Human Capital Complementarities in Wealth Production

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Abstract

The experimental and quasi-experimental literature finds few positive effects of financial education on financial behavior and wealth. I posit that the lack of measurable effects may result from the underlying relationship between financial literacy and asset accumulation. Theoretical literature suggests that the effect of financial education and traditional education are complements in the wealth production function, so the effect of financial education is conditional on years of schooling. To empirically test this theoretical hypothesis, I provide the first experimental evidence on the elasticity of substitution between traditional schooling and financial education in terms of wealth production in this paper. I am able to credibly identify this elasticities with data from the Learn$ave IDA program, a randomized control trial that exogenously shifted the costs of these inputs to wealth production. Using a dual translog cost function specification, I estimate an iterated 3SLS strategy to measure individual elasticities of substitution between wealth production inputs. The mean value of all of the individual estimated elasticities of substitution is greater than zero, indicating that, on average, the inputs are substitutes

I. Introduction

Financial education is currently a hot topic in both the political and academic worlds. Over the past several years, an increasing number of states have mandated that financial literacy be a core part of the high school curriculum (Brown et. al, 2013), and there are calls to begin financial education programs as early as primary school (Schwartz, 2013). The objective of these measures is to increase the rate and amount of asset accumulation through an increase in the financial literacy level of the participants. These changes to asset accumulation are expected to increase lifetime wealth accumulation (Delavande et. al, 2008), the focus of many public programs.

The empirical findings on the causal relationship between financial literacy, developed at some point in the life-cycle through formal and informal financial education, and wealth accumulation are mixed. Much of the empirical evidence on the relationship between financial literacy and financial outcomes is suggestive of a large causal effect. A number of studies find that financial literacy is strongly correlated with greater wealth (Lusardi and Mitchell 2006, 2007; Van Rooij at al., 2007; Alessie et. al, 2011). Indeed, there is evidence to suggest that financial sophistication is associated with higher-return investments. Lusardi (2003) and Ameriks et. al (2003) show that financial education is related to a higher level of financial market participation, including stock ownership. Clark and Schieber (1998) find that financial education increases participation rates and contributions in pension plans, and Lusardi and Mitchell (2009) show that financial literacy is related to a higher-degree of retirement preparation. However, these findings are based on observational data and, as a whole, condition the effect of financial literacy only on observable characteristics. There likely exist unobservable characteristics that are correlated with both financial literacy and wealth, such as individual ability or discount rates. The existence of such factors would induce omitted variable bias.

Despite the large number of suggestive correlations, there exists no definitive empirical evidence to date which confirms the hypothesis that financial education leads to behavior that increases asset accumulation (Gale and Levine, 2011; Hathaway and Khatiwada, 2008; Willis, 2009). Fernandes et. al (2013) compile 168 papers, many of which are observational, and conduct a meta-analysis of the short- and long-term effects of financial education and financial literacy. Unlike the aforementioned analyses, the authors find that financial education explains little of the variation in financial behaviors and outcomes. In fact, experimental and quasi-experimental evidence suggests that financial education has no effect on asset accumulation (Agarwal, 2009; Cole and Shastry, 2008; Engelhardt et. al, 2013; Mandell and Klein, 2009) and may increase debt accumulation in some circumstances (Bell et. al, 2009; Brown et. al, 2013). Consequently, there are fundamental doubts that financial literacy actually increases wealth.

The failure of financial education to produce positive effects on savings behaviors and wealth in the experimental and quasi-experimental data may result from the fact that these results are, in general, average treatment effects for low-income, low-asset populations.[1] Theoretical work posits that greater financial knowledge will increase expected returns on investment, but that the total level of this increase in income depends upon wealth that can be invested (Delavande et. al, 2008; Jappelli and Padula, 2013). If the efficacy of financial education is conditional on wealth in the way these models suggest, the expected effects of even a large amount of financial education on low-wealth individuals may be small.

In this paper, I test the theoretical prediction that the effect of financial education is dependent upon individual wealth. The ideal way to test the this impact would be to randomly

[1] It is natural that these individuals should be the primary focus of many of these studies, as they are the focus of many public programs that aim to increase lifetime wealth accumulation for those on the lower end of the skill and income distribution

assign hours of financial education to new lottery winners and measure the effect of each hour on asset and debt outcomes. In the absence of such an experiment, I exploit the Learn$ave Individual Development Program (IDA) program, a randomized control trial, to credibly identify the relationship between financial education and years of traditional schooling in the wealth production function. The Learn$ave experiment exogenously decreased the costs, and consequently increased the consumption, of both financial education and traditional schooling for members of its treatment groups. It is well established that education is positively correlated with earnings and that each additional year of schooling has positive returns in the labor market.[2] In addition, cognitive skills developed through traditional education are also shown to be related to lifetime wealth (McArdle et. al, 2009; Smith et. al, 2010).[3] As a result, the increase in schooling for members of the treatment groups can be thought of as an increase in wealth that can be invested.

