11,273	9,602
Net worth (\$)						
25th percentile	-21,851	-18,421	-3,429	0.553	-1,450	-702
50th percentile	-6,065	-5,305	-760	0.678	-2,209	-228
75th percentile	8,055	5,175	2,881	0.509	-226	6,876
<b>Material hardship (household)</b>						
Any hardship (%)	45.5	51.9	-6.4	0.114	-4.0	-7.7
Number of hardships (#)	1.9	2.5	-0.6 *	0.061	-0.4	-0.8 *
Food hardship (%)	14.1	13.9	0.2	0.943	4.2	-2.0
Housing hardship (%)	12.3	13.7	-1.5	0.585	-3.6	-0.6
Housing hardship (#)	0.3	0.3	0.0	0.605	0.0	0.0
Utilities hardship (%)	30.2	27.3	2.9	0.409	8.1	-1.5
Utilities hardship (#)	1.0	0.9	0.0	0.815	0.2	-0.2
Medical hardship (%)	25.3	35.7	-10.3 ***	0.005	-7.3	-13.0 ***
Medical hardship (#)	0.7	1.2	-0.5 ***	0.008	-0.4	-0.6 **
<b>Means-tested benefit receipt (household)</b>						
Receive benefits (%)	63.6	67.4	-3.7	0.306	-7.0	-3.8
<b>Alternative financial service use (participant and spouse/partner)</b>						
Use nonbank check cashing (%)	3.4	6.4	-3.0 *	0.084	-0.5	-5.9 **
Nonbank check cashing (#)	0.1	0.2	-0.1	0.180	0.0	-0.2 **
Use AFS credit (%)	14.2	15.9	-1.7	0.539	-2.7	-1.5
AFS credit (#)	0.4	0.5	-0.1	0.526	-0.5 **	0.1

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** AFS = alternative financial services. Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and third-year follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for binary outcome variables (for which we report the marginal effects), and quantile regression models. All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). There are statistically significant differences between the Albuquerque and Los Angeles impact estimates for AFS credit (#) and nonbank check cashing (#).

\*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

A different pattern emerges across the two sites. In Los Angeles, AFI significantly reduced material hardship for the same three material hardship outcome measures: the average number of hardships experienced by 0.8 (30 percent), the likelihood of medical hardship by 13 percentage points (35 percent), and the number of medical hardships by 0.6 (45 percent). In Albuquerque, however, we find no statistically

significant declines in material hardship. There was a similar pattern across the two sites in the first-year follow-up evaluation, where AFI significantly reduced material hardship for multiple outcome measures in Los Angeles and for none in Albuquerque.

## Means-Tested Public Benefit Receipt

Study participants' receipt of means-tested benefits (e.g., Temporary Assistance for Needy Families, Supplemental Security Income, and the Supplemental Nutrition Assistance Program) is measured in the month before the third-year follow-up survey. We hypothesize that by promoting investments in human and business capital, AFI works to increase employment and earnings opportunities for participants and reduce public benefit receipt. Consistent with this hypothesis, the impact estimates on benefit receipt are negative in the two sites combined and each site separately, although none of these estimates are statistically significant.

This pattern differs from the first-year follow-up evaluation results. At the first-year follow-up, AFI participants (treatment group members) had significantly higher levels of benefit receipt than nonparticipants (control group members). This may have been because AFI program rules require that IDA savings be disregarded when determining eligibility for means-tested program benefits, which can help AFI participants retain their means-tested benefits. It is also possible that program staff helped participants retain benefits.

## Use of Alternative Financial Services

We hypothesize that AFI reduces the use of alternative financial services (AFS), both alternative nonbank check-cashing services and alternative credit products (i.e., payday, auto title, and pawnshop loans). For low-income people, alternative products and services are easier to access than traditional financial services (e.g., from a bank or credit union) but are generally more expensive. 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, and by providing lessons in financial education classes on how to maximize savings and avoid costly financial products. We examine whether study participants and their spouse or partner used these services and the number of times they used such services, separately for use of AFS check-cashing services and AFS credit.<sup>38</sup> We measure AFS use 25 to 36 months after enrollment (i.e., in the 12 months before the third-year follow-up survey).

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<sup>38</sup> 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 the note to table 3.4 for details.

The results suggest that AFI led to reduced use of nonbank check-cashing services. AFI led to a 3 percentage-point (47 percent) reduction in the share of participants who used nonbank check-cashing services (table 6.2). With this decline, 3.4 percent of treatment group members were using nonbank check-cashing services in the medium term, compared with 6.4 percent of control group members.<sup>39</sup> This finding suggests that by connecting participants to a bank account, AFI reduces the need for nonbank check-cashing services. Examining each site separately, we find AFI led to a statistically significant decline in the use of nonbank check-cashing services in Los Angeles but not in Albuquerque. We estimate that the AFI project in Los Angeles led to a 5.9 percentage-point (64 percent) reduction in the share of participants who used check-cashing services and reduced the frequency of use (scale from 1 to 5) by an average of 0.2 points (66 percent).<sup>40</sup> Our finding of no statistically significant effect in Albuquerque could result from the fact that all AFI savings accounts in Albuquerque were owned by Prosperity Works as a custodian (at Wells Fargo). With this setup, if an AFI IDA participant went into the bank without an account number to cash a check, the teller could not identify the participant as a customer.

In our analyses of AFS credit product use, we find AFI had no statistically significant effect on credit use with the two sites combined. Examining the two sites separately, however, we find that AFI reduced the frequency with which participants in Albuquerque used high-cost AFS credit, such as a payday or auto title loan. Specifically, AFS credit use declined an average of 0.5 points (64 percent) in Albuquerque. Declines in AFS credit use could be particularly beneficial, as use of high-cost credit, such as payday and auto title loans, can trap consumers in a cycle of debt.

## Employment, Earnings, and Income

AFI is not an employment intervention, so we do not expect the program to *directly* affect participants' employment, earnings, or income. But AFI could have secondary effects on these outcomes. For example, an AFI participant might earn more because of additional education or training. On the other hand, AFI could lower medium-term earnings for participants enrolled in postsecondary education or starting a new business. We measure employment at the third-year follow-up and earnings and income in the month before the third-year follow-up.

Our analyses provide no evidence that the AFI program affected employment or earnings at the third-year follow-up (table 6.3).<sup>41</sup> We find no statistically significant program effects for the combined sample or

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<sup>39</sup> None of these of treatment effects are significant at the 10 percent level after adjusting for multiple comparisons.

<sup>40</sup> After adjusting for multiple outcomes in the Los Angeles analysis, AFI led to a significant decline in the number of times participants used check-cashing services but not in the likelihood of using nonbank check-cashing services.

<sup>41</sup> Participants' employment status is measured at the third-year follow-up survey, and household earnings are measured in the month before the third-year follow-up survey.

for either site. In analyses of income, the results suggest that AFI decreased average household income by more than \$1,000 in the medium term. Income could fall as participants start a business or if participants who are in school take a flexible, low-paying job or cut back on hours so they can better manage their workload. But we find no statistically significant difference in income between the treatment and control groups at the 25th, 50th, or 75th percentiles of the income distribution.

TABLE 6.3

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

Secondary outcomes	Sites Combined				Albuquerque	Los Angeles
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Impact <sup>b</sup>	Impact <sup>b</sup>
<b>Employment (participant) and earnings and income (household)</b>						
Employed (%)	78.9	75.2	3.7	0.256	5.9	2.9
Monthly earnings amount (\$)	2,523	3,240	-717	0.187	-486	-834
Monthly earnings (\$)						
25th percentile	450	450	0	1.000	0	0
50th percentile	1,555	1,455	100	0.574	38	74
75th percentile	2,719	2,711	7	0.980	-227	277
Monthly income amount (\$)	2,658	3,816	-1,157 *	0.057	-412	-1,325 *
Monthly income (\$)						
25th percentile	863	849	14	0.942	105	-104
50th percentile	1,712	1,738	-26	0.880	-72	48
75th percentile	3,052	3,226	-174	0.592	-297	-84
<b>Credit score (participant)<sup>c</sup></b>						
Vantage 2.0 score (501–990)					11	
Change in Vantage 2.0 score (#)					14	

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least-squares regression models for continuous outcome variables, probit models for the binary outcome variable (for which we report the marginal effects), and quantile regression models. All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

<sup>c</sup> Credit score impact is based on the Vantage 2.0 score for all scoreable randomly assigned participants in Albuquerque and is not regression adjusted.

\*  $p < 0.1$ . No differences are significant at the  $p < 0.05$  or  $p < 0.01$  level.

## Credit Score

Credit-building support, financial education, or other financial counseling provided through AFI could improve participants' credit scores. Participation in AFI could also improve medium-term 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 or training.

