Center for Social Development
GEORGE WARREN BROWN SCHOOL OF SOCIAL WORK

THE UNIVERSITY OF KANSAS
School of Social Welfare

Individual Development Accounts and Post-Secondary Education

Evidence from a Randomized Experiment

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This paper is part of the Assets and Education Research Symposium, sponsored by the University of Kansas School of Social Welfare and the Center for Social Development at the George Warren Brown School of Social Work, Washington University in St. Louis

Subsequent publication: Grinstein-Weiss, M., Sherraden, M., Gale, W. G., Rohe, W. M., Schreiner, M., & Key, C. C. (2013). Long-term effects of Individual Development Accounts on postsecondary education: Follow-up evidence from a randomized experiment. *Economics of Education Review, 33*, 58–68. doi:10.1016/j.econedurev.2012.12.007

2012
CSD Working Papers
No. 12-21

Acknowledgements

This publication is part of the Assets and Education Research Symposium held at the University of Kansas on March 28 and 29, 2012. The symposium is co-organized by the School of Social Welfare at the University of Kansas and the Center for Social Development at Washington University in St. Louis. Other supporters are CFED, New America Foundation, and the Office of the Provost at the University of Kansas.

For financial support, we thank Annie E. Casey Foundation, F. B. Heron Foundation, John D. and Catherine T. MacArthur Foundation, Charles Stewart Mott Foundation, The National Poverty Center at the University of Michigan, Rockefeller Foundation, The Smith Richardson Foundation, and the University of North Carolina. We thank Ben Harris, Krista Holub, Lissa Johnson, Andrea Taylor, and Jenna Tucker for helpful comments, Steven Dow and Brandy Holleyman at the Community Action Project of Tulsa County for invaluable help throughout the study, and Leah Puttkammer for administrative support.

Individual Development Accounts and Post-Secondary Education: Evidence from a Randomized Experiment

This paper presents evidence from a randomized field experiment testing the impact of a 3-year matched savings program on educational outcomes 10 years later. We examine the effect of an Individual Development Account (IDA) program on educational enrollment, degree completion, and increased education level. The IDA program, which ran from 1998 to 2003 in Tulsa, Oklahoma, provided low-income households with financial education and matching funds for qualified savings withdrawals, including a 1:1 match for educational uses. We find a significant impact on education enrollment and positive, but non-significant impacts on degree completion and increase in level of education. We also examine the interaction between gender and treatment assignment and find that the IDA had a strong positive effect on increased educational attainment for males, but not for females.

Key words: *Individual Development Accounts, asset effects, educational attainment*

Introduction

Why should IDAs increase education?

Individual Development Accounts (IDAs) were designed to help households overcome the barriers that keep them from building and sustaining assets (Sherraden, 1991). Generally, assets are conceptualized as financial investments that can provide some measure of financial security, and which have the potential to contribute to economic mobility (Sherraden, 1991). IDAs provide incentives to encourage low-income individuals to save for and invest in a limited set of assets. These subsidized assets include down payment on a home, investment in a small business, and dedicated retirement savings vehicles. While not a financial or physical asset, education is also treated as an asset in the literature on asset building and in the operation of asset-building programs. This is consistent with human capital theory, which argues that an individual's skills and knowledge can be converted into financial capital through the labor market (Becker, 1964), increasing lifetime income and facilitating the accumulation of assets and financial security over the life course.

Compared to homeownership and business ownership, high educational attainment may represent a more flexible and enduring asset. According to data from the Federal Reserve Bank of St. Louis (2012), delinquency rates on single-family mortgages had risen to roughly 10% by 2010, at the end of the Great Recession, and delinquency rates on business loans were roughly 3.75% (up from about 1.75% pre-recession). The large increases in home and business loan delinquency rates demonstrate

that even traditionally safe investments can be precarious in difficult economic times. In contrast, educational attainment cannot be foreclosed upon, and it is directly related to income and job security. According to 2010 data, the unemployment rate for individuals with only a high school degree was nearly twice the rate for those with a Bachelor's degree, while median weekly earnings of Bachelor's degree holders were 1.5 times those of high school graduates (U.S. Department of Labor, 2011). Even after the recent financial crisis, returns to educational attainment in terms of both reduced incidence and duration of unemployment and increased wages have been consistent.

In addition, research suggests that education has benefits beyond financial returns. Increased knowledge and skills from education can be applied to a variety of domains, and this phenomenon is reflected in better health outcomes in middle age (Herd, 2010) and more positive and constructive parenting practices (Carr & Pike, 2011; Chen, Liu, & Kaplan, 2008) among adults with greater educational attainment. Furthermore, Mangino (2010) argues that higher educational attainment can serve as a way for African Americans to overcome the disproportionate assignment of privilege to white Americans in other domains.

Despite the substantial economic and social advantages associated with the completion of postsecondary education, low-income adults seldom pursue further education once they have entered the workforce. For example, among adults age 25 and over not enrolled in full-time education programs in 2005, 28% reported participation in work-related courses, but only 4% reported enrollment in a part-time degree or diploma-granting program (U.S. Department of Education, 2007). Adults with higher levels of education were more likely to participate in continued education: those with at least some college participated in degree-granting programs at a rate of roughly 7%, while those with only a high school degree participated at a rate of only 3%. In addition, younger adults and those with higher incomes were more likely to participate in adult education, and individuals with professional occupations were more likely to be enrolled in education programs than those with service, sales, or trade occupations (U.S. Department of Education, 2007).

Low-income adults face several substantial barriers to education, both in resuming educational enrollment and in persisting to degree completion. Life course theorists suggest that when educational pursuits occur "out of sequence," non-traditional students experience more competing financial responsibilities and role conflicts than traditional full-time students (Jacobs & King, 2002). There are opportunity costs inherent in returning to education once one has entered the workforce. In a study of female adult learners, Jacobs and King (2002) found that post-secondary students over 25 were more likely to be married, have preschool-age children at home, and to be working full-time. As a result of these factors, non-traditional students may have to pay for additional child care, and forego wages they could have earned had they not been in class. These factors may make enrolling in education seem daunting and costly to lower-income adults who may consider it.