Accordingly, if the assumptions made in the theoretical literature hold true in practice, human capital and financial education are complements in the production of lifetime wealth. I test this hypothesis by exploiting the duality of the translog cost function.[4] I use randomly assigned treatment status as instruments for the cost of traditional and financial education with an iterated 3SLS strategy to consistently identify the nature of the relationship between the inputs and determine whether they are complements or substitutes. I find that the estimated mean elasticity of schooling with respect to financial education is about 1.7. While the elasticity of substitution

[2] In aggregate, the literature that measures the returns to schooling typically finds estimated wage increases in the 5 to 15 percent range for each additional year of school completion (Card, 1999).
[3] Numeracy is particularly important; empirical evidence shows that accuracy of responses to simple mathematical questions is a strong predictor of total wealth and investment wealth (Lusardi, 2012; Christelis et al., 2010; McArdle et. al, 2009).
[4] I assume that the optimization problem facing the individual is to choose inputs so that the costs of wealth production are minimized, given input prices, the level of wealth, and the form of the production function. The solution to this optimization problem yields a cost function that is dual to the wealth production function.

is not constant across individuals, the mean value indicates that the inputs are substitutes in terms of wealth production.

The remainder of the paper is structured as follows: Section II describes the experimental design, data, and the experiment's financial education and traditional schooling components. My estimation strategy and results are contained in Section III, and Section IV concludes.

II. Experimental Design

A. Program Characteristics

I use data from a field experiment conducted in Canada as part of the Learn$ave IDA program, funded in 2000 by Human Resources and Skills Development Canada (HRSDC). The program was designed by Social and Enterprise Development Innovations (SEDI) and implemented as a randomized control trial, in partnership with local community-based organizations, in Halifax, Toronto, and Vancouver. To be eligible for the Learn$ave program, applicants had to have household income at or below 120% of the Low-Income Cut-off (LICO). The LICO is a measure of poverty that varies by local area and household size. In the early 2000's, when the participants were selected, the LICO for a household of three was $36,000 in Toronto and Vancouver, and $31,000 in Halifax. Applicants could not have liquid assets that exceeded the lesser of $3,000 or 10% of annual income, and the value of the householder's home could not exceed the median value of the homes in the area. Program participation was restricted to those between 21 and 65 years of age because the primary focus of the demonstration was adult education and small business development.

Participants were assigned with equal probability into one of three groups: control, Treatment, and Treatment-plus. Members of both the Treatment and Treatment-plus groups were provided with a 3:1 match credit for every dollar deposited in their Learn$ave accounts during the three years after program enrollment. They were encouraged to save on a regular basis by the requirement that participants had to make net deposits of at least $10 in twelve (not necessarily consecutive) months before their withdrawals would qualify for these matched credits. Participants received a match for deposits up to $250 per month, and up to $1,500 in total. The earned match credits were held in trust until program participants withdrew them for approved expenses.[5] Both treatment groups received case management services, common to other IDA programs, which were designed to reinforce participants' savings goals. These services included help filling out forms and applications, a review of monthly account statements, other reminders to fulfill program goals and take advantage of program benefits, and help in fulfilling the requirements to access program benefits. Case management can be understood as very basic financial planning assistance. In addition to access to matching credits and case management services, the Treatment-plus group also was eligible for financial education training. The financial education training consisted of a fifteen-hour curriculum that was developed by SEDI and administered by local site staff, discussed later in Section II C. Control group participants were not eligible for the Learn$ave account's match credits, case management, or financial education, but were interviewed in follow-up surveys.

Upon enrollment, program participants were assigned to either the education program stream or the microenterprise program stream according to their specific saving goal, as indicated on their application forms. Those in the microenterprise stream were allowed to use their savings

[5] The accounts were held with RBC Royal Bank, which was selected by SEDI as the lead partnering financial institution. The Learn$ave accounts were deposit accounts with very low fees, limited account features, and earning very low interest. When they exited the project, participants were able to convert their Learn$ave accounts into regular deposit accounts within RBC Royal Bank.

and match credits for either education or starting a small business. Participants in the education stream were required to use their savings and match credits for education or training expenses.

The Learn$ave program took place over nine years. From June 2001 to December 2003, participants were recruited and screened. During the recruitment stages of the program, if an applicant was deemed eligible for enrollment, the individual was contacted for the baseline survey interview. The follow-up survey was conducted 54 months after the participant's enrollment in the program, the last month in which participants were able to use the matched credits. The last applicants were enrolled in February 2004, the last participant's saving period ended in February 2004, and the last participant's cash-out period ended in February 2008.

