Learning *to* Save, Saving *to* Learn

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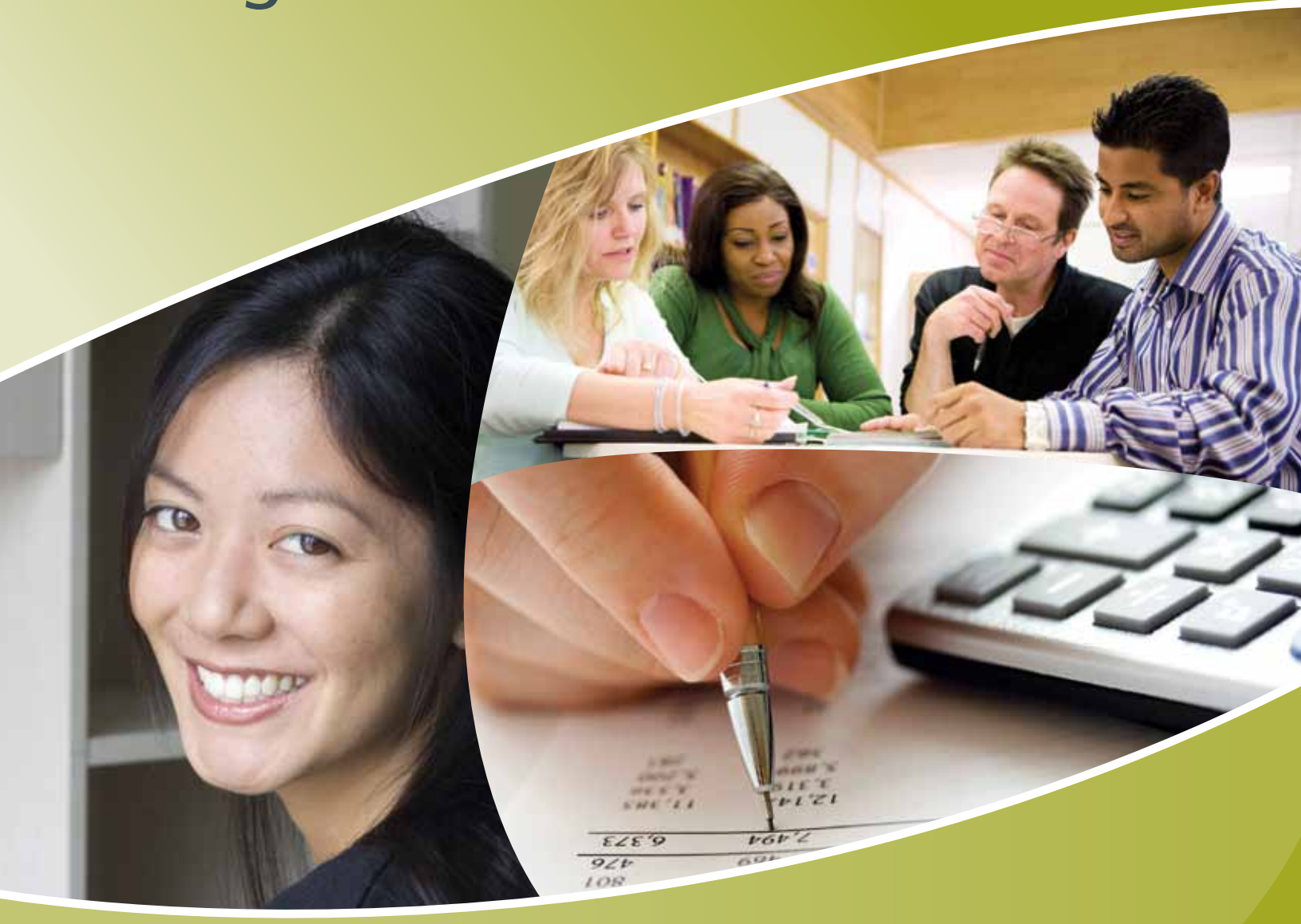

learn$ave

Individual Development Accounts Project

Final Report

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A project sponsored by

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Preface

In today's economy, people who lack sufficient education and basic skills are exposing themselves to lower earnings and higher risk of unemployment. This has been a source of concern for Canadian governments over the last two decades. That concern, along with the desire to build a competitive workforce, explains why governments have been looking for ways to encourage Canadians to invest in their own human capital.

learn$ave was introduced as a demonstration project to test the effectiveness of a new instrument – Individual Development Accounts (IDAs) – to encourage low-income adults to save for their own education or training. The use of IDAs was pioneered in the United States in the 1990s and introduced in Canada on a small scale more recently. In general, IDAs work as regular saving accounts, with account holders receiving a matching grant for every dollar they deposit. To benefit from the matching grant, savings have to be used for specific purposes. In *learn*$ave, savings could be used for education, training or starting a small business.

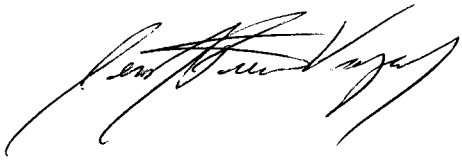
There has been much discussion of the promise of IDAs, but little proof of their alleged effectiveness, particularly in Canada and particularly in regard to incentivizing adult education and business start-ups. Would the offer be appealing to the target group? Would the program contribute to increasing education enrolment and small business start-ups among participants? Would it improve labour market outcomes? This is the reason why, in 2000, Human Resources and Skills Development Canada (HRSDC) funded *learn*$ave, a nine-year demonstration project to test the IDA approach.

This report presents final results of the *learn*$ave project covering the entire 54-month period after participants' entry into the project. It summarizes findings based on all lines of evidence, including participant surveys, focus groups, and administrative data. While the emphasis is placed on impacts on participants' savings and education enrolment, important implementation issues around recruitment and take-up as well as cost-effectiveness issues are also addressed.

We are grateful to HRSDC for funding the *learn*$ave project, in particular Satya Brink and Urvashi Dhawan-Biswal who provided the authors with advice and comments along the way, as well as Patrick Brussière who provided support in the latter stages of this project. We would also wish to thank our major partner, Social and Enterprise Development Innovations (SEDI), which developed the initial project idea, and the community-based organizations that SEDI worked with to deliver and administer *learn*$ave in 10 sites across Canada. Thanks should also go to the financial institutions that held and administered the *learn*$ave accounts, namely, RBC Royal Bank, Assiniboine Credit Union, and Caisse d'économie Desjardins.