Once enrolled, those who resume their studies face unique challenges in earning a degree. The same study by Jacobs and King (2002) found that older students were less likely to complete their degrees

than younger students (under 25). The authors concluded that this was largely due to the fact that older students were much more likely to attend school part-time. Failure to complete a degree after enrollment may leave LMI households in a more precarious financial position than they would have been had they not reentered the education system. For those who enroll but do not achieve higher educational attainment, the time, effort, and money invested is less likely to yield increased wages or a more advantaged labor market position.

Perhaps most importantly for both initial re-enrollment and completion of a degree, low-income households seldom have the financial resources necessary to finance education. In the 2009-10 school year, the average yearly cost of full-time undergraduate tuition was $2,923 at a 2-year public institution, and $12,467 for a 4-year public institution (U.S. Department of Education, 2010). In contrast, the median transaction account balance for individuals in the bottom income quintile in 2009 was only $1,000, and the median of all non-housing financial assets combined was only $2,100 (Bricker, Bucks, Kennickell, Mach, & Moore, 2011). The median asset values are similar for those in the bottom wealth quintile as well: $900 in transaction accounts, and $1,900 in combined financial assets.

IDAs may contribute to increased education in four ways. The most straightforward way occurs if participants receive matching funds from the program to directly subsidize education expenditures. Similarly, these direct incentives may nudge participants to save more for education expenses than they would in the absence of the program. Even if participants do not receive a subsidy for or save for education costs, savings generated by the IDA that are intended for other uses but are not used for those purchases could be used to defray the costs of education, or to compensate for the opportunity costs associated with returning to school. Likewise, once those other expenses have been met using the subsidy, future savings that would have been used for them may be freed up and could be substituted as education spending. Finally, asset theory predicts that saving in IDAs should have positive psychological effects, increasing goal-setting and self-efficacy among participants (Sherraden, 1991). Although the hypothesized link between IDAs and these psychological outcomes has received little attention in the empirical literature, this mechanism could contribute to increased investment in human capital for IDA participants in the long-term.

Past findings on IDAs and education

To date, there are few studies on the impacts of IDAs on adult education. Research has focused primarily on non-experimental findings from the Assets for Independence program, the Canadian *learn*$ave experiment, and the short-term findings of the American Dream Demonstration.

The federal Assets for Independence (AFI) program has funded 611 state and local IDA programs, which have opened roughly 60,100 IDAs, between its inception and the close of fiscal year 2009 (U.S. Department of Health and Human Services, 2010). A non-experimental study comparing AFI participants to a comparison group drawn from the Survey of Income and Program Participation

found that AFI participation was associated with a greater likelihood of enrollment in postsecondary education (Mills et al., 2008b). Contrary to trends in the general population, however, the study also found that the treatment effect was strongest for participants with only a high school diploma at baseline.

One reason for the limited evidence regarding the effect of IDAs on education is that the most popular savings purpose in IDAs is homeownership (U.S. Department of Health and Human Services, 2010). The *learn*$ave experiment, which took place in Canada, was one of the few IDA programs focused primarily on education. Of the 3,584 participants in the experimental study, only a small proportion were permitted to save for investment in small business (Leckie, Hui, Tattrie, Robsen, Voyer, 2010). The rest saved specifically for adult post-secondary education expenses. Six months after the program ended, there was a significant treatment effect on enrollment in community college and university programs, as well as on educational program completion (Leckie et al., 2010).

The only large-scale experimental test of IDAs in the US thus far is part of the American Dream Demonstration, which included 14 IDA programs across the country. The experimental portion of the evaluation focused on an IDA program based at Community Action Program of Tulsa County (CAPTC) in Oklahoma; the other 13 programs were non-experimental. Using data gathered at the 48-month follow-up, just after the end of the program, Mills and colleagues (2008a) found no significant effect on enrollment in degree-granting or non-degree education programs. However, there was a marginally significant effect of treatment on enrollment in non-degree-granting courses among participants who were homeowners at baseline. Harris (2012) similarly found no significant effect of treatment assignment in ADD on participation in postsecondary education.

Zhan and Schreiner (2005), using data from all 14 ADD programs, (the experimental program in Tulsa and the 13 non-experimental programs), found that the factors predicting savings performance of participants who were saving for education differed from the factors predicting performance in the sample overall. Specifically, among all ADD participants, being female and being married were both associated with higher average monthly net deposits (AMND; total deposits less unmatched withdrawals, averaged across the savings period). However, among education savers, females saved less than males, and married participants saved less than unmarried participants. The authors attempt to explain this finding by suggesting that females and married participants face unique barriers to pursuing postsecondary education, such as child care responsibilities and TANF work restrictions, that they may not fully appreciate until after they have committed to saving for education, at which point their commitment to saving in the IDA is reduced. It is worth noting, however, that neither the number of children in the household nor TANF receipt had significant effects on AMND among education savers. Furthermore, because these analyses included participants across all ADD sites, it is unclear to what extent these results reflect the savings performance of participants at the experimental site in Tulsa.

To summarize, research found significant associations between participation in both AFI and *learn*$ave IDA programs and enrollment in postsecondary education programs. Past research on ADD found no overall effect of the program on enrollment in degree-granting programs and a marginal effect on enrollment in non-degree-granting courses for a subsample of participants.

Although past research on ADD found no significant effect on outcomes related to degree-granting educational programs, there are several reasons to suspect an effect of treatment on such outcomes in the longer term. First, past research on ADD relied on data from the 48-month follow-up, about a year after the program's end. While this may be an appropriate time to assess effects on enrollment in postsecondary education, it does not provide adequate time for the program to have an effect on longer-term outcomes such as degree completion or increases in educational level. Although treatment and control group members may enroll in education programs at equal rates during the program period, they may not persevere equally over time. The reason for this is related to a second observation: positive treatment effects on one form of asset, such as homeownership or the clearing of debt, immediately after the program may result in positive effects on other assets, such as education, later on. It is also possible that these earlier assets contribute to greater financial security for treatment group households, which may facilitate perseverance in education (for those who enrolled during the program period) or free up funds for educational pursuits in the future. These effects would not be detected until years later.

We use the latest wave of ADD data, Wave 4, to explore the impact of eligibility for IDA on educational outcomes 10 years after baseline (and 6 years after the end of the program). We focused on three key outcomes: the effect of assignment to a treatment consisting of access to an IDA and financial education on enrollment in education, completion of a degree or certificate, and increase in reported level of educational attainment at the 10-year follow up compared to that reported at baseline.

