Effects of an Individual Development Account Program on Retirement Saving:

Follow-Up Evidence from a Randomized Experiment

Michal Grinstein-Weiss
Center for Social Development

Michael Sherraden
Center for Social Development

William Gale
Brookings Institution

William M. Rohe
University of North Carolina at Chapel Hill

Mark Schreiner
Center for Social Development

Clinton Key
University of North Carolina at Chapel Hill

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Abstract

Using data from a randomized experiment that ran from 1998 to 2003 in Tulsa, Oklahoma, we examine the 10-year follow-up effects on retirement saving of an Individual Development Account (IDA) program. The IDA program included financial education, encouragement to save, and matching funds for several qualified uses of the savings, including contributions to retirement accounts. The results indicate that, as of 2009, 6 years after the program ended, the IDA program had no impact on the propensity to hold a retirement account, the account balance, or the sufficiency of retirement balances to meet retirement expenses.

Key words: *American Dream Demonstration (ADD), asset effects, Community Action Program of Tulsa County (CAPTC), home ownership, Individual Development Account (IDA), low-income households, retirement, savings*

Concerns among researchers and policy makers about the inadequacy of household savings for retirement have only been heightened by the recent financial crisis. The National Retirement Risk Index (NRRI; Munnell, Soto, Triest, & Zhivan, 2008) suggests that many or most households are at risk of suffering a significant reduction in their standard of living upon retirement. Prospects for low-income households are a particular concern. The index shows that 72 percent of those in the bottom third of the income distribution are at risk of significant reductions in living standards, and other evidence (Butrica, Zedlewski, & Issa, 2010) shows that low-income households are less likely to hold retirement accounts and less likely to make persistent contributions to retirement accounts. Low-income households may be worse than others at predicting the level of financial assets needed for retirement (Bell, Carasso, & Steuerle, 2005) and are more likely than others to cash out preretirement distributions (Burman, Coe, Dworsky, & Gale, 2012). In contrast, Scholz, Seshadri, and Khitatrakun (2006) and Scholz and Seshadri (2008) find that almost all adult American households have accumulated, or are on a path to accumulate, net worth sufficient to maintain preretirement living standards in retirement.

Despite the controversy about the adequacy of saving for retirement, policy makers have shown strong interest in policies designed to boost private saving, including among low-income households. Individual Development Accounts (IDAs) provide an example. They are matched savings accounts, often paired with financial education and individual counseling, that offer holders matching funds as incentives if withdrawals are used for certain purposes—typically, for home purchase, postsecondary education, or small-business ownership. Between 1999 and 2009, more than 60,000 IDAs were opened under the Assets for Independence Act (U.S. Department of Health and Human Services 2010), although the Assets for Independence program does not allow for matched retirement savings.

Most experimental evidence on the effect of IDAs is based on a randomized field experiment that took place in Tulsa, Oklahoma, between 1998 and 2003.[1] In the experiment, treatment group members could contribute up to $750 per year for 3 years to an IDA.

They also received financial education and encouragement to save by program staff. Withdrawals for housing down payments were matched at a 2:1 rate. Withdrawals for other qualified uses—contributions to retirement savings accounts, home repair, small business ownership, and education—were matched on a 1:1 basis. Sample members were randomized into treatment and control groups and interviewed at baseline. Follow-up interviews took place at 18 months, 4 years, and 10 years.

Using data from the first 4 years of the Tulsa experiment, Grinstein-Weiss et al. (2008) and Mills et al. (2008) show that the Tulsa program had a positive effect on home ownership rates as of 2003. However, using data from the 10-year follow-up, Grinstein-Weiss et al. (in press) show that the home ownership effect declined rapidly after the program ended in 2003. They also show that the longer-term (as of 2009) effects on home ownership rates were close to zero and not statistically significant.

In *learn$ave*, a randomized IDA experiment that started in Canada in 2001, exposure to IDAs has positive impacts on postsecondary education and small-business start-ups, two of the qualified uses of contributions in that program (Leckie, Hui, Tattrie, Robson, & Voyer, 2010).[2] Retirement saving is not a qualified use of funds in this program.