With the limitation of aggregated credit scores from the Albuquerque site only, there is no evidence that AFI increased participants' credit scores three years after enrollment (table 6.3). The treatment-control difference is in the expected positive direction (i.e., higher credit scores in the treatment group than in the control group), but the differences are not statistically significant. That the Albuquerque control group members were also eligible for coaching and referral services may help explain the absence of significant differences.

## Personal Outlook and Community Involvement

We hypothesize that AFI will improve medium-term personal financial outlook via improved financial stability, as measured by financial well-being, self-esteem, and physical health. We find a positive impact of AFI on only one of the four financial well-being outcomes (table 6.4). Specifically, we find that AFI led to a 6.4 percentage-point (27 percent) lower share of participants who reported that their financial situation has worsened in the past year.<sup>42</sup> We find no evidence that AFI participants were more likely to feel their financial situation improved in the past year or that AFI increased participants' ability to make ends meet or improved their perceived level of financial security.

We found more robust evidence that AFI improved financial well-being at the first-year follow-up. The higher level of financial well-being among treatment (versus control) group members at the first-year follow-up may have been because of the higher level of savings among treatment group members at the first-year follow-up but not at the third-year follow-up. Although the first-year follow-up results show that AFI increased participants' savings roughly one year after study enrollment (e.g., median savings increased \$657), we do not find that participants had statistically significantly higher savings than the control group three years after enrollment (table 6.1).

The results also provide no evidence that AFI led to improvements in physical health, self-esteem, or community involvement for the combined sample or for either site separately.

## Time Preference

We hypothesize that through financial education, AFI can shift time preferences so participants are more willing to pay a given cost today than pay a higher cost in the future. In a question about financing the purchase of a refrigerator, we found that AFI led to a 4.8 percentage-point (35 percent) decrease in the share of people willing to accept an interest rate of 25 percent or more (at the third-year follow-up survey). This impact is statistically significant in the combined sample and in Albuquerque. In other words, AFI participants were more willing to pay for an item today (e.g., pay \$100 today) than to put off paying today and pay 25 percent more for it (\$125) in a year. Shifting behavior this way can lead to long-term improvements in AFI participants' financial well-being, as high-interest-rate products can lead to financial difficulties down the road.

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<sup>42</sup> After adjusting for multiple comparisons, none of these measures are statistically significant.

TABLE 6.4

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

Secondary outcomes	Sites Combined				Albuquerque	Los Angeles
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Impact <sup>b</sup>	Impact <sup>b</sup>
<b>Personal outlook (participant)</b>						
Ability to make ends meet (%)	57.6	57.9	-0.3	0.941	1.8	0.4
Perceived financial security (1–10)	5.4	5.3	0.1	0.685	-0.4	0.4
Better off financially (%)	43.4	38.3	5.2	0.187	8.7	4.8
Worse off financially (%)	17.3	23.7	-6.4 **	0.046	-7.9	-6.5
Good health (%)	76.4	78.2	-1.7	0.588	-3.8	-0.7
Self-esteem (0–30)	23.3	23.2	0.1	0.863	0.2	0.0
<b>Community involvement (participant)</b>						
Community involvement (%)	57.9	56.3	1.6	0.663	-4.2	3.1
<b>Time preference (participant)</b>						
Present-oriented time preference (%)	8.8	13.6	-4.8 *	0.058	-10.0 **	-1.7

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for binary outcome variables (for which we report the marginal effects), and quantile regression models. All models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .



# Chapter 7. Study Limitations

The large scale of the evaluated AFI projects and changes in the type of participants and the project intervention could affect participants' experiences, project outcomes, and evaluation results. Project sites also had some difficulty offering the promised service to the served population. Below, we further describe these issues.

## Lack of Nonfederal Funds in the Los Angeles Site after 2015

The Los Angeles site faced major challenges, leading to a failure to implement the project as intended. Many participants could not make asset purchases because of RISE's inability to provide the required nonfederal funds. With few exceptions, only enrollees who finished saving before May 2015 could make a matched withdrawal. In addition, beginning in April 2015, RISE staff departures made it difficult for participants to get in touch with staff about withdrawals or other concerns. The fact that matched withdrawals were unavailable to most Los Angeles participants is expected to dampen the third-year impact estimates.

## Scale of the Project

Expanding the AFI projects to attract and serve more participants was a challenge for both sites and had unintended consequences. The sites were accustomed to having up to two years to enroll a limited number of AFI-eligible participants: about 15 at the Albuquerque site (CNM Connect) and about 80 at the Los Angeles site (RISE). In contrast, the evaluation called upon the Albuquerque and Los Angeles sites to enroll 150 and 250 AFI project participants, respectively, along with an equal number for the control group, all within 18 months. This required new recruitment strategies and 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, staff enrolled some participants whom staff believed were less likely to succeed. Although this concerned project staff, all enrolled participants met the AFI eligibility criteria. Further, this broader population of AFI participants better represents the full AFI-eligible population and thus leads to broader external validity of the evaluation results (how much the evaluation population represents not only the nationwide AFI population but the AFI eligible population).

Albuquerque staff noted that in the past, CNM academic coaches had been able to select the most saving-ready students among those already receiving CNM Connect services 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 the students enrolled later in the study period were less ready or less motivated to save. Such enrollees were responding to the stepped-up marketing efforts undertaken by the site in the late stages of enrollment to meet its recruitment targets. For these reasons, we controlled in the regressions for when during the study period the sample member enrolled.

In Los Angeles, RISE had to recruit from the public, not only 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 it normally would have, however, because of its goal of enrolling 500 study 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 services 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 giving participants 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 lower than in the past. Another staff member reported that some effort was made to shift client communications online to deal with the expansion. This was limited, however, as direct interaction with clients was considered an important project element.

## Changes in the Project Intervention

In both sites, the intervention changed over time. The slower pace of enrollment in the study's first year meant staff in both sites could offer more individual attention to earlier enrollees.<sup>43</sup>

Los Angeles underwent additional changes. 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 required participants to complete the class before making a matched withdrawal. This delay in financial education for some participants may have affected their savings habits. Participants'

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<sup>43</sup> 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 in all of 2013.

ability to make a matched withdrawal also changed. As mentioned, with few exceptions, only enrollees who finished saving before May 2015 could make a matched withdrawal.

## Control Group Access to Services

The three key components of the AFI program were individual development accounts, potential of match funds, and financial education and other supportive services. Although control group members at both sites could not access the accounts or the match funds, they could access the financial education and other services. By the third-year follow-up survey, for example, 41 percent of control group survey respondents reported having received financial education since enrollment (versus 79 percent of the treatment group). This evaluation cannot disentangle the impacts of these separate components, but the cumulative effect may be reduced by the control group's access to financial education and other supportive services.

# Chapter 8. Conclusions and Implications

This chapter summarizes key medium-term impact evaluation findings, interprets the results in the light of earlier IDA research, and suggests potential implications for policy, practice, and research.

## Emerging Insights on AFI's Role

Among the full sample of study participants, we find AFI has no statistically significant effect on asset ownership—homeownership, business ownership, or education or training. These statistically insignificant findings are, however, in the context of a moderate share of program participants who were already homeowners (8 percent) or business owners (24 percent) or had a college education (29 percent) when they entered the program (appendix table B.1). For this reason, we also estimate AFI's effect among study participants without assets at study enrollment. We find that AFI increased the homeownership rate among renters and the business ownership rate among non-business owners at the third-year follow-up. Specifically, AFI increased homeownership among renters by 4.7 percentage points (52 percent) and increased business ownership among non-business owners by 5.1 percentage points (53 percent). Though sizable, these impact estimates may be reduced by Los Angeles's inability to provide match funds to many of its AFI participants. The increase in homeownership among renters and business ownership among non-business owners in the medium term, coupled with the early evaluation finding that AFI increases savings, suggests that AFI helped low-income people build savings in the short term and assets in the medium term among families who have yet to make the asset purchase.

These results are consistent with fourth-year impact estimates from the American Dream Demonstration Tulsa, Oklahoma, experimental site. The American Dream Demonstration evaluation did not find statistically significant increases in asset ownership among the full sample of study participants but did find statistically significant increases in homeownership among renters (Grinstein-Weiss et al. 2008). The American Dream Demonstration evaluation did not, however, find statistically significant increases in business ownership among non-business owners (Grinstein-Weiss et al. 2012).

We do not find any effects of AFI participation on education or training outcomes, even among those without a college degree at study enrollment. One possible explanation is that AFI IDAs could be used only for a limited set of purposes regarding postsecondary education or training: tuition, fees, and books and supplies bought directly from an eligible educational institution. A second possibility is that educational interventions should start earlier than age 18, as done in Arizona's Earn-to-Learn program and Italy's

ACHAB/Percorsi matched savings program, which begin working with students in high school. For example, the randomized controlled evaluation of the ACHAB/Percorsi program found increased postsecondary education participation as measured with college enrollment (i.e., the likelihood of enrolling in college right after high school) and persistence (i.e., the likelihood of being still enrolled in the second year) (Azzolini et al. 2018). A third explanation is that IDA program impacts on education and training take longer to materialize. The American Dream Demonstration did find a significant positive impact on education enrollment at the 10-year follow-up (Grinstein-Weiss et al. 2012). They find that treatment group members with a baseline education level of high school or less are likely to gain “some college” compared with the control group. This suggests that the AFI program could increase education long term, just not in the medium term, which this evaluation measures.