B. Data

Table 1 presents demographic and economic characteristics as recorded during the baseline interview. [6] The first column of Table 1 shows characteristics of the control group, while the second column shows the characteristics of the combined treatment groups, Treatment and Treatment-plus. Column 3 displays the difference between Columns 1 and 2, and suggests that randomization between the control group and the pooled treatment groups was effectively implemented. Relative to the control group, pooled treatment group members were significantly more likely to have graduated from high school, although they were not significantly more likely to have completed additional years of education beyond high school. Although there were no

[6] Learn$ave participants' average age was approximately 35 years old, and the sample was split fairly evenly between men and women. The average number of children living in a participant's household was 0.6. Only 15 percent of Learn$ave participants were Canadian citizens and almost 30 percent had arrived in Canada within the last two years. 35 percent of the sample identified themselves as White, with self-identified Blacks and Asians each comprising an additional 4 percent of the sample. The remainder of Learn$ave participants identified with some other race or ethnic group. About 45 percent of participants were married and 42 percent were single. Almost every participant was a high school graduate, and 77 percent had completed years of education beyond high school. Approximately 7 percent of the sample was drawn from Halifax, while the remainder was evenly drawn from Toronto and Vancouver. Just over half of participants were employed, although the yearly total income reported by participants (both employed and unemployed) was just $11,212. 6 percent reported receiving welfare and 3 percent reported receiving some other type of government assistance.

significant differences in employment, the pooled treatment group had a significantly lower average yearly income than the control group. The control group members were also significantly less likely to have student loans than the pooled treatment group members. Significant differences in baseline characteristics of control group and pooled treated group members were about as frequent as would be expected based on chance. Additionally, Columns 1 – 3 of Table 2 suggest that randomization was effectively implemented between the two treatment groups, Treatment and Treatment-plus. The first column of Table 2 reports characteristics of the Treatment group while the second column displays the characteristics of the Treatment-plus group. Column 3 shows the difference between Columns 1 and 2. There were no significant differences in the baseline characteristics shown in Table 2 between the Treatment and Treatment-plus groups.

Of the 3,583 individuals in the aggregate baseline sample, 63 percent completed the 54-month survey. As shown in Table 3, there are large differences in the 54-month response rates for the control group and the pooled treatment groups. 71 percent of the pooled treatment group participants completed the 54-month survey while only 48 percent of the control group participants did. This 24 percent difference in 54-month survey completion rate is significantly different at the 1 percent significance level. Of the treatment groups, the Treatment group participants were about 1 percent more likely to complete the 54-month survey than the Treatment-plus group participants, but this difference is statistically insignificant at conventional significance levels. The substantial attrition rate and the differences in attrition between control and treatment groups are examined in detail in Leckie et al. (2010). As a result of the high level of attrition, especially among control group members, I calculate a propensity of attrition score based on a vector of baseline characteristics and conduct my analysis using only those in the region of common support, as suggested by Crump et. al (2009), a final analysis sample of 2,263.

C. *Traditional and Financial Education*

For all of the experimental groups, control, Treatment, and Treatment-plus, the average level of education at baseline was fifteen years. Individuals in the pooled treatment groups acquired an average of .4 years more education during the course of the Learn$ave program than the control group members (Engelhardt et. al, 2013). This increase in education was exogenous to other individual characteristics due to the random assignment of program participants to the experimental groups. There was no significant additional effect of Treatment-plus group membership in education acquired.

The financial education course available to the Treatment-plus group was not available to either the Treatment group or the control group, but nothing precluded these individuals from having accessed a financial education program elsewhere during the course of the program. However, the follow-up surveys for all the groups did ask participants details about any continuing education program they engaged in during the course of the experiment and what topic that education program covered. Thus, if any of the Treatment or control group program participants did engage in financial education, these additional hours of education would have been recorded in the surveys. Six individuals in the control group participated in an average of 20.5 hours of outside financial education. Nine individuals in the Treatment group participated in an average of 17.1 hours of outside financial education. And, five individuals in the Treatment-plus treatment group participated in an average of 11.7 hours of outside financial education.

All participants in the program participated in an initial orientation session which explained the nature of the program, the goals of the program, the purpose and nature of a randomized experimental trial, and the purpose of an IDA. After the orientation session, potential program participants were given application forms and conditions and were able to apply for program admittance. Consequently, I assign each program participant one hour of financial education for that orientation session. Potential program participants were all made aware of the potential benefits to saving, the goals which could be accomplished by saving for their education, and the structure of a randomized control trial.

The final source of financial education that I measure was only available to the Treatment-plus group. This group was expected to complete fifteen hours of a financial education course that was developed for them in order to make matched withdrawals. The financial management training curricula was adapted from existing financial education programs, many of which had been developed for use in previous IDA programs. A curriculum was developed with consultants from the Prior Learning and Assessment Centre (PLA Centre) in Halifax. All sites used the same curriculum. The Treatment-plus group received an average of fourteen hours more financial education during the Learn$ave experiment than Treatment and control group members (Engelhard et. al, 2013). This increase was exogenous to other individual characteristics by virtue of the random assignment to the treatment and control groups.