In this paper, we examine the 10-year effects of the 3-year Tulsa IDA program on retirement savings. This analysis provides, to our knowledge, the first longer-term experimental analysis of the impacts on retirement saving of any type of subsidy program; this despite a large literature on the possible effects of the billions of dollars of annual public tax expenditure for subsidies for private retirement saving (Office of Management and Budget 2010). The paper also offers a rare opportunity to learn about retirement saving incentives in the absence of sample-selection issues (see the discussion in Engen, Gale, & Scholz [1996] and Poterba, Venti, & Wise [1996]). Finally, the analysis provides a rare opportunity to examine the impact of IDAs on retirement saving, as most IDAs (including all in the Assets for Independence Act) do not include retirement saving as a qualified use of matching funds.

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[1] Individual Development Accounts have also been studied using nonexperimental methods. A number of studies (e.g., Mills et al., 2008b; Rademacher, Wiedrich, McKernan, Ratcliffe, & Gallagher, 2010) compare IDA participants to samples of non-IDA participants. These comparisons are less than ideal because people who sign up for IDAs are not a random sample of low-income households, even after analyses control for observables (as shown in Grinstein-Weiss et al., in press; Mills et al., 2008a). Other studies examine associations of IDA program and participants' characteristics with IDA saving outcomes (Schreiner and Sherraden 2007). These studies are informative but are not designed or reported as impact tests. Another set of studies (Sherraden & Moore McBride 2010; Sherraden, Moore McBride, Hanson, & Johnson, 2005) report results of in-depth interviews with IDA participants. These analyses illuminate participation patterns in the IDA program and document participants' assessment of results but do not claim to test impacts.

[2] In related work, Engelhardt, Eriksen, Gale, & Mills (2010) use Tulsa IDA treatment status as an instrument for home ownership in analysis of 2003 data and find no net impact of home ownership on the provision of social capital.

In prior work, Mills et al. (2008) show that the IDA program had no effect on retirement saving at the end of the 3-year saving period in 2003. Nevertheless, the longer-term effects are still of interest. The 2003 effects are short-term impacts, since people had 3 years to accumulate funds in the IDA before using them for retirement savings or other qualified uses. Even with insignificant short-term effects, the longer-term effects could be positive if the effects of financial education cumulate over time or if people use the IDA to meet their economic priorities in a sequential manner—perhaps obtaining more education or a house first and then moving on to other saving goals, such as retirement. Alternatively, the longer-term effects could be negative if pursuing the other subsidized uses of the funds causes treatment group members to draw resources away from retirement saving.

Our central result comes from 2009 data: approximately 10 years after treatment assignment, there is no impact of the Tulsa IDA program on the presence of retirement saving, the level of retirement account balances, or measures of the sufficiency of retirement saving to meet expenses.

The results of this study should contribute to the larger body of literature on the impact of matching contributions on retirement saving contributions. Duflo, Gale, Liebman, Orszag, and Saez (2006) and Saez (2009) show that a matching contribution for individual retirement account (IRA) contributions, offered at the time of tax filing, can raise the participation level in IRAs as well as contribution levels among those who contribute. Several factors may cause their results to differ from ours: the IRA contribution in their studies is the only match-qualified use of funds, and the match is made more salient by the direct focus of the tax preparer and the extremely time-limited nature of the offer. In the Tulsa IDA program, in contrast, there were multiple saving goals and more diffuse attention paid to any one use of the funds.

Engelhardt and Kumar (2007) find that raising matching contributions in 401(k) plans increases participation and contribution levels. The primary ways their sample differs from ours are is that their sample respondents have higher average earnings and that 401(k) contributions are made by automatic payroll deductions, whereas IDA contributions generally were not.

A related literature examines the extent to which contributions to incentivized saving accounts, like IRAs and 401(k) plans, represent net additions to private and national saving. Poterba et al. (1996) and Engen et al. (1996) reach differing conclusions on this issue. Benjamin (2003) finds that about a quarter of aggregate 401(k) balances represent net additions to national saving.[3] We do not examine the impact of increased retirement saving contributions on net worth in this paper for the simple reason that we do not find any increase in retirement saving contributions.