Our third-year findings also show that AFI led to reduced use of nonbank check-cashing services. By connecting people to a bank or credit union, incentivized savings programs help low-income people stay out of the alternative financial services market. Because AFS products tend to be more expensive, diversion can have long-term benefits. Our finding that AFI shifted participants’ time preferences to be more future oriented (i.e., more willing to pay today versus accepting a higher cost later) can also have long-term benefits, because delaying financial burdens can cause greater financial strain later.

Our finding that AFI reduced material hardship and improved financial well-being, in both the short and medium term, yields important new insights in IDA research. Neither the short- or medium-term American Dream Demonstration evaluation nor the national AFI nonexperimental impact evaluation examined material hardship. Behavioral economics research on economic scarcity and cognitive depletion (Mullainathan and Shafir 2013) shows that financial stress and worry tend to sap families’ cognitive resources during urgent economic need, when families can least afford to make poor choices. And reduced material hardship and improved financial well-being are important in the context of toxic stress and its impact on children. Per the American Academy of Pediatrics, toxic stress in early childhood can lead to permanent changes in brain structure and function. These alterations can “create a weak foundation for later learning, behavior, and health” (Shonkoff et al. 2012, e236). Our findings that AFI participants have reduced material hardship and improved financial well-being become even more salient in these contexts.

## Potential Implications for Policy, Practice, and Research

These findings suggest implications regarding building assets, connecting to the mainstream financial sector, securing nonfederal funds, using allowable matched savings for education, and integrating asset building into other service delivery systems. We also discuss potential fruitful areas for future research based on this study’s findings.

## Helping Low-Income People Build Assets and Connect to Mainstream Financial Services

Incentivized savings programs such as IDAs can help low-income people build assets, particularly among those who have yet to make an asset purchase. AFI participation also helps people enter and remain in the traditional banking system by connecting participants with a savings account and financial education on how to avoid expensive financial products. It familiarizes people with banking options—so they cash checks at a bank rather than a nonbank check-cashing outlet.

## Securing Nonfederal Funds and Allowable Uses

Among Los Angeles participants who made an unmatched withdrawal, 30 percent made the withdrawal because match funds were not available. The inability of Los Angeles to secure the nonfederal match funds echoes a challenge documented in earlier AFI research. Difficulty in raising nonfederal funds was an impediment to AFI grant applications and to the successful expansion and completion of AFI grants (DeMarco, Mills, and Ciurea 2008). Moreover, the requirement for nonfederal funding also limits the number of potential sites for a rigorous evaluation, as fewer AFI projects are able to attain the scale (in terms of funded program slots) needed to enroll a research sample of sufficient size.

Also, the requirement that AFI IDA dollars for postsecondary education be used only for tuition, fees, and books and supplies bought directly from an eligible educational institution was a program limitation (Mills et al. 2019). It may also be a reason that we do not find any effects of AFI participation on education or training outcomes, even among those without a college degree at study enrollment. Community colleges are already free or nearly free to many low-income students eligible for Pell grants. Future incentivized savings programs that include a postsecondary education focus could consider accommodating additional expenses not paid directly to an eligible educational institution, such as internet, computers, transportation and parking, standardized tests, and living expenses (e.g., rent, groceries) to improve education and training success. The evaluation of the ACHAB/Percorsi matched savings program in Italy allowed any education-related expenses and found that, after tuition fees, a large part of participants' funded expenses were related to computers, internet, or software; transportation; and additional training and rent.

## Integrated Service Delivery

Though not a fundamental part of AFI, some grantees integrated their AFI projects into other activities that serve low-income households by providing public benefits, social services, loans, higher education, and other assistance. The Albuquerque site, for example, operated at Central New Mexico Community College, which served AFI participants in a student center that offers various services. Based on qualitative interviews with IDA staff at six community colleges, Mills and coauthors (2019) offer lessons to colleges

interested in offering integrated IDA services to students. Integrating incentivized savings programs into existing college and social safety net programs may be a mechanism for promoting health, economic, and social well-being.

## **Mechanisms for Reducing Economic Hardship and Future Research Suggestions**

Understanding how AFI participation reduced economic hardship could inform future program design. The evaluation findings do not show what mechanisms reduced hardship. Potential mechanisms are unmatched withdrawals; asset building, financial education, and other support services; and the ability to retain federal means-tested benefits by excluding AFI savings from countable assets. Future research could disentangle the role of each of these mechanisms.

The AFI intervention consisted of a bundle of services offered to low-income households. Disentangling the relative roles of the IDA account, promise of a savings match, and financial education or coaching would be a fruitful area for future research. By cross-randomizing each element of the intervention, future work might identify the importance of each component, as well as their interactions, on the financial well-being of low-income families. Large scale and sample size would likely be needed for such an evaluation.

Relatedly, future research could investigate the tradeoffs between expanding AFI to more participants (low service level) or providing more resources and services to a limited number of participants (higher service level). For example, the initial proposal design for this evaluation included two treatments: (1) financial match plus financial education brochure only (low service level) and (2) financial match plus mandatory financial education classes and the availability of case management and peer support (traditional service level). We determined early on that the scale of the programs we evaluated were too small to provide the sample size needed to evaluate two treatments.

Finally, future asset-building research should build in subgroup analyses that account for differences in study participants' asset holdings at study enrollment. With the American Dream Demonstration subgroup findings in mind, this evaluation conducted subgroup analyses and found that understanding initial assets helps us understand AFI's impacts. Having explored these subgroups within the overall experimental design, future evaluations should be designed to randomly assign within subgroups to provide confirmation of impacts within each group.

## **Concluding Observations**

The AFI program was one of the few federal efforts that encouraged 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).<sup>44</sup> Savings penalties in many benefit programs create a disincentive for low-income people to save (Ratcliffe et al. 2016).

Sherraden's original proposal for IDAs in 1991 included universal, progressive, lifelong accounts. The AFI program provides a small dose of his vision. The evidence presented here indicates that the program improved the financial well-being of low-income earners and their households in the short and medium term and can inform current and future incentivized savings programs (e.g., child development accounts, Earn to Learn, SaverLife) and evaluations.

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<sup>44</sup> 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. Evaluation of Random Assignment

Differences between the treatment and control groups in their characteristics at random assignment (i.e., at baseline) and differential sample attrition can make the two groups unequal at the third-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 third-year follow-up survey). We examine the baseline demographic characteristics and the baseline values of the third-year outcome variables. These tests are performed for the combined analysis sample of 621 cases and separately for the Albuquerque analysis sample of 240 cases and the Los Angeles analysis sample of 381 cases. We use t-tests for univariate variables and chi-squared tests for categorical variables.

Results show six statistically significant treatment-control differences (table A.1). Of the 33 characteristics tested, two are statistically significant at the 1 percent level, one is statistically significant at the 5 percent level, and four are statistically significant at the 10 percent level. These are more differences than would have occurred by chance alone, meaning that despite the program's randomization, the treatment and control groups are different from each other in their baseline characteristics. Given the 33 tests, statistical fluctuation is expected to result in significant treatment-control differences for fewer than one attribute at the 1 percent level, about two attributes at the 5 percent level, or three attributes at the 10 percent level.

Among participants' demographic and economic characteristics, treatment group members had higher (less negative) baseline net worth but had higher rates of food hardship. Treatment group members were worse off in terms of personal outlook, were more likely to be married, and had more adults at baseline. To account for these differences, the regression models 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, three baseline characteristics have significant treatment-control differences at the 10 percent level, while in Los Angeles, five baseline characteristics have significant treatment-control differences. The site-specific analyses follow the combined-site analyses and include the same baseline characteristics as covariates in the regression models.