The financial education curriculum focused both on teaching financial knowledge and behavioral budgeting tactics. The financial knowledge components covered topics such as compound interest, tracking expenses, setting and staying within a household budget, understanding credit rating systems and interacting with financial institutions. This part of the course encouraged participants to wisely allocate both assets and debt in order to maximize returns. The behavioral portion of the course focused on setting goals and planning to work around personal and interpersonal obstacles to saving and spending wisely.[7] Although the majority of the material covered in the financial education course administered by the Learn$ave program was standard

[7] Complete curricula are available upon request.

among the material administered to low-income, low-asset populations, it was developed specifically for the needs and financial sophistication of the program's participants. As such, the financial education provided in this experiment was in many ways similar to "typical" financial training programs, but it did not necessarily correspond precisely.

III. Method

In order to tractably estimate the elasticities of substitution between years of traditional schooling and financial education for wealth production, I use a translog cost function that is homogeneous of degree one in prices, as suggested by Berndt (1991). Because it is tractable for the purposes of empirical estimation, the translog functional form is commonly used in empirical work. This function allows the elasticity of substitution to vary between pairs of inputs, and does not impose constant returns to scale. The general form is

(1) $$lnC = ln\alpha_0 + \sum_{i=1}^{N} a_i lnP_i + \frac{1}{2}\sum_{i=1}^{N}\sum_{j=1}^{N}\gamma_{ij} lnP_i lnP_j + \alpha_Y lnY + \frac{1}{2}\gamma_{YY}(lnY)^2,$$

where C is total costs, P_i is the price of input i, and Y is total income. The translog cost function is a second-order Taylor's series approximation in logarithms to an arbitrary cost function. I use a homothetic function which constrains relative input demand to be independent of the level of output. Estimating a dual cost function is an alternative to the estimation of the production function itself. If I assume that the optimization problem facing the individual is to choose inputs so that the costs of wealth production are minimized, given input prices, the level of wealth, and the form of the production function, the solution to this optimization problem yields a cost function that is dual to the wealth production function.

I estimate the optimal, cost-minimizing input demand equations, transformed into cost share equations. To obtain these, I employ Shephard's lemma, which states that the optimal, cost-minimizing demand for input *i* can simply be derived by differentiating the cost function with respect to P_i. I logarithmically differentiate (1), to obtain cost share equations of the form

(2) $$\frac{\partial lnC}{\partial lnP_i} = \frac{P_i}{C} \cdot \frac{\partial C}{\partial P_i} = \frac{P_i X_i}{C} = \alpha_i + \sum_{j=1}^{N} \gamma_{ij} lnP_j,$$

where X_i is the amount of input *i* purchased, and . If I assume that the costs shares are determined with some error, u_i, the parameters of (2) can be consistently estimated if P_j is uncorrelated with u_i. In practice, however, it is likely there exists some unobservable individual characteristics, such as ability, that are correlated with both P_j and u_i. The existence of such factors would result in omitted variable bias. In order to solve this problem and obtain consistent parameter estimates, I use treatment-group membership in the Learn$ave experiment as instruments for price.

In order to implement an instrumental variables strategy with (2), the model must include three or more production inputs. Accordingly, I include case management, in addition to financial and traditional schooling, as my third input. From (2), the individual cost share equations for the three inputs are

(3) $$\frac{P_1 X_1}{C} = \alpha_1 + \gamma_{11} lnP_1 + \gamma_{12} lnP_2 + \gamma_{13} lnP_3,$$

(4) $$\frac{P_2 X_2}{C} = \alpha_2 + \gamma_{21} lnP_1 + \gamma_{22} lnP_2 + \gamma_{23} lnP_3, \text{ and}$$

(5) $$\frac{P_3 X_3}{C} = \alpha_3 + \gamma_{31} lnP_1 + \gamma_{32} lnP_2 + \gamma_{33} lnP_3.$$

In the absence of symmetry restrictions there are twelve parameters to estimate, four in each of the three equations. When the three cross-equations symmetry conditions are imposed, so that $\gamma_{ij} = \gamma_{ji}$, the number of parameters to estimate drops to nine. The underlying economic theory also requires that the translog function be homogeneous of degree one in input prices. This specification is consistent with any returns to scale, although I am not able to isolate this parameter. In this framework the homogeneity restrictions are

(7) $$\alpha_1 + \alpha_2 + \alpha_3 = 1,$$

(8) $$\gamma_{11} + \gamma_{12} + \gamma_{13} = 0,$$

(9) $$\gamma_{21} + \gamma_{22} + \gamma_{23} = 0, \text{ and}$$

(10) $$\gamma_{31} + \gamma_{32} + \gamma_{33} = 0.$$

Using (7)-(10), the number of free parameters to be estimated is reduced from nine to five. If I posit an additive random disturbance term, u_i, in each equation that is multivariate normally distributed with mean vector zero and constant covariance matrix Ω, the fact that only two of the cost share equations are linearly independent implies that the disturbance covariance matrix Ω is singular and non-diagonal. Moreover, because the cost shares sum to one in each observation, when the three symmetry restrictions are not imposed, the simple arithmetic of equation-by-equation OLS yields parameter estimates that must obey the following column sum adding up conditions:

(11) $$a_1 + a_2 + a_3 = 1, \text{ and}$$

(12) $$g_{11} + g_{12} + g_{13} = g_{21} + g_{22} + g_{23} = g_{31} + g_{32} + g_{33} = 0,$$

where a_i and g_{ij} are estimates of the α_i and γ_{ij} parameters. These relationships also imply that the OLS residuals across equations will sum to zero at each observation. Thus, the residual cross-products matrix resulting from OLS equation-by-equation estimates will be non-diagonal and singular. So, because the disturbance covariance and residual cross-products matrices will both be singular, maximum likelihood estimation are not feasible. The most common procedure for handling this singularity problem is to drop an arbitrary equation and then estimate the remaining two equations by maximum likelihood (Berndt, 1991). Accordingly, I estimate the following two equations, synonymous to (3) and (4):

(13) $$\frac{p_S x_S}{C} = \alpha_S + \gamma_{SS} \ln\left({p_S}/{p_C} \right) + \gamma_{SF} \ln\left({p_F}/{p_C} \right) + u_S, \text{ and}$$

$$\frac{p_F X_F}{C} = \alpha_F + \gamma_{SF}\ln({}^{p_S}/_{p_C}) + \gamma_{FF} ln({}^{p_F}/_{p_C}) + u_F, \quad (14)$$

where S, or input 1, denotes traditional schooling, F, or input 2, denotes financial education, and C, or input 3, denotes case management. The values of $X_s, X_F,$ and X_C are clearly defined and easily measurable using the Learn$ave data.

I define p_C, the cost of case management, as the person-specific opportunity cost of time, equal to the individual's post-tax baseline yearly earnings. p_C does not vary by treatment status, as individuals can independently access free resources, such as customer service representatives from financial planning institutions or reminder systems, which approximate the basic financial planning services the Learn$ave case management provided. I define p_F, the cost of financial education, as the sum of the person-specific opportunity cost of time and the cost of a year of financial planning education.[8] For the Treatment-plus group, however, p_F is simply the person-specific opportunity cost, because these individuals were provided with free financial education. The values of p_S, the cost of an additional year of school, conditional on baseline education level, are the sum of the cost of a year of schooling and the person-specific opportunity cost of time.[9] I use two different constructed values of p_S in my estimations. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of twelve. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment. Both of these measures vary by treatment status, as both those in the Treatment and Treatment-plus groups were provided with a

[8] This unit of measure for the costs in the estimating equations is years. The cost of financial education is found using the cost of the first financial literacy program created by the Financial Consumer Agency of Canada and introduced at George-Brown College in Toronto during the winter, spring, and fall sessions of 2008.
[9] Matching monies were not taken into account in the determination of eligibility for all Canadian federal and province educational grant and loan programs.

3:1 match on educational savings. Thus, the cost of an equivalent year of schooling is 25 percent as much for a member of the treatment groups relative to a control group member. The person-specific opportunity cost of time in each of the prices is total baseline post-tax earned income. Because I cannot observe baseline wages for those who are unemployed, I construct predicted wages using a Heckit estimator.[10] These predicted wages and other participant demographic characteristics are used to construct predict post-tax earnings using Kevin Milligan's Canadian Tax and Credit Simulator (CTaCS).

In order to provide consistent and unbiased estimates of the parameters in (13) and (14), I use an iterated 3SLS procedure in which Treatment and Treatment-plus status serve as instruments for the independent variables.[11] The iteration makes the selection of the dropped cost share equation irrelevant in terms of the parameter estimates.

The Allen partial elasticities of substitution for the translog cost function are equal to

(15) $$\sigma_{ij} = \frac{\gamma_{ij}+S_i S_j}{S_i S_j}, i, j = 1, 2, 3, i \neq j,$$

where $S_i = \frac{P_i X_i}{C}$. The price elasticities are calculated as

(16) $$\epsilon_{ij} = \frac{\gamma_{ij}+S_i S_j}{S_i}, i, j = 1, 2, 3, i \neq j, \text{ and}$$

(17) $$\epsilon_{ii} = \frac{\gamma_{ii}+S_i S_i}{S_i}, i = 1, 2, 3.$$

These equations are individual-specific and computed using parameter estimates and fitted cost shares. Hicks (1970) shows that, in the case of Allen elasticities, the inputs are complements if $\sigma < 0$ and substitutes if $\sigma > 0$. Hicks also shows that it is possible that one pair of inputs, though not more than one pair, may be complements.

[10] I follow the process suggested by Kimmel and Kniesner (1998) to create these predicted wages.
[11] My IV strategy allows me to consistently estimate the parameters in (13) and (14) even if there is measurement error in the constructed prices .

First-stage estimates of the effect of treatment status on the cost shares shown in (13) and (14) are displayed in Table 4. With the exception of the effect of Treatment status on $\frac{p_F \cdot x_F}{C}$, these estimated coefficients are all significant at the 1 percent level.