These three outcomes were chosen because each represents a unique aspect of educational attainment. If treatment has a significant effect on enrollment, the finding suggests that IDAs provided participants with the financial and/or psychological resources they needed to take the risk of returning to school. However, enrollment in an educational program does not necessarily translate to program completion, as evidenced by the low rates of completion among non-traditional students (Jacobs & King, 2002). IDAs could have an effect on enrollment without significantly increasing the likelihood of program completion. Lack of such an effect on completion would suggest that additional resources, not provided by the IDA program, are needed to facilitate program completion. Finally, an effect of treatment on enrollment and completion would not necessarily translate to an increase in educational attainment. For example, an individual who earned a second associate's degree would have enrolled in and completed a program, but would not have increased their level of education. Although investment in education yields returns in lifetime income and increased economic security, these returns seem to attach to educational levels or

credentials rather than to years of education. Therefore an increase in educational attainment is an additional key criterion in judging the efficacy of a program which subsidizes education as an asset.

Methods

Program details

As mentioned above, Individual Development Accounts (IDAs) are incentivized savings accounts designed to help low-income individuals save and invest in asset purchases (Sherraden, 1991). Their design is based on the institutional theory of asset building, which suggests that low-income populations face a variety of barriers to building wealth, including a lack of connections with mainstream financial services, a lack of knowledge about financial management, and perhaps most importantly, a lack of institutional incentives to save and build assets. IDAs provide a bundle of services to address these issues, including a low-cost, 3-year savings account that incentivizes savings by matching withdrawals that are used on qualified asset investments. In addition, IDA programs usually provide general case management services and financial education, which is often targeted to the particular asset purchase the participant plans to make (e.g., individuals saving for homeownership receive financial education about improving credit to prepare to purchase a home).

Data for this study come from the baseline and 6-year follow-up data collection of the ADD experiment in Tulsa, OK. ADD was the first large-scale test of IDAs in the United States and was implemented through 14 programs across the US. Only one program, based at the Community Action Program of Tulsa County (CAPTC), used a randomized experimental design and is the source of the data for this study.

Participants in the ADD experiment were randomized into either the treatment group, which was offered an IDA matched savings account, financial education, and case management services, or the control group, which was barred from participating in these services until the end of the four-year study period. Treatment group members had 36 months in which to save in their IDAs. Participants were expected to deposit at least $10 per month for at least 9 months per year. Six months after opening the account, they were free to begin making matched withdrawals for microenterprise start-up or investment, down payment on a home, home repair and improvement, or postsecondary education costs. They could continue making matched withdrawals for 6 months following the end of the three-year savings period. Participants who withdrew money for education costs were matched at a 1:1 rate, meaning they could double their money if they put it toward education. Participants who saved the maximum matchable amount ($750 per year) could save up to $4,500 to put toward education. Business purchases and home repair were matched at the same rates as education, while home purchases were matched at a rate of 2:1 (for a maximum of $6,750 to put toward home purchase). At the end of the program, remaining balances could be matched at a 1:1 rate and rolled over into a Roth Individual Retirement Account. All participants were expected to

participate in 12 hours of general financial education, as well as some asset-specific financial education.

Data

A total of 1,103 individuals volunteered to participate in the ADD experiment. After randomization, there were 537 treatment group members and 566 control group members. The baseline survey was administered prior to randomization. The wave 4 survey, used for these analyses, was administered 6 years after the end of the spending period, 10 years after randomization. At the wave 4 follow-up, 407 treatment group members and 448 control group members were surveyed, resulting in an overall response rate of 80.1% (855 in total) of the original sample. The analytic sample for this paper was limited to those respondents with non-missing data on necessary covariates (presented in Table 1). This reduced the analytic sample to 824 respondents.

Measures

Treatment. The primary independent variable is treatment assignment. All participants who were assigned to the treatment group are coded as 1 and included in the analysis, regardless of their level of participation in the IDA program. Control group members are coded as 0.

Gender. Gender was assessed at baseline. An indicator variable for female gender was created, for which women are coded as 1 and men are coded as 0.

Treatment by gender interaction. To test the interaction between treatment and gender, we created an interaction term by multiplying the treatment and gender variables for each respondent. The interaction variable therefore took on a value of 1 for female treatment group members, and a value of 0 for all other respondents.

Education level. At baseline and wave 4, respondents were asked to report their highest level of completed education using a 9-level categorical variable. Categories included "8th grade or less," "grade school, middle school, or junior high," "some high school," "high school diploma or GED," "some college," "graduate of a 2-year college," "graduate of a 4-year college," "some graduate school," and "completed graduate school." For the purposes of these analyses, the bottom three categories are combined into one category labeled "less than high school," and the remaining six categories were left intact.

Enrollment. At wave 4, respondents were asked whether they had been enrolled in school since baseline. This variable was coded 1 for enrollment and 0 for no enrollment.

Table 1. Descriptive Statistics on Baseline Covariates by Treatment and Control

	Treat (n = 392)	Control (n = 432)	Difference	p
Categorical Variables (proportions)				
Own a home	0.214	0.262	-0.047	0.112
Age				
Less than 25	0.151	0.125	0.026	0.288
25-35	0.347	0.370	-0.023	0.484
35-45	0.314	0.289	0.024	0.445
45-55	0.140	0.162	-0.022	0.385
55+	0.048	0.053	-0.005	0.756
Less than median income ($1,421/month)	0.474	0.495	-0.021	0.549
Female	0.793	0.806	-0.012	0.662
Banked	0.862	0.831	0.031	0.215
Race				
White	0.426	0.465	-0.039	0.258
Black	0.444	0.394	0.050	0.143
Other	0.130	0.141	-0.011	0.642
Married	0.270	0.266	0.004	0.892
Study Cohort				
Cohort 1-3	0.151	0.176	-0.025	0.325
Cohort 4-6	0.212	0.225	-0.013	0.657
Cohort 7-9	0.163	0.167	-0.003	0.895
Cohort 10-12	0.278	0.264	0.014	0.647
Cohort 13	0.196	0.169	0.027	0.308
Total Assets				
Less than $1421	0.227	0.227	0.000	0.995
$1422-$2841	0.117	0.097	0.020	0.350
$2842-$4262	0.092	0.088	0.004	0.846
$4263 and up	0.423	0.491	-0.067	0.053
Assets missing	0.140	0.097	0.043	0.055
Total Debt				
Less than $1421	0.194	0.201	-0.008	0.787
$1422-$2841	0.074	0.076	-0.002	0.896
$2842-$4262	0.064	0.060	0.004	0.831
$4263 and up	0.487	0.484	0.003	0.921
Debt missing	0.181	0.178	0.003	0.914
Housing unsubsidized	0.753	0.750	0.003	0.933
Health insurance	0.602	0.576	0.026	0.455
Own a business	0.077	0.063	0.014	0.428
Own other property	0.051	0.032	0.019	0.180
Have retirement savings	0.092	0.079	0.013	0.500