TABLE A.1

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

Variable	Sites Combined		Albuquerque		Los Angeles	
	Treatment	Control	Treatment	Control	Treatment	Control
<b>Asset ownership (participant or participant and spouse/partner)</b>						
Homeownership (%)	8.3	8.2	13.9	11.0	4.9	6.3
Business ownership (%)	24.4	22.8	18.0	21.4	28.2	23.7
Education (associate's or higher) (%)	28.8	28.8	18.0	22.0	35.3	33.3
<b>Savings (participant and spouse/partner)</b>						
Liquid asset amount (\$)	1,628	1,570	1,493	1,877	1,721	1,346
<b>Net worth (participant and spouse/partner)</b>						
Net worth (\$)	-2,551	-9,262 **	-1,145	-12,274 **	-3,492	-6,594
<b>Material hardship (household)</b>						
Any hardship (%)	56.6	52.5	65.2	59.2	51.4	48.1
Food hardship (%)	21.1	15.5 *	31.7	22.9	14.9	10.4
Housing hardship (%)	16.9	15.6	25.4	17.7	11.7	14.2
Utilities hardship (%)	27.9	28.9	32.2	31.3	25.3	27.3
Medical hardship (%)	38.1	32.9	46.6	40.0	33.0	28.2
<b>Alternative financial services (participant and spouse/partner)</b>						
Use nonbank check cashing (%)	8.2	7.6	4.1	5.9	10.7	8.7
Nonbank check cashing (#)	0.3	0.2	0.2	0.2	0.4	0.3
Use AFS credit (%)	17.9	19.1	19.8	18.3	16.8	19.7
AFS credit (#)	0.6	0.5	0.6	0.5	0.5	0.4
<b>Means-tested benefit receipt (household)</b>						
Receive benefits (%)	70.1	65.9	66.4	55.5 *	72.4	73.4
<b>Employment (participant) and earnings and income (household)</b>						
Employed (%)	89.5	92.8	88.5	89.8	90.1	94.8 *
Monthly earnings (\$)	1,192	1,081	971	960	1,335	1,170
Monthly income (\$)	1,410	1,281	1,208	1,116	1,533	1,405

TABLE A.1

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

Variable	Sites Combined		Albuquerque		Los Angeles	
	Treatment	Control	Treatment	Control	Treatment	Control
<b>Credit score (participant)<sup>a</sup></b>						
Vantage 2.0 score (501–990)	—	—	666	663	—	—
<b>Personal outlook (participant)</b>						
Ability to make ends meet (%)	53.0	56.4	45.9	44.9	57.3	64.3
Perceived financial security (1–10)	4.8	4.9	4.3	4.5	5.1	5.2
Better off financially (%)	32.9	40.5 *	25.4	30.5	37.5	47.4 *
Worse off financially (%)	20.8	19.4	27.0	28.0	17.0	13.5
Good health (%)	81.5	86.7 *	81.1	89.0 *	81.7	85.1
Self-esteem (0–30)	23.6	24.8 ***	22.9	23.9	24.1	25.5 ***
<b>Community involvement (participant)</b>						
Community involvement (%)	70.8	68.4	70.0	61.0	71.3	73.5
<b>Time preference</b>						
Present-oriented time preference (%)	9.7	11.2	9.2	11.1	10.0	11.2
<b>Demographic explanatory variables</b>						
Age (%)	37.4	37.2	33.1	33.0	40.0	40.1
Female (%)	71.8	71.9	68.0	63.6	74.0	77.4
Race/ethnicity (%)						*
Black	28.2	31.5	11.6	7.0	38.4	48.8
Hispanic	47.6	43.4	52.9	47.8	44.4	40.2
White/other	24.1	25.1	35.5	45.2	17.2	11.0
Married (%)	22.2	13.7 ***	20.5	12.7	23.2	14.3 **
Number of adults in household	1.90	1.77 *	1.93	1.74	1.89	1.79
Number of children in household	1.10	1.04	0.90	0.70	1.22	1.27

**Source:** Authors' tabulations of the AFI baseline survey.

**Note:** AFS = alternative financial service.

<sup>a</sup>Credit score is the Vantage 2.0 scores reported for randomly assigned participants in Albuquerque.

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

# Appendix B. 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 enrollees, comparing those who responded to the third-year follow-up survey with those who did not respond. These tests are performed for the combined analysis sample of 807 cases and separately for the Albuquerque analysis sample of 229 cases and the Los Angeles analysis sample of 508 cases. We use *t*-tests for univariate variables and chi-squared tests for categorical variables.

We examined 33 outcomes and demographic characteristics for the combined sample (table B.1) and found one variable with a difference statistically significant at the 1 percent level, four variables with differences statistically significant at the 5 percent level, and one variable with a difference statistically significant at the 10 percent level. These are more differences than would have occurred by chance alone.

Individuals who did not respond to the survey tend to be more disadvantaged—they had lower liquid assets and education and higher rates of food hardship and AFS credit use (e.g., payday loans or pawnshops). Nonrespondents were not, however, more likely to experience other types of hardship (housing, utility, medical) or use alternative check cashing. Nonrespondents were also less likely to be female and had fewer children.

Examining differences by site yields more differences. In Albuquerque, five variables were significant at the 1 percent level, two variables were significant at the 5 percent level, and two variables were significant at the 10 percent level. In Los Angeles, one variable was significant at the 1 percent level, five variables were significant at the 5 percent level, and two variables were significant at the 10 percent level. Nonrespondents in Albuquerque tended to be more disadvantaged (had lower liquid assets, earnings, and educational attainment), while in Los Angeles, the findings were mixed. For example, nonrespondents had lower liquid assets but higher homeownership rates.

TABLE B.1

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

Variable	Sites Combined		Albuquerque		Los Angeles	
	Respondent	Nonrespondent	Respondent	Nonrespondent	Respondent	Nonrespondent
<b>Asset ownership (participant or participant and spouse/partner)</b>						
Homeownership (%)	8.23	9.89	12.5	8.62	5.53	10.5 *
Business ownership (%)	23.6	26.5	19.7	20.3	26.1	29.4
Education (associate's or higher) (%)	28.8	20.4 **	20.0	5.1 ***	34.4	27.6
<b>Savings (participant and spouse/partner)</b>						
Liquid asset amount (\$)	1,853	899 ***	1,896	861 **	1,822	920 ***
<b>Net worth (participant and spouse/partner)</b>						
Net worth (\$)	-5,196	-7,605	-6,391	-9,406	-4,285	-6,534
<b>Material hardship (household)</b>						
Any hardship (%)	54.6	59.0	62.3	80.0 ***	49.9	48.6
Food hardship (%)	18.4	26.1 **	27.3	33.9	12.8	22.4 **
Housing hardship (%)	16.3	19.7	21.6	26.8	12.9	16.2
Utilities hardship (%)	28.4	29.8	31.8	40.7	26.2	24.6
Medical hardship (%)	35.6	35.8	43.4	44.8	30.8	31.4
<b>Alternative financial services (participant and spouse/partner)</b>						
Use nonbank check cashing (%)	7.9	10.9	5.0	11.9	9.8	10.4
Nonbank check cashing (#)	0.28	0.41	0.18	0.49 *	0.35	0.38
Use AFS credit (%)	18.5	21.4	19.1	32.8 **	18.1	16.1
AFS credit (#)	0.52	0.78 *	0.56	1.16 *	0.49	0.60
<b>Means-tested benefit receipt (household)</b>						
Receive benefits (%)	68.1	62.9	61.0	64.2	72.9	62.3 **
<b>Employment (participant) and earnings and income (household)</b>						
Employed (%)	91.1	94.1	89.2	91.5	92.3	95.3
Monthly earnings (\$)	1,204	1,098	1,014	676 ***	1,333	1,293
Monthly income (\$)	1,402	1,365	1,198	861 ***	1,540	1,591
<b>Credit score (participant) <sup>a</sup></b>						
Vantage 2.0 score (501–990)	—	—	664	631 ***	—	—
<b>Personal outlook (participant)</b>						
Ability to make ends meet (%)	54.6	53.0	45.4	47.5	60.5	55.6
Perceived financial security (1–10)	4.85	4.74	4.39	4.41	5.14	4.89
Better off financially (%)	36.5	34.4	27.9	23.7	42.0	39.5
Worse off financially (%)	20.1	23.0	27.5	25.4	15.4	21.8
Good health (%)	84.0	83.6	85.0	81.4	83.3	84.7
Self-esteem (0–30)	24.2	23.6	23.4	23.4	24.7	23.7 *

Continued on next page

TABLE B.1

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

Variable	Sites Combined		Albuquerque		Los Angeles	
	Respondent	Nonrespondent	Respondent	Nonrespondent	Respondent	Nonrespondent
<b>Community involvement (participant)</b>						
Community involvement (%)	69.7%	63.5%	65.5%	57.9%	72.3%	66.1%
<b>Time preference (participant)</b>						
Present-oriented time preference (%)	10.4	10.0	10.1	11.9	10.6	9.1
<b>Demographic characteristics</b>						
Age	37.3	37.5	33.1	31.9	40.0	40.1
Female (%)	71.8%	63.4% **	65.8%	59.3%	75.6%	65.4% **
Race/Ethnicity (%)						**
Black	29.8%	26.3%	9.3%	7.1%	43.1%	35.7%
Hispanic	45.7%	42.1%	50.4%	48.2%	42.5%	39.1%
White/other	24.6%	31.6%	40.3%	44.6%	14.4%	25.2%
Married (%)	18.1%	20.7%	16.7%	15.5%	19.0%	23.0%
Number of adults in household	1.84	1.95	1.83	1.95	1.84	1.95
Number of children in household	1.07	0.80 **	0.80	0.62	1.24	0.89 **

**Source:** Authors' tabulations of the AFI baseline survey for third-year survey respondents.

**Notes:** AFS = alternative financial service. "Respondent" includes only those who completed the AFI third-year follow-up survey, and "nonrespondent" includes only those who did not complete it.

