

Assets for Independence Act Evaluation: Design Phase

Concept Paper

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Section 1: Evaluation Objectives

This report provides the conceptual framework for the evaluation of the Assets for Independence Act. We focus here on the research questions to be addressed in the evaluation, the evaluation methods appropriate for addressing these questions, and the measures to be used in the research. This paper does not detail the data collection strategies that might be appropriate for obtaining these measures. Such strategies will be developed and tested during the remainder of the twelve-month evaluation design phase, which extends through August 2000.

The Assets for Independence Act (AFIA) provides federal funds for the operation of individual development account (IDA) programs at the state and local levels, subject to requirements regarding who can participate and how the accounts will be financed and structured. The premise of the Act is that the offer of matching funds and the provision of program support services (counseling and training) will promote savings, enable participants to purchase homes, start businesses, and advance their education, and thus improve their lives.

The primary question that the AFIA evaluation must address can be stated as follows:

Has the Assets for Independence Act led to programs that achieved their intended results in a cost-effective manner?

From this basic question, a series of other questions follow, as listed below.

- (1) Were programs implemented as intended under the Act?
 - (a) What programs were created or expanded as a result of the Act?
 - (b) Did these programs indeed conform to the provisions of the Act?
- (2) Did the participants improve their lives in expected ways as a result of the program?
 - (a) Did the participants experience improved outcomes?

What were the savings rates of individuals . . . based on demographic characteristics including gender, age, family size, race or ethnic background, and income?

What were the economic, civic, psychological, and social effects of asset accumulation? How did such effects vary among different populations or communities?

(b) Did the improved outcomes among participants result from the program?

What were the effects of incentives and organizational or institutional support on savings behavior?

What were the effects of individual development accounts on savings rates, homeownership, level of postsecondary education attained, and self-employment?

How do such effects vary among different populations and communities?

(3) Are the programs cost-effective?

(a) What were the benefits and costs associated with these programs?

(b) Do the program benefits outweigh their associated costs?

What are the potential returns to the Federal Government and to other public sector and private sector investors in individual development accounts over a 5-year and 10-year period of time?

With this as the general evaluation framework, the remainder of this first section describes the statutory mandate for evaluation (as contained in Section 414 of the Assets for Independence Act), the relevant evaluation methods, and issues of site selection and timing of the evaluation activities to be conducted following completion of the design phase.

Statutory mandate for evaluation

Section 414 of the Assets for Independence Act establishes the statutory mandate for evaluation of AFI-funded projects, as follows:

- X Section 414(a)—“In General”—indicates that, within 10 months of legislative enactment, the HHS Secretary is to “contract with an independent research organization to evaluate the demonstration projects conducted under this title, individually and as a group.”
- X Section 414(b)—“Factors to Evaluate”—specifies the factors that the research organization is to address “in evaluating any demonstration project” conducted under the Act.
- X Section 414(c)—“Methodological Requirements”—states that, in at least one site, the

research organization will “use control groups to compare participants with nonparticipants” and that the evaluation work will be based on both quantitative analysis and qualitative assessments, with the latter including in-depth interviews.

- X Section 414(d)—“Reports by the Secretary”—indicates that the HHS Secretary will provide to the Congress (1) “interim reports” each year summarizing the results of annual progress reports submitted by each grantee to the Secretary and (2) a “final report setting forth the results and findings of all reports and evaluations” undertaken with respect to the Act.
- X Section 414(e)—“Evaluation Expenses”—sets aside 2 percent of the congressionally appropriated amount for the Act to carry out the evaluation activities called for under Section 414.

Factors to evaluate and corresponding evaluation activities

With respect to the design of the evaluation, it is Sections 414(b) and 414(c) that are most important. In Exhibit 1-1, we have listed the “factors to evaluate” from Section 414(b), as rows in a matrix. The columns of this matrix indicate the evaluation activities that we have identified as relevant in carrying out the statutory mandate, as follows:

- X process analysis
 - on-site observation of program operations, interviewing of program staff, and examination of written materials to determine how the program was implemented and how the program operates
- X in-depth participant interviews
 - lengthy personal interviews with program participants to examine their understanding of the program, their reasons for participating, and their experiences as a participant
- X program and participant tracking and monitoring
 - collection and analysis of information regularly maintained about the status of program participants, the flow of funds into and out of the accounts, and administrative operational details, including staffing and costs
- X experimental impact analysis
 - collection and analysis of information on program-eligible persons randomly assigned to a treatment group (participating in the program) and a control group (not participating in the program) for the purpose of estimating the effects of the program on its participants, relying on random assignment as the

**Exhibit 1-1:
Factors to Evaluate and
Corresponding Evaluation Activities**

	Corresponding evaluation activities:					
	Process analysis	In-depth participant interviews	Program and participant tracking and monitoring	Experimental impact analysis	Nonexperimental impact analysis	Benefit-cost analysis
Factors to evaluate—In evaluating any demonstration project conducted under this title [the Assets for Independence Act], the research organization shall address the following factors:						
(1) The effects of incentives and organizational or institutional support on savings behavior	✓ especially, at experimental site(s)	✓ especially, at experimental site(s)		✓ treatment-control differences	✓ participant-nonparticipant differences	
(2) The savings rates of individuals . . . based on demographic characteristics including gender, age, family size, race or ethnic background, and income		✓	✓	✓ treatment group data	✓ participant-nonparticipant differences	
(3) The economic, civic, psychological, and social effects of asset accumulation, and how such effects vary among different populations or communities		✓		✓ treatment subgroup data	✓ participant-nonparticipant differences	
(4) The effects of individual development accounts on savings rates, homeownership, level of postsecondary education attained, and self-employment, and how such effects vary among different populations or communities	✓ especially, at experimental site(s)	✓ especially, at experimental site(s)		✓ treatment-control differences	✓ participant-nonparticipant differences	
(5) The potential financial returns to the Federal Government and to other public sector and private sector investors in individual development accounts over a 5-year and 10-year period of time	✓ cost estimates		✓ cost estimates	✓ benefit estimates	✓ benefit estimates	✓
(6) The lessons to be learned from the demonstration projects . . . and if a permanent program of individual development accounts should be established	✓	✓	✓	✓	✓	✓

means of obtaining groups that are comparable on both observable and unobservable traits and thus as the means of enabling one to attribute to the program any differences in observed outcomes between the two groups.

- X nonexperimental impact analysis
collection and analysis of information on persons participating in the program and a comparison group of persons identified in data as not participating in the program, for the purpose of estimating the effects of the program on its participants, relying on statistical techniques to take account of demographic and socioeconomic differences between groups and thus to isolate the effects of the program.

- X benefit-cost analysis
collection and analysis of information on the benefits of the program to its participants and the costs of the program to the federal government (i.e., to federal taxpayers), to other public sponsors (including state and local), and to private funders.

This following sections of this report focus in turn on these research activities. Within Exhibit 1-1, a check mark (✓) indicates those evaluation activities that are relevant for each factor to evaluate.

Among the six “corresponding evaluation activities” shown in Exhibit 1-1, three follow directly from the statutory requirements. These are: in-depth participant interviews, experimental analysis of effects, and benefit-cost analysis. Section 414(c) makes specific reference to in-depth participant interviews. The same subsection also calls for experimental analysis of effects, through the requirement for control groups, which is conventionally interpreted to mean the construction of a research sample with cases randomly assigned to treatment and control groups. Benefit-cost analysis is also a necessary research component, as the fifth factor to evaluate identified in Section 414(b) calls for the evaluation to address the “potential financial returns to the Federal Government and to other public sector and private sector investors.”

The inclusion in this design of the other three evaluation activities identified in Exhibit 1-1—process analysis, program and participant tracking and monitoring, and nonexperimental analysis of effects—requires brief explanation.

Process analysis is a valuable counterpart to any quantitative analysis of program effects. One needs to establish how the program was implemented, whether the program was in fact implemented as intended in any given site, and whether the program operates differently across sites. Information about the specific workings of the program, especially as it pertains to the interface with program participants, is valuable—if not essential—to the interpretation of program effects, whether

estimated experimentally or nonexperimentally. Cost information obtained through on-site staff interviews is also essential to the benefit-cost analysis.

Program and participant tracking and monitoring is a means of obtaining administrative data on program participation and costs, account usage, and participant outcomes. In the AFI programs, the information system in use by virtually all sites will presumably be MIS IDA—the Management Information System for Individual Development Accounts. The data collected through this system (or any other equivalent) will indicate the pattern among participants of deposits into accounts and assets purchased from the accounts. The system also provides some program cost information.

The nonexperimental impact analysis is one possible approach, in addition to an experimental design, to estimating the impact of the program on savings and asset accumulation. In a nonexperimental analysis, the effects of AFI programs would be derived by using available national databases to compare the experiences of AFI program participants with the experiences of similar individuals and families in the population of AFI nonparticipants. Statistical methods would be used to adjust for the effect on savings and asset accumulation of differences between participants and nonparticipants in their demographic and socioeconomic characteristics, in seeking to estimate the effects that are specific to the program.

Issues of site selection and timing

Section 414 of the Act says little about matters of site selection and the timing of evaluation activities. These issues are raised here at the outset and will then be addressed later, in the final section of this paper.

As to site selection, Section 414(a) calls for research activities that serve the following purpose:

to evaluate the demonstration projects conducted under this title, individually and as a group, including evaluating all qualified entities participating in and sources providing funds for the demonstration projects conducted under this title.

As noted earlier, Sections 414(b) and 414(c) then establish the factors to evaluate and the methodological requirements, respectively, to be used “in evaluating any demonstration project conducted under this title,” where control groups are to be used “for at least one site.”

We interpret this language to mean that the evaluation design should provide for the following:

In *all* funded sites, there will be some level of evaluation activity. This will be the program and participant monitoring and tracking that can be provided through MIS IDA or its equivalent.¹

- X In *one site*, and most likely *only* one site, an experimental design will be implemented.
- X In the experimental site and also *in some limited number of additional sites*, the following evaluation methods will be applied: process analysis, in-depth participant interviews, and benefit-cost analysis.

As to matters of timing, the Act requires the Secretary to submit interim reports to the Congress annually, by the end of March 2000 and every 12 months thereafter, “until all demonstration projects conducted under this title are completed.” The final evaluation report is then due “not later than 12 months after the conclusion of all demonstration projects conducted under this title.”

As shown in Exhibit 1-2, because projects are funded for five years and because there are five cohorts of funded projects, it is not until September 2008 that “all demonstration projects conducted under this title are completed.”² The final evaluation report would then come one year later (2009), if one interprets the statutory language literally.

This literal interpretation, however, seems an implausible one. The Congress will presumably want evaluation findings in a timely manner for purposes of reauthorizing the Act, which passed in October 1998 and will come up for reauthorization during 2003. This suggests that interim evaluation reports would be submitted in March of 2000, 2001, and 2002, with a final evaluation report submitted in March 2003.

Under this scenario, the final evaluation report (if submitted in March 2003) would encompass operational data through September 2002, including:

- X three years of operation of the first cohort of grantees (October 1999-September 2002);
- X two years of operation of the second cohort of grantees (October 2000-September 2002); and
- X one year of operation of the third cohort of grantees (October 2001-September 2002).