The remainder of this paper is organized into four sections. The first describes the Tulsa IDA program and the data collection. The second describes our empirical measures and the analytic methods. The third section presents the main results. The final section provides discussion and concludes.

Experimental Design and Data Collection

The Community Action Program of Tulsa County (CAPTC) administered the IDA program as part

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[3] Ashraf, Karlan, and Yin (2006) examine the 12-month effects of a savings commitment device implemented at a bank in the Philippines.

of the American Dream Demonstration (ADD).[4] Begun in the late 1990s, ADD was a set of 14 philanthropically funded local IDA programs. The Tulsa site was the only local ADD program implemented as a random-assignment experiment. Eligibility rules required applicants to be employed and to have household income below 150 percent of the federal poverty guideline.

Participants in the treatment group received access to financial education, case management, and an IDA held at the Bank of Oklahoma.[5] Deposits in these accounts earned an interest rate of 2–3 percent.[6] Participants could receive matches for up to $750 in deposits each year. If a participant deposited more than $750 in a given year, the program considered that excess eligible for a match in subsequent years. The program matched participant deposits for 36 months after the opening of the account. Participants could make an unmatched withdrawal at any time, but the program only allowed them to make matched withdrawals 6 or more months after opening the account. The Tulsa program matched withdrawals for home purchase at a 2:1 rate. It matched withdrawals for home repair, small-business ownership, postsecondary education, or retirement saving at a 1:1 rate. If a participant made the maximum matchable deposit in all 3 years, he or she could accumulate $6,750 (plus interest) for a home purchase or $4,500 (plus interest) for other qualified uses. At the end of the program, participants could ask the program to place the remaining IDA balance into a Roth IRA and receive a 1:1 match. If the participant made no request, the funds remained in the account, and the participant received no matching funds. Sample members had to agree not to use other matched-savings programs at CAPTC during the 4-year study period.[7]

Recruitment of participants took place from October 1998 to December 1999. Program applicants were assigned to one of 13 cohorts based on the timing of their applications. After completing a baseline interview (Wave 1), sample members were randomly assigned to either the treatment or the control group. The baseline sample consisted of 1,103 respondents. Wave-2 interviews took place between May 2000 and August 2001, and Wave-3 interviews occurred between January and

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[4] See Mills, Patterson, Orr, and DeMarco (2004) and Grinstein-Weiss et al. (in press) for more information on the data and survey methods.

[5] General money-management training and asset-specific training were part of the Tulsa program's financial-education component. Program staff sent out monthly deposit-reminder postcards and provided case management, including assistance and consultation by phone or in person. The bank paid matches directly to the vendor.

[6] The bank charged no monthly maintenance fee for the account. It charged no fees to open or withdraw from the account if the respondent made fewer than three withdrawals in 1 year (the third and subsequent withdrawals induced a $3 fee). The bank allowed participants to transfer money automatically into the IDA via direct deposit.

[7] In particular, CAPTC provided a housing-subsidy program that offered low-income households up to $4,000. As a result, during the experimental period through 2003, treatment group members had access to the CAPTC IDA while both control and treatment group members were restricted from other CAPTC housing-subsidy programs available to other low-income households. After 2003, treatment and control group members were again eligible for all CAPTC programs. All sample members could use CAPTC services for tax preparation, employment, education, child care, and other purposes during the experiment period. Control group members could also receive individual counseling from CAPTC and, if they requested it, general financial information and referrals to other agencies in the Tulsa area that provided similar services. At these agencies, control group members were free to seek any service for which they qualified.

September 2003, about 48 months after random assignment. Computer-assisted-telephone and personal interviewing methods were employed.

The current study reports analyses of data from a fourth wave of interviews that took place between August 2008 and March 2009, approximately 10 years after random assignment and about 6 years after the experiment ended. Interviews with the treatment and control groups occurred at an even pace. The pace is relevant because the recent economic downturn developed and worsened during data collection. Most participants living in the greater Tulsa area completed in-person interviews. Respondents who lived elsewhere (about 20 percent of the sample) completed telephone interviews. To increase response rates and improve data quality, especially for income and wealth (Biemer, Groves, Lyberg, Mathiowetz, & Sudman, 1991), the primary interview method was changed from telephone interviews (employed in earlier waves) to personal interviews in the fourth wave. The format and content of the Wave-4 survey were the same as those in the earlier surveys, but this survey included some new questions.