Iterated 3SLS estimates of the system of (13) and (14) are show in Table 5. This system is first estimated with unconstrained coefficients, in Panel A, and then estimated with γ_{SF} constrained to be identical in both equations, in Panel B. Shown in Columns 1 and 2, respectively, specification 1 uses $\tilde{p}_S$ and specification 2 uses $\hat{p}_S$. All of the parameter estimates are significant at the 5 percent level. I conduct a Wald test to determine the validity of the parameter restrictions. The p-value of this Wald test for each specification is reported at the bottom of Table 5. Both Wald tests indicate that the difference in the constrained and unconstrained coefficients is not systematic, so the constrained specification is legitimate. Accordingly, I obtain the indirect parameter estimates of the four parameters in the omitted case management cost share equation with the directly estimated parameters from the constrained systems. These omitted parameters are reported in Table 6.

Finally, I calculate the individual Allen cross-partial elasticities of substitution from (15) for the three inputs. These values are reported in Table 7.[12] While these elasticities of substitution are not constant across individuals, the mean values indicate that all of the inputs are substitutes in terms of wealth production. $\overline{\sigma_{SF}}$ = 2.0 in specification 1, shown in Column 1, and $\overline{\sigma_{SF}}$ = 1.7 in specification 2, in Column 2; $\overline{\sigma_{SC}}$ = 0.7 in specification 1 and $\overline{\sigma_{SC}}$ = 0.9 in specification 2; $\overline{\sigma_{FC}}$ = 1.6 in specification 1 and $\overline{\sigma_{FC}}$ = 1.5 in specification 2. The mean elasticities of substitution are quite similar across specifications. Overall, traditional schooling and financial education appear

[12] The other Allen partial elasticities and price elasticities from (15) – (17) are reported in Table 8.

to have a higher degree of substitutability than traditional schooling and case management. However, these values vary quite widely from individual to individual. Both traditional schooling and financial education and traditional schooling and case management are complements for some individuals. Only financial education and case management are substitutes for every individual in the sample. The minimum elasticity of substitution for financial education and case management is 0.6.[13]

IV. **Conclusion**

This paper provides the first experimental evidence, using the Learn$ave IDA randomized control trial, of the elasticities of substitution between traditional schooling and financial education in terms of wealth production. Using a dual translog cost function specification, I am able to use an iterated 3SLS strategy to estimate individual elasticities of substitution between the three inputs in terms of wealth production. The mean values of all of the individual estimated elasticities of substitution are greater than zero, indicating that, on average, traditional schooling and financial education are substitutes in terms of wealth production. In addition, case management is, on average, a substitute input to both financial and traditional education. Only financial education and case management are substitutes for every individual in the sample.

Overall, my results suggest that the experimental and quasi-experimental literature's lack general finding that financial education has little impact on financial behavior and asset accumulation is not due to the resource constraints of participants in these programs. Because financial and traditional education act as substitutes, on average, in the wealth production function, I do not expect that more positive effects of financial education would be found in if the wealth of study populations was also exogenously increased, as the theoretical literature has suggested.

[13] The distribution of elasticities does not differ significantly across groups sorted by baseline level of education.

Consequently, the added emphasis on financial education and case management services in IDA experiments and other programs designed to increase the total wealth of low-income individuals is not necessary, on average. For some individuals these different types of human capital are "greater than the sum of their parts," but for others combining different types of education may not be the most cost effective strategy.[14] As a result, more time could be spent at the beginning of programs designed to increase wealth production through education to determine the best or most effective human capital input. In addition, the results suggest that the recent push to have primary schools provide financial education (Schwartz, 2013) is not necessary in order to increase students' lifetime wealth, as simply accruing additional years of traditional schooling may be as effective for the majority of students.[15]

I have several remarks regarding the external validity of these results. The sample of program participants consists low-income individuals in major Canadian cities, many of whom are immigrants. This population may differ in important ways from other populations who may benefit in different ways from the inputs in this study. Also, the voluntary nature of the experiment implies that the sample consists of individuals who are highly motivated to increase their years of traditional schooling, which may have made this population uniquely receptive to other types of assistance, such as financial education and case management, as well.

Most importantly, the financial education provided to the participants of the Learn$ave program was not necessarily representative of other financial education programs. The financial aid offered in this experiment was aimed at low-income individuals, who had little experience interacting with financial institutions. Accordingly, the inclusion of a substantial amount of

[14] Unfortunately, my results do not vary significantly by education level, so initial level of education is not a good predictor of the most effective was to increase wealth production for an individual.
[15] This is especially true if providing financial education requires additional costs (training, personnel, materials, etc.) to administer that could be more efficiently spent improving traditional instruction.

behavioral financial education offered may contribute to the finding that case management and financial education are found to be substitutes in terms of wealth production for every member of this sample. However, much of the content provided in the financial education program was similar to that of "typical" programs aimed at this type of audience. As such, the Learn$ave curriculum may be subject to the main critique other programs have faced in this literature: financial education courses tend to be general rather than focused on specific decision-making processes. The effects of financial education tend to be highly dependent on the content of financial training (Brown et. al, 2013), and so the substitutability of financial education may be dependent on the content of the financial education program as well. Further research is needed to determine if my elasticity estimates hold for all types of financial education.