Table 1. Descriptive Statistics on Baseline Covariates by Treatment and Control (continued)

	Treat (n = 392)	Control (n = 432)	Difference	p
Categorical Variables (proportions)				
Welfare receipt	0.260	0.278	-0.018	0.570
Own a car	0.844	0.847	-0.003	0.910
Continuous Variables (means)				
Number of adults in household	0.48	0.53	-0.05	0.317
Number of children in household	1.75	1.64	0.11	0.250
Household goods ownership scale	2.71	2.70	0.01	0.960
Economic strain scale	0.56	0.56	-0.01	0.726
Giving help in the community scale	0.56	0.54	0.02	0.154
Getting help in the community scale	0.36	0.36	0.00	0.929
Satisfaction with health	0.86	0.86	0.00	0.971
Satisfaction with financial situation	0.63	0.60	0.03	0.450
Community involvement scale	0.40	0.41	-0.01	0.548

Notes: The asset and debts variables were categorized based on median monthly income ($1,421), such that categories represent values less than 1 month's income, 1-2 months' income, 2-3 months' income, and more than 3 months' income. Each of the seven scales listed at the bottom of the table have scores which range from 0 to 1.

Degree completion. Respondents who reported enrollment in school since baseline were asked whether they graduated from that program with a degree or certificate. An indicator variable was created which was coded as 1 if the respondent graduated from school and 0 if the respondent had never been enrolled in school or did not graduate.

Increase in education level. An indicator variable was created to reflect whether respondents had experienced an increase in education between baseline and wave 4. This variable was created by comparing values on the education level variable at both time points. If respondents moved to a higher category (e.g., from "some college" to "graduate of a 4-year college"), then they were coded as 1. If respondents remained in the same category, they were coded as 0. Respondents were coded as missing if they had the highest possible level of education at baseline (and therefore could not have increased their education by this measure; n = 20), or if they reported an education level at wave 4 that was lower than the one they reported at baseline (n = 102). Six respondents fall into both categories, having reported graduate-level education at baseline and a lower level of education at wave 4. It is also important to note that in some cases, the increase in education level variable is not consistent with the enrollment and degree completion variables. For example, 29.5% of those who said they received a degree did not report an increase in education (n = 73). This may be because their new degree was redundant; for example, they may have earned second bachelor's degree. In addition, 28.8% of respondents who increased education level did not report enrolling in school (n = 88).

Analysis

Past research with the wave 4 sample of the ADD study has demonstrated some minor but statistically significant differences between treatment and control groups at baseline (e.g., Grinstein-Weiss, Sherraden, Gale, Rohe, Schreiner, & Key, forthcoming). To address this issue, propensity score weighting was used in all analyses. Propensity scores were created by estimating a logistic regression model that predicted treatment assignment from a variety of baseline covariates. These predictors included baseline homeownership, age, income, gender, education level, banked status, race, marital status, total assets, total debts, household size, welfare receipt, property ownership (e.g. cars, businesses, etc), and scale measures of community involvement and support (a full list of covariates are presented in Table 1). The model was then used to predict each person's probability of being assigned to the treatment group, based on the characteristics listed above. Finally, average treatment effect weights were created for the treatment group by taking the inverse of the probability of treatment assignment, and for the control group by taking the inverse of 1 minus the probability of treatment assignment (Guo & Fraser, 2010). These weights were applied in all regression models described below.

We conducted bivariate comparisons between treatment and control group members on baseline covariates using tests of proportions for categorical variables, and t-tests for continuous variables. Similar bivariate comparisons were conducted to compare the groups on baseline and wave 4 education levels, as well as the primary educational outcomes – enrollment, degree completion, and increased education. For the education outcomes, bivariate tests were repeated using the propensity score weights. The "tabstat" command in Stata 10.1 was used to generate weighted proportions on each variable for both groups, and simple OLS regressions with propensity score weights were used to determine significance levels of the weighted comparisons (StataCorp, 2009a).

To test the effect of treatment on each of the education outcomes, propensity score-weighted marginal probit regression was used. Each regression model controlled for the same covariates, which were entered into the propensity score, including baseline education level. In order to test the potential interaction between treatment and gender, the interaction term was added to the model for each education outcome. For each outcome, the two models (with and without interaction) were compared using a Wald test, in order to determine whether the addition of the interaction term significantly improved model fit. For outcomes on which the interaction effect was significant, the command "margins" was used in Stata 11.0 to estimate the marginal effect of treatment on males or females, and the significance of that effect, when all other covariates are held at the mean (StataCorp, 2009b). The same command was also used to generate predicted probabilities of positive education outcomes for male and female members of each treatment group. Throughout analyses, we use a p-value of .10 to test for significance.

Results

Descriptives and bivariate comparisons

Baseline covariate descriptive statistics are presented in Table 1. The majority of both treatment and control groups fall between 25 and 45 years old. Roughly 80% of both groups are female. In both groups, around 40% of participants are Black, an additional 40% are White, and the remaining participants are some other race. Only one-quarter of both groups are married. Over 40% of both groups reported having assets valued at greater than $4,263, but control group members were slightly more likely to fall into this high asset category ($p = 0.053$). Treatment group members were more likely to have missing data on assets at baseline ($p = 0.055$). Along with high levels of assets for some participants, there were also high levels of debt for a large proportion of participants. Nearly 50% of both groups reported debts of $4,263 or more. The majority of the sample had some material advantages. Three-quarters of participants reported living in unsubsidized housing, roughly 60% reported that they had health insurance, and 84% owned cars at baseline. Nevertheless, around one-quarter of participants received some public assistance at baseline. Among the covariates reported here, treatment and control group members differed only with regard to assets, as noted above.