We appreciate the contributions of Christopher Mallory (production manager), Stéphanie Navarro (executive assistant), Eliza Bennett (editor), Jeff Hammell (designer), and Jennifer Robson (consultant) who very capably handled the production, revision and dissemination of this report. We would also like to thank Saul Schwartz, professor at the Carleton University School of Public Policy and Administration who provided very insightful and useful comments on an earlier draft of this report. We are grateful as well to Connie Cheng at POLLARA who was responsible for conducting the participant surveys. The participants who dutifully responded in the various surveys conducted for this project should also be thanked.

Thanks are also due to SRDC colleagues who played earlier key roles in this project, particularly Michael Dowie and Hongmei Cao. Finally, special thanks to my colleagues at SRDC who performed the analysis and co-authored this report, namely, Norm Leckie (project manager), Doug Tattrie, and Taylor Shek-Wai Hui as well as Jennifer Robson who is now a private consultant.



Jean-Pierre Voyer
President
Social Research and Demonstration Corporation
November 2010

Chapter 1 Policy context and rationale

In the last 30 years, employment outcomes for those with high school education and particularly post-secondary qualifications have consistently proven better than outcomes for less educated and lower skilled workers. According to the most recent Census data, median annual earnings for Canadians with a high school diploma reached just over $37,400 in 2005, nearly 1.5 times less than median earnings for Canadians with a bachelor's degree ($56,000) (Statistics Canada, 2009). Other investments in post-secondary education (PSE) such as trades certifications and college also increase median earnings, although less so than university degrees. Thus, higher education, while no guarantee, is still the best route to a good job, which, in turn, continues to be the surest guarantee of household financial well-being.

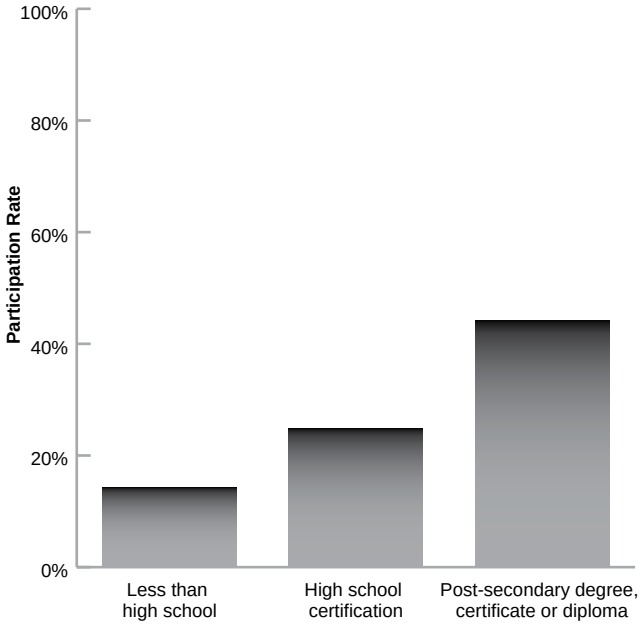
Despite the significant returns to higher education, very few adult Canadians return to school to upgrade their education after they have entered the workforce. Canadians tend to go through the formal education system and then enter the labour market, rarely investing in any more formal education during their working lives, particularly when their credentials are very low (de Broucker and Myers, 2006). In so doing, many Canadians may miss opportunities to increase their lifetime earnings and overall productivity. Those with fewer skills expose themselves to lower earnings and employability and increase their risk of unemployment during their working lives. By best estimates, just 7 per cent of working adults in Canada (aged 25 or older and working full-time) reported taking part in formal education programs through high school, business or trade schools, college or university in 2003 (Drewes, 2008, Table 4.1). Participation in job-related training is somewhat higher at 21 per cent of all working age adults, but the training typically lasts no more than a work week. More worrisome still, adult education and training are associated with prior education, labour force attachment, and household income (Figure 1.1). In other words, often it is the best skilled workers in Canada who are most likely to be investing in their own ongoing learning, further increasing the gap between high-skilled and low-skilled workers.

A new instrument to encourage adult learning

The *learn*$ave demonstration project was launched in 2000 by Human Resources Development and Skills Development Canada (HRSDC)[1] to test the effectiveness of a new instrument — Individual Development Accounts (IDAs) — in promoting adult learning among low-income Canadians. At their core, IDAs are means-tested programs that offer restricted savings accounts with matching credits on all deposits made by an accountholder. As first proposed by Michael Sherraden in his 1991 book *Assets and the Poor: A new American welfare policy*, IDA funds were to be limited to low-income and low-asset individuals who wanted to save for certain "productive" uses such as homeownership, education or small business start-up that might improve their long-term self-sufficiency and well-being. In addition to the matching credits, Sherraden argued in favour of providing training on household financial management for all IDA accountholders (Sherraden, 1991).

Since the 1991 book, IDAs have been piloted or rolled out in several countries and have taken on several variations. For example, in the United States (U.S.), large numbers of IDAs have been introduced following the original Sherraden model first piloted through the American Dream Demonstration (ADD) and now supported by state and federal legislation such as the Assets for Independence Act (AFIA). Matched savings incentives have also been implemented in Taiwan, Uganda, New

Figure 1.1 Proportion of Canadians 25-64 Years Old in Job-Related Training or Education, by Prior Education Level, 2008



Source: Tamara Knighton, Filsan Hujaleh, Joe Iacampo and Gugsa Werkneh (November 2009). *Lifelong Learning Among Canadians Aged 18 to 64 Years: First Results from the 2008 Access and Support to Education and Training Survey*, Statistics Canada Catalogue no. 81-595-M — No. 079, Table A.1.4, p. 47.

1 At the time, the federal government department was known as Human Resources Development Canada (HRDC); however, this department will be referred to as it is presently known (HRSDC) throughout this report.

Zealand, Australia, Israel and the United Kingdom (UK), among others, in various forms and for various policy purposes. For example, the UK, after two pilot phases, implemented its national Savings Gateway program, an income-tested matching grant on eligible deposits that can be used for any purpose and without financial management training. Its intent is to generate a saving habit among low-income Britons, not to build a particular asset.

In contrast to the Savings Gateway, the *learn*$ave project is quite faithful to the original IDA concept, combining matched savings, financial management training for participants, and restricted uses corresponding to the objectives of encouraging enrolment in education or starting a small business, although not to other productive uses proposed by Sherraden.