Table 1: Sample Means for Baseline Characteristics of Program Participants

Explanatory Variable	Control	Treatment	Difference
	(1)	(2)	(3)
Male	0.485	0.473	0.012
	(0.014)	(0.01)	*0.5*
New Immigrant	0.304	0.282	0.022
	(0.013)	(0.009)	*0.178*
Canadian Citizen	0.14	0.159	-0.019
	(0.01)	(0.007)	*0.122*
Single	0.437	0.463	-0.026
	(0.014)	(0.01)	*0.139*
Married	0.441	0.414	0.027
	(0.014)	(0.01)	*0.117*
High School Graduate	0.936	0.951	-0.015
	(0.007)	(0.004)	*0.059*
Greater than High School Education	0.885	0.901	-0.016
	(0.009)	(0.006)	*0.154*
Employed	0.551	0.556	-0.005
	(0.014)	(0.01)	*0.784*
Paid Hourly	2.301	2.31	-0.009
	(0.02)	(0.013)	*0.691*
Weekly Earnings	195.129	208.715	-13.586
	(7.826)	(5.808)	*0.171*
Welfare Recipient	0.055	0.066	-0.011
	(0.007)	(0.005)	*0.204*
Government Program Member	0.028	0.026	0.002
	(0.005)	(0.003)	*0.828*

Note: Standard errors for each of the means in columns 1 and 2 are shown in parentheses. P-values for a t-test of the difference between the two means in column 3 are shown in italics.

Table 2: Sample Means for Baseline Characteristics of Program Participants

Explanatory Variable	Learn$ave-only	Learn$ave-plus	Difference
	(1)	(2)	(3)
Male	0.464	0.482	-0.018
	(0.014)	(0.014)	*0.379*
New Immigrant	0.287	0.277	0.01
	(0.013)	(0.013)	*0.594*
Canadian Citizen	0.162	0.157	0.005
	(0.011)	(0.011)	*0.702*
Single	0.459	0.466	-0.007
	(0.014)	(0.014)	*0.729*
Married	0.418	0.41	0.008
	(0.014)	(0.014)	*0.691*
High School Graduate	0.955	0.948	0.007
	(0.006)	(0.006)	*0.444*
Greater than High School Education	0.902	0.899	0.003
	(0.009)	(0.009)	*0.832*
Employed	0.546	0.566	-0.02
	(0.014)	(0.014)	*0.332*
Paid Hourly	2.287	2.333	-0.046
	(0.021)	(0.016)	*0.005*
Weekly Earnings	201.891	215.6	-13.709
	(8.119)	(8.308)	*0.238*
Welfare Recipient	0.066	0.066	0
	(0.007)	(0.007)	*0.996*
Government Program Member	0.023	0.029	-0.006
	(0.004)	(0.005)	*0.37*

Note: Standard errors for each of the means in columns 1 and 2 are shown in parentheses. P-values for a t-test of the difference between the two means in column 3 are shown in italics.

Table 3: Learn$ave Survey Responses and Response Rates, by Survey and Research Group

Survey	Control	Treatment	Treatment-plus	Total
Baseline	1,195	1,194	1,195	3,584
54-month	568 (47.5%)	842 (70.5%)	859 (71.9%)	2,269 (63.3%)

Table 4: First-stage Estimates of the Effect of Treatment Status on Cost Shares

	(1)	(2)	(3)
Explanatory Variables	$\frac{\tilde{p}_S \cdot X_S}{C}$	$\frac{\hat{p}_S \cdot X_S}{C}$	$\frac{p_F \cdot X_F}{C}$
Treatment	-0.122***	-0.059***	-0.000
	(0.002)	(0.006)	(0.000)
Treatment-plus	-0.122**	-0.061***	-0.104***
	(0.002)	(0.006)	(0.000)
Constant	0.064***	0.089***	0.104***
	(0.002)	(0.004)	(0.000)
N	2,263	2,263	2,263

Note: Standard errors are in parentheses. A propensity to attrit score was calculated for each individual and only those individuals in the area of common support for the treatment and control groups are included. *** p<0.01, ** p<0.05, * p<0.1. Specification 1 is estimated with $\tilde{p}_S$ for the individual price of a year of schooling. Specification 2 is estimated with $\hat{p}_S$ for the individual price of a year of schooling. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of 12. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment.