Table 2 presents descriptive statistics on education variables of interest. On the left side of the table are unadjusted proportions for treatment and control. The right panel includes propensity score-weighted proportions. At baseline, the largest proportion of both groups reported having some college education (treatment: 41%, control: 42%). The next largest groups were high school graduates (treatment: 26%, control: 25%), and graduates of 2-year colleges (treatment: 14%, control: 13%). At wave 4, respondents with some college education remain the largest proportion of both treatment (37%) and control (33%) groups. However, the proportion of respondents with only a high school degree or only a 2-year degree are reduced compared to baseline, with the result that 4-year college graduates become the second-largest group at wave 4 (treatment: 23%, control: 26%). Bivariate comparisons reveal no significant differences between treatment and comparison group by education level at either baseline or wave 4. Results are the same in unweighted and propensity score-weighted comparisons, although the p-values of weighted comparisons are larger.

With regard to educational outcomes, roughly half of both groups reported enrolling in school since baseline, although a larger proportion of treatment group members enrolled (treatment: 52%, control: 45%). Propensity score-weighted bivariate comparisons suggest that the difference between the two groups is marginally significant ($p = 0.108$). A slightly larger proportion of treatment group members also reported receiving a degree or certificate (treatment: 35%, control: 30%). This difference was not significant in either the unadjusted bivariate comparisons ($p = 0.182$), nor in the propensity score weighted comparisons ($p = 0.220$). Roughly the same proportion of both groups reported increased education compared to baseline (treatment: 45%, control: 41%), and this was reflected in non-significant bivariate comparisons.

Table 2. Descriptive Statistics on Education Variables at Baseline and Wave 4, with and without Propensity Score Weighting

		Without Propensity Score				With Propensity Score Weighting			
	N	Treat	Control	Difference	p	Treat	Control	Difference	p
Baseline education									
Less than H.S.	824	0.07	0.07	0.01	0.701	0.07	0.07	0.00	0.941
H.S. or GED	824	0.26	0.25	0.01	0.861	0.26	0.26	0.00	0.969
Some college	824	0.41	0.42	-0.01	0.702	0.42	0.41	0.00	0.978
2-year college graduate	824	0.14	0.13	0.01	0.721	0.14	0.14	0.00	0.998
4-year college graduate	824	0.07	0.08	0.00	0.799	0.07	0.08	0.00	0.965
Some graduate school	824	0.02	0.03	-0.01	0.306	0.02	0.02	0.00	0.987
Graduate degree	824	0.03	0.02	0.01	0.501	0.02	0.02	0.00	0.987
Wave 4 outcomes									
Enrolled in education program	824	0.52	0.45	0.06	0.066	0.51	0.45	0.06	0.108
Received a degree	824	0.35	0.30	0.04	0.182	0.34	0.30	0.04	0.220
Increased education	707	0.45	0.41	0.04	0.346	0.44	0.42	0.02	0.612
Wave 4 education									
Less than H.S.	823	0.08	0.07	0.01	0.693	0.08	0.08	0.00	0.853
H.S. or GED	823	0.20	0.20	0.00	0.925	0.20	0.20	0.00	0.966
Some college	823	0.37	0.33	0.03	0.318	0.37	0.33	0.04	0.295
2-year college graduate	823	0.04	0.04	0.00	0.931	0.04	0.04	0.00	0.979
4-year college graduate	823	0.23	0.26	-0.03	0.362	0.23	0.27	-0.03	0.280
Some graduate school	823	0.04	0.03	0.01	0.526	0.05	0.03	0.01	0.398
Graduate degree	823	0.03	0.05	-0.02	0.157	0.03	0.05	-0.02	0.193

The likelihood of each educational outcome by baseline education level is shown in Table 3. Numbers in the table are unweighted proportions of people at each education level who enrolled in school, completed a degree, and/or increased their education. When interpreting these proportions, it is important to cross-reference them with the relative proportions of each education level that were present at baseline. For example, while 22.4% of respondents with less than a high school degree enrolled in school, this group made up only 7% of the total sample at baseline. That being said, there appears to be roughly linear relationship between baseline education level and enrollment and degree completion. The exception is the group of respondents in the top education category, who already have a graduate degree at baseline. Over half of respondents with at least some college education (including those with 2- or 4-year degrees) reported enrolling in school since baseline. About one-third of those with only high school degrees, and those with graduate degrees, enrolled. Even among those with less than a high school degree, more than one-fifth enrolled in school.

Degree receipt shows a trend similar to enrollment, wherein respondents with higher levels of education at baseline are more likely to receive a degree, with the exception of respondents who already had a graduate degree. Because degree receipt was contingent on enrollment, the proportion

who enrolled serves as an upper bound on the proportion who received a degree. Fully 64.7% of respondents with some graduate education reported receiving a degree. Degree completion rates were lower for other groups, with one-fifth of high school graduates receiving a new degree, and 12.1% of those with less than a high school degree receiving a new credential.

Increase in education is measured separately from enrollment and degree completion, and shows a different pattern. Respondents with a 2-year degree at baseline were most likely to report an increase in education level, with 88.9% having a higher level of education at wave 4. Within every baseline education level, more than 25% of respondents reported an increased education level at wave 4.

Table 3. Proportion who Enrolled in School, Received a Degree, and Increased Education, by Baseline Education Level

	Enrolled	Received a Degree	Increased Education
Less than high school	0.224	0.121	0.263
High school or GED	0.357	0.219	0.419
Some college	0.556	0.374	0.350
2-year college degree	0.605	0.421	0.889
4-year college degree	0.508	0.365	0.304
Some grad school	0.765	0.647	0.615
Graduate degree	0.350	0.200	---
N	824	824	707

Note: The increase in education variable is missing for individuals who had the highest possible education level at baseline, and for those who reported lower levels of education at Wave 4 than at baseline.

Regression analyses

Propensity score-weighted marginal probit regression analyses were conducted to estimate the effect of treatment on education outcomes. These results are reported in Table 4. Treatment has a significant positive effect on enrollment in school ($p = 0.062$). The marginal effect is 0.071, indicating that after controlling for all covariates, the proportion of treatment group members who enrolled in education is 7.1 percentage points higher than the proportion of control group members who enrolled. Results of the models predicting degree completion and increased education also estimate positive effects of treatment, although the effects are not statistically significant (degree completion: $p = 0.156$; increased education: $p = 0.236$).