Our analysis uses data from the baseline and the 10-year follow-up surveys. Of the 855 baseline respondents in the 10-year survey, we exclude 156 with missing data on key outcome variables or covariates as well as all 39 who were over age 55 at baseline (and, thus, are over retirement age at the 10-year follow-up).[8] The remaining analytic sample consists of 660 respondents, 311 in the treatment group and 349 in the control group.

Survey questions ask respondents (a) whether they had any savings in "IRAs, 401(k), 403(b), or other pension plans at work," and, (b) if so, how much. Public funds, such as Social Security benefits, are not included in the question. All values are presented in 2009 dollars.

We use responses to these items to construct measures of retirement savings in the baseline and 10-year surveys. The measures include the presence of any retirement savings balances, the level of retirement account balances, and a constructed measure of the sufficiency of retirement-account balances. The last item is a rough measure of the ability of retirement saving to replace income at baseline. It is constructed by dividing the level of retirement account balances in each survey wave by 75 percent of baseline monthly household income. This measure is intended to suggest, in the absence of income or other sources of support, how long the retirement savings might sustain the respondent at a given level of consumption. From the ratio, indicator variables are created for sufficient saving for 1 month, 6 months, and 1 year.

Results

Preliminary data analysis

About 22 percent of respondents owned their residence, and about 28 percent reported income from welfare payments. Most respondents are nonwhite (55 percent), and about 80 percent are women. About 28 percent were married, and about 81 percent had children under 18 living in their home. Average monthly total household income was $1,824. The mean age at baseline was 35 years; thus, the average respondent was about 45 at the 10-year follow-up.

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[8] Among Wave-4 respondents who met age criteria, data are missing on the retirement assets of 69, or 8.5 percent. The measure of retirement assets is the central dependent variable in this study.

Appendix A compares the baseline characteristics of treatment and control group members in the 10-year survey. The two groups significantly differ at the 10 percent level on only one characteristic: presence of children in the household. This number of significant differences is actually less than would be expected to occur due to sampling variation under random assignment. In the regression analysis below, we control for all of the baseline demographic and financial characteristics listed in Appendix A.

Appendix B compares baseline characteristics of those who were missing from the Wave-4 sample to those of individuals who are included. Excluded from the sample are both those who did not respond to the Wave-4 survey and those who responded to the survey but for whom data are missing on variables included in the analysis. The table shows that, relative to the full baseline sample, members of the analyzed sample—at baseline—had lower income, had less retirement saving, were more likely to be married baseline, and were less likely to be in the last two cohorts of data collection. All of the variables in the table are controlled for in the regressions.

Table 1 describes treatment group members' intended use of the IDA. About 90 percent of treatment group members opened an IDA. Among the 279 who opened accounts, 17 percent said at account opening that they planned to use the account to save for retirement. Account holders could make multiple matched withdrawals and could make them for any of the qualified purposes. They could also make unmatched withdrawals for other purposes. About 12 percent of account holders made withdrawals for more than one use. About 15 percent of all of those who opened accounts made at least one matched withdrawal for retirement. Among respondents saving for retirement, the average monthly net deposit was $30. Among respondents who made a matched withdrawal for retirement, the average amount of match-eligible withdrawals (aggregated across all withdrawals and all uses) was $664.

Bivariate analysis

Table 2 presents retirement saving information for the analytic sample at baseline and at the 10-year follow-up. The leftmost panel shows there were no observed differences between the treatment and control groups on retirement saving at baseline. About 17 percent of the treatment group held retirement balances; by comparison, 18 percent of the control group did so. Fewer than 10 percent of respondents in either group held balances in excess of 75 percent of 1 month's income.