In funding *learn*$ave, HRSDC sought to discover whether or not incentives to save can lead low-income working-age adults to invest their own resources in their human capital development. IDAs are just one among a myriad of approaches to encouraging higher learning and skills development. This is well illustrated in our review of existing approaches to encourage higher education and learning in Canada presented in Appendix A. None of these approaches, however, seems to have resolved the policy question of how to improve access to and participation in adult education for low-wage and low-skilled adults. Appendix A also indicates that no prior pilot or program has yet tried to use a matched savings instrument to try to increase participation in training and education among adults.

The *learn*$ave context

Within the context of adult education and training policy, the *learn*$ave project must be viewed as a test of a new policy instrument to fill a gap in the range of supports for adult education and training. Broadly speaking, there are few education and training policy measures designed with the needs of low-income working age adults in mind. Most supports for higher education participation benefit much younger learners newly out of high school or adults with higher incomes. The balance offers some support for adult learning, but through a patchwork of programs for which low-incomes adult learner may or may not qualify, depending on their citizenship, insurable employment hours, age, province and receipt of Employment Insurance (EI) or Income Assistance (IA) benefits. While some very small IDA projects have been tried in Canada and many included uses of the matched saving credits for adult education and training (see Box 2.1 in Chapter 2), *learn*$ave was the first one in which an assisted savings instrument was designed to encourage education participation among low-income adults on a large scale.

Another important part of the context is a recent general trend towards greater uses of assisted savings

Table 1.1 Summary of Recent Canadian Savings Policies

Name	Description	Year Introduced
Canada Education Savings Grant	New grant that matched (at 20 per cent) each dollar of household savings in an eligible RESP for a dependent child up to annual and life-time maximums.	1998
	The grant was later enhanced to add a higher matching rates (of 30 per cent and 40 per cent) on the first $500 saved annually by low and modest income families	2004
Canada Learning Bond	Annual bond that is deposited into the RESPs of eligible children receiving the National Child Benefit Supplement. The first payment is $500 and subsequent payments are $100 annually in each year the children remains eligible. No co-payment is required but the policy hopes to kick-start RESP savings.	2004
Alberta Centennial Education Savings Program	A grant of $500 at birth and top-ups of $100 into RESPs at the ages of 8, 11, and 14 years for all children born or living in the province after 2005	2005
Registered Disability Savings Plan and associated grant and bond	A tax sheltered savings account that enables families caring for children with disabilities to save for their longer-term needs and security. Savings can be matched by a Disability Savings Grant and low-income families may be eligible for the Disability Savings Bond.	2007
BC Children's Education Fund	A provincial pooled savings program that invests $1,000 at birth for each child born or adopted in the province after January 1, 2007. Funds are disbursed to recipients when they are between 17 and 26 years of age and enter into PSE. The end value of the account is expected to be $2,200 per child.	2007
Quebec Education Savings Incentive	A provincial matching contribution of 10 per cent on family deposits into a Quebec child's RESP up to $250 annually for most families and $300 annually for low-income families.	2007
Tax-Free Savings Account	A tax pre-paid account that provides no credits or deductions but shelters all investment income and withdrawals from taxation. An annual limit of $5,000 can be deposited into a TFSA but no lifetime limit is in place. The account can only be opened by or for adults over 18.	2008

in Canadian public policy. Canada has long had policies aimed at encouraging individual savings or wealth where public expenditures act as a subsidy for the individual capital accumulation (Axworthy, 2005). These include the RRSPs (introduced in 1957), RESPs (introduced in 1974) and the former Assisted Homeownership Program (1970–1982), which can all be described as asset-based in their approach. Since 1998, there have been seven new federal or provincial policies aimed at increasing individual and household savings for a range of purposes. These are summarized in Table 1.1 below.

There are at least four potential reasons why governments might prefer to use assisted savings instruments to achieve certain policy objectives:

1. *The financial benefits may be more transparent and easier to communicate.* Other measures such as needs-tested loans and income tax credits offer very different levels of assistance depending on individual circumstances. Assisted savings measures may take income into account but generally tie the payout to savings behaviour. In other words, "if you put in a dollar, you get this amount."

2. *Administrative costs to government may be lower.* Because these instruments almost exclusively rely on financial institutions (for example banks, credit unions and investment firms who sell RRSPs and other registered savings products), much of the administrative burden of the accounts is offloaded to the private sector who in turn recover its cost in their product design and fees. While administrative costs to the government for student loans are very high, comparable administrative costs have been reported to be a few cents for each dollar of savings grant distributed under the Canada Education Savings Grant (Burton, 2004).

3. *Stakeholding effects may be possible and there may be fewer "free-rider" problems.* Because savings instruments generally demand a personal contribution to trigger a public contribution, they may filter out less enthusiastic participants and may foster a sense of ownership or commitment among those who do take part in attaining their goals. Windfall gains are present in nearly every publicly funded program and are often a result of the fact that participants self-select — meaning that the most motivated participate and benefit when they might have succeeded on their own without the program. At least if costs are shared by participants, the costs to the public purse might be reduced.

4. *Opportunities for employer and third-party participation may be greater than with other instruments.* Since administrative costs are already lowered and risk is already shared, assisted savings mechanisms may provide an instrument that better enables policy-makers to engage the private sector in common goals. Subsidies can be cost-shared, for example, with philanthropic sources or with employers.

Observing the trend toward assisted saving does not suggest it is either good or bad, but that *learn*$ave should be seen within both the larger contexts of the increasing importance of skills and education as well as of the more frequent use of assisted savings instruments to address a range of policy issues. However, this does not yet answer the question of why or how a savings instrument might be suited to encouraging learning among lower-income earners.

One line of reasoning behind the *learn*$ave demonstration was that the IDAs being tested could represent a complementary approach to existing policies and programs using matched savings to encourage higher education for the broad population by targeting a sub-group most in need of education and higher skills. Having already invested in this policy approach, it would be natural to ask whether or not some design changes could create programs that both fit within the overall policy framework and fill a niche for low-wage, low-skilled workers. If successful, such programs might help to address the general problem of skew in the current savings policy that delivers benefits largely to those who have higher incomes and already have a propensity to invest in human and other forms of capital.

learn$ave within the theory of asset-building

A second line of reasoning behind *learn*$ave was whether or not there was something unique about savings instruments that might yield better outcomes in terms of education and training participation and well-being, than traditional means. Might there be something important about the act of saving itself that could lead to greater behavioural change in adult learning and financial security?