¹ Nonexperimental impact analysis, to the extent that it makes use of national data, may also potentially include information for all funded sites.

² The first cohort (of 40 grantees) was announced on September 21, 1999. These five-year programs will thus be completed in September 2004. One could then expect that the fifth and final cohort of grantees under the current law would be announced around September 2003; these programs would not end until late 2008.

Exhibit 1-2:
Assets for Independence Act Grantees--
Project Periods, by Cohort

Grantee cohort	Federal fiscal year funding	Date of grant announcement	Project period				
			1st year	2nd year	3rd year	4th year	5th year
First	1999	Sep 1999	Oct 1999-	Oct 2000-	Oct 2001-	Oct 2002-	Oct 2003-
			Sep 2000	Sep 2001	Sep 2002	Sep 2003	Sep 2004
Second	2000	Sep 2000	Oct 2000-	Oct 2001-	Oct 2002-	Oct 2003-	Oct 2004-
			Sep 2001	Sep 2002	Sep 2003	Sep 2004	Sep 2005
Third	2001	Sep 2001	Oct 2001-	Oct 2002-	Oct 2003-	Oct 2004-	Oct 2005-
			Sep 2002	Sep 2003	Sep 2004	Sep 2005	Sep 2006
Fourth	2002	Sep 2002	Oct 2002-	Oct 2003-	Oct 2004-	Oct 2005-	Oct 2006-
			Sep 2003	Sep 2004	Sep 2005	Sep 2006	Sep 2007
Fifth	2003	Sep 2003	Oct 2003-	Oct 2004-	Oct 2005-	Oct 2006-	Oct 2007-
			Sep 2004	Sep 2005	Sep 2006	Sep 2007	Sep 2008

Within this projected schedule, the time available to conduct an experimental evaluation in one or more sites is quite limited, in the following respects. Once an experimental site is selected and is ready to serve participants under an experimental regime, it will likely take one year to enroll a research sample of adequate size. Moreover, the startup of sample enrollment could itself not begin until some time after HHS has contracted with an independent research organization to conduct the Section 414 evaluation activities; this lead time would be necessary to establish the procedures for baseline interviewing and random assignment. At the very earliest, this evaluation contract would be awarded in September 2000, immediately following the year-long evaluation design phase. This best-case scenario implies that the experimental sample would be enrolled from October 2000 through September 2001, with follow-up data extending through September 2002. The members of the research sample would thus be observed for a period of 12 to 24 months, depending on their month of random assignment.

Note that only the grantees in the first or second funding cohorts would be candidates for the experimental site, if experimental findings are to be available for Congress to consider in its reauthorization of the Act. For sites in the third cohort, follow-up data collection would need to end at the close of the first project year, a year that would be required simply to enroll the research sample.

Section 2: Literature Review

How Can the Poor Save?

A large body of work attempts to explain patterns of saving behavior and asset accumulation. Existing theories, which are actually at various stages of theoretical development, may be classified into four categories: 1) neoclassical economic, 2) psychological and sociological, 3) behavioral, and 4) institutional.

Neoclassical Economic Theories

Neoclassical economic models assume that individuals are rational beings who respond in predictable ways to changes in incentives. From this perspective, there are two broad determinants of individual behavior: opportunities (or constraints) and individual preferences (Pollak, 1998). Preferences are generally assumed to be stable and exogenous. Many economic models also assume that individuals have perfect knowledge and access to perfect markets. With regard to theories of saving and asset accumulation, it is important to note that individual utility (i.e., happiness or satisfaction) is assumed to be a function of consumption. Therefore, economic models generally treat saving as a residual, resources that are left over after consumption decisions have been made.

The two most well-known neoclassical theories of saving are the life cycle hypothesis, or the LCH, (Ando & Modigliani, 1963; Modigliani & Ando, 1957; Modigliani & Brumberg, 1954), and the permanent income hypothesis, or the PIH (Friedman, 1957). Both of these theories assume that individuals and households are concerned about *long-term* consumption opportunities and therefore explain saving and consumption in terms of expected future income. Proponents of these models view saving as a way to “smooth” consumption in the face of income fluctuations. Since consumption is determined by anticipated lifetime resources (rather than current resources), saving over short periods of time (e.g., a year) is expected to reflect departures of current income from average lifetime resources. In other words, according to these theories, when current income falls below average expected lifetime income, saving decreases, and individuals and households may even borrow to finance consumption. When current income exceeds average expected lifetime resources, individuals and households save. In recent years, a small number of economists have proposed alternatives to the LCH and PIH, the so-called “buffer-stock” models of saving (e.g., Carroll, 1997; Carroll & Samwick, 1997; Deaton, 1991). These models emphasize a precautionary motive for saving, particularly for younger households and for households facing greater income uncertainty.

Psychological and Sociological Theories

Psychological and sociological theories of saving posit that the effects of external stimuli on economic behavior are conditioned by intervening variables such as motives, aspirations, and expectations (Katona, 1975; Olander & Seipel, 1970; Strumpel, 1972; 1975; Van Raaij, 1989). However, unlike neoclassical economic theories, these theories do not assume that preferences and aspirations are fixed. In fact, psychological and sociological theories of saving explicitly seek to explain saving-related preferences, aspirations, and expectations.

The most well-known economic psychologist, George Katona (1951; 1975), has noted that saving is a function of two sets of factors, ability to save and willingness to save. The emphasis on ability to save acknowledges that some individuals, because of limited economic resources or special consumption needs, find it more difficult to defer consumption than others. At the same time, those individuals who can postpone consumption still must *choose* to do so, a decision that requires some degree of willpower. Katona claims that consumer sentiment (i.e., the evaluation and expectations people have regarding the economic circumstances of the nation and their own households) determines households' willingness to save. For example, households are expected to postpone consumption and save for future security if their perceptions of household finances, interest rates, unemployment, inflation, and so forth are pessimistic.. Other psychological and sociological propositions consider the effects of families (Cohen, 1994), peers (Duesenberry, 1949), and past savings experiences (Furnham, 1985; Katona, 1975) on consumption patterns, saving-related beliefs, and aspirations for saving.

Behavioral Theories

In addition to economic, psychological, and sociological theories, there are a few behavioral theories of saving. These theories note that individuals have trouble resisting temptations to spend, even when they want to save, and may therefore create their own behavioral incentives and constraints (Maital & Maital, 1994; Shefrin & Thaler, 1988; Thaler & Shefrin, 1981). For example, individuals may open Christmas saving accounts, arrange for over-withholding of income taxes, or adopt “rules-of-thumb,” such as avoiding borrowing or restricting borrowing to specific purchases. With these strategies in mind, behavioral theories view household saving as “the result of the successful and sophisticated imposition of welfare-improving, self-imposed constraints on spending” (Maital & Maital, 1994, p. 7).

Institutional Theories

Finally, Sherraden (1991) has proposed a theory of welfare based on assets which emphasizes the role of institutions (i.e., formal and informal socioeconomic relationships, rules, and incentives) in asset accumulation. This perspective is part of a larger body of institutional theory emphasizing that societal institutions shape, and give meaning to,

individual behavior (see, e.g., Gordon, 1980; Neale, 1987). According to Sherraden, “asset accumulations are primarily the result of institutionalized mechanisms involving explicit connections, rules, incentives, and subsidies” (p. 116). He emphasizes the subsidies provided through housing- and retirement-related tax benefits, including deductions for home mortgage interest and property taxes, deferment and exclusion of capital gains on sales of principal residences, exclusions for employment-sponsored pension contributions and earnings, deferments for Individual Retirement Accounts and Keogh Plans, and employer contributions to employee pension plans. Because these mechanisms for asset accumulation are subsidized or receive preferential tax treatment, Sherraden claims that it is rational for individuals who have access to these institutions to accumulate assets:

...institutionalized arrangements provide tremendous access and incentives to accumulate assets. People participate in retirement pension systems because it is easy and attractive to do so. This is not a matter of making superior choices. Instead, a priori choices are made by social policy, and individuals walk into the pattern that has been established. (p. 127)

More recently, Beverly and Sherraden (1999) have identified four major categories of institutional variables that are expected to shape saving and asset accumulation: (1) incentives, (2) information, (3) access, and (4) facilitation. These authors have also documented differences in access to these institutions. For example, low-income individuals have less access to attractive saving incentives, and those receiving means-tested welfare benefits often face saving *disincentives* in the form of asset restrictions. Members of low-income households are less likely to have access to financial education (see, e.g., Bernheim & Garrett, 1996) and to mechanisms that facilitate saving, such as payroll deduction and mortgage-financed home purchases. Beverly and Sherraden (see also Beverly, 1997) argue that limited access to institutions that promote and facilitate saving may help explain the low saving rates and limited asset accumulation of low-income households.

Existing Theories and Saving in Low-Income Households

In their current stages of development, none of the existing theories provides a suitable explanation for saving and asset accumulation in low-income households (Beverly & Sherraden, 1999). The mainstream economic theories make unrealistic assumptions regarding the knowledge, foresight, and self-control of individuals. They also assume that individuals have incomes during their later working years that exceed their consumption needs (enabling them to pay off debts and save for retirement) and have savings which can act as a cushion—or access to credit—when current income is low. In reality, imperfect credit markets and uncertainties regarding future income may prevent households from borrowing against future income, and many low-income individuals may never have earnings that substantially exceed their consumption needs. Perhaps most importantly, empirical evidence indicates that most low-income households have very low saving rates and very limited asset accumulation. The fact that these patterns are observed even among households

nearing the age of retirement challenges conventional life cycle models and the more recent buffer-stock models of asset accumulation.

Psychological, sociological, and behavioral propositions may complement economic theories of saving and asset accumulation by explicitly considering the role of social, cultural, and personal norms, motives, aspirations, financial management strategies, precommitment constraints, and so forth. Again, however, few of these propositions explicitly attempt to explain the saving or asset accumulation of low-income households. In addition, few have been subjected to rigorous empirical tests. The only theory that explicitly attempts to explain patterns of low-income saving is the institutional theory. Although existing evidence suggests that institutionalized asset accumulation is more substantial than discretionary saving (Bernheim & Shoven, 1988; Bosworth, Burtless, & Sabelhaus, 1991; Kotlikoff, Spivak, & Summers, 1982; Shefrin & Thaler, 1988; Thaler, 1990), scholars are still working to develop and test a bona fide institutional theory of saving.

Assets and the Well-Being of Children and Families

There is a growing body of literature addressing the relationship between assets and well-being. Much, but not all, of this research focuses on homeownership and its association with various measures of individual and social well-being. The research emphasis on assets in the form of homeownership has been noted in earlier reviews of studies on assets and well-being by Page-Adams and Sherraden (1997), Boshara, Scanlon, and Page-Adams (1998), and Page-Adams, Scanlon, and Moore (1999). Further, Scanlon (1998) offers a thorough summary of research on homeownership and community well-being.

The research reviewed here includes homeownership studies as well as research using other asset measures. This review focuses on the well-being of children and families. Studies are briefly summarized and tables detailing research on (1) assets and children's well-being (2) assets and marital stability (3) assets and health and (4) assets and economic security are provided.