The treatment group held mean balances of $1,212 in retirement accounts, and the control group's mean is $789. In both groups, however, a small number of outliers inflate the estimated mean and may not be balanced between treatment and control. Because the original data are highly dispersed (retirement savings balances at baseline range from $0 to $66,000), winsorized versions of the variable were created, taking the 75th and 95th percentiles of the analytic sample's distribution at the 10-year follow-up ($4,450 and $26,000, respectively) as the cutoffs. In winsorizing, cases with a value greater than the cutoff are recoded so their value equals the cutoff. Before adopting the cutoffs, numerous different cutoffs were tested in the regression models. None of the tested cutoff values produced impact estimates substantively different from those presented in the paper. As Table 2 shows, with winsorizing, the mean value of retirement savings at baseline falls for both groups and the differences shrink. There continue to be no significant differences between the treatment and control group.

Table 1. Account Use

Use	Proportion/Amount ($)
Opened IDA (*n* = 311)	.90
Planned to save for retirement (*n* = 279)[a]	.17
Made a matched withdrawal for retirement (*n* = 279)[a]	.15
Mean AMND (*n* = 48)[b]	30
Mean matched withdrawal amount (*n* = 42)[c]	664

Notes: IDA = Individual Development Account; AMND = average monthly net deposit.

[a] Those who opened IDA account

[b] Those saving for retirement.

[c] Those who took matches for retirement

The middle panel of Table 2 shows similar data at the 10-year follow-up. There remain no significant differences between treatment and control group members: 45 percent of the treatment group and 43 percent of the control group held retirement accounts. About 36 percent of treatment group members and 35 percent of control group members had at least 75 percent of 1 month's income in retirement balances.

The rightmost panel of Table 2 presents changes between baseline and the 10-year Wave-4 survey. Both groups raised their retirement saving over the period, which is not surprising, given that the data cover 10 years for a working-age population. But there was no significant difference in share of people reporting a higher level of retirement savings at Wave-4; 41 percent of each group reported higher balances. The control group actually experienced significantly larger growth in retirement savings balances than did the treatment group (*p* < .06): an increase of $4,966 for controls compared to an increase of $3,120 for treatment group members. This difference, though, is largely driven by outlying values. We observe small differences between the two groups in the change in retirement savings when winsorized values are examined.

Regression analysis

We use regression models to estimate the treatment effect on all of the 10-year follow-up outcome variables. The models take the following form:

$$Y_{4i} = \alpha + \beta T_i + \gamma \mathbf{X}_i + \delta Y_{1i} + \varepsilon_i, \tag{1}$$

where i indexes households, Y_4 is the outcome variable at the 10-year survey, Y_1 is the equivalent baseline value of the variable, T takes a value of 1 for treatment group members and 0 for control group members, and $\mathbf{X}$ is a vector of covariates measured at baseline and reported in Appendix B. Controlling for these characteristics improves the precision of the estimates.

For the dichotomous outcome variables—presence of retirement saving, increase in retirement savings, and indicator variables for sufficiency of retirement savings—treatment effects are

Table 2. Bivariate Analysis of Retirement Savings at Baseline, 10-Year Follow-up, and Change for 1998–2008

	1998 Retirement Savings				2008 Retirement Savings				Change in Retirement Savings, 1998–2008*			
	Treatment ($n = 311$)	Control ($n = 349$)	Diff.	p	Treatment ($n = 311$)	Control ($n = 349$)	Diff.	p	Treatment ($n = 311$)	Control ($n = 349$)	Diff.	p
Categorical variables (proportion)												
Have any dedicated retirement savings	.17	.18	-.01	.807	.45	.43	.02	.548				
Have enough retirement savings to replace 75% of												
1-month income	.10	.09	.01	.727	.36	.35	0	.904				
6-months income	.05	.03	.02	.262	.20	.21	-.01	.745				
1-year income	.03	.02	.01	.327	.11	.13	-.02	.377				
Increase in retirement savings (2008 > 1998)									.41	.41	0	.971
Continuous variables (mean)												
Retirement savings	1,212	789	423	.307	4,332	5,756	-1,423	.163	3,120	4,966	-1,846	.060
Retirement savings winsorized at												
75th percentile ($4450)	340	327	13	.874	1,419	1,374	45	.767	1,115	1,210	-95	.558
95th percentile ($26,000)	855	721	134	.620	3,625	4,019	-394	.481	2,747	3,481	-733	.194

Note: Diff. = difference.