Low-wage workers must address several hurdles to participating in higher education. They face two kinds of financial barriers: they are less able to afford both the out-of-pocket costs of education (including tuition, books, transportation and all the other associated costs); and the earnings interruptions (forgone earnings) from taking time off work to attend a school program. There are also non-financial barriers to attending higher education. For example, a recent survey of working age adults by Statistics Canada and the Canadian Council on Learning finds that adults who do not take part in formal learning are most likely to state that they do not see the need for

Box 1.1 Proposed effects of assets

- A cushion in times of unexpected strain (such as job loss, environmental catastrophe, marital dissolution or critical illness).
- A platform for productive risk-taking (such as entrepreneurial endeavours or temporarily leaving the workforce to return to school).
- Household stability by reducing financial strain.
- Well-being for dependent children by providing intergenerational transfers of wealth.
- Self-efficacy, hopefulness, and longer-term planning.
- Tending behaviours that take care of assets that are owned and valued, possibly including greater civic engagement, greater care to one's primary residence or simply shaping values in favour of assets that are held over other forms of capital.
- Income for investment in new capital, creating a virtuous cycle of wealth creation.

it (Canadian Council on Learning, 2009). Low-skilled workers with less formal education may have had negative experiences in school that lead them to carry beliefs that more education is "not for" them. Finally, it also appears as though low-income earners differ from higher-income earners in projecting returns on investments in education. Lower-income earners expect higher education to be more costly than do higher-income earners, and also anticipate that increases in earnings after more education will be much smaller (Usher, 2005).

Can a matched savings instrument address the financial barriers to higher education and also influence the non-financial barriers (such as negative attitudes toward education or saving) that shape personal choices about whether to invest in more human capital? According to models developed by Michael Sherraden and others, assets play a key role in shaping a wide range of attitudes and behaviours (Sherraden, 1991). Traditional economic theory views savings as stores of income left over after current consumption that can then be used for future consumption. The primary issues from this perspective are whether and how it is possible or even desirable to influence people's preferences to consume more now or to save now and consume even more later. However, Sherraden's model suggests that the presence of, or access to, assets (in the form of human, physical and financial capital) can have a number of benefits as described in Box 1.1.

While there may be certain advantages to savings instruments, as discussed earlier, these instruments are not the only way to transfer or stimulate wealth. Direct transfers of lump sums that are not conditional on participant contributions (such as the Canada Learning Bond and Disability Savings Bond) might result in increased capital without any savings at all. Similarly, gains made passively as a result of market conditions (for example rising housing or stock prices) can increase wealth with little or no effort on the part of the beneficiary. The theory on asset effects does not necessarily differentiate how the assets are acquired or their value is raised. In this view, it may be simpler to not require participants to save at all but instead to just transfer an equivalent lump sum — e.g., as an education grant, scholarship or voucher.

However, the act of saving, by making regular deposits into a savings instrument, may be an important mechanism for acquiring productive assets. Saving up smaller amounts towards a large goal may allow households, particularly those with less disposable income, to smooth the "lumpy" costs of a home or PSE over a period of time, making it more affordable to participate in a program using ongoing income flows.

More importantly, according to Sherraden, the very act of saving becomes a self-reinforcing behaviour. In his model, saving increases the value of the desired savings goal, and the chances of attaining it, by requiring repeated personal contributions. It also promotes self-efficacy, as measurable progress is made toward a valued goal. Finally, saving over a period of time, says Sherraden, sustains the longer-term thinking and planning that he believes are crucial to exits from cycles of poverty.

Sherraden (1991) had proposed the use of a restricted savings account that he termed an "Individual Development Account" or IDA that embraces the above concepts. He suggested that IDAs be used by policy-makers, as an addition to traditional income support policy, to provide a subsidized vehicle to enable low-income and low-asset households to save and acquire certain productive assets that might improve longer-term well-being. He argued that these "productive assets" might include homeownership, small business development, higher education for dependent children, and adult education or training. Sherraden suggested that deposits into the IDA be matched from public funds or philanthropic sources at a relatively generous rate such as $3 for each $1 saved and that withdrawals of the matched funds be restricted to the above-mentioned menu of human capital or tangible assets. He further proposed that financial education be delivered to accountholders to reduce the risk of hardship, for example, by teaching budgeting

skills and smart consumption to reallocate resources and enable regular IDA deposits.

In his writing, Sherraden and like-minded others have been clear to state that IDAs are just one of many ways of increasing savings and asset development among low-income households. However, IDAs and similar matched savings mechanisms remain the most common instrument of what has been termed asset-building or asset-based policy worldwide.

Sherraden does not discount the role of institutional factors such as wage rates, access to education, labour markets, welfare walls, and social supports in improving well-being, but rather views the cycle of poverty as a dynamic exchange between these factors and individuals, in which self-defeating behaviours become rationalized and rewarded. In the 19 years since *Assets and the Poor* was published, Sherraden has tended to place increasing emphasis on the importance of institutional factors in shaping individual financial behaviours and wealth outcomes, perhaps downplaying his own earlier assertions about the degree to which saving might "change the heads of the poor" (see for example Beverly, Sherraden, Zhan, Williams Shanks, Nam & Cramer, 2008).

Research results from previous IDA initiatives

The research literature on the effects of assets holding is sparse and mixed. A review by Scanlon and Paige-Adams (2001) of the literature on the effects of assets found promising but fairly weak evidence that assets, particularly housing equity, were associated with household stability, improved child outcomes, and certain care-taking behaviours such as increased voting among homeowners versus renters. Research commissioned by the Canadian Mortgage and Housing Corporation (CMHC, 2006) further suggests that housing wealth is strongly and positively associated with other savings, investments and pension wealth. In other words, homeowners built more non-housing wealth compared to their counterparts in the rental market. Finally, in a study for the UK Institute for Public Policy Research, Bynner (in Bynner and Paxton, 2004) looked at panel data for a cohort of youth in the UK and found that the presence of even a small amount of savings in early adulthood was associated with significantly better employment, education, and even health outcomes later on.