Table 4. Propensity Score-Weighted Marginal Probit Regressions Predicting Wave 4 Educational Outcomes

	Enrollment			Degree Completion			Increased Education		
	dF/dx	se	p	dF/dx	se	p	dF/dx	se	p
Treatment Status	0.071	0.038	0.062	0.048	0.034	0.156	0.050	0.042	0.236
Homeownership	-0.069	0.057	0.229	-0.033	0.049	0.509	-0.048	0.065	0.468
Age (less than 25)									
25-35	-0.173	0.063	0.007	-0.181	0.047	0.000	-0.273	0.061	0.000
35-45	-0.380	0.056	0.000	-0.277	0.042	0.000	-0.335	0.058	0.000
45-55	-0.298	0.060	0.000	-0.230	0.040	0.000	-0.433	0.043	0.000
55+	-0.468	0.032	0.000	-0.308	0.022	0.000	-0.422	0.037	0.000
Less than median income	-0.049	0.045	0.275	-0.033	0.038	0.390	-0.053	0.048	0.271
Female	0.230	0.051	0.000	0.099	0.044	0.036	-0.053	0.062	0.391
Baseline education (less than H.S)									
H.S. grad	0.111	0.086	0.200	0.082	0.091	0.356	0.092	0.091	0.308
Some college	0.308	0.079	0.000	0.234	0.086	0.006	-0.031	0.090	0.728
2-year college graduate	0.388	0.073	0.000	0.322	0.105	0.002	0.563	0.054	0.000
4-year college graduate	0.332	0.086	0.001	0.316	0.118	0.007	-0.052	0.118	0.661
Some graduate school	0.500	0.043	0.000	0.601	0.087	0.000	0.294	0.157	0.096
Graduate degree	0.215	0.134	0.138	0.139	0.158	0.353			
Bank account ownership	0.000	0.057	0.997	0.061	0.048	0.220	0.028	0.062	0.651
Race (white)									
Black	0.073	0.046	0.111	0.093	0.041	0.023	0.063	0.051	0.219
Other	0.068	0.063	0.281	0.084	0.058	0.134	0.120	0.067	0.075
Married	0.089	0.055	0.104	0.008	0.049	0.872	0.005	0.061	0.937
Study Cohort (1-3)									
Cohort 4-6	0.049	0.063	0.431	0.022	0.057	0.701	0.080	0.070	0.254
Cohort 7-9	-0.001	0.068	0.988	-0.030	0.056	0.603	-0.029	0.073	0.694
Cohort 10-12	-0.052	0.060	0.382	-0.034	0.052	0.517	-0.036	0.070	0.608
Cohort 13	0.021	0.064	0.743	-0.001	0.058	0.993	0.116	0.076	0.127
Total assets (less than $1421)									
$1422-$2841	0.079	0.079	0.317	0.047	0.072	0.506	0.062	0.081	0.443
$2842-$4262	0.013	0.081	0.870	0.003	0.073	0.965	-0.029	0.087	0.739
$4263 and up	0.006	0.068	0.928	0.010	0.059	0.865	0.068	0.074	0.357
Assets missing	0.052	0.081	0.522	-0.027	0.070	0.704	0.004	0.086	0.958
Total debt (less than $1421)									
$1422-$2841	-0.063	0.081	0.444	-0.035	0.071	0.635	-0.149	0.083	0.095
$2842-$4262	0.067	0.090	0.454	0.103	0.087	0.219	0.073	0.098	0.453
$4263 and up	-0.029	0.057	0.610	0.019	0.052	0.716	0.033	0.062	0.590
Debt missing	0.045	0.065	0.491	0.092	0.062	0.126	0.077	0.071	0.278

Table 4. Propensity Score-Weighted Marginal Probit Regressions Predicting Wave 4 Educational Outcomes (continued)

	Enrollment			Degree Completion			Increased Education		
	dF/dx	se	p	dF/dx	se	p	dF/dx	se	p
Number of adults in the household	-0.054	0.032	0.092	-0.076	0.028	0.006	-0.045	0.035	0.190
Number of children in the household	-0.012	0.018	0.488	-0.008	0.015	0.616	-0.041	0.020	0.041
Housing unbsubsidized	0.067	0.052	0.199	0.053	0.044	0.238	0.040	0.059	0.492
Health insurance	-0.022	0.042	0.610	-0.053	0.038	0.161	0.010	0.047	0.837
Own a business	0.075	0.083	0.370	-0.001	0.076	0.985	-0.092	0.084	0.290
Own other property	0.004	0.099	0.967	0.003	0.089	0.972	-0.071	0.104	0.510
Have retirement savings	-0.064	0.069	0.357	-0.062	0.058	0.311	-0.049	0.082	0.550
Welfare receipt	0.026	0.046	0.565	0.007	0.040	0.863	0.098	0.051	0.055
Own a car	0.128	0.062	0.044	0.101	0.050	0.063	-0.037	0.072	0.608
Household goods ownership scale	-0.008	0.010	0.382	-0.005	0.008	0.590	0.012	0.011	0.279
Economic strain scale	0.024	0.088	0.784	0.093	0.079	0.239	0.027	0.098	0.779
Giving help in the community scale	-0.017	0.116	0.883	-0.178	0.104	0.087	-0.038	0.133	0.773
Getting help in the community scale	0.102	0.111	0.359	0.276	0.098	0.005	0.081	0.127	0.521
Satisfaction with health	0.083	0.057	0.153	0.039	0.048	0.426	0.111	0.061	0.079
Satisfaction with financial situation	0.127	0.045	0.006	0.077	0.039	0.057	0.134	0.051	0.010
Community involvement scale	0.003	0.101	0.978	0.050	0.088	0.566	0.245	0.111	0.027
N	824			824			707		

Notes: Results of propensity score-weighted marginal probit regression. Reference groups for categorical variables are listed in parentheses.