* Change is measured at the individual level, not difference in aggregates.

Table 3. Regression on Retirement Saving Outcomes for Treatment Group at 10-Year Follow-up

	B	SE	p
Tobit regressions on level of retirement saving ($N = 660$)			
Amount of retirement savings	–1,961	1,841	.287
Winsorized at 75th percentile ($4,450)	155	297	.602
Winsorized at 95th percentile ($26,000)	–326	1,034	.753
Regressions on presence of and increase in retirement saving ($N = 660$)			
Presence of retirement savings	.025	.038	.502
Increase in retirement savings	–.001	.038	.986
Regressions on sufficiency of retirement saving ($N = 660$)			
Retirement savings to replace 75% of income for 1 month	.005	.036	.884
Retirement savings to replace 75% of income for 6 months	–.011	.030	.724
Retirement savings to replace 75% of income for 1 year	–.025	.025	.311

estimated using ordinary least squares (OLS). Though the dichotomous outcome measure violates the distributional assumptions of OLS, OLS results are similar to those from probit regressions and offer a more intuitive and familiar interpretation and presentation. To provide robust estimates when examining levels of retirement saving at Wave-4, where there are a large number of cases that report no dedicated retirement savings, we estimate Tobit models. In those models, a latent variable Y_{4i}^{*} is estimated using the form in equation (1). When $Y_{4i}^{*} > 0$, $Y_{4i} = Y_{4i}^{*}$; when $Y_{4i}^{*} \leq 0$, $Y_{4i} = 0$. The model parameters are estimated using maximum likelihood.

Table 3 presents the results of regressions on saving outcomes. The top panel of the table shows Tobit regressions on level of dedicated retirement savings at the 10-year follow-up at two cutoffs for winsorizing: the 75th and 95th percentiles. The main result is that access to the Tulsa IDA had no statistically significant effect on the level of dedicated retirement savings at the 10-year follow-up.

The middle and bottom panels of Table 3 show that access to the Tulsa IDA had no statistically significant or practically important impact on whether households held dedicated retirement savings or whether they increased their retirement savings balances. Also, access had no effect on the sufficiency of retirement assets relative to baseline income.

Conclusion

Using follow-up evidence from the Tulsa IDA program, which ran between 1998 and 2003, this paper provides the first longer-term experimental evidence on the impact of a matching incentive program on retirement saving. Although both treatment and control groups raised the retirement savings over time, retirement saving contributions were matched by the program on a 1:1 basis, and financial education was provided as part of the program, as of 2009, there were no significant differences in several measures of retirement savings between treatment group members and control group members.

The presented findings also highlight the crucial role of random assignment design in the evaluation of the effects of social programs. Without the randomly assigned control group, the increases in the presence and amount of savings in the treatment group might mistakenly be interpreted as evidence of the Tulsa IDA's effectiveness. Moreover, the results highlight the potential financial peril that retirement represents for participants in the Tulsa IDA. The median respondent holds no balance in

dedicated retirement savings at the 10-year follow-up, and only about 12 percent of each group has enough retirement savings to replace 75 percent of 1 year's baseline income. These facts should inform policy and research considerations regarding social security and other retirement support programs.

The generalizability of the results (that is, the external validity of the experiment) is worth discussing. One issue is that other IDA programs (and, more generally, other subsidy or matching programs) may differ with respect to design features, and these differences could affect outcomes. For example, most of the IDA programs funded through the federal Assets for Independence program now offer a 5-year saving period (U.S. Department of Health and Human Services, 2010). It is also possible that the existence of multiple qualified uses of the Tulsa IDAs—and in particular, the higher incentive for home purchase—diluted the effect of the program on retirement saving. Future experimental research is needed to examine the efficacy of other programs, particularly those focused exclusively or predominantly on bolstering retirement saving.