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

<sup>a</sup> Credit score is the Vantage 2.0 score reported for randomly assigned participants in Albuquerque.

# Appendix C. AFI Project Services

This appendix draws heavily from the AFI first-year evaluation (Mills et al. 2016) in describing AFI services study participants could receive. AFI services included individual development accounts with matched withdrawals for asset purchases; unmatched withdrawals, generally for emergencies; financial education; specific asset-related training on homeownership, small business operation, and postsecondary education; credit-building support; and coaching or case management.

## 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.

In Los Angeles, clients and staff faced 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.<sup>45</sup> When participants opened accounts, RISE could not automatically 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 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.<sup>46</sup>

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<sup>45</sup> 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 to monitor the account.

<sup>46</sup> 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.

## 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.

## 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 (e.g., job loss or medical needs), they could take a leave of absence for six months and not make any deposits. In Albuquerque, Prosperity Works monitored savers' accounts and notified coaches of savers who had missed deposits for three months.

## 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 for 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. 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 academic 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 to the appropriate party directly by Prosperity Works. The match 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 asset purchase 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. During our interviews with Albuquerque staff, they noted that some participants made unmatched withdrawals because they no longer had a purpose for the match funds (e.g., they got a job or scholarship and no longer needed the money).

Many participants in Los Angeles made unmatched withdrawals because RISE failed to secure nonfederal funds. Among Los Angeles participants who made an unmatched withdrawal, 30 percent reported in the AFI third-year follow-up survey doing so because match funds were not available. Another 11 percent reported taking out unmatched withdrawals out of concern their own savings may be inaccessible if they wait.

### FINANCIAL EDUCATION

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 course involved 21 hours of class time, and 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 for financial aid, which helped more participants cover the cost.

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 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.

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.

## 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 be a mortgage lender. The classes and counseling were free. 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 a home.

RISE gave Los Angeles participants saving for a home a list of homeownership training providers approved by the City of Los Angeles Housing Authority, and staff advised the participants to attend as many training sessions as they could. Some providers 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. The 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 free small business training—only to AFI participants—at the organization’s offices. The training lasted 10 hours and covered such topics as business missions, elevator pitches, financial projections and statements, cash flow, and business plans. RISE also required business savers to write a business plan, often involving several drafts.

## POSTSECONDARY EDUCATION TRAINING

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

## 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 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 (both treatment and control) 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.

**Coaching and case management services.** In Albuquerque, CNM academic coaches guided AFI participants through the project. They also connected students to resources such as financial aid,

scholarships, and public benefits and provided tutoring and mentoring. Although coaching is available to all CNM students, only some students take advantage of the services, and AFI offered a pathway to do so.

Los Angeles project staff offered AFI participants support 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 clients' needs and guiding them toward resources. Lack of individual time spent with staff was a problem noted by both staff and participants. In addition to individual case management, staff reported sending participants email 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 email address.

# Appendix D. Analytic Approach

This appendix describes our analytic approach for estimating intent-to-treat effects, our treatment-on-treated results, and minimum detectable effects.

## 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  $\alpha_1$  is the intent-to-treat effect. All models also include a site indicator variable ( $S_i$ ), baseline value of the outcome ( $Y_{ib}$ ),<sup>47</sup> a vector of individual-level characteristics measured at baseline ( $X_{ib}$ ;; see table D.1 for list and definition of characteristics)<sup>48</sup> and variables that identify the mode of the follow-up survey (over the phone, online, or in person) and enrollment cohort (whether the person entered the study in months 1 to 12, months 13 to 16, or months 17 to 19 of the enrollment period), which are represented by  $Z_i$ .

We estimate the program's treatment effect using ordinary least squares when  $Y_i$  is a continuous variable and using a probit model when  $Y_i$  is dichotomous.<sup>49</sup> In addition, we estimate quantile treatment effects of the program for liquid assets, net worth, earnings, and income using quantile regressions.<sup>50</sup> This method provides estimates of AFI's impact at different points of the distributions. Specifically, we estimate the program's impact at the 25th, 50th, and 75th percentiles of the liquid asset, net worth, earnings, and income distributions. We highlight impact estimates that are statistically significant at the 1 percent, 5 percent, and 10 percent levels.<sup>51</sup>

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<sup>47</sup> 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.

<sup>48</sup> We use multiple imputation to impute missing values for eight of the individual-level baseline characteristics used in the multivariate models. On average, 10 responses were imputed for each variable.

<sup>49</sup> We calculate and present the average marginal effect for the probit models.

<sup>50</sup> The quantile regression models include only 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 the results are qualitatively the same (no impact of AFI) and, in many cases, including them substantially reduces the precision of the impact estimates.

<sup>51</sup> 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.

**TABLE D.1**  
**Explanatory Variable Measures**

Variable	Definition
<b>Demographic explanatory variables</b>	
Age	Participant's age
Female	Participant's gender
Race/ethnicity (multiple dummy variables)	Participant's race or 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, license/certificate or associate'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 in household	Number of children 17 or younger living in household
Annual household income	Total household income in the past 12 months, including job earnings, child support, money received from family and friends, and public benefits such as welfare, Supplemental Security Income, and unemployment insurance (dummy variables: less than \$15,000, \$15,000–\$24,999, \$25,000 and over)
<b>Study participation explanatory variables</b>	
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)
Third-year follow-up survey mode	Means of administering third-year follow-up survey (dummy variables: in person, online, by phone)

## Estimation of Treatment-on-Treated Effects

To obtain TOT effects, we use an instrumental variables (IV) model (Angrist, Imbens, and Rubin 1996), where the first stage is:

$$P_i = \beta_0 + \beta_1 T_i + \pi_1 S_i + \pi_2 Y_{ib} + \pi_3 X_{ib} + \pi_4 Z_i + u_i.$$

and the second stage is:

$$Y_i = \delta_0 + \delta_1 \hat{P}_i + \theta_1 S_i + \theta_2 Y_{ib} + \theta_3 X_{ib} + \theta_4 Z_i + e_i.$$

In this model,  $P_i$  is an indicator for whether respondent  $i$  ever opened an IDA, and  $\hat{P}_i$  is the prediction of  $P_i$  obtained in the first stage. This method accounts for nonparticipation in the treatment group (those who did not open an IDA) and those in the control group who “crossed over” and opened an IDA (from a non-AFI provider). All other variables in the model are defined as above. The parameter of interest is  $\delta_1$ , which is the TOT effect (i.e., effect for participants who opened an IDA). We estimate the TOT using a linear IV approach when the outcome ( $Y_i$ ) is a continuous variable and a probit IV model when  $Y_i$  is dichotomous.

## Treatment-on-Treated Results

For the TOT analyses, we consider participation as making at least one deposit in an IDA. The rate of treatment group nonparticipation is roughly 16 percent, and the crossover rate (i.e., share of participants assigned to the control group who opened an IDA) is 7 percent. The TOT estimates suggest that AFI increased the likelihood of being a homeowner among renters (at baseline) by 6.1 percentage points (table D.2). This is higher than the ITT estimate of 4.7 percentage points (table 6.1). In our TOT analyses, we do not find a statistically significant increase in business ownership among non-business owners (at baseline), as we did in our ITT analysis. The TOT analyses also suggest that AFI decreased the number of hardships by 0.8 and number of medical hardships by 0.6. The ITT estimates are -0.6 and -0.5, respectively. Finally, we estimate that AFI participation decreased the likelihood of nonbank check cashing by 4 percentage points. We present the TOT estimates for all other outcomes in table D.2.

TABLE D.2

### Treatment-on-Treated Impacts of the AFI Program on Selected Outcomes at the Third-Year Follow-Up

Outcomes	TOT estimates <sup>b</sup>	P-value
<b>Primary outcomes</b>		
<b>Asset ownership (participant or participant and spouse/partner)</b>		
<i>Full sample (%)</i>		
Homeownership	3.5	0.317
Business ownership	5.3	0.174
Education or training	1.2	0.787
Homeownership, business ownership, or education or training	2.5	0.605
<i>By selected baseline characteristics (%)</i>		
Homeownership–Renter	6.1	*
Business ownership–Non-business owner	6.2	0.116
Education or training–No college degree	2.1	0.692
Homeownership, business ownership, or education–No assets	6.8	0.275
<b>Liquid assets (participant and spouse/partner)</b>		
Has liquid assets (%)	4.6	0.318
Liquid asset amount (\$)	1,521	0.178
<b>Secondary outcomes</b>		
<b>Net worth (participant and spouse/partner)</b>		
Net worth amount (\$)	-677	0.966
<b>Material hardship (household)</b>		
Any hardship (%)	-8.2	0.111
Number of hardships (#)	-0.81	*
Food hardship (%)	0.2	0.957
Housing hardship (%)	-1.9	0.583
Housing hardship (#)	-0.056	0.605
Utilities hardship (%)	3.7	0.414
Utilities hardship (#)	0.052	0.815
Medical hardship (%)	-13.1	***
Medical hardship (#)	-0.641	***
<b>Means-tested benefit receipt (household)</b>		
Receive benefits (%)	-4.8	0.3
<b>Alternative financial service use (participant and spouse/partner)</b>		
Use nonbank check cashing (%)	-4.1	*
Nonbank check cashing (#)	-0.116	0.18
Use AFS credit (%)	-2.2	0.543
AFS credit (#)	-0.108	0.525

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

Notes: AFS = alternative financial service. Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and third-year follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>b</sup> The TOT estimates are obtained using linear instrumental variable regression models for continuous outcome variables, and instrumental variable 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 or 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, enrollment date, and survey mode).