The current research on the effects of savings incentives for low-income populations is even less compelling. The first demonstration of IDAs was the U.S. Down payments on the American Dream Demonstration (ADD),[2] referred to above, which ran from 1997 to 2003. The demonstration used the IDA model with a range of match rates (averaging at 2:1), financial management training, and the traditional list of IDA savings goals of homeownership, small business start-up, and PSE enrolment. There were 13 sites, one of which (at Tulsa, Oklahoma) was an experimental one that generated evidence based on comparisons of outcomes between the program participants and a control group. Analysis of the data from experimental and non-experimental ADD sites continues, but recent papers suggest mixed results and many more questions to explore.

Sherraden (2008) provides an overview of the most up-to-date findings from the wider ADD research. A sample of his findings regarding design and delivery issues follows:

- Participants with IDA accounts were nearly evenly split between "savers" and "non-savers"; the distinction was based on a relatively modest benchmark of just U.S.$100 in net IDA savings over the project.
- Administrative costs are high. The ADD IDA project had administrative costs of U.S.$64 per month for each account before adding the cost of the matching funds. By comparison, 401K accounts (personal retirement savings accounts with employers) are estimated to cost just U.S.$10 per month per account in administration.
- Participants appear to be more responsive to the match cap or maximum (the ceiling on the amount that will be matched in the IDA) than to the IDA match rate. Sherraden hypothesizes that the ceiling acts as a concrete goal for IDA participants to aim for where the IDA functions as a deterrent to short-term spending.
- Participant savings seem to increase with up to 10 hours of financial management training and then are stagnant or even decline with further training.
- Participant income, education, employment, and welfare dependence were not predictive of savings outcomes.

As for the effects of the IDAs, the controlled ADD IDA experiment at Tulsa found no significant impacts on overall net worth (Mills, Gale, Patterson, Engelhardt, Eriksen, and Apostolov, 2008a). However, the IDA had positive effects on homeownership rates and home purchases among African American participants but negative effects on non-retirement assets. The fact that participants showed increases in housing wealth but decreases in financial wealth suggests that they had shifted savings into housing equity. Also, it may be that the assets purchased with the IDA funds will lead to increases in net

2 For a description of the ADD, see its website: http://add.cfed.org/index.html

worth over time but, concedes Sherraden (2008), it is also possible that IDA participants were drawn by the saving matches into making ill-advised investments with their new savings (e.g., in homes they could not carry).

Given that homeownership was the most common use of the ADD savings, Grinstein-Weiss, Lee, Irish, and Han (2007) examined the impacts on homeownership for ADD participants in the experimental study. They find that after 18 months in the program, IDA accountholders were much more likely to be reducing debts and to be engaged in activities to find a new home. After 48 months in the program, IDA accountholders were much more likely than non-accountholders to have purchased a home.

There have been mixed results for the impacts of IDAs on PSE enrolment. The experimental results from ADD showed no effects on participation in higher education and training (Mills et al., 2008a). This included degree and non-degree courses, despite the fact that earlier reports from the study and the same data had indicated significant increases in participation in non-degree courses (Mills, Gale, Patterson, and Apostolov, 2006). However, results from a study funded under the Assets for Independence (AFI) Program, a major federal source of current IDA funding in the U.S., did find large positive effects of IDAs on PSE enrolment (Mills, Lam, DiMarco, Rodger, and Kaul, 2008b). These estimates are based on a quasi-experimental design in which there is a matching comparison group, not a control group as in the case of the Tulsa results. Under the AFI Program, close to 500 IDA projects have been implemented through to the end of fiscal year 2007, with about 42,500 IDAs set up providing match rates ranging from $1 to $8 (although typically $2) for every $1 deposited in the IDA up to an average of $1,625, along with financial education, with asset goals of homeownership, business capitalization or PSE or training.[3]

Other research has been conducted in the UK and Australia on pilot projects with similar characteristics to American IDAs, but with the focus primarily on encouraging a saving habit. In the UK, Savings Gateway (SG) ran as a pilot initiative in two separate waves. The second SG pilot (Saving Gateway 2 or SG2) was introduced in 2005 and ran for 18 months to promote saving among working age people on lower incomes. It was offered to people with individual incomes below £25,000 a year and household incomes below £50,000, or those on income support benefits.[4] Individuals were provided with a saving match but it also varied by area in terms of the match-rate offered (ranging from 20p to £1 for each £1 contributed) and in terms of the monthly contribution limit (ranging from £25 a month to £125 a month). Participants could use their match funds for anything. Alongside the financial incentive to place funds in a SG2 account, the pilot also offered financial education in the form of a CD Rom, and tailored courses. Based on a comparison of outcomes between randomly assigned program and control groups less than a year after the conclusion of the program, SG had an incremental impact on participants' savings level (Harvey, Pettigrew, Richard, Emmerson, Tetlow, and Wakefield, 2007). As sources of the savings, there was evidence of diversion of funds from other assets among higher-income individuals and reduced purchases of food outside the home by lower-income individuals. There were no impacts on overall net worth. Results from a two-year follow-up research indicated that 61 per cent of participants were still saving regularly two years after their accounts matured and that about 30 per cent of those who were not saving regularly prior to the scheme were regular savers (i.e. saving at least monthly) at the time of the follow-up research (Ipsos MORI, 2009).

In Australia, Saver Plus has been introduced on a wide scale following two phases of pilot projects. This program is aimed at encouraging a saving habit, but with a focus on increasing savings for children's education.[5] The program includes three components: matched savings at a ratio of $1 for every $1 saved up to $1,000; financial literacy education; and support from the delivery organization. To be eligible to participate, an individual must be a parent or guardian of children enrolled in a government secondary school; have earnings through part-time, casual or self employment; and be able to demonstrate a capacity to save. Up to 2008, about 4,600 people started IDAs under the program. The results of non-experimental follow-up research (with no control group) of participants who indicated a willingness to participate in the research after leaving the program suggest lasting saving effects (Russell, Harlim, and Brooks, 2008). About 70 per cent were still saving 1–3 years after the program. Of these, half were saving regularly and another half were still saving towards education costs for their children. Only 7 per cent were saving for their own education.