Assets and Children

A number of studies suggest that assets have a positive effect on child welfare. (See Exhibit 2-1.) First and foremost, there appear to be important educational benefits associated with parental homeownership. Green and White (1997), in an analysis of four large, national data sets, find that controlling for parental education and income, 17 and 18 year old children of homeowners are less likely than the children of renters to drop out of school. For girls, parental homeownership was also associated with avoiding early child bearing. The effects of homeownership on staying in school and avoiding early child bearing were particularly strong for low-income children.

Other studies on assets and educational outcomes for children include one by Mayer (1997) who reports that investment income and inherited wealth predict educational test scores and educational attainment better than income. Similarly, an evaluation of Panel Study of Income Dynamics data demonstrates that income from assets, which can be taken as a proxy for asset holding, positively impacts children's educational attainment (Hill & Duncan, 1987). Assets had a particularly notable effect on the educational attainment of girls. The findings from these US studies are consistent with those of an earlier study in England suggesting a positive, independent effect of homeownership on children's educational attainment (Essen, Fogelman & Head, 1977). Scanlon's (1997) work suggests that the residential stability associated with homeownership may provide a partial explanation for the relationship between housing tenure and children's educational outcomes (Scanlon, 1997).

Another way that asset holding may be associated with child welfare is through its effects on the well-being of parents. Homeowners appear to have higher levels of life satisfaction (Rossi & Weber, 1996; Rohe & Stegman, 1994; Potter & Coshall, 1987), physical and emotional well-being (Page-Adams & Vosler, 1997; Vitt, 1994; Pugh, et al., 1991; Rodgers, 1991; Greene & Ondrich, 1990) and future orientation and self-efficacy (Clark, 1997). It would seem likely that children benefit from living in homes with parents who are healthier and more satisfied with their lives.

Other studies addressing the possible intergenerational effects of assets suggest that, controlling for education and socio-economic status, parental assets predict the likelihood that adult daughters who are single parents will be able to maintain their families above the poverty level (Cheng, 1995). A study of factors associated with teen-agers' savings and consumption patterns reveals that parental savings, particularly for college, is predictive of teen savings behavior (Pritchard, Myers & Cassidy, 1989). Henretta (1984) finds that parental homeownership is predictive of adult children's likelihood to own homes, even controlling for income and parental gifts.

Assets and Families

Some of the research on assets and family well-being addresses homeownership, while other studies focus on assets in the form of savings, net worth, or small business ownership. In general, financial and property assets appear to have effects on: (1) marital stability (2) health and (3) economic security. The following section reviews research on these three outcomes for families and households.

Marital stability. Married couples with assets appear to be less likely to divorce than couples without assets. Galligan and Bahr (1978) find that assets, rather than income, have significant effects on marital dissolution among a representative sample of married women in the US. In this study, the effect of net worth on marital stability is strong even when controlling for income, race/ethnicity, and education. Galligan and Bahr's findings are

consistent with earlier theoretical and empirical work by Cutright (1971), Cherlin (1977), and Ross and Sawhill (1975) on the significance of assets in explaining marital stability. In a more recent study using PSID data from a sample of 575 married couples, Hampton (1982) finds that property and financial assets are negatively associated with marital disruption for African American couples.

Bracher and his colleagues (1993) find that buying and owning a home outright reduce the risk of marital dissolution in Australia. The effect of homeownership on marital stability is significant even when controlling for the effects of a number of other social and economic factors. The researchers note that homeownership may increase stability by increasing the rewards within marriage or by creating financial or emotional disincentives to divorce. Alternately, couples who are experiencing marital distress may avoid making a joint investment in a home. If this is the case, homeownership may simply demonstrate that marital stability already exists.

A similar caution in interpretation is noted by Page-Adams (1995) whose findings suggest that homeownership has an effect on marital stability through its negative association with conflict and violence between spouses. It may be that homeownership makes couples reticent to put their marriages, and their marital homes, at risk by arguing and using violence. Alternately, serious marital conflict and physical violence may preclude homeownership for many couples. Further, Rossi and Weber (1996) find slightly higher levels of marital disagreements among homeowners, speculating that homeowner couples may have additional stresses associated with home upkeep and repair.

In any case, a negative relationship between assets and marital violence has also been found in a random sample study of married women in the U.S. (Petersen, 1980) and in a control group study of rural married women in a developing country (Schuler & Hashemi, 1994). The latter follows Levinson's (1989) conclusion from a study of ethnographic data that wealth and property ownership patterns in marriage are causally related to domestic violence. Given the strong association between domestic violence and marital dissolution in the U.S., such a relationship between assets and violence would have important implications for marital stability in this country. To summarize, assets may increase marital stability by reducing divorce and by decreasing domestic violence. Exhibit 2-2 provides an overview of studies on assets and marital stability.

Family health. As summarized in Exhibit 2-3, studies from both the U.S. and from Europe indicate a positive relationship between asset holding and physical health. In a review of health research, Joshi and Macran (1991) note that assets are related to lower mortality and that these effects are partially independent of other socio-economic resources. This is consistent with findings from the Office of Population Censuses and Surveys' Longitudinal Study in England showing positive, independent effects of assets on men's and women's physical health (Goldblatt, 1990; Moser, Pugh & Goldblatt, 1990).

Some studies in this literature point to homeownership as a particularly strong socioeconomic measure in health research. For example, Baker and Taylor (1997) find that, of seven measures of socioeconomic status, homeownership is the most consistently related to health among mothers of infants in England. Homeownership is significantly related, sometimes positively and sometimes negatively, to five of the six common ailments studied. The finding of some negative relationships between assets and health parallels that of Johnston, Grufferman, Bourguet, Delzell, Delong and Cohen (1985) who find that, of seven socioeconomic status measures, only homeownership is significantly associated with multiple myeloma and the association is positive.

However, most of the research reviewed not only points to the strength of homeownership as a health related socioeconomic measure, but also shows a positive relationship between homeownership and health. For example, a study in the Netherlands controls for occupation, education, and employment status and finds that male homeowners report fewer chronic conditions and better general health and that female homeowners perceive themselves to be in better general health than those without homes (Stronks, van de Mheen, van den Bos & Mackenbach, 1997). Hahn (1993) finds that, controlling for income and education, homeownership is modestly but significantly associated with women's health in the US. Further, homeownership helps to explain the generally positive relationship between marriage and physical health for women.

In research from England, asset holding is a better predictor of lung cancer mortality for married women than occupational measures of socio-economic status (Pugh, Power, Goldblatt & Arber, 1991). For example, married women living in owner occupied housing with access to a car are two and a half times less likely to die from lung cancer as those living in rented housing without access to a car. Pugh and her colleagues also find that there are substantial differences in the percentage of women who smoke based on occupational status, but much larger differences based on home ownership. Fifty-seven percent of women who rent are smokers compared with 31 percent of women who own homes. Turning to smoking uptake and cessation, Pugh and her colleagues (1991, pp. 1106-1107) find that "... among women in rented accommodation the rate of uptake was 23 percent while the cessation rate was 12 percent; among owner occupiers these percentages were reversed (12 and 24 percent respectively)." These findings are consistent with research by Yadama and Sherraden (1996) showing that assets in the form of savings have a positive effect on prudence as measured, in part, by smoking habits.

Turning to research on older family members, Robert and House (1996) find that financial assets have positive health effects on U.S. adults when controlling for the effects of income and education. Although assets and health are always positively related, the effects of assets on health are particularly strong for older adults between the ages of 65 and 84. In a study of relatively frail older adults, Greene and Ondrich (1990) control for income and education and

find that homeownership is negatively associated with nursing home admission and positively associated with successful nursing home exit back to the community. In this study, neither income nor education significantly affect the likelihood of either nursing home admission or discharge when controlling for the effects of homeownership.

Although this review has focused on research from the U.S. and Europe, findings of positive asset effects on health are consistent with results of studies from developing countries linking assets to increased childhood immunization (Amin & Li, 1997), improved nutritional status of women and children (Quanine, 1989) and decreased infant and child mortality (Amin & Li, 1997; Lee & Amin, 1981). Further, findings of asset effects on physical health parallel those from studies demonstrating relationships between assets and positive mental health outcomes for family members including reduced stress (Berger, Powell & Cook, 1988), increased life satisfaction (Potter & Coshall, 1987; Rohe & Stegman, 1994; Rossi & Weber, 1996), and reduced neurosis (Rodgers, 1991).

Economic security. In an earlier review, Page-Adams and Sherraden (1997) noted that assets appear to increase the economic security of families on public assistance (Raheim & Alter, 1995), female-headed families (Cheng, 1995), as well as other families in the U.S. and in other countries (Krumm & Kelly, 1989; Massey & Basem, 1992; Sherraden, Nair, Vasoo, Liang & Sherraden, 1995). Exhibit 2-4 provides an overview of additional studies on assets and economic security for families in the U.S.

Three of the studies in this review that address family economic security use homeownership as the measure of assets. While Rossi and Weber (1996) find limited differences between homeowners and renters, one important difference between the two groups has to do with asset holding. Controlling for age and socioeconomic status, homeowners have about \$6,000 more in savings and about \$5,000 more invested in mutual funds than renters. Homeowners are more likely to carry debt on credit cards, installment purchases, and personal bank loans, but less likely to have unpaid educational loans and overdue bills than renters.

Other studies addressing homeownership also control for a number of social and economic factors and find that homeownership reduces the length of joblessness for unemployed workers by some 11.6 weeks (Goss & Phillips, 1997) and increases high school graduation and college entry rates for African American youths (Kane, 1994). Kane's findings are consistent with those of Green and White (1997) who find that children of homeowners are less likely to drop out of school or to have children before the age of 18 than children of renters.

In studies using asset measures other than homeownership, wealth is positively associated with financial transfers to both adult children and parents in their older years (McGarry & Schoeni, 1995) and with the ability of single mothers to maintain their families above the federal poverty level (Rocha, 1997). Rocha controls for age, education, number of weeks

worked during the past year, and a number of other socioeconomic factors and finds that single mothers with money in a savings account are significantly more likely to have incomes above the poverty line than those without savings. Neither homeownership nor child support payments were strongly associated with living above the poverty level for female-headed families in this study.

Although this review has focused on research from the U.S., findings of positive asset effects on family economic security are consistent with results of studies from developing countries, especially those linking the mother's assets to enhanced material conditions of families (Quanine, 1989; Noponen, 1992; Schuler & Hashemi, 1994).

To summarize, assets appear to have positive effects on several measures of children and family well-being. Much more research is needed, especially on the effects of financial assets such as savings on well-being. Future research focusing on the relationships between assets and (1) children's educational attainment (2) domestic violence and (3) poverty among families that are maintained by women appears as though it will be particularly promising.

Do IDAs Work?

Can the poor save in a program of IDAs? This policy-analytic question defines the critical policy test of IDAs and other asset-building strategies. Given an IDA program design, do IDA participants save and accumulate assets more than they would otherwise have done? And do they use these assets for homeownership, education, and business start-up (or other allowed uses) more than they would otherwise have done? Only limited evidence exists on IDA evaluations at this time (Sherraden et al. 1999; Lazear, 1999). Other studies of the use of financial institutions by the poor will also be useful (e.g., Caskey, 1994). As evaluation studies continue to be available for the American Dream Demonstration and other IDA initiatives, these will be included.