A second issue is that the overall population most interested in IDAs (that is, the population represented by those who signed up for the Tulsa program) is not a random sample of all low-income households. Instead, relative to other low-income households, participants in the Tulsa study are self-selected and probably more highly motivated to save. Also, they are more educated and more likely to be black, single, and female than are the low-income population at large (Grinstein-Weiss et al., in press; Mills et al., 2008a). Whether the IDA would have an impact on the overall population of low-income households is an open question.

Lastly, the results highlight the need for a better understanding of the choices and constraints faced by lower-income households as they make retirement-saving and other asset-accumulation decisions. Such research would elucidate the role of different barriers preventing the accumulation of adequate savings for retirement and would examine the impact of subsidy programs on other outcome measures.

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Appendix A

Balance between Treatment and Control on Baseline Characteristics

	Treatment (N = 311)	Control (N = 349)	Difference	p
Categorical variables (proportions)				
Own a home	.19	.24	−.05	.113
More than median income ($1,683/month)	.56	.5	.06	.134
Female	.79	.81	−.01	.726
Education (less than HS)				
HS degree	.32	.32	.00	.931
Some college	.41	.42	−.01	.795
College degree or more	.27	.27	.01	.843
Banked	.84	.83	.02	.553
Race (nonwhite)				
White	.43	.48	−.05	.220
Married	.29	.28	.01	.683
Children in household	.84	.78	.06	.052
Study cohort (other cohorts)				
Cohort 12–13	.29	.26	.03	.368
Housing unsubsidized	.74	.73	.01	.788
Health insurance	.59	.56	.03	.442
Own a business	.08	.07	.01	.574
Own other property	.04	.03	.01	.481
Welfare receipt	.27	.29	−.02	.573
Own a car	.83	.85	−.02	.591
Continuous variables (means)				
Number of adults in household	.49	.51	−.02	.692
Household goods ownership scale	2.61	2.70	−.09	.606
Economic strain scale	.55	.56	−.01	.506
Giving help in the community scale	.56	.54	.02	.231
Getting help in the community scale	.35	.36	−.01	.563
Satisfaction with health	.86	.88	−.02	.489
Satisfaction with financial situation	.64	.60	.04	.351
Community involvement scale	.39	.40	−.01	.619
Total monthly household income	1,458	1,369	89	.110
Total assets	15,661	17,979	−2,318	.307
Total debts	13,452	15,330	−1,878	.267
Age	34.3	34.7	−.40	.596

Note: HS = high school.

Appendix B

Analysis of Missing Data

Baseline characteristic	Missing	In Sample	Difference	p
Categorical variables (proportions)				
Treatment Status	.50	.47	.03	.478
Own a home	.24	.22	.02	.599
More than median income ($1,683/month)	.64	.53	.11	.024
Female	.81	.80	.01	.738
Education (less than HS)				
HS degree	.30	.32	-.02	.611
Some college	.47	.41	.06	.21
College degree or more	.23	.27	-.03	.384
Banked	.87	.83	.04	.239
Race (nonwhite)				
White	.43	.46	-.03	.577
Married	.19	.28	-.09	.019
Children in household	.77	.81	-.04	.323
Study Cohort (other cohorts)				
Cohort 12–13	.34	.27	.07	.076
Housing unsubsidized	.78	.74	.04	.277
Health insurance	.65	.57	.08	.074
Own a business	.07	.07	.00	.876
Own other property	.05	.04	.02	.327
Have retirement savings	.13	.07	.06	.03
Welfare receipt	.23	.28	-.04	.292
Own a car	.84	.84	.00	.952
Continuous variables (means)				
Number of adults in household	.51	.50	.01	.872
Household goods ownership scale	2.68	2.66	.02	.939
Economic strain scale	.58	.56	.02	.315
Giving help in the community scale	.57	.55	.02	.236
Getting help in the community scale	.40	.36	.04	.032
Satisfaction with health	.85	.87	-.02	.468
Satisfaction with financial situation	.62	.62	.00	.932
Community involvement scale	.42	.39	.03	.151
Total monthly household income	1,553	1,411	141	.048
Total assets	19,772	16,897	2,875	.376
Total debts	11,844	14,444	-2,600	.193
Age	35	35	0	.902
Amount of retirement savings	667	989	-322	.528

Note: HS = high school.