Beside the matched incentive, evidence on the effectiveness of the other important component of IDAs, the financial education, has been even more limited to date. None of the research cited above was able to measure the specific role played by financial education on saving and

3 For more on the AFI program, see its website: www.acf.hhs.gov/programs/ocs/afi/

4 For more details on Saving Gateway, see its website: www.hm-treasury.gov.uk/saving_gateway.htm

5 For more on the Saver Plus program, see its website: www.anz.com/about-us/corporate-responsibility/community/financial-literacy-inclusion/programs/saver-plus/

other outcomes. Separate survey research on financial literacy finds that, while levels of financial literacy are very low overall, adults with low incomes and even those with low literacy may still match or even out-perform high-income adults on some elements of financial management such as keeping track of income and expenses (Atkinson, 2007; Atkinson, McKay, Kempson, and Collard, 2006), likely because their lack of income forces them to. This finding calls into question the traditional IDA program requirement of financial management training, or at least suggests that the need for increases in financial literacy may be as great or greater among higher-income individuals outside IDA programs. Other research suggests that financial education, such as the kind offered in IDA programs, may increase financial knowledge and contribute to improvements in financial behaviour or actions but other factors can also come into play (Hilgert, Hogarth, and Beverly, 2003).

While the above research is informative, it raises more questions than it answers about IDAs. Without further and rigorous research like the *learn*$ave demonstration, there is insufficient evidence on the utility of IDAs (or related savings instruments) for reaching certain social policy goals, including encouraging adult education and training. The *learn*$ave project is unique in at least three ways. First, it is one of only two rigorous research projects on this type of matched savings instruments, the other one being the ADD controlled IDA experiment at Tulsa referred to previously. Second, it is a demonstration of the IDA model in a mainly adult learning context, whereas most other IDAs have other purposes in addition to adult PSE, if PSE as a use for the credits was permitted at all. Third, it is distinguished by its rigorous test of the separate additional impact of financial literacy training and enhanced case management services on top of the incentives, whereas past projects can measure only the combined effect of the incentives and services when delivered as a package. As such, *learn*$ave can uniquely inform policy-makers in Canada and abroad about the potential for IDAs as a social policy instrument to promote participation in adult learning and education.

Outline of this report

The rest of this report presents the description of the *learn*$ave program design and its operations (Chapter 2), followed by a description of the research questions and the methodology used to address them (Chapter 3). Findings from the demonstration project are reported in subsequent chapters: Chapter 4 reviews lessons learned from the program implementation of *learn*$ave and examines the role of community-based non-profit organizations in the delivery of *learn*$ave; Chapter 5 presents saving and cash-out activity on IDAs accounts and the role of financial management training, case management, and program parameters in that activity; Chapter 6 presents impacts on budgeting, total savings, assets, debts, net worth, and hardship; and Chapter 7 looks at education and labour market impacts. Chapter 8 includes a cost-effectiveness analysis of the program. Chapter 9 draws key policy lessons and insights gathered through all the study phases.

Chapter 2 Program design and operations

The first part of this chapter describes the *learn*$ave program design and key features. The latter part presents details on program operations, such as participants' recruitment and assignment, as well as the establishment of a project management information system.

Program design

As mentioned in Chapter 1, *learn*$ave's design was very faithful to the original IDA model first proposed by Michael Sherraden (1991). It was also informed by the implementation of the Downpayments on the American Dream Demonstration (ADD) in the U.S. that had pre-dated the launch of *learn*$ave by several years. Like ADD and the more than 500 smaller IDA programs that have been implemented in the U.S. (Corporation for Enterprise Development or CFED, 2007), *learn*$ave combined a restricted matched savings account, case management services, and financial management training. Unlike any other IDA or asset-based policy project before or since, however, *learn*$ave explored two different ways of delivering IDAs: (1) the traditional approach of coupling a financial savings incentive with ancillary services, such as financial management training; and (2) a more streamlined approach of a financial savings incentive alone that more closely resembles other account-based policy measures such as RESPs and RRSPs.

The project was delivered in 10 communities. These included a mix of large and medium-sized urban areas, rural communities, and a range of local economic and educational environments across seven Canadian provinces.[1] At three of the sites — Halifax (Nova Scotia), Toronto (Ontario), and Vancouver (B.C.) — the project was designed and delivered using an experimental design with two different program groups and a matching control group. At the other seven sites — the Digby-Annapolis region (Nova Scotia), Fredericton (New Brunswick), Montreal (Quebec), the Grey-Bruce region (Ontario), Kitchener-Waterloo (Ontario), Winnipeg (Manitoba), and Calgary (Alberta) — *learn*$ave was delivered as a non-experimental pilot program. Findings from all 10 sites have been used to develop this final report; however, the estimation of the project impacts drew only on data from the experimental sites.

Participants in the *learn*$ave research experiment receiving the more traditional IDA model of an incentive with services were said to be in the *learn*$ave-plus program group, while those receiving the more streamlined model of just the credits were said to be in the *learn*$ave-only program group. All participants not participating in the experiment (i.e., Income Assistance recipients at the experimental sites and all participants at the non-experimental sites) received the equivalent of what the *learn*$ave-plus participants received.

Project partners

There were two main partners in *learn*$ave: Social and Enterprise Development Innovations (SEDI) and Social Research and Demonstration Corporation (SRDC). The partnership was the first to combine a voluntary sector agency's abilities in idea development, community networking and program implementation and management with a research organizations skills in designing and implementing rigorous evaluation methodologies. Funding for the project design, implementation and research was provided by HRSDC as a research project under Part II of *The Employment Insurance Act*.

SEDI had lead responsibility for the design of the IDA program model and its delivery. SEDI had introduced the IDA policy innovation idea to Canada, undertaking much prior stakeholder consultation and research on the concept of IDAs. In *learn*$ave, SEDI, in consultation with HRSDC, selected the 10 communities where the project would be introduced; in developing partnerships with third-party organizations to deliver frontline services, financial services, information technology in these communities; and in financial training curriculum development. SEDI also oversaw project implementation, supervising the delivery of the project in each site, ensuring compliance with the project operations guidelines, and administering project funds to cover both project operation expenses as well as the matching credits to participants.

SEDI worked closely with the other lead partner, SRDC. SRDC was solely responsible for the evaluation of the project and maintained ongoing responsibility for the collection, management and analysis of *learn*$ave and the generation of the research results presented in this report.

Participant eligibility

To be eligible for *learn*$ave, applicants had to meet the following criteria:

- *Income:* Annual household income could not exceed 120 per cent of the before-tax Low-income Cut-off (LICO),[2]

1 See Kingwell, Dowie, Holler, Vincent, Gyarmati, and Cao (2005) for profiles of each community.

2 The LICO is one measure of poverty, which varies by local area and size of household. In the early 2000s, the LICO was in the low to mid $30,000s for the communities in question. For more explanation, see, for example: Statistics Canada (2006). "Low-income

a measure of poverty tied to community and household size. At the experimental sites, this limit translated to about $36,000 for a household of three in Toronto and Vancouver at the time of recruitment, and to $31,000 in Halifax. This threshold was considered high enough to include families with sufficient income to save without serious hardship. In Winnipeg, household income had to be less than the LICO and two-thirds of participants had to be earning less than 60 per cent of the LICO, reflecting the clientele the local agency expected to recruit into the project (Kingwell et al., 2005).