Exhibit 2-1: Assets and Children's Well-Being

Study	Description	Findings
Green & White (1997)	Examines four large data sets-(Panel Study of Income Dynamics, High School and Beyond Survey, 1980 Census, and the National Bureau of Economic Research-Boston Youth) to determine whether homeownership affects outcomes for 17 and 18 year olds.	Finds that children of homeowners are less likely to drop out or to have children than children from renter households.
Henretta (1984)	Examines Panel Study of Income Dynamics (PSID) to determine whether children of homeowners are more likely than children of renters to become homeowners.	Reports that children of homeowners are more likely to become homeowners, controlling for parental income and gifts.
Essen, Fogelman & Head (1977)	Study of 16,000 British youth to determine whether housing tenure impacts educational outcomes of children between 11 and 16 years.	Finds that 16 year old children of homeowners are significantly more likely to have higher math and reading scores than other children.
Mayer (1997)	Study of two large national data sets (PSID and the National Longitudinal Survey of Youth or NLSY) determine the relative impact of factors other than income on well-being outcomes of parents and children.	Finds that investment income and inherited income explain more variance in educational attainment and outcomes than did income measures.
Cheng (1995)	Studies effects of parental socioeconomic status (SES), education and asset holding on poverty among adult children with daughters. Examines 836 female heads of household using National Survey of Families and Households (NSFH) data.	Controlling for SES and education, assets have a negative relationship to likelihood of adult daughters living in poverty.
Pritchard, Myers & Cassidy (1989)	Study of 1,619 teens and parents in the 1982 cohort of the High School and Beyond Survey to determine the impact of family factors on saving and spending patterns of teens.	Finds that parental savings, particularly for college, predicted teens savings patterns.
Hill & Duncan (1987)	Study of 845 PSID cases to test effects of asset income on children's educational attainment, controlling for other factors.	Finds parental income from assets impacts education but not wages of adult children.

Exhibit 2-2: Assets and Marital Stability

Study	Description	Findings
Bracher, Santow, Morgan & Trussell (1993)	Examines marriage dissolution using data from a representative sample of 2,547 Australian women aged 20 to 59 years.	Controlling for a number of other social and economic factors, homeownership reduces the risk of marital dissolution.
Galligan & Bahr (1978)	Longitudinal study of marital stability among 1,349 married U.S. women using data from the National Longitudinal Survey of Labor and Market Experience.	Income has little effect on marital stability, but assets as measured on the basis of net worth have a substantial effect even when controlling for income, race and education.
Hampton (1982)	Study of marital disruption among African Americans using PSID data with a sample of 575 married couples in the U.S.	Controlling for income, property and financial assets have a significant negative effect on marital disruption.
Page-Adams (1995)	Examines domestic violence using data from 2,827 married women and their husbands who responded to the National Survey of Families and Households.	Homeownership has significant negative effects on marital conflict and on domestic violence controlling for income and women's independent economic resources.
Petersen (1980)	Exploration of several measures of socio-economic status and wife abuse among a random sample of 602 married women.	Homeownership has a stronger negative relationship with wife abuse than other SES measures including income and education.

Exhibit 2-3: Assets and Family Health

Study	Description	Findings
Baker & Taylor (1997)	Examines socioeconomic status and health among 11,040 mothers of infants in southwest England.	Of seven measures of socioeconomic status, homeownership had the strongest and most consistent relationship to health.
Greene & Ondrich (1990)	Study of nursing home admissions and exits among 3,332 frail older adults in the U.S. who were enrolled in The National Long Term Care Channeling Demonstration.	Homeownership, but not income or education, is negatively associated with nursing home admission and positively associated with nursing home discharge.
Hahn (1993)	Using National Medical Expenditure Survey data from 9,356 U.S. women, this study examines relationships between marriage, assets and women's health.	Controlling for income and education, homeownership has a positive effect on women's health and helps explain the relationship between marriage and health.
Pugh, Power, Goldblatt & Arber (1991)	Study of SES and lung cancer mortality among 10,212 married women in England using data from the Office of Population Censuses and Surveys' Longitudinal Study.	Assets explain lung cancer mortality better than other SES measures. Women with assets are 2.5 times less likely than those without assets to die from lung cancer.
Robert & House (1996)	Explores health effects of assets using data from 3,617 U.S. adult participants in the Americans' Changing Lives Study.	Controlling for income and education, assets have positive effects on health especially for adults ages 65 to 84.
Stronks, van de Mheen, van den Bos & Mackenbach (1997)	Examines relationships between various socioeconomic measures and health among 13,391 men and women in the Netherlands who participated in the Longitudinal Study on Socio-Economic Health Differences.	Controlling for the effects of occupation, education, and employment status, an SES measure that includes homeownership is positively related to health (fewer chronic conditions and better perceived health).

Exhibit 2-4: Assets and Economic Security

Study	Description	Findings
Goss & Phillips (1997)	Using a sample of 1,134 unemployment workers from the PSID, the authors examine the effect of homeownership on the duration of unemployment.	Homeownership reduces the duration of unemployment, controlling for education, occupation, race, gender, home equity, and many other social and economic variables.
Kane (1994)	Examines the role of family background, college costs, and local economic conditions on college entry using Current Population Survey data for 18 and 19 year old African American youths.	Homeownership is significantly and positively associated with high school graduation and with college entry for African Americans, controlling for other resources.
McGarry & Schoeni (1995)	Using data from the PSID and the Health and Retirement Study, the authors examined intergenerational transfers.	Controlling for a number of social and economic factors, wealth is significantly associated with financial gifts to both adult children and to parents in their older years.
Rocha (1997)	Study of economic well-being among 670 female-headed households using data from the National Survey of Families and Households (NSFH).	Single mothers with savings are significantly more likely to maintain their families above the federal poverty level than other single mothers, controlling for many social and economic factors.
Rossi & Weber (1996)	Using data from the General Social Survey and the NSFH, this study explores the social and economic benefits of homeownership.	Controlling for age and other measures of socioeconomic status, homeowners have about \$1,000 more in financial assets and more debt than renters.

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Section 3: Process Analysis

The process analysis will provide a comprehensive picture of the development, planning, start-up, and on-going operations of AFIA programs. It will help HHS staff understand how the programs work and the factors influencing effective operations. In describing how clients interact with program staff and receive program services, the process analysis also will help us interpret the findings of the impact analysis (to be described in Section 6).

Rationale

The contribution of the process analysis will be:

- X to describe the goals of the AFIA legislation and the program features prescribed by the Act.
- X to assess the implementation of AFIA-funded programs among grantees, as viewed in relation to the provisions of both the statute and grantee applications;
- X to compare the experiences of different grantees in developing and implementing their IDA programs; and
- X to help interpret the findings of the impact analysis by providing programmatic context.

In short, the process analysis can both explain and expand upon the impact analysis. More specifically, the process analysis should speak to several of the “factors to evaluate” indicated in the Act. As noted previously in Exhibit 1-1, the process analysis will provide insight regarding:

- X the effects of incentives and organizational or institutional support on savings behavior—It will do so particularly at the experimental site, where process-related findings will provide the programmatic context for interpreting the impact findings.
- X the effects of IDAs on savings rates, homeownership, level of post-secondary education attained, and self-employment, and how such effects vary among different populations or communities—Again, the findings of the process analysis would be particularly insightful at the experimental site.
- X the potential financial returns to the Federal Government and to other public and private sector investors in IDAs over a 5-year and 10-year period of time; and

- X the lessons learned from the demonstration projects, and whether a permanent program of IDAs should be established.

We view the process and impact analyses as closely complementary. The impact analysis will provide estimates of the effects of IDA incentives. Although the impact analysis can indicate *whether* IDAs affect participant savings and asset accumulation, it is of limited use to explain *why* and *how* those effects accrue. The process analysis will ***indicate the dynamics of program-client interactions and suggest the mechanics by which behavioral changes occur***. It is therefore crucial that the process analysis be conducted at the experimental site, among others.

The value of the process analysis goes beyond it tells us about the dynamics of change at any *one* site. It can also ***illustrate the variety of program models that evolve under AFIA***. Existing research into IDA programs--for example, from Abt Associates' study of asset accumulation initiatives (sponsored by the USDA) and CSD's evaluation of the American Dream Demonstration--indicates that current IDA program models are quite diverse. For example, programs may vary significantly in the strictness with which staff monitor and enforce the requirements of program participation (e.g., minimum deposit amounts, frequency of deposits, attendance at counseling and training sessions). Programs may also vary philosophically in their approaches to the use of funds for family emergencies. Some programs adopt a laissez faire approach to emergency withdrawals, on the premise that the accountholder should have ultimate discretion over the use of funds. Others adopt a more interventionist approach, seeking to help the accountholder avoid an emergency withdrawal if at all possible. To the extent that programs vary on these and other important features, it is important to ask whether these differences appear to influence participant outcomes. Of course, without an experimental design in each site, we cannot definitively attribute causality to the program. What we observe in the process analysis, however, can serve to narrow and sharpen our focus on those aspects of the program that appear to offer the most plausible explanation of effects.

Second, a process analysis that traces the development of an IDA program over time, can ***provide valuable lessons for other programs***. It may identify issues that were found problematic across all sites or only under certain conditions. For example, establishing relationships with financial institutions, or devising procedures for efficient verification of account use, may prove to be more difficult than sites anticipated. The lessons learned about how sites overcame these challenges (or the implications of *not* overcoming them) would be extremely useful to both current and future sites and may have policy implications, to the extent some policy elements appear to promote or impede success.

Approach

Perhaps the greatest challenge of the process analysis will be to make sense of the great variety of program features observed. We seek to avoid a research design that produces

cacophony of descriptive findings, but stops short of deriving meaningful, salient, generalizable insights from them.

Accordingly, it is useful to think of the process analysis as composed of two complementary activities: a *description* of the program models that emerge under AFIA and an *analysis* of what these program models imply about program outcomes, replicability, and long-term sustainability.

A variety of data sources, collection techniques, and analytic approaches will be needed to meet the multiple objectives of the process analysis. The following data collection activities will be necessary to support the process analysis:

- X a program survey to all grantees;
- X multiple site visits to selected programs;
- X a treatment-group module in the follow-up surveys planned primarily for the impact analysis.

These three data collection efforts are described below.

Program Survey

The descriptive task can be effectively met by the use of a brief program survey sent to each year's cohort of grantees. The survey will supplement findings from the site visits, which will necessarily be restricted to a small group of grantees. Our experience is that short program surveys can be very effective to capture straightforward descriptive information about program features. Having this information available for the universe of grantees can be useful in several ways. First, it can identify the degree to which IDA programs are implemented consistently with what was intended (as articulated in grantees' applications). Second, it can place the program features identified in the process analysis in a broader context. (For example, how common is a particular program feature that was observed during visits to several sites?) Third, it can identify any differences in cohorts over time. It may be expected that, as each successive cohort's experiences become known, IDA programs "mature" over time. That is, newer sites will take into account their predecessors' successes and challenges. Cohort differences may also occur if certain AFIA requirements, over time, tend to encourage or discourage certain types of program models.