To test potential interaction effects between gender and treatment, an interaction term was added to each model, and the two forms of the model (with and without interaction) were compared using a Wald test to see if the inclusion of the interaction term significantly improved the fit of the model. Table 5 reports regression results for each of the outcomes after including the interaction term. The interaction was not significant in predicting enrollment ($p = 0.819$), and this conclusion was supported by the Wald test, which indicated that the addition of the interaction term did not significantly improve model fit ($p = 0.818$). The findings with regard to degree completion were similar. The interaction term was not a significant predictor of degree completion ($p = 0.395$), and the Wald test indicated no improvement in model fit ($p = 0.395$). Results differed for increase in education, however. When the female by treatment interaction term was added to the model, the Wald test indicated that model fit was significantly improved ($p = 0.005$). Furthermore, the treatment effect reported in Table 5 became quite large and significant ($dF/dx = 0.282$, $p = .002$). It is important to note that, due to the inclusion of the interaction term, this is the effect of treatment on men only. The interaction term was similar in magnitude, but negative, and also highly

significant (dF/dx = -0.288, p = 0.005). The marginal effect of treatment on females, controlling for covariates, is the sum of the treatment and interaction effects reported in Table 5, bearing in mind that the Table 5 presents exponentiated coefficients (Treatment effect among women= -0.012, p = 0.805). The overall treatment effect is the weighted average of the treatment effects on men and on women. In the regression predicting the effect of treatment assignment on increase in education that includes the female*treatment interaction term, the overall marginal treatment effect is 0.051 (p=0.233).

Figure 1 illustrates the marginal effect of treatment on increased education for males and females, holding all other covariates at the mean. Taken together, these estimates suggest that treatment had a strong positive effect on increased education in males, but no effect on females. While male control group members have slightly lower rates of increased education than females in both groups, male treatment group members experience a much higher incidence of increased education than all other groups.

In addition to the interaction term presented here, the authors evaluated the effect of IDA on education on sub-groups using a sub-sampling approach. In a sub-sampling approach, the treatment effect is evaluated separately in the two groups, then compared. Whereas the interaction term estimates the effect of control variables jointly, the sub-group approach does so separately for each group, sometimes affecting the results. Using the sub-sample approach, we found that the treatment effect on both degree completion and increase in educational level among men was significantly stronger than the treatment effect observed on the sub-sample of women. Similarly, regressions performed without propensity score weights produced results consistent with those performed with the weights.

Results of regression analyses in Table 4 show that females are significantly more likely than males to enroll and to complete a degree, suggesting that the gender difference in treatment effect on increased education is not due to lower overall rates of education activity among females. Because 2-year college graduates and those with some graduate education at baseline were the groups most likely to increase in education, we compared males and females on these categories of education. Females were actually more likely to have 2-year college degrees at baseline (15.2% of females compared to 8.5% of males, $X^2(1) = 4.95$, p = 0.026). Males and females were equally likely to have some graduate education at baseline (2.1% of females and 1.8% of males, $X^2(1) = 0.061$, p = 0.805). There was also no difference by gender in the likelihood of saving for education (7.2% of females and 6.5% of males, $X^2(1) = 0.052$, p = 0.820). However, men were slightly more likely to make a matched withdrawal for education (5.1% of females compared to 11.7% of males, $X^2(1) = 4.33$, p = 0.038).

Table 5. Propensity Score-Weighted Marginal Probit Regressions Predicting Wave 4 Educational Outcomes, with Treatment by Gender Interaction

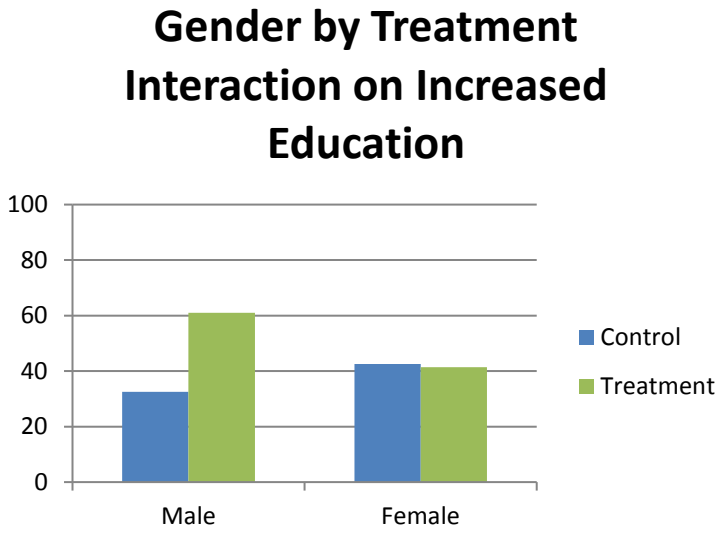
	Enrollment			Degree Completion			Increased Education		
	dF/dx	se	p	dF/dx	se	p	dF/dx	se	p
Treatment status	0.090	0.092	0.330	0.112	0.081	0.170	0.282	0.088	0.002
Homeownership	-0.069	0.057	0.227	-0.033	0.049	0.506	-0.056	0.065	0.397
Age (less than 25)									
25-35	-0.173	0.063	0.007	-0.181	0.047	0.000	-0.276	0.061	0.000
35-45	-0.380	0.056	0.000	-0.277	0.042	0.000	-0.332	0.058	0.000
45-55	-0.298	0.060	0.000	-0.229	0.040	0.000	-0.430	0.043	0.000
55+	-0.468	0.032	0.000	-0.308	0.021	0.000	-0.426	0.035	0.000
Less than median income	-0.048	0.045	0.282	-0.031	0.038	0.415	-0.043	0.049	0.378
Female	0.241	0.067	0.001	0.135	0.057	0.034	0.103	0.079	0.202
Baseline education (less than H.S)									
H.S. grad	0.113	0.086	0.194	0.087	0.091	0.328	0.104	0.091	0.253
Some college	0.309	0.079	0.000	0.237	0.085	0.006	-0.026	0.091	0.773
2-year college graduate	0.389	0.073	0.000	0.327	0.105	0.002	0.572	0.053	0.000
4-year college graduate	0.333	0.086	0.001	0.319	0.118	0.006	-0.053	0.119	0.658
Some graduate school	0.500	0.043	0.000	0.597	0.088	0.000	0.273	0.164	0.131
Graduate degree	0.218	0.134	0.134	0.150	0.159	0.319			
Bank account ownership	-0.001	0.057	0.992	0.061	0.048	0.222	0.025	0.062	0.693
Race (white)									
Black	0.073	0.046	0.111	0.094	0.041	0.023	0.068	0.051	0.186
Other	0.068	0.063	0.281	0.085	0.058	0.132	0.122	0.068	0.072
Married	0.090	0.055	0.102	0.010	0.049	0.843	0.009	0.061	0.880
Study Cohort (1-3)									
Cohort 4-6	0.049	0.063	0.434	0.021	0.057	0.711	0.082	0.070	0.241
Cohort 7-9	-0.001	0.068	0.993	-0.029	0.057	0.616	-0.019	0.074	0.801
Cohort 10-12	-0.053	0.060	0.379	-0.036	0.052	0.505	-0.037	0.070	0.596
Cohort 13	0.021	0.064	0.748	-0.002	0.058	0.978	0.113	0.077	0.139
Total assets (less than $1421)									
$1422-$2841	0.079	0.079	0.319	0.046	0.072	0.516	0.051	0.081	0.528
$2842-$4262	0.012	0.081	0.878	0.001	0.073	0.984	-0.038	0.086	0.658
$4263 and up	0.007	0.068	0.918	0.013	0.059	0.831	0.085	0.074	0.255
Assets missing	0.053	0.081	0.517	-0.026	0.070	0.718	0.011	0.086	0.901
Total debt (less than $1421)									
$1422-$2841	-0.063	0.081	0.443	-0.035	0.071	0.629	-0.153	0.082	0.085
$2842-$4262	0.067	0.090	0.456	0.102	0.088	0.225	0.070	0.099	0.476
$4263 and up	-0.029	0.057	0.609	0.019	0.052	0.716	0.031	0.062	0.611
Debt missing	0.045	0.065	0.488	0.094	0.062	0.121	0.084	0.071	0.236