Given that there is considerable annual mobility within the low-income population in Canada (see for example Finnie, 2000), the income threshold was applied to the year of application and to the previous calendar year. This criterion effectively excluded those whose income had only temporarily fallen.

Household income, rather than individual income, determined eligibility for *learn*$ave, consistent with most income-tested programs (e.g., income tax credits, income assistance, student loans and daycare subsidies). Generally, household members pool financial resources to meet household costs. An individual may have a low-income job but have access to considerable funds in relatively wealthy households. Examining household income, therefore, reduces the likelihood of low-income individuals in otherwise wealthy households entering a program such as this that is geared to those in true poverty. Household members were limited to those related by kinship or marriage, consistent with Statistics Canada's definition of "economic family."

Newcomers to Canada who had immigrated in the year prior to application or the year of application were assessed using a special formula that took into consideration "world income" as well as funds brought into Canada at the point of entry and funds transferred from overseas between the time of arrival and the date of application completion. Newcomers with combined income and liquid assets of less than 120 per cent of LICO plus $3,000 in the year of and prior to application were eligible. For further description of this formula, see Kingwell et al. (2005, p. 48–50).

- *Liquid assets:* The reported value of household liquid assets (e.g., deposit accounts, Guaranteed Income Certificates, Registered Retirement Savings Plans and trust funds) could not exceed the lesser of $3,000 or 10 per cent of annual household income. The limit was intended to exclude those who had already access to a pool of fungible capital that could otherwise be used for the same purposes as *learn*$ave and to limit the possibility of participants simply shifting capital into a *learn*$ave account to benefit from a higher rate of return.[3]

- *Home:* On the basis that housing equity forms the largest pool of capital against which homeowners can borrow at reduced costs, the value of the household's home could not exceed the median value of homes in the area.

- *Age:* Applicants had to be of working age (21–65 years), since the primary focus of the demonstration was adult learning and small business development. Applicants 18–20 years of age had to be out of school for at least two years. This approach is consistent with other programs targeted to mature students who are re-entering formal education, rather than continuing current educational plans.

- *Educational enrolment:* Applicants could not be full-time students at the time of application, i.e., carrying at least 60 per cent of a full course load, as per the definition used by the Canada Student Loans Program and most provincial and territorial loans. Again, the objective was to target the project to those who were out of the formal education system. Full-time students were deemed to have already found a way to finance their education and therefore not truly in need of *learn*$ave.

  Part-time students were eligible for the project on the basis that they could still be expected to use the benefits from *learn*$ave to increase their participation in adult learning by moving toward more intensive full-time studies.

- *Residence:* Applicants had to be residing within the recruitment boundaries of a *learn*$ave site (although they could have moved later). This criterion was largely to facilitate the delivery of the services of the project including any liaison with participating financial institutions, financial management training and case management services. Given the relatively small and community-based nature of the delivery agencies, it may have been challenging to offer consistent levels of service to clients living at greater distances. In both the Vancouver and Toronto experimental sites, the recruitment boundaries included very large metropolitan areas. In the Digby-Annapolis and Grey-Bruce sites, the recruitment included large, rural and more sparsely populated areas.

Cut-offs for 2005 and Low Income Measures for 2004." http://www.statcan.gc.ca/pub/75f0002m/75f0002m2006004-eng.pdf.

3 Some participants who were admitted into the project did hold larger assets but in these instances the capital was deemed to be held in a locked-in account (usually a trust fund or locked-in retirement account) that could not have been accessed during the course of the project.

Table 2.1 *learn*$ave Program Saving Parameters[1], by Site

Site	Income Eligibility	Match Rate	Match Cap	Maximum Saving Period	Other Unique Features
Experimental Sites					
Halifax, NS	<= 120% of LICO	3:1	$1,500	3 years	
Toronto, ON	<= 120% of LICO	3:1	$1,500	3 years	
Vancouver, BC	<= 120% of LICO	3:1	$1,500	3 years	
Non-Experimental Sites					
Digby-Annapolis, NS	<= 120% of LICO	*4:1*	*$1,125*	3 years	
Fredericton, NB	<= 120% of LICO	3:1	*$2,000*	3 years	
Montreal, QC	<= 120% of LICO	*5:1*	*$900*	3 years	
Grey-Bruce, ON	<= 120% of LICO	*2.5:1*	$1,500	3 years	*Extra $0.50 match per dollar if savings goals met*
Kitchener-Waterloo, ON	<= 120% of LICO	*2:1*	$1,500	3 years	*Enhanced counselling available to clients*
Winnipeg, MB	*< LICO*[2]	3:1	$1,500	3 years	*Focused on very low income client group*[2]
Calgary, AB	<= 120% of LICO	3:1	$1,500	*2 years*	

1 Differences in aspects of the program and setting from the experimental sites are indicated in italics. Another basic difference is that IA recipients represent varying proportions of participants across the 10 sites, though the maximum was established at 25 per cent in the non-experimental sites (see later in the text for more on these exceptions).

2 Two-thirds of participants must have income that is 60 per cent of the LICO or less.

- *Other:* Only one person per household could apply. Applicants had to have a Social Insurance Number, thus allowing non-permanent residents (such as refugee claimants) to apply for *learn*$ave, along with those born in Canada.

Matched saving credits

At the core of the *learn*$ave program model, as for other IDAs, was the matched saving credit incentive. Participants were provided with credits for every dollar they saved in their designated *learn*$ave account. The match rate varied from 2:1 in the Kitchener-Waterloo site to 5:1 in Montreal (see Table 2.1). In most sites, including all three experimental sites, the match rate was 3:1. In Grey-Bruce, participants received an extra $0.50 credit per $1 saved if they met their saving goal. For a definition of this and other IDA terms used in this report, see the glossary near the end of this report. For a discussion of other IDA programs in Canada, see Box 2.1.