The program survey could collect the following types of information:

- X funding levels (federal, state, and local);
 - X eligibility requirements;
 - X program requirements (minimum deposits, counseling requirements, etc.);
 - X number of account holders currently anticipated;
 - X length of time the program has been operational;
 - X financial institutions involved in the program; and

X terms of the savings accounts.

Our experience is that program surveys can be most effective when the information sought is straightforward and descriptive. Such surveys, however, have limited ability to indicate the dynamics of program operations. Accordingly, the process analysis would certainly require site visits to a limited number of sites. Important considerations with regard to the site visits are described below.

Site Visits

Site visits to selected AFIA programs are a major component of the process analysis. These visits will allow us to view first-hand how programs operate. The visits will also allow us to obtain a comprehensive description of each program, including its planning phase, organizational aspects, service provisions, and program operations. In addition, we will document key contextual factors, such as the social, economic, and political aspects that may have a bearing on program performance.

The programs to be visited will need to be selected carefully. For obvious reasons, the experimental site must be included. Other programs selected should encompass to some meaningful extent the variation in program features and operational settings found among funded sites. We recommend developing a *typology of salient program features* that are hypothesized to affect program “success” (however defined). These parameters might include, for example:

- X program age;
- X organization size;
- X urban/rural location; and
- X program cohort (Year 1 grantee, Year 2 grantee, etc.)

Site selection for the process analysis might even include one or several of these features as “stratification factors.” These factors can be collected in the program survey and validated in the course of the three site visits conducted during the research design phase.

Even if not used for site selection per se, the typology forms the conceptual framework that should be used to guide the research. Site visits should be conducted with that typological framework in mind, while still remaining open to the importance of unexpected factors. This can be done by developing research protocols that probe certain pre-identified program features very carefully, while also including open-ended queries that allow for unanticipated issues.

We recommend multiple site visits to capture the evolution of programs over time. Analyzing the trajectory of programs’ development over time can shed light on the common challenges and successes, and can suggest areas in which technical assistance might be targeted. The evolution of the early sites in particular can provide important lessons for subsequent AFIA applicants trying to determine how to configure their program.

Specific topics that might be examined during site visits include:

- X local context (community, economic, social, and physical features);
- X “closeness of fit” of IDA goals with organization’s overall mission;
- X resources available (material resources as well as expertise and links with other groups);
- X eligibility criteria;
- X degree of monitoring/verification;
- X financial counseling;
- X relationships with financial institutions;
- X magnitude/type of other funding and any associated constraints;
- X program strengths and weaknesses.

Treatment-Group Module in the Follow-up Surveys

A special module for treatment-group respondents will be included in the follow-up surveys used for the impact analysis. The module will contain a series of questions related to the services that participants receive through the program. The module will help us understand the services provided by the program from the participants’ point of view. More intensive and selective input from participants will be provided from the in-depth participant interviews, as described in detail in Section 4. The combination of the treatment-group module in the follow-up surveys and the in-depth participant interviews are the vehicles used to collect the participants’ perspectives on the program.

Some topics that could be covered in the treatment-group module include the following:

- X how did they hear about the program?
- X what motivated them to apply?
- X what was involved in enrolling in the program?
- X what, in their minds, are the objectives of the program?
- X how frequently did they attend counseling or training sessions?
- X what types of counseling did they receive?
- X what were the burdens of participating in the program, in time and expense?
- X how easy/difficult was it to meet the requirements of the program?

In brief, the process analysis described in this section will contribute importantly to the evaluation in three ways. The first is to describe and document the process of planning, implementing, and operating IDA programs. The second is to evaluate the extent to which the programs were implemented as planned. The third is to provide useful information for interpreting the impact findings.

Section 4: In-Depth Interviews with Participants

In-depth interviews are semi-structured or “focused” informal conversations with a trained interviewer (Merton et al. 1990, Rubin & Rubin 1995). In-depth interviews are a principal research tool for social scientists to learn how people respond to complicated and often understudied issues. This method utilizes guided, but open-ended interviews, that reflect on events and ways that respondents understand their world and how and why they do certain things. In these interviews, participants are “conversation partners,” not respondents as they are in survey interviews (Rubin & Rubin, 1995).

In-depth interviews utilize a grounded theory approach emphasizing the role of human actors in shaping and giving meaning to the world around them, stressing the interrelationships among conditions, meaning, and action (Strauss & Corbin 1990). Initially outlined by Glaser and Strauss (1967), grounded theory was extended and further specified by many other writers such as Lincoln and Guba (1985) and Strauss and Corbin (1990). In Strauss and Corbin’s words “the grounded theory approach is a qualitative research method that uses a systematic set of procedures to develop an inductively-derived grounded theory about a phenomenon” (1990, p. 24). In order to produce new understandings, grounded theorists interview respondents, listening carefully and systematically before developing concepts and hypotheses. Although grounded theorists enter the field with as few “filters” as possible, this does not suggest that the social researcher begins with a “tabula rasa.” On the contrary, the qualitative social researcher begins with existing theory and examines results within and beyond preconceived constructs.

Interviews

Interview topics and open-ended questions will be carefully derived from the study questions. Questions will be constructed in such a way as to provide direction to respondents, but not to restrict responses. Each question will have several open-ended probes that may be used to encourage further discussion of the topic. Despite the explicit design of the instrument, the interview itself will be informal and relaxed. The interview topics will be memorized to facilitate the conversation flow. It is important that respondents feel they are in control: free to talk about topics in the order they prefer, and comfortable bringing up other issues (Sherraden & Barrera 1995). Typically, interviews will be conducted in respondents’ homes in one session, totaling two to three hours. Occasionally a second interview may be needed. Interviews will follow a chronological sequence, emphasizing topics that shed light on attitudes toward savings and savings behavior. Some closed-ended questions may be asked at the end of the interview in order to confirm very specific content, such as demographics, and information on family income and expenses. Any pretest interviews will be undertaken in a manner consistent with the approved protocol of the Washington University Human Research Committee, which serves as the university’s institutional review board (IRB).

In-depth interviews will be designed to explore the IDA holder's own descriptions, definitions, and understanding of saving, the IDA program, and their experiences (Sherraden, 1995). Included in the interview will be social psychological concepts such as self, socialization, identity, reference group, and attitudes. Moreover, the interview will include macro-sociological concepts such social network and institutional support and access to resources. These complex phenomena are tied to past experiences and beliefs about both the past and the future and this form of interviewing is designed to capture these webs of meaning (Stack & Burton 1993). Interviews will help identify constellations of factors that together define "qualitative differences" in attitudes and behaviors influencing savings among IDA holders.

Interviewers conducting in-depth interviews have refined listening skills, are open to hearing things that challenge assumptions (both practical and theoretical), and are able to help respondents articulate their lived experiences. Interviewers focus on hearing the particular meaning, interpretation, and understanding presented by the participant. This includes developing a shared understanding of words (e.g., family, savings) and phrases so that the interview can be interpreted correctly. For example, is the respondent talking about savings as "putting money aside," or "saving for a rainy day," or "saving for the future"? Most studies begin by assuming that the concept of saving is well understood and that the challenge is to accurately measure its effects. However, little is really known about how the poor save, the meanings of saving to the poor, and the various experiences of saving among the poor. A grounded theory approach using in-depth interviews will begin to answer these questions.

Qualitative interviews are designed to build trust and collaboration between the interviewer and participant, thus encouraging honesty and forthright discussion. At the same time, the interview has enough structure so that the respondent fairly quickly understands the purpose. As a result, in-depth interviews are successful in helping participants discuss sensitive topics, such as financial pressures and family issues that might affect saving. In-depth interviews also allow the interviewer to help participants clarify recollections. Apparent contradictions, for example, can be gently probed, permitting the respondent to clarify a narrative. Clarification is particularly important in the case of low-income savers, whose experiences and perspectives have been little studied (Beverly & Sherraden, 1999).

In-depth interview content

The purpose of the IDA in-depth interviews is to learn about the circumstances that brought participants into an IDA program, their personal experiences with saving, how they view the successes and failures of the program, and the effects of IDAs. The interviews explore details of how participants manage their IDAs in the context of their everyday lives. In-depth interviews are aimed at understanding the following relationships from the respondents' point of view:

- X the impact of earlier life experiences (e.g., family, education, neighborhood) on respondents' savings behavior;

- X the impact of organizational and institutional support on savings behavior;
- X the effects of matching savings on respondents' saving behavior; and
- X the economic, psychological, and social effects of savings on respondents.

Below, we sketch out the conceptual issues that will be covered in the interview.

Because it is easier for most people to follow a chronology (Merton et. al., 1990), the interview will start “in the beginning,” in childhood and move progressively through independence and young adulthood to the present. The second part of the interview will cover the IDA experience.

Respondents typically jump from one section to another, despite the overall chronological design. The interviewer is responsible for tracking what has been covered and what has not and for eventually steering the respondent back to cover all the topics.

The aim of the first part is to understand the impact of life experiences on savings behavior. Several areas of the participant's background will be covered, including basic demographics, family, education, housing, and neighborhood characteristics as well as history of saving. The focus is on the participant's understanding of financial decision making, consumption, and savings behavior. Specific topics include family history of savings, respondent's savings as a young person, and family attitudes about money and money matters.

Next, the interview will examine young adulthood and growing physical, emotional and financial independence from the family. This typically involves a discussion of education and work, as well as family formation. A wide variety of topics may emerge at this point, but a centerpiece of the discussion is financial transitions. These include exploring how and why the respondent began to make financial decisions to purchase small consumer purchases, larger consumption items, and longer-term investment decisions.

Finally, following the basic chronology, the interview continues with a discussion about the respondent's current family, including demographic background, socio-economic well-being, and financial decision making. This section will cover participant and family spending, budgeting, unplanned expenses, financial strain, and surplus.

The second part of the interview focuses on the respondent's experience in the IDA program. Again, following a chronological sequence, the interview begins with how the respondent learned about IDAs and the decision process that led to enrolling in the program. Following this, the interview explores savings patterns, sources of saving, and planned (or actual) uses of IDA savings. Another part explores the content of the IDA program, including economic literacy, personnel, administration, access to staff, access to accounts, links with other programs, and so forth. The focus is how the program facilitates or hinders saving in an IDA. Respondents also will evaluate how well the program operates overall, including perceptions about how the program is structured, the feasibility of goals, and the value of the program.

Finally, the participant is asked to reflect on the positive and negative effects of IDAs on their lives and the lives of their families and communities. Effects explored are economic personal (emotional and cognitive), familial, intergenerational, social, and civic. This evaluation includes discussion of how IDAs relate to participants' views of their short-term and long-term goals.

Section 5: Program and Participant Tracking and Monitoring

Tracking IDA programs, IDA participants, savings patterns, and uses of IDAs will yield basic information about success or failure of the IDA demonstration. According to Section 412 of the Assets for Independence Act (AFIA), reporting is separate and distinct from evaluation.