Table 5. Propensity Score-Weighted Marginal Probit Regressions Predicting Wave 4 Educational Outcomes, with Treatment by Gender Interaction (continued)

	Enrollment			Degree Completion			Increased Education		
	dF/dx	se	p	dF/dx	se	p	dF/dx	se	p
Number of adults in the household	-0.054	0.032	0.091	-0.077	0.028	0.006	-0.046	0.034	0.178
Number of children in the household	-0.012	0.018	0.508	-0.006	0.016	0.688	-0.037	0.020	0.066
Housing unbsubsidized	0.067	0.052	0.197	0.054	0.043	0.230	0.048	0.058	0.414
Health insurance	-0.021	0.042	0.610	-0.053	0.038	0.160	0.014	0.047	0.762
Own a business	0.073	0.083	0.379	-0.005	0.076	0.945	-0.111	0.086	0.217
Own other property	0.003	0.099	0.975	0.001	0.089	0.991	-0.088	0.100	0.397
Have retirement savings	-0.064	0.069	0.355	-0.062	0.058	0.312	-0.050	0.082	0.547
Welfare receipt	0.026	0.046	0.571	0.005	0.040	0.895	0.097	0.051	0.057
Own a car	0.128	0.062	0.044	0.101	0.050	0.063	-0.036	0.071	0.610
Household goods ownership scale	-0.008	0.010	0.382	-0.004	0.008	0.596	0.012	0.011	0.266
Economic strain scale	0.024	0.088	0.783	0.093	0.079	0.236	0.031	0.098	0.750
Giving help in the community scale	-0.018	0.116	0.876	-0.182	0.104	0.080	-0.052	0.135	0.700
Getting help in the community scale	0.102	0.111	0.359	0.275	0.098	0.005	0.074	0.128	0.562
Satisfaction with health	0.083	0.057	0.151	0.040	0.048	0.422	0.112	0.061	0.076
Satisfaction with financial situation	0.127	0.046	0.006	0.078	0.039	0.054	0.138	0.051	0.008
Community involvement scale	0.004	0.101	0.972	0.054	0.088	0.536	0.254	0.111	0.023
Female x Treatment	-0.024	0.102	0.819	-0.076	0.088	0.395	-0.288	0.095	0.005
N	824			824			707		

Note: Regression models are identical to those in Table 3, with the addition of the treatment by gender interaction. Reference groups for categorical variables are listed in parentheses.

Figure 1. Predicted probabilities of increased education for males and females in treatment and control groups



Discussion

This paper provides the first empirical evidence from a randomized, longitudinal experiment on the long-term impacts of IDAs on educational outcomes, including enrollment in an educational program, degree completion, and increased education level, among low-income adults. We find that only 7.1% (n=25) of treatment group participants intended to save for education and about 6.5% (n=23) took a matched withdrawal for educational uses. Even with this small group saving for education, we find a significant impact on education enrollment 10 years after baseline assignment (6 years post program completion). These results are similar to results from *learn*$ave, a randomized IDA experiment in Canada.

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

Third, our sample size may be too small, and therefore the power too weak to detect an impact. For example, the estimated treatment effect on degree completion is 4.8 percentage points, but it is not statistically significant. The required sample size to have a power of 0.8 at the observed difference on degree completion would be about 4,800 cases (2,556 for the treatment group and 2,224 for

control group). Finally, the structure of the Tulsa IDA program, which allowed for five different qualified uses, could make effects even harder to detect. In this study, only 8.3% of the IDA treatment group reported saving for education. As a comparison, with IDAs more focused on education and with a larger sample size, the *learn*$ave experiment yields more definite positive impacts on education, including strong impacts on enrollment and program completion.

Our study also examines the interaction between gender and treatment assignment. We find that assignment to the treatment had a strong positive effect on increased educational attainment for males, but not for females. The effect size is also quite large; while females in the treatment and control groups increased their educational level at similar rates over the study period, males in the treatment group were twice as likely to increase their education level compared with males in the control group. Also worth mentioning, males were also more likely than females to take a matched withdrawal for education. This result suggests that males may benefit more from the IDA program in terms of educational attainment. This is an important finding given that there is a disturbing current trend in the United States of minority and lower-income males declining in educational attainment (Kim, 2011; King, 2000). Our current data cannot assess through what channels IDAs may have this effect, and this is an important question for future research.

In conclusion, our findings suggest that IDAs may increase educational enrollment for lower-income households and improve educational attainment, especially for males. It may be that using IDAs for educational purposes is easier and more accessible compared to, for example, buying a home, and can be done incrementally over a longer period of time. The results suggest that IDAs may be a desirable policy strategy to help increase education among low-income adults.

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