Table 5: Iterated 3SLS Estimates of Translog Cost Share Equations

Explanatory Variables	(1) $\frac{\tilde{p}_S \cdot X_S}{C}$	(1) $\frac{p_F \cdot X_F}{C}$	(2) $\frac{\hat{p}_S \cdot X_S}{C}$	(2) $\frac{p_F \cdot X_F}{C}$
<u>*A. Unconstrained*</u>				
$ln\hat{p}_S - lnp_C$	-0.296***	0.177**	-0.620***	0.371**
	(0.110)	(0.073)	(0.235)	(0.156)
$lnp_F - lnp_C$	0.272***	-2.357***	0.285**	-2.364***
	(0.115)	(0.077)	(0.122)	(0.081)
Constant	0.044***	0.734***	0.079***	0.713***
	(0.011)	(0.007)	(0.011)	(0.007)
<u>*B. Constrained*</u>				
$ln\hat{p}_S - lnp_C$	-0.308***	0.192***	-0.546***	0.310***
	(0.109)	(0.071)	(0.194)	(0.113)
$lnp_F - lnp_C$	0.192***	-2.313***	0.310***	-2.375***
	(0.071)	(0.058)	(0.113)	(0.078)
Constant	0.049***	0.732***	0.074***	0.717***
	(0.009)	(0.006)	(0.007)	(0.004)
p-value of Wald Test	*0.993*		*0.999*	
N	2,263	2,263	2,263	2,263

Note: Standard errors are in parentheses. A propensity to attrit score was calculated for each individual and only those individuals in the area of common support for the treatment and control groups are included. Specification 1 is estimated with $\tilde{p}_S$ for the individual price of a year of schooling. Specification 2 is estimated with $\hat{p}_S$ for the individual price of a year of schooling. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of 12. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment. Randomly assigned Treatment and Treatment-plus status are used as instruments for the independent variables in each of the equations. The Wald test shows the p-value of a Wald test for the equivalence of the coefficients in the unconstrained and constrained systems of equations for each specification. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Indirect Parameter Estimates for the Translog Cost Function

	(1)	(2)
a_C	2.20***	0.209***
	(0.004)	(0.004)
g_{SC}	0.116**	0.236**
	(0.053)	(0.109)
g_{FC}	2.120***	2.065***
	(0.039)	(0.053)
g_{FC}	-2.237***	-2.301***
	(0.043)	(0.085)
N	2,263	2,263

Note: A propensity to attrit score was calculated for each individual and only those individuals in the area of common support for the treatment and control groups are included. Indirect parameter estimates in the omitted case management share equation are obtained by rearranging the homogeneity restrictions in terms of the directly estimated coefficients from the constrained systems. Standard errors are in parentheses. Specification 1 is estimated with $\tilde{p}_S$ for the individual price of a year of schooling. Specification 2 is estimated with $\hat{p}_S$ for the individual price of a year of schooling. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of 12. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment.

Table 7: Allen Cross-Partial Elasticities of Substitution for the Translog Cost Function

	(1)	(2)
σ_{SF}	1.982	1.726
	8.341	*46.871*
σ_{SC}	0.717	0.876
	5.232	*13.408*
σ_{FC}	1.562	1.529
	0.718	*0.701*
N	2,263	2,263

Note: Standard deviations are in italics. A propensity to attrit score was calculated for each individual and only those individuals in the area of common support for the treatment and control groups are included. Column 1 shows the mean estimated Allen cross-partial elasticities of substitution for the translog cost function estimated with $\tilde{p}_S$ for the individual price of a year of schooling. Column 2 shows the mean estimated Allen cross-partial elasticities of substitution for the translog cost function estimated with $\hat{p}_S$ for the individual price of a year of schooling. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of 12. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment.

Table 8: Allen Partial Elasticities of Substitution and Price Elasticities for the Translog Cost Function

	(1)	(2)
σ_{SS}	-0.373	-0.607
	0.017	*0.066*
σ_{FF}	-2.544	-2.604
	0.023	*0.025*
σ_{CC}	-2.453	-2.517
	0.038	*0.037*
ϵ_{SS}	-5.403	-5.613
	27.230	*112.065*
ϵ_{SF}	3.365	3.234
	16.973	*63.589*
ϵ_{SC}	2.038	2.379
	10.258	*48.476*
ϵ_{FF}	-4.582	-4.721
	0.890	*0.987*
ϵ_{FC}	4.167	4.090
	0.821	*0.850*
ϵ_{CC}	-7.802	-7.990
	2.738	*2.768*
N	2,263	2,263

Note: Standard deviations are in italics. A propensity to attrit score was calculated for each individual and only those individuals in the area of common support for the treatment and control groups are included. Column 1 shows elasticities estimated with $\tilde{p}_S$ for the individual price of a year of schooling. Column 2 shows elasticities estimated with $\hat{p}_S$ for the individual price of a year of schooling. $\tilde{p}_S$ uses the average cost of a year of schooling in the participant's chosen education level and city less the participant's expected financial aid, determined by his location, income level, and number of dependents under the age of 12. $\hat{p}_S$ uses a predicted cost of a year of schooling based on the actual education costs incurred by members of the control group during the experiment.

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