The purpose of reporting on IDA program progress and goal attainment is to answer the following questions:

- X How much do participants save?
- X What is the pattern of savings?
- X How does savings vary by program and participant characteristics?
- X For what purposes do participants use their savings?

In order to collect the basic information necessary to answer these questions, the Center for Social Development (CSD) spent several years developing and refining a tracking and reporting instrument. In 1996, a committee was formed to assist CSD in determining what data from IDA programs and participants should be tracked. Initially in paper form, the tracking tool was to become a comprehensive program management tool. With significant time and investment, the Management Information System for Individual Development Accounts (MIS IDA) was developed, revised, and finally released in 1998. MIS IDA was designed not only to assist IDA program staff in keeping track of program and participant progress but also to provide account statements to participants, reports to funding sources, data integration with financial institutions, and centralized data management in multi-site settings. A database from MIS IDA can then be sent to a central location and merged with other databases from other IDA programs for analysis.

Strengths of this reporting method include:

- X a standardized data collection method;
- X data comparison across a variety of settings; and
- X an integrated tool used for tracking and in daily program functioning.

Where possible, MIS IDA uses standard lists from which staff can select to describe program and participant characteristics. Although programs may differ somewhat in design and in participant population characteristics, MIS IDA provides a general framework for reporting. This framework allows data from various programs to be aggregated and compared.

The strategy in creating MIS IDA has been to make a management information system so practical that IDA programs will want to use it. Research shows that field personnel are more likely to use an “evaluative” tool when it assists them in their daily functioning (Austin,

et al., 1982; Clark, 1985; Freel & Epstein, 1993). The tension between reporting and daily program functioning is lessened when processes are fully integrated (Grasso and Epstein, 1993). The expansion of MIS IDA from a paper “tracking and reporting instrument” to a robust centralized information system increases the likelihood of its use and therefore its usefulness in data tracking. Version 2.02 of MIS IDA is the current standard, and is now being used in IDA programs around the country.

Data to be collected in MIS IDA and reported include: program characteristics, program costs, numbers of participants, characteristics of participants, longevity of participation, patterns of savings; amounts saved, and uses of IDAs (typically for home ownership, education, or business capitalization). Specific program information includes program design features, administrative costs, and funding partner contributions. Specific participant information includes: demographics, financial assets/liabilities, account information, IDA uses, participation in economic education, and program exit status. Analysis will generate information on savings patterns and how program and participant characteristics are related to savings. “Savings” may be measured in various ways, including:

- X total participant savings (participant closing balance + matched withdrawals);
- X total IDA savings (participant closing balance + matched withdrawals + match dollars);
- X average monthly (net) savings (total participant deposits + interest – unmatched dollars, averaged by the number of months the participant is in the program); and
- X deposit regularity (number of months of positive deposits divided by the number of months the participant is in the program).

There are several important considerations in using a reporting instrument such as MIS IDA. The first is that MIS IDA should be distributed to the field prior to program startup. A corollary is the need for training and technical assistance in getting the program properly installed and set up. Many programs already use MIS IDA but will require instruction in how to separate their current IDA database from the AFIA database. And some will have multi-site configurations that differ from single-site IDA programs. Second is data validity. MIS IDA is based on self-report. Program staff will be entering program design characteristics of their own program as well as data collected from participants. Training and technical assistance on the use of MIS IDA will increase understanding of the content of questions and will increase awareness of the importance of accuracy. Third is ensuring data quality. MIS IDA is coded with multiple data edit features, but data entry errors can and do occur. It is essential that data are reviewed for accuracy and “cleaned” prior to reporting. These issues point to the need for immediate technical assistance and training on the use of MIS IDA to enhance the level of accuracy in later reporting.

MIS IDA will require some revisions based on the implementation of AFIA. However, in its current form, it is usable for AFIA sites to begin the data collection process; in fact, many already have. As noted at the beginning of this section, reporting is clearly outlined in

Section 412 of the AFIA legislation and the evaluation is separately delineated in Section 414. Given this distinction and the immediate deployment of MIS IDA in the field, it is recommended that the implementation of tracking and reporting occur as soon as possible and under separate contract from the evaluation, which will not begin until after the year-long evaluation design phase.

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Section 6: Impact Analysis

This section describes the general proposed strategy for estimating program effects. We first introduce the statutory mandate for an experimental design. Subsequent materials address the research questions and outcome measures. We then discuss the challenges of experimental research and develop one possible option for estimating impacts through a nonexperimental approach.

Mandated experimental design

The Assets for Independence Act specifies that the research organization shall “for at least one site, use control groups to compare participants with nonparticipants.” In the experimental site(s), individuals will be randomly assigned to either a treatment group, which is allowed to participate in the program, or a control group, which is not. In addressing the research questions through an experimental design, Congress has properly sought to establish the strongest empirical foundation for drawing policy implications from the demonstration.

Experimental impact analyses are used to estimate the effects of a program as measured against the outcomes that would have happened in its absence. Measures of this sort provide the best indication possible of the effectiveness of a program in achieving its desired outcomes. For policy makers, the experimental evaluation provides the best policy counterfactual: a control group whose experiences can be interpreted as representing what would have happened to the treatment group in the absence of the demonstration. Any observed differences between the treatment and control groups can be attributed to the program.

Properly implemented, an experimental design through random assignment assures that the control group does not differ from the treatment group in any systematic way other than the receipt of program services. Thus, any subsequent differences in outcomes between the two groups that exceed the bounds of statistical fluctuation can be confidently attributed to the intervention. With any non-random comparison group, there is always a chance that differences in outcomes are the result of pre-existing differences between the two groups, rather than the program itself.

An experimental impact analysis will strive to answer the key research questions posed by the evaluation by collecting data from the research sample over a period of time, initially at baseline (i.e., immediately prior to random assignment) and then at one or more prescribed follow-up interval(s). Experimental impact studies typically consist of four elements: baseline data collection; random assignment of program applicants to treatment and control groups; follow-up data collection; and impact estimation.

Research questions

In general, the experimental component of the evaluation will seek to quantify program impacts, or the influence of IDA programs on participating individuals. As a result, many of the research questions concern the difference between participants' pre-program baseline status and their status after participating in an IDA program.

Most fundamentally, AFI programs—and IDA programs more generally—are intended to increase the savings rates and assets of program participants. The experimental research questions will address whether these effects occur, and whether they have longer-term implications for individual well-being. Three major categories of program effects have been identified from the “factors to evaluate” in the AFI legislation. These categories, described below, include effects on savings and asset accumulation, on employment and income, and on the personal well-being of IDA program recipients.

Effects on savings and asset accumulation

The most immediate, short-term impact of IDA program participation is expected to be an increase in the savings of the individual participant. The increased savings then enable the participant to acquire the types of assets favored by the program. These include financial assets, purchased assets, and human capital that reflect allowable uses of IDA funds. Key questions the experimental evaluation will address include:

- X What is the difference between baseline (pre-participation) savings account balances and post-participation savings account balances? What portion of IDA program participants' savings balances are their own deposits? What portion are matching funds? What portion (if any) reflect other public investments?

- X What is the difference between baseline (pre-participation) purchased assets and post-participation purchased assets?
 - X Do IDA program participants experience greater improvements in their education levels (including employment training) than members of the control group?
 - X Do IDA program participants purchase homes at a greater rate than control group members?
 - X Do IDA program participants start businesses at a greater rate than control group members?

- X Does IDA program participation affect individuals' debt holdings?

Effects on employment and income

It is hypothesized that a number of longer-term changes will occur as a result of IDA program participation. Some of these changes concern one's employment status and income

situation. The experimental evaluation will seek to monitor such conditions over time. These issues are addressed by a number of experimental research questions which, as above, will be answered through comparisons between the experiences of IDA program participants and a control group:

- X What is the effect of program participation on total income levels?
- X To what extent does IDA program participation influence levels and rates of employment?
- X What effect does program participation have on earned income? On self-employment, sole-proprietorship, or other microenterprise business income?
- X To what extent does IDA program participation influence dependency on public assistance programs (e.g., cash assistance, food stamps, Medicaid)? To what extent does any difference in public assistance usage reflect avoidance of receiving public assistance versus helping people to leave public assistance?

Effects on personal well-being

Finally, it is suggested that IDA program participation can result in a second type of long-term impact concerning participants' quality of life. By providing people with working assets, IDA programs are intended to result in increased feelings of self-efficacy, community involvement, future orientation, and other effects. These impacts may be somewhat less tangible than those cited above, but will nonetheless be addressed in the experimental evaluation. Key research questions will include whether IDA program participation influences any of the following outcomes:

- X Participants' feelings of self-efficacy?
- X Participants' future orientation, planning horizon, or timeline?
- X Participants' maintenance and utilization of their personal assets?
- X Participants' feelings of financial well-being or financial planning activities?

Measures

To answer these research questions, a variety of measures will be employed to reach a series of program impact estimates. These estimates will seek to quantify the effect of the program versus the 'counterfactual' -- what would have happened to the IDA participants in the absence of the IDA program. This will be accomplished by comparing the differential between the pre- and post-program experiences of program participants (the "treatment group") versus the same differential experienced by a similar group of people who do not receive IDA services (the "control group").

Importantly, all measures used to draw comparisons between program participants and nonparticipants require data sources that are equivalent for both groups. That is, the method of data collection must be the same for both groups to avoid the possibility of biases inherent in the collected data (“measurement bias”).

Two methods are available for collecting the required information and satisfying the requirement of identical sources for the treatment and control groups: 1) survey data (baseline and follow-up); 2) common administrative data (as reported by employers through State unemployment insurance systems). The majority of measures required are not available from existing public data sources, but are conducive to survey collection. As a result, surveys will be the primary source of data required for the experimental evaluation. Original survey instruments will be developed for this project based on its specific requirements.

To the extent that public data can be utilized, however, it may capture certain important measures well (such as earned income and unemployment). Public data sources have several benefits. First, they provide the potential to track individuals’ employment record longitudinally. Second, they may be a more reliable and consistent source of information, given that income-related questions can sometimes be difficult and/or uncomfortable for people to answer in a survey. Third, they would reduce the burdens of original data collection from the treatment and control groups.

A focused set of measures flows directly from the research questions posed for the study, as discussed in Section One. Key measures are presented below. Unless otherwise indicated, these measures will be collected using baseline and follow-up surveys:

Effects on savings and asset accumulation

- X Savings level at baseline and followup
- X Self-investment between baseline and follow-up
- X Matching funds received (treatment group only)
- X Funds from any other sources
- X Net savings increase: savings at follow-up, minus savings at baseline, plus self-investment between baseline and follow-up
- X Home ownership and improvement/maintenance
- X Business startup
- X Other assets and their value (e.g., vehicles, property, other accounts)
- X Own educational activity, including employment training
- X Debts, by type
- X Effects on employment and income
- X Employment status, tracked over time (public data source)
- X Earned income, tracked over time (public data source)
- X Hours worked per week and hourly wage
- X Other private (own) income

- X Public assistance use (cash assistance, food stamps, Medicaid)
- X Other income sources
- X Effects on personal well-being
- X Outlook (feelings of self-efficacy, regard for the future, expectations for children)
- X Financial well-being / avoidance of hardship
- X Activities to improve status (e.g., looked at home purchase or job change opportunities)
- X Financial planning activities (e.g., budgeting, goal-setting, encouraging children to save)

Challenges of an experimental design

Implementation of random assignment and an experimental impact analysis in the context of an ongoing program is not a simple task. It requires careful design and planning, in close consultation with program staff, to ensure that the approach taken is consistent with the overall design and institutional context of the program, both to minimize the intrusion of the evaluation on program activities and to ensure that randomization is not compromised by events in the field.