\*  $p < 0.1$ , \*\*\*  $p < 0.01$ . No differences are significant at the  $p < 0.05$  level.

## 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. A sample's adequacy 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 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 effects for two illustrative third-year outcomes: one binary measure and one continuous measure (table D.3). The binary outcome has an assumed control mean of 0.5.<sup>52</sup> 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 and 19.8 percent of the control standard deviation. For single-site analyses of Albuquerque and Los Angeles, the corresponding MDEs are 0.158 and 0.126, respectively.<sup>53</sup>

**TABLE D.3**  
**Minimum Detectable Effect for Treatment-Control Differences**

	Sites combined	Albuquerque	Los Angeles
<b>Analysis sample</b>			
Treatment	326	122	204
Control	295	118	177
Total	621	240	381
<b>Illustrative binary outcome</b>			
Control mean	0.5	0.5	0.5
Minimum detectable effect <sup>a</sup>	0.099	0.158	0.126
MDE as % of control mean	20%	32%	25%
Standardized MDE (MDE/control SD)	0.198	0.316	0.252
<b>Illustrative continuous outcome</b>			
Control mean	\$3,000	\$3,000	\$3,000
Control standard deviation	\$6,000	\$6,000	\$6,000
Minimum detectable effect <sup>a</sup>	\$1,200	\$1,932	\$1,536
MDE as % of control mean	40%	64%	51%
Standardized MDE (MDE/control SD)	0.200	0.322	0.256

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

**Note:** MDE = minimum detectable effect; SD = standard deviation.

<sup>a</sup> Assumes 80 percent power and 10 percent significance (two-tailed test).

<sup>52</sup> 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.

<sup>53</sup> The indicated MDEs are reasonable approximations. The actual MDEs will be 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.

For the illustrative continuous outcome, we use assumptions that correspond approximately to the available survey findings for monthly household income. We assume a control mean of \$3,000 and a standard deviation of \$6,000. For the combined sample, the MDE is \$1,200, or 40 percent of the control mean and 20 percent of the control standard deviation. For single-site analyses of Albuquerque and Los Angeles, the corresponding MDEs are \$1,932 and \$1,536, respectively.

The MDEs from table D.3 are in the range of the medium-term impact estimates from the Tulsa experimental site in the American Dream Demonstration. To put standardized MDE into perspective, Cohen (1977) suggests that standardized effect sizes of 0.20 should be considered a “small effect.” Standardized effect sizes around 0.50 indicate a “medium effect.”

Because we are also interested in the effect of AFI participation for some subgroup of respondents, we calculate the minimum detectable effects for the sample of renters, non-business owners, and respondents without a college degree and without assets in table D.4. We focus only on the binary outcome, as we only estimate the program’s effect on dichotomous variables for these subgroups.

We find sizable minimum detectable effects even when restricting the sample to these four subgroups. Standardized MDE varies from 0.21 for renters (the group with the largest sample size) to 0.28 for respondents with no assets (the group with smaller sample sizes). Those are standardized MDE considered between small and medium effects in the literature (Cohen 1977).

**TABLE D.4**  
**Minimum Detectable Effect for Treatment-Control Differences, Subgroups**

Subgroup	Renters	Non-business owner	No college degree	No assets
<b>Analysis sample</b>				
Treatment	298	244	274	180
Control	268	223	248	189
Total	566	467	522	369
<b>Illustrative binary outcome</b>				
Control mean	0.5	0.5	0.5	0.5
Minimum detectable effect <sup>a</sup>	0.104	0.114	0.1081	0.128
MDE as % of control mean	21%	23%	22%	26%
Standardized MDE (MDE/control SD)	0.208	0.228	0.2162	0.256

**Source:** Authors’ calculations from AFI third-year follow-up survey.

**Note:** MDE = minimum detectable effect; SD = standard deviation.

<sup>a</sup> Assumes 80 percent power and 10 percent significance (two-tailed test).

# Appendix E. Means of Outcome Variables by Group and Site

TABLE E.1  
Outcomes at the Third-Year Follow-Up Survey, Means by Group and Site

Dependent variable	Sites Combined				Albuquerque				Los Angeles			
	Treatment		Control		Treatment		Control		Treatment		Control	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
<b>Asset ownership (participant or participant and spouse/partner)</b>												
Homeownership (%)	18.2	(38.6)	14.4	(35.2)	27.9	(44.9)	21.6	(41.2)	12.3	(32.9)	9.7	(29.6)
Business ownership (%)	22.0	(41.4)	17.0	(37.6)	20.5	(40.4)	14.5	(35.3)	22.9	(42.0)	18.7	(39.0)
Education or Training Increase (%)	36.7	(48.2)	36.9	(48.3)	56.6	(49.6)	55.9	(49.7)	24.8	(43.2)	24.0	(42.7)
Homeowner, business owner, or education or training increase (%)	56.1	(49.6)	54.4	(49.8)	68.0	(46.7)	70.7	(45.6)	48.7	(50.0)	43.3	(49.6)
<b>Liquid assets (participant and spouse/partner)</b>												
Has liquid assets (%)	81.6	(38.8)	76.6	(42.4)	83.5	(37.2)	81.8	(38.6)	80.3	(39.8)	72.4	(44.8)
Liquid asset amount (\$)	5,462	(11,727)	3,680	(7,773)	5,139	(10,713)	3,740	(7,768)	5,688	(12,390)	3,631	(7,782)
Liquid assets (\$) <sup>a</sup>												
25th percentile	70	--	9	--	54	--	25	--	70	--	0	--
50th percentile	800	--	725	--	705	--	750	--	900	--	692	--
75th percentile	4,500	--	3,855	--	5,750	--	4,300	--	4,400	--	3,800	--
<b>Net worth (participant and spouse/partner)</b>												
Net worth amount (\$)	2,702	(140,835)	-2,166	(75,131)	-8,386	(80,472)	-1,804	(78,906)	10,536	(170,740)	-2,486	(71,709)
Net worth (\$)												
25th percentile	-24,124	--	-25,447	--	-30,750	--	-37,695	--	-16,400	--	-13,340	--
50th percentile	-3,000	--	-3,000	--	-9,415	--	-8,250	--	-40	--	0	--
75th percentile	5,200	--	5,800	--	1,400	--	4,850	--	6,723	--	5,926	--
<b>Material hardship (household)</b>												
Any hardship (%)	45.7	(49.8)	52.1	(50.0)	44.5	(49.7)	46.6	(49.9)	46.4	(49.9)	55.7	(49.7)
Number of hardships (#)	1.88	(3.47)	2.46	(4.44)	1.74	(3.08)	2.00	(3.76)	1.97	(3.68)	2.77	(4.81)
Food hardship (%)	13.8	(34.4)	14.4	(35.2)	14.3	(35.0)	11.0	(31.3)	13.4	(34.1)	16.8	(37.4)
Housing hardship (%)	12.2	(32.7)	13.9	(34.6)	11.1	(31.5)	11.5	(31.9)	12.8	(33.4)	15.5	(36.2)
Housing hardship (#)	0.29	(0.96)	0.36	(1.07)	0.26	(0.89)	0.26	(0.85)	0.30	(1.01)	0.43	(1.19)
Utilities hardship (%)	29.7	(45.7)	27.9	(44.9)	28.6	(45.2)	19.6	(39.8)	30.4	(46.0)	33.3	(47.2)
Utilities hardship (#)	0.96	(1.99)	0.97	(2.27)	0.94	(2.11)	0.59	(1.65)	0.98	(1.91)	1.22	(2.56)
Medical hardship (%)	26.0	(43.9)	34.9	(47.7)	27.4	(44.6)	33.6	(47.3)	25.1	(43.4)	35.7	(47.9)
Medical hardship (#)	0.79	(2.02)	1.18	(2.51)	0.78	(1.85)	1.16	(2.40)	0.79	(2.11)	1.20	(2.59)
<b>Means-tested benefit receipt (household)</b>												
Receive benefits (%)	64.2	(48.0)	66.7	(47.2)	58.6	(49.3)	59.6	(49.1)	67.6	(46.8)	71.7	(45.1)

Continued on next page

TABLE E.1

**Outcomes at the Third-Year Follow-Up Survey, Means by Group and Site (continued)**

Dependent variable	Sites Combined				Albuquerque				Los Angeles			
	Treatment		Control		Treatment		Control		Treatment		Control	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
<b>Alternative financial service use (participant and spouse/partner)</b>												
Use nonbank check cashing (%)	3.5	(18.3)	6.5	(24.7)	3.3	(17.9)	2.5	(15.8)	3.6	(18.6)	9.2	(29.0)
Nonbank check cashing (#)	0.12	(0.71)	0.21	(0.92)	0.15	(0.82)	0.09	(0.65)	0.10	(0.64)	0.30	(1.06)
Use AFS credit (%)	13.9	(34.6)	16.4	(37.0)	14.0	(34.8)	14.9	(35.7)	13.8	(34.5)	17.3	(37.9)
AFS credit (#)	0.43	(1.68)	0.54	(1.61)	0.29	(1.17)	0.67	(2.04)	0.52	(1.92)	0.45	(1.24)
<b>Employment (participant) and earnings and income (household)</b>												
Employed (%)	78.8	(40.9)	75.3	(43.1)	82.0	(38.5)	76.3	(42.6)	76.8	(42.2)	74.7	(43.5)
Monthly earnings amount (\$)	2,739	(5,948)	3,004	(6,670)	2,986	(7,045)	3,065	(6,750)	2,578	(5,099)	2,960	(6,616)
Monthly earnings (\$)												
25th percentile	217	--	0	--	400	--	0	--	13	--	0	--
50th percentile	1,500	--	1,500	--	1,200	--	1,350	--	1,630	--	1,600	--
75th percentile	3,000	--	2,982	--	2,400	--	2,982	--	3,200	--	3,000	--
Monthly income amount (\$)	2,870	(5,307)	3,582	(7,387)	3,040	(6,238)	3,584	(7,567)	2,764	(4,631)	3,581	(7,259)
Monthly income (\$)												
25th percentile	800	--	743	--	788	--	677	--	800	--	906	--
50th percentile	1,709	--	1,800	--	1,460	--	1,700	--	1,944	--	1,900	--
75th percentile	3,200	--	3,095	--	2,775	--	3,000	--	3,500	--	3,100	--
<b>Credit score (participant)<sup>b</sup></b>												
Vantage 2.0 score (501–990)	—	—	—	—	675	(87)	663	(111)	—	—	—	—
Change in Vantage 2.0 score (#)	—	—	—	—	16	(102)	2	(117)	—	—	—	—
<b>Personal outlook (participant)</b>												
Ability to make ends meet (%)	57.1	(49.5)	58.8	(49.2)	62.8	(48.4)	64.4	(47.9)	53.6	(49.9)	55.0	(49.8)
Perceived financial security (1–10)	5.38	(3.05)	5.35	(3.31)	5.45	(2.94)	5.84	(3.19)	5.34	(3.11)	5.01	(3.35)
Better off financially (%)	42.4	(49.4)	39.4	(48.9)	43.0	(49.5)	37.3	(48.4)	42.0	(49.4)	40.9	(49.2)
Worse off financially (%)	17.1	(37.7)	23.9	(42.6)	15.7	(36.4)	22.9	(42.0)	18.0	(38.4)	24.6	(43.1)
Good health (%)	76.2	(42.6)	78.5	(41.1)	78.5	(41.1)	84.7	(36.0)	74.8	(43.5)	74.3	(43.7)
Self-esteem (0–30)	23.06	(4.90)	23.40	(5.37)	22.62	(5.11)	22.79	(5.82)	23.35	(4.74)	23.82	(4.99)
<b>Community involvement (participant)</b>												
Community involvement (%)	59.1	(49.2)	55.1	(49.8)	59.7	(49.1)	55.9	(49.7)	58.8	(49.3)	54.4	(49.8)
<b>Time preference (participant)</b>												
Present-oriented time preference (%)	8.5	(14.1)	27.9	(34.8)	5.9	(14.7)	23.5	(35.4)	10.1	(13.8)	30.1	(34.5)

Source: AFI third-year follow-up survey.

Notes: AFS = alternative financial services. Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values. Standard deviations are presented in parentheses.

<sup>a</sup>Standard deviations cannot be calculated for percentiles.

<sup>b</sup>Credit score values are based on the Vantage 2.0 scores for all scoreable randomly assigned participants in Albuquerque.

TABLE E.2

**Outcomes at the Third-Year Follow-Up Survey, Means by Site**

Variable	Sites Combined		Albuquerque		Los Angeles	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
<b>Asset ownership (participant or participant and spouse/partner)</b>						
Homeownership (%)	16.4	(37.0)	24.8	(43.2)	11.1	(31.4)
Business ownership (%)	19.6	(39.7)	17.6	(38.1)	21.0	(40.7)
Education or training increase (%)	36.9	(48.3)	56.3	(49.6)	24.7	(43.1)
Homeowner, business owner, or education or training increase (%)	55.2	(49.7)	69.5	(46.1)	46.2	(49.9)
<b>Savings (participant and spouse/partner)</b>						
Has liquid assets (%)	79.2	(40.6)	82.7	(37.9)	76.7	(42.3)
Liquid asset amount (\$)	4,624	(10,100)	4,454	(9,408)	4,751	(10,590)
Liquid assets (\$)ª						
25th percentile	25	--	50	--	15	--
50th percentile	773	--	710	--	800	--
75th percentile	4,500	--	4,500	--	4,200	--
<b>Net worth (participant and spouse/partner)</b>						
Net worth (\$)	407	(114,637)	-5,074	(79,704)	4,719	(135,841)
Net worth (\$)						
25th percentile	-24,500	--	-31,993	--	-15,400	--
50th percentile	-3,000	--	-8,500	--	0	--
75th percentile	5,650	--	2,880	--	6,000	--
<b>Material hardship (household)</b>						
Any hardship (%)	48.7	(50.0)	45.5	(49.8)	50.7	(50.0)
Number of hardships (#)	2.2	(4.0)	1.9	(3.4)	2.3	(4.3)
Food hardship (%)	14.1	(34.8)	12.7	(33.3)	15.0	(35.7)
Housing hardship (%)	13.0	(33.6)	11.3	(31.7)	14.0	(34.8)
Housing hardship (#)	0.3	(1.0)	0.3	(0.9)	0.4	(1.1)
Utilities hardship (%)	28.8	(45.3)	24.2	(42.9)	31.8	(46.6)
Utilities hardship (#)	1.0	(2.1)	0.8	(1.9)	1.1	(2.2)
Medical hardship (%)	30.2	(45.9)	30.4	(46.0)	30.0	(45.9)
Medical hardship (#)	1.0	(2.3)	1.0	(2.1)	1.0	(2.3)
<b>Alternative financial services (participant and spouse/partner)</b>						
Use nonbank check cashing (%)	4.9	(21.7)	2.9	(16.9)	6.2	(24.2)
Nonbank check cashing (#)	0.2	(0.8)	0.1	(0.7)	0.2	(0.9)

TABLE E.2

**Outcomes at the Third-Year Follow-Up Survey, Means by Site (continued)**

Variable	Sites Combined		Albuquerque		Los Angeles	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Use AFS credit (%)	15.1	(35.8)	14.5	(35.2)	15.4	(36.1)
AFS credit (#)	0.5	(1.6)	0.5	(1.7)	0.5	(1.6)
<b>Means-tested benefit receipt (household)</b>						
Receive benefits (%)	65.3	(47.6)	59.1	(49.2)	69.5	(46.1)
<b>Employment (participant) and earnings and income (household)</b>						
Employed (%)	77.1	(42.0)	79.2	(40.6)	75.9	(42.8)
Monthly earnings amount (\$)	2,866	(6,304)	3,025	(6,898)	2,756	(5,859)
Monthly earnings (\$)						
25th percentile	0	--	200	--	0	--
50th percentile	1,500	--	1,200	--	1,600	--
75th percentile	3,000	--	2,700	--	3,000	--
Monthly income amount (\$)	3,208	(6,388)	3,310	(6,932)	3,139	(5,995)
Monthly income (\$)						
25th percentile	775	--	730	--	903	--
50th percentile	1,800	--	1,551	--	1,900	--
75th percentile	3,200	--	3,000	--	3,347	--
<b>Personal outlook (participant)</b>						
Ability to make ends meet (%)	57.9	(49.4)	63.6	(48.1)	54.2	(49.8)
Perceived financial security (1–10)	5.4	(3.2)	5.6	(3.1)	5.2	(3.2)
Better off financially (%)	41.0	(49.2)	40.2	(49.0)	41.5	(49.3)
Worse off financially (%)	20.3	(40.3)	19.2	(39.4)	21.0	(40.8)
Good health (%)	77.3	(41.9)	81.6	(38.8)	74.5	(43.6)
Self-esteem (0–30)	23.2	(5.1)	22.7	(5.5)	23.6	(4.9)
<b>Community involvement (participant)</b>						
Community involvement (%)	57.2	(49.5)	57.8	(49.4)	56.7	(49.6)
<b>Time preference</b>						
Present-oriented time preference (%)	11.1	(31.5)	10.2	(30.3)	11.7	(32.2)

Source: AFI third-year follow-up survey.