Along with the benefits of an experimental impact study come some drawbacks that need to be considered. Careful design of random assignment procedures and thorough training of program staff in those procedures will help ensure that the experimental design is implemented as intended. Nevertheless, it is essential to anticipate potential threats to the experimental design and to develop procedures that will minimize the likelihood of their occurrence.

The following is a list of issues or concerns that need to be considered in the design of any experimental impact study.

- X *Recruiting representative site(s) to participate in the study.* Selecting appropriate site(s) to participate in the experimental impacts study is always a challenge. Site(s) selected for the experimental impact study must have the capacity to recruit and serve large numbers of individuals and be willing to participate in the study. However, any site(s) large enough to enroll a sufficient research sample may not be representative of the other programs being funded. The findings may therefore have limited generalizability (specific to the particular program model at the experimental site). As a result, the most important task in site selection will be to establish a broad understanding of the programs funded to date under AFIA and thereby assess the appropriateness of particular programs to serve as evaluation sites.
- X *Ethical issues of denying services to control group members.* This is especially a concern among program staff. However, one must recognize that the programs

- funded under the Assets for Independence Act have limited funding; as a result, not everyone who wants to be enrolled in a program will necessarily be served, even in nonexperimental sites.
- X *Need to compensate sites for additional burden placed on them for participating in the experimental study.* Sites participating in the experimental study will need additional resources for outreach and recruitment of the research sample, as they are required to recruit twice as many eligible applicants as non-experimental sites. Staff from the research organization must be available to provide technical assistance to staff at the experimental site on various issues pertaining to establishing and maintaining the treatment-control regime. Prior to implementing the study, staff from both organizations will be involved in designing an efficient approach to recruitment and random assignment that will be the least burdensome for the experimental site(s). This includes a recruitment strategy and operational procedures for transferring data from the program site to the research organization and back again.
 - X *Need to minimize mid-course program changes.* Experimental impacts are based on the treatment-control differences that emerge over time. To ensure the validity of the results, it is important that all individuals in the treatment group receive a consistently administered set of program benefits and services. If the program intervention shifts after enrollment of the research sample, it becomes extremely difficult to interpret the observed treatment-control differences in outcomes. One way to avoid such confounding is to select site(s) that appear to have settled into a stable set of program rules and are less likely to make changes. Again, this emphasizes the need for a careful and comprehensive site selection process. It is important to discuss this requirement with the site(s) before the study begins, so that any contemplated changes can be incorporated into the program before recruitment of the sample. At the initial stages of recruitment, a site visit should be conducted to document all recruitment efforts, program eligibility requirements, and services provided by the program.
 - X *Inability to attribute estimated program impacts to specific program features.* The experimental design is likely to include only one treatment group at each experimental site. As a consequence, it will not be possible to determine whether any estimated treatment-control differences in outcomes are attributable, for instance, to the availability of matching funds, to the counseling and training services offered to account-holders, or to some combination of these features. Although multiple treatment groups would address this limitation, they require either a larger research sample or some loss of "statistical power" (i.e., a reduced ability to detect program effects).
 - X *Expense of primary data collection.* It is costly to conduct primary data collection over a multi-year period with households in both a treatment group and a control

group, even at minimally acceptable initial sample sizes and survey response rates. To provide appreciable statistical power to detect program effects under conventional assumptions, impact estimates are typically based on at least 300 households per group, with available data at both baseline and one or more followup intervals. Followup response rates of 70 percent or higher are generally considered necessary to minimize the risk of nonresponse bias (i.e., the risk that those not interviewed differ systematically from the respondents). These requirements entail a substantial, labor-intensive effort by telephone interviewers and field survey staff to track, locate, and interview sample cases.

Nonexperimental impact analysis

As noted above, there are a number of drawbacks to an experimental design. One option is to undertake *nonexperimental* impact analysis. Under this alternative approach, instead of using a randomly assigned control group to represent the policy counterfactual, one uses available data on nonparticipants within the general population. Comparable data would then be collected on program participants. Multivariate statistical techniques would be employed to account for observable differences between participants and nonparticipants on individual background characteristics and other contextual factors, such as local economic conditions.

Nonexperimental analysis requires that one has adequate data to parcel out program effects from non-program “external” effects on savings and asset outcomes. If one is unable to control adequately for the external factors, the resulting impact estimates could falsely attribute to the program the effects of underlying demographic or socioeconomic differences between participants and nonparticipants. This is especially problematic in programs such as IDAs, where one expects that participants have greater motivation and initiative than nonparticipants. Such personal traits are typically unmeasured in available data; without any means to properly control for them, one tends to overstate the program’s effects.

With these limitations in mind, it is nonetheless worth considering the merits of nonexperimental approaches. To be feasible, this strategy requires a database that would enable one to measure the savings and asset patterns among households who participate in an AFIA-funded program and also among those who would qualify for, but are not participating in, such a program. For the program participants, as noted above, comparable data would need to be collected through a separate primary data collection effort, to the extent that participants would be found in very small numbers in any national database.

Such a database would need to meet the following criteria:

- X It would contain national data with oversampling of the low-income population, to provide sufficient numbers of AFIA-eligible households.
- X It would provide detail on income, savings, assets, and liabilities, both to identify the AFIA-eligible households and to track outcomes on savings and asset accumulation.

- X It would follow households longitudinally (i.e., over multiyear intervals), to enable one to profile the year-to-year changes in household savings and asset-holdings.
- X The data for nonparticipants would cover a time period that coincides with the operation of AFIA programs, to avoid the need to control for the effects of recent changes in external factors such as economic conditions, institutional arrangements, or technology (such as direct deposit or other electronic transfers).

The one dataset that appears to meet these requirements is the Survey of Income and Program Participation (SIPP), which is administered by the U.S. Census Bureau. The features of SIPP that make it well-suited for such analysis are as follows:

- X The survey is a series of national Apanels@ or household samples. The members of each panel are interviewed in successive Awaves@ every four months, over periods of 22 to 4 years. The most recent panel, the 1996 panel, was introduced in April 1996 and will be interviewed over 12 waves, encompassing 4 years. The twelfth and final wave is about to begin in December 1999. (A 2000 panel will be introduced in February 2000; a 2001 panel will be introduced in February 2001. Each of these is now assumed to cover 22 years.)
- X Each panel is a stratified sample of the U.S. civilian noninstitutional population, with oversampling of low-income households. The 1996 panel consists of 36,700 households. (The sizes of the 2000 and 2001 panels have not yet been announced.)
- X Detailed financial information is collected for each household. The Acore module@ of questions administered to each panel at each wave includes items on income sources and amounts, labor force status, living arrangements, and participation in income support programs. Such basic information is recorded for each of the last four months. Additionally, asset information is asked as of the last day of the four-month reference period. The latter items include checking account balances, value of U.S. savings bonds, amounts in individual retirement accounts (IRAs), and outstanding debts and obligations, including unpaid bank loans and credit card bills.
- X At each wave, the core questions are supplemented by several Atopical modules@ that address particular household circumstances. One of the topical modules pertains to AAssets and Liabilities.@ It is administered every year (i.e., every third wave for each panel).³ The items include savings accounts, stocks, mutual funds, bonds, Keogh and IRA accounts, and unsecured liabilities (e.g., loans, credit cards, medical bills). One can calculate each individual=s net worth in conjunction with other information, including the value of homes and automobiles, collected through another topical module on AHousing Costs and Energy Usage.@

³For the 1996 panel, the AAssets and Liabilities@ topical module was administered during Waves 3, 6, 9, and 12, which occurred in December of 1996, 1997, 1998, and 1999, respectively. For the 2000 and 2001 panels each, this topical module will be administered during Waves 3 and 6. These waves will occur in October of 2000 and 2001 for the 2000 panel, and in October of 2001 and 2002 for the 2001 panel.

One possible approach would be to combine the forthcoming SIPP data from the 2000 and 2001 panels with data that could be collected on a supplementary national sample of AFIA program participants. The intent would be to conduct the supplementary data collection contemporaneously with the SIPP data collection, using the same survey instruments (i.e., the core module and the topical modules on Assets and Liabilities and Housing Costs and Energy Usage). The joint dataset would then become the basis for a statistical analysis of program effects. One would model savings and asset outcomes as a function of household-specific explanatory variables, including whether or not one participates in an AFIA-funded project.

Undeniably, this proposed strategy for nonexperimental analysis would be an ambitious undertaking. One drawback of relying on SIPP data from upcoming panels is that such data typically do not become available for analysis until approximately two years after collected. Thus, the information on Assets and Liabilities from Waves 3 and 6 of the 2000 panel (collected in October of 2000 and 2001) would not become available until late 2002 and 2003, respectively. For the 2001 panel, the corresponding dates would be one year later.

Another drawback of a SIPP-based approach is the risk of measurement bias in conducting the supplementary data collection for AFIA program participants separately from the data collection (through SIPP) for nonparticipants. Even with identical instruments, different interviewing methods between Census and non-Census interviewers might lead to different response patterns and thus spurious estimates of program effects. The preferable strategy for any SIPP-based approach of this type would be to arrange, if possible, for Census interviewers to conduct the supplementary data collection for AFIA program participants.

Section 7: Benefit-Cost Analysis

Stakeholders in Individual Development Accounts (IDAs) include government, private donors, users, non-users, administrators, and society as a whole. Each group has its own roles and its own goals, and so each group has different benefits and costs. If IDAs are to succeed, then each group must play its part; for a given group to play its part, its benefits must exceed its costs (Schreiner, 1997).

The benefit-cost analysis will rigorously analyze outcomes for all six groups. It will ask not only whether benefits exceed costs (sign of impact), but also by how much benefits exceed costs (size of impact). It will also try to explain the causes of impact; perhaps the most important output of the demonstration will be improved knowledge of how to increase benefits and decrease costs through improved rules of policy, design of IDA contracts, technology of supply, organization of administration, and implementation of IDA-related services.

Like the rest of the evaluation, the benefit-cost analysis aims at rigor. This is not to pretend to quantify all benefits and costs. Such exhaustive measurement is impossible; real knowledge is always incomplete and imperfect. Also, most benefits and costs are intangible and subjective, unquantifiable even to those who experience them.

A rigorous analysis will not eschew all measurement, but rigor goes far beyond mere quantification. Rigor is the care to make explicit the inevitably subjective judgments and assumptions required to reach conclusions (Schreiner, 1999a). The goal of rigor is to expose the building blocks of arguments so as to make them susceptible to improvement through open discussion and criticism. Rigor and explicit arguments—not falsificationist experiments and numbers—are the heart of the social scientific method.