Notes: AFS = alternative financial services. Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values. Standard deviations are presented in parentheses.

<sup>a</sup>Standard deviations cannot be calculated for percentiles.

# Appendix F. Impact Estimates by Site

TABLE F.1

Impacts of AFI Program on Liquid Assets and Asset Ownership at the Third-Year Follow-Up, by Site

Primary outcomes	Albuquerque				Los Angeles			
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value
<b>Asset ownership (participant or participant and spouse/partner)</b>								
<i>Full sample</i>								
Homeownership (%)	27.7	21.6	6.1	0.216	11.6	10.3	1.3	0.653
Business ownership (%)	21.1	14.0	7.2	0.127	22.0	19.6	2.3	0.542
Education or training (%)	55.2	57.3	-2.1	0.725	24.7	24.0	0.8	0.860
Homeowner, business owner, or education or training (%)	66.7	72.0	-5.3	0.348	48.2	43.9	4.2	0.389
<i>By selected baseline characteristics</i>								
Homeownership (%)— <i>Renter</i>	22.6	13.9	8.7 *	0.088	9.1	6.0	3.1	0.288
Business ownership (%)— <i>Non-business owner</i>	16.1	11.7	4.4	0.374	13.9	8.4	5.6	0.146
Education or training (%)— <i>No college degree</i>	61.1	61.2	0.0	0.995	30.9	30.2	0.8	0.883
Homeownership, business ownership, or education (%)— <i>No assets</i>	68.2	71.9	-3.7	0.596	46.5	37.0	9.5	0.136
<b>Liquid assets (participant and spouse/partner)</b>								
Has liquid assets (%)	83.3	82.1	1.2	0.815	79.2	74.2	5.0	0.304
Liquid asset amount (\$)	5,173	3,705	1,469	0.209	5,349	4,036	1,312	0.309
Liquid assets (\$)								
25th percentile	622	591	30	0.915	130	99	31	0.801
50th percentile	1,801	1,617	184	0.723	1,037	958	79	0.852
75th percentile	6,366	4,517	1,849	0.210	4,685	3,225	1,460	0.370

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

Notes: Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for binary outcome variables (for which we report the marginal effects), and quantile regression models. These models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). Quantile regressions for liquid assets are estimated at the 25th, 50th, and 75th percentiles of the liquid asset distribution and may control only for the baseline value of liquid assets. There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

\*  $p < 0.1$ .

TABLE F.2

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

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value
<b>Net worth (participant and spouse/partner)</b>								
Net worth amount (\$)	-10,747	526	-11,273	0.402	9,008	-594	9,602	0.643
Net worth (\$)								
25th percentile	-27,443	-25,993	-1,450	0.903	-14,722	-14,020	-702	0.927
50th percentile	-9,035	-6,825	-2,209	0.623	-3,999	-3,770	-228	0.924
75th percentile	3,791	4,018	-226	0.979	12,403	5,528	6,876	0.552
<b>Material hardship (household)</b>								
Any hardship (%)	43.5	47.5	-4.0	0.535	46.8	54.6	-7.7	0.144
Number of hardships (#)	1.7	2.1	-0.4	0.431	1.9	2.8	-0.8 *	0.078
Food hardship (%)	14.7	10.4	4.2	0.304	13.9	15.9	-2.0	0.589
Housing hardship (%)	9.6	13.1	-3.6	0.398	13.7	14.4	-0.6	0.855
Housing hardship (#)	0.3	0.3	0.0	0.882	0.3	0.4	0.0	0.799
Utilities hardship (%)	28.0	19.9	8.1	0.123	31.0	32.6	-1.5	0.742
Utilities hardship (#)	0.9	0.7	0.2	0.358	1.0	1.2	-0.2	0.522
Medical hardship (%)	26.8	34.2	-7.3	0.216	24.1	37.3	-13.0 ***	0.006
Medical hardship (#)	0.8	1.2	-0.4	0.162	0.7	1.3	-0.6 **	0.020
<b>Means-tested benefit receipt (household)</b>								
Receive benefits (%)	55.7	62.6	-7.0	0.229	67.8	71.7	-3.8	0.415
<b>Alternative financial service use (participant and spouse/partner)</b>								
Use AFS credit (%)	13.1	15.8	-2.7	0.534	14.6	16.0	-1.5	0.676
AFS credit (#)	0.3	0.7	-0.5 **	0.037	0.5	0.4	0.1	0.637
Use nonbank check cashing (%)	2.5	3.0	-0.5	0.811	3.4	9.2	-5.9 **	0.029
Nonbank check cashing (#)	0.1	0.1	0.0	0.646	0.1	0.3	-0.2 **	0.037

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** AFS = alternative financial service. Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> The impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for binary outcome variables (for which we report the marginal effects), and quantile regression models. The models control for the baseline value of the dependent variable, baseline characteristics (i.e., age, sex, race or 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, enrollment date, and survey mode). There are statistically significant differences between the Albuquerque and Los Angeles impact estimates for AFS credit (#) and nonbank check cashing (#).

\*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .



TABLE F.3

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

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value
<b>Employment (participant) and earnings and income (household)</b>								
Employed (%)	82.2	76.3	5.9	0.244	77.2	74.3	2.9	0.484
Monthly earnings amount (\$)	2,785	3,271	-486	0.602	2,366	3,200	-834	0.208
Monthly earnings (\$)								
25th percentile	345	345	0.0	1.000	539	539	0.0	1.000
50th percentile	1,348	1,310	38	0.879	1,645	1,571	74	0.772
75th percentile	2,413	2,639	-227	0.727	3,022	2,745	277	0.372
Monthly income amount (\$)	3,105	3,517	-412	0.690	2,530	3,855	-1,325 *	0.077
Monthly income (\$)								
25th percentile	764	659	105	0.700	942	1,046	-104	0.656
50th percentile	1,559	1,631	-72	0.774	1,859	1,812	48	0.865
75th percentile	2,634	2,931	-297	0.663	3,189	3,273	-84	0.819
<b>Credit score (participant)<sup>c</sup></b>								
Vantage 2.0 score (501–990)	675	663	11	0.329	—	—	—	—
Change in Vantage 2.0 score (#)	16	2	14	0.309	—	—	—	—

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

<sup>b</sup> With the exception of credit score, the impact estimates are obtained using ordinary least squares regression models for continuous outcome variables, probit models for the binary outcome variable (for which we report the marginal effects), and quantile regression models. 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, enrollment date, and survey mode). Credit score impact estimates are not regression adjusted. There are no statistically significant differences between the Albuquerque and Los Angeles impact estimates.

<sup>c</sup> Credit score values are based on the Vantage 2.0 scores for all scoreable randomly assigned participants in Albuquerque.

\*  $p < 0.1$ .

TABLE F.4

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

Secondary outcomes	Albuquerque				Los Angeles			
	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value	Treatment <sup>a</sup>	Control <sup>a</sup>	Impact <sup>b</sup>	P-value
<b>Personal outlook (participant)</b>								
Ability to make ends meet (%)	64.6	62.8	1.8	0.759	54.2	53.8	0.4	0.939
Perceived financial security (1–10)	5.5	5.8	-0.4	0.368	5.4	5.0	0.4	0.226
Better off financially (%)	44.6	35.9	8.7	0.157	43.7	38.9	4.8	0.343
Worse off financially (%)	15.4	23.2	-7.9	0.115	18.0	24.6	-6.5	0.126
Good health (%)	79.9	83.6	-3.8	0.412	74.2	74.8	-0.7	0.880
Self-esteem (0–30)	22.8	22.6	0.2	0.745	23.6	23.6	0.0	0.996
<b>Community involvement (participant)</b>								
Community involvement (%)	55.8	60.0	-4.2	0.471	58.2	55.1	3.1	0.520
<b>Time preference (participant)</b>								
Present-oriented time preference (%)	5.4	15.3	-10.0 **	0.016	10.9	12.6	-1.7	0.615

**Source:** AFI third-year follow-up and baseline surveys.

**Notes:** Sample includes respondents from Albuquerque (N = 240) and Los Angeles (N = 381) who completed the baseline and follow-up surveys. Sample sizes for specific outcomes may vary because of missing values.

<sup>a</sup> Values in these columns are the regression-adjusted means and quantile values at the third-year follow-up.

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

\*\*  $p < 0.05$ .

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