Of course, the benefit-cost analysis here will measure as much as it can. In particular, it is inexpensive to measure cash flows. Most effects, however, are expensive to measure because they are so subtle, diffuse, complex, and intangible. For example, probably the most important hypothesis in asset-based welfare theory is that IDAs spark hope (Sherraden, 1991). People with an IDA expect to have resources in the future, and this knowledge may change their choices and efforts. Hope is real; unfortunately, it is difficult to capture in a single number. Whether IDAs are worthwhile, however, may hinge on how they increase hope, so the benefit-cost analysis must include subjective judgments of the worth of changes in hope caused by IDAs. The subjectivity of these judgments does not mean that their support should be arbitrary or opaque; rather, the analysis must be even more careful to explain carefully and explicitly the experiences, reasons, and assumptions behind the judgments.

The first benefit-cost analysis of IDAs was by Clones *et al.* (1995). Although the work uses almost no measurements because few data on IDAs were then available, the *pro forma* analysis is extraordinarily rigorous. Clones *et al.* carefully identify different sources of benefits and costs for different groups and then discuss which ones they might measure, which ones they cannot measure, and the reasons and logic behind the assumptions that stand in for measurements. The new work here will build on this solid base, replacing some assumptions with previously unavailable measurements and improving on the methods used to compare benefits and costs for different groups.

Alternatives to IDAs

IDAs are just one of many tools to help the poor. Resources used for IDAs could have been used in some other way. For example, they could have been distributed to the poor as cash, allocated to public schools, used to pay down the national debt, spent on roads, or even returned to taxpayers. Because there are alternatives, it is not enough that IDAs simply have some benefits; benefits must be so great as to exceed costs.

Benefits and costs take place at different points in time, so the analysis will use discounting to ensure that all figures are in comparable units.

Benefits and costs for different groups

Government

Benefits and costs for the government are relatively simple to measure because most of the effects involve cash flows. Examples include cash entrusts to IDA administrators and wages paid to government employees who manage IDA policy.

The government also receives benefits from IDAs. These include increases in tax receipts and decreases in expenses for such things as public assistance and prisons. These benefits are measured as the differences caused by IDAs in cash flows between the government, members of the experiment group, and members of the control group.

Private investors

Under AFIA, government funds must be matched by funds from private donors. Private donors do not expect a financial return, but they do expect a return in terms of improved well-being for some target group. It is difficult to compare non-financial benefits with financial costs. Whether benefits exceed costs for private donors can, however, be inferred from whether the donor makes donations more than once.

Participants

The evaluation framework developed by Schreiner (1999b and 1999c) compares benefits and costs as seen by participants. The framework highlights trade-offs between six aspects of costs and benefits that parallel standard concepts in benefit-cost analysis (Belli, 1996; Brent, 1996; Gittinger, 1982):

- Worth to participants (willingness to pay)
- Cost to participants (price, transaction, and opportunity costs)
- Depth (Social value of worth minus cost for a given user)
- Breadth (Number of participants)
- Length (Time frame of participation)
- Scope (Range of services supplied and used)

Non-participants

IDAs affect not only but also non-participants. For example, costs for the government are costs for taxpayers. Costs are also borne by non-participants who receive less assistance than if funds for IDAs were used for some other program.

People lucky enough to have access to IDAs will displace some non-participants without access. For example, microentrepreneurs with IDAs will squeeze the market share of some other microentrepreneurs. Likewise, people who use IDAs to buy homes will bid up prices and squeeze out even poorer people who otherwise might have bought homes. As a rule, the impacts of IDAs on non-participants depend on the general-equilibrium response of the supply of the assets that IDAs subsidize. Unless supply expands, IDAs may merely shift well-being from low-resource non-participants to participants. Of course, if prices rise, then supply will expand, but prices rise only if some people crowd others out.

Non-participants, especially family members of participants, also get benefits from IDAs. For example, children of homeowners and of college graduates tend to have more success otherwise (Page-Adams and Vosler, 1997; Cheng and Page-Adams, 1996; Page-Adams and Sherraden, 1996; Scanlon, 1996). Furthermore, homeownership has positive affects on children in the neighborhood.

Administrators

The non-government employees who administer IDA programs will perform best when their own benefits exceed their own costs. Benefits as seen by administrators include wages, perks such as travel to conferences, and satisfaction from the attempt to help low-resource people. Costs include their time and effort at work on IDA programs.

Society

In the end, IDAs are meant to produce social benefits in excess of social costs. The only reason to measure net benefits for government, private investors, participants, non-participants, and administrators is because the cooperation of all these groups is needed if IDAs are to be worthwhile for society as a whole. Any one group of stakeholders can sabotage IDAs, so each group must be satisfied if IDAs are to succeed.

In the simplest terms, benefits net of costs for society as a whole is the aggregate of benefits net of costs for the other five groups of stakeholders.

Benefit-cost accounting

As an illustration, Exhibit 7-1 breaks down costs and benefits from cash flows (or from implicit cash flows) that result from the use of IDAs for home purchase, microenterprise, and post-secondary education. The exhibit lists basic categories of measures of cash flows from the point of view of participants, the federal government, other public and private investors, and society as a whole. (The exhibit excludes nonparticipants and administrators of IDA programs because changes in their cash flows will not be estimated.) For any given group, net benefits equal benefits minus costs. For society as a whole, benefits, costs, and net benefits equal the corresponding effects summed across the three individual groups.

For participants, the cash costs of participation in an IDA program are the funds placed on deposit. Deposited funds have an opportunity cost because they are unavailable for other uses. Participants get benefits when they use deposits for home purchase, microenterprise, or post-secondary education. Implicit cash benefits from home purchase include the home equity that results from the use of IDA funds and part of the tax breaks that result from home-mortgage interest deductions. These benefits are net of reductions in transfer payments received, such as the loss of direct housing subsidies. For microenterprises, cash benefits include the value of increases in net worth caused by asset purchases made with IDA funds and increases in business income. Netted out of these benefits are business taxes and reductions in transfer payments and in non-business income caused by the use of IDAs. For post-secondary education, cash benefits include projected lifetime increases in income, net of increased taxes and decreased transfer payments. For all three uses of IDAs, the cash benefits net of taxes and transfer payments will be estimated based on observed differences between members of the treatment and control groups.

For the federal government, costs result from budget outlays for matching funds and for administrative expenses of IDA programs, as well as for the expenses of the federal government's own program administrators. Cash benefits for the federal government include net increases in tax receipts and net decreases in transfer payments. For example, increased microenterprise and post-secondary education may increase incomes and thus tax receipts. They may also decrease outlays for means-tested transfer payments. At the same time, home purchase may lead to decreased tax receipts due to home-mortgage interest deductions.

For state and local governments, cash costs and benefits resemble those of the federal government in that they result from changes in tax receipts and transfer payments and from budget outlays for matching funds and administrative expenses. Private investors bear costs in that they provide cash for matching funds and administrative expenses, but of course the cash flows of private donors are not directly affected by changes in tax receipts and transfer payments.

**Exhibit 7-1:
Benefit-Cost Accounting**

Measure	Participants	Federal government	Other public and private investors	Society
Discounted present value of benefits or costs, in dollars per participant				
Benefits (+):				
Home purchase	housing value, net of taxes and transfer payments	federal taxes and transfer payments	state and local taxes and transfer payments	sum (row)
Micro-enterprise	imputed income, net of taxes and transfer payments	federal taxes and transfer payments	state and local taxes and transfer payments	sum (row)
Post-secondary Education	imputed income, net of tax and transfer payments	federal taxes and transfer payments	state and local taxes and transfer payments	sum (row)
Costs (-):				
	deposited funds	matching funds, administrative costs	matching funds, administrative costs	sum (row)
Net benefits:	sum (column)	sum (column)	sum (column)	sum

For society as a whole, the benefits or costs are the summed effects for participants, the federal government, and other investors. At the societal level, the effects on taxes and transfer payments net to zero. Societal benefits thus equal the sum of increased housing value and higher incomes for participants; societal costs equal are the sum of participant deposits, matching funds, and administrative costs.

All benefits and costs will be expressed as discounted present values, in dollars per participant. This will require the choice of an appropriate discount rate. Sensitivity analysis will be conducted using alternative assumptions for the discount rate. As specified in the Assets for Independence Act, the benefit-cost calculations will be carried out for time horizons of five years and ten years.

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Section 8: Overall Implications for Evaluation Design

This concluding section highlights a series of issues relating to the scope and timing of the evaluation activities that would commence following the design phase, which ends in August 2000. These aspects of the evaluation strategy will require further discussion with the Task Order Officer and other HHS staff.

Timing of evaluation activities

As noted in Section 1, a literal interpretation of Section 414 of the Act would call for the final evaluation report to be completed in 2009 (i.e., “not later than 12 months after the conclusion of all demonstration projects conducted under this title”). Presumably, however, the Department will want to present some intermediate findings available at the time that Congress is considering the reauthorization of the Act, scheduled for 2003.

Extensive findings will indeed be available by 2003 from the process analysis, in-depth participant interviews, and the program and participant tracking and monitoring. These findings will reflect project experience from the first, second, and third grantee cohorts. By 2003, the findings from the impact analysis would at best reflect only 12 to 24 months of followup data on an enrolled experimental sample. These are the findings that would then form the basis for the benefit-cost analysis.

This issue of timing is important with respect to upcoming decisions regarding the scope of the impact analysis, and thus the extent of findings ultimately available for the benefit-cost analysis. Simply stated, the question is whether 2003 should be regarded as the date for a final evaluation report, or whether the evaluation would extend further. As discussed below, the answer to this question has implications for selecting one or more experimental sites and for considering possible nonexperimental impact analysis.

Selection of one or more experimental sites. If experimental impact findings are to be available by 2003, the experimental site(s) must be chosen from the first or second cohort of grantees—i.e., those announced in September 1999 and those to be announced during the year 2000. The enrollment of a randomly assigned research sample would take place during 2000-2001, and data would then be collected for followup periods of 12 to 24 months. This schedule would require that the experimental site(s) be selected in mid-2000. If the timeframe for impact findings is extended beyond 2003, it allows the selection of the experimental site(s) to incorporate more information about the variation in program models that states and localities are implementing under the Act.

Consideration of nonexperimental impact analysis. If 2003 is regarded as the date for a final evaluation report, it raises questions as to the feasibility of undertaking any nonexperimental impact analysis of the type discussed in Section 6. The time lags in

availability of SIPP data from the year 2000 panel (the first panel to be observed within the AFIA period) suggest that one could make use of information on assets and liabilities from the Wave 3 topical module (covering the first followup year for panel members), but not the Wave 6 topical module (covering the second followup year).

Balancing the statutory requirements with evaluation resources

One of the products of this task order (under Subtask 3.4, due in late April 2000) will be “an overall cost estimate for implementing and conducting all aspects of the evaluation project.” At this early stage, we can already foresee that the estimated costs to fulfill the statutory requirements will exceed the funds available through the 2 percent evaluation setaside, even if one assumes that the statutory mandate is minimally met and even if one assumes that annual appropriations are as high as \$20 million (twice the level of annual appropriations thus far). This suggests that possible enhancements to the evaluation design, such as more than one experimental site or nonexperimental impact analysis, may not warrant serious consideration unless there is some expectation of additional evaluation funding.