To encourage participants to save on a regular basis, participants had to make net deposits (all deposits less all withdrawals) in their *learn*$ave account of at least $10 in each of 12 months (non-consecutive) before their withdrawals would qualify for the matched credits. As well as encouraging regular saving, this restriction was also expected to manage the risk of participants borrowing or shifting assets to quickly meet the maximum savings and exiting with a large return for little or no real "new" savings. The maximum saving period — the period during savings in the *learn*$ave account qualify for match credits — was three years in all sites except Calgary, where it was two years to better enable the community partner to coordinate services for *learn*$ave clients alongside other IDA clientele.

There were upper limits on the account deposits that qualified for matched saving credits, also referred to as the match cap. It was $250 each month at all sites, again largely to minimize opportunities to transfer existing assets or borrowed amounts. Over the lifetime of each account however, the match cap varied by site, generally depending on the local match rate in place. For example, at a 5:1 match rate, the match cap was $900 in Montreal and at 4:1 was $1,125 in Digby-Annapolis. At the 3 experimental sites, it was $1,500.

Despite the variation in match rate and cap, in most sites the maximum amount of credits that could be earned did not vary, at $4,500. The exceptions were in Fredericton and Kitchener-Waterloo.[4]

4 In the Kitchener-Waterloo site, the match rate was reduced to 2:1 to a maximum of $3,000 on the first $1,500 of participant savings. The difference in the maximum matched credits (up to $1,500 per participant) was transferred to the operating budget of the Kitchener-Waterloo delivery agency to support more intensive personal counselling services above and beyond the standard case management and financial management training offered in other sites (these are discussed below). In the Fredericton site, a contribution from the provincial government to the local *learn*$ave delivery agency enabled the site to increase the match cap to $2,000 as well as increasing the maximum matching credits to $6,000, using a 3:1

Box 2.1 Other Canadian IDA programs

While the ***learn***$ave project is by far the largest IDA program, there are several smaller IDA programs in communities across Canada, including the BC Asset-Building Collaborative (BC ABC), the Fair Gains IDA program in Calgary and SEED Winnipeg's IDA program, some of which ***learn***$ave has inspired.

BC ABC was formed in 2004 as an association of some 15 BC IDA program providers to share best practices, identify funding sources and advocate for public policy on IDAs. Fair Gains and the Winnipeg programs are the longest-running and best-established local IDA programs in Canada.

Fair Gains was launched by Momentum Calgary in 1999 in the wake of consultations on ***learn***$ave organized by SEDI. The program offers IDAs for youth and working age adults with match rates between 3:1 and 5:1. Fair Gains accountholders can use their savings (of up to $2,400 including matching credits) for education, training, small business start-up, and employment-related tools or to save for a dependent child's higher education after saving for one year and taking part in extensive financial education workshops. A two-year IDA program is also offered for accountholders saving up to $8,400 (including matching credits) towards a down payment on a home. Each year, Momentum accepts just over 100 new accountholders, delivers case management and financial management training to nearly another 100 clients, and administers just over 100 cash-outs, most of which are for adult learning.

SEED Winnipeg launched its local IDA program as ***learn***$ave was starting up in 2000. Similar to Momentum's IDA program, SEED accountholders are offered a 3:1 match rate on savings (over 2 years) for adult education, children's education, self-employment and homeownership. Given Winnipeg's stock of low-cost but rundown housing, the Winnipeg IDA program also encourages current homeowners to use their IDA savings to invest in repairs and renovations that will improve the value of their housing equity. Thirty-five new accountholders entered the program in 2008 while another 134 graduated. SEED also runs a savings circle program that provides a 3:1 match on smaller savings over just 6 months to kick-start a savings habit and help very low-income participants cover larger household purchases such as furniture or computer equipment.

In recent years, the Alberta and Manitoba provincial governments have funded local IDA programs. Momentum's Youth Fair Gains now receives provincial funding for the delivery costs but not the matching credits. After providing funding to SEED for several years for its local IDA, the Manitoba Government announced that in 2008–09 SEED will be running the Manitoba Saves program, offering IDAs to low-income residents (including social assistance recipients) in Winnipeg, Brandon and Thompson.

Finally, the matching amounts accumulated in learn$ave accounts as virtual credits. The credits could be accessed only when participants could demonstrate they were ready to withdraw them (or "cash them out") for approved purposes.

Saving streams

At enrolment, participants were required to select one of two saving streams: (1) education or training and (2) micro-enterprise (small business) start-up. However, the proportion of small-business savers within the project was capped at 20 per cent in each project site.[5] This cap was implemented to ensure that the project retained its main purpose as a test of a policy instrument to promote adult learning. That said, it was acknowledged that some participants might benefit as much or more from the knowledge and experience gained from developing and launching a new small enterprise as from a formal education or training experience. Where these limited spaces in micro-enterprise were over-subscribed, they were awarded by lottery or on a first-come-first-served basis. Those participants saving towards microenterprise start-up continued to be able to access their savings credits for formal education or training as well. However, the reverse was not true for participants enrolled in the education or training stream: they were obliged to use their credits solely for education or training.[6]

Account structure and financial institution partners

The vast majority of accounts (and indeed all accounts in the three experimental sites) were held with RBC Royal Bank which had been selected by SEDI as the lead partnering financial institution. In the Winnipeg and Montreal project sites, local *learn*$ave delivery agencies developed relationships with regional financial institutions (Assiniboine Credit Union and the Caisse populaire Desjardins, respectively) to offer account services that were comparable to the RBC product adapted for the demonstration project.

The *learn*$ave accounts were, in all important ways, deposit accounts with very low fees, and limited account

match rate. This was the only site that received any additional funding for the delivery of the *learn*$ave project.

5 With the exception of the rural communities of Grey-Bruce and Digby-Annapolis where, because there were few education providers and self-employment prospects might be better than local employment opportunities even for better educated residents, it was decided this cap could be exceeded, up to 40 per cent in the micro-enterprise stream.

6 In non-experimental sites, participants could transfer from the education stream to the micro-enterprise stream if space became available.

features, and earning very low interest. Participants were able to make deposits and withdrawals of their own funds directly from the account using a wide range of options including branch banking, automated-banking machines, on-line banking, pre-authorized transfers from other accounts, etc. The financial institutions did not charge for deposits, but did charge a flat rate of $1 for each withdrawal, in an effort to discourage frequent unmatched withdrawals.

In addition to meeting the eligibility requirements for the project, those participants who were entitled to *learn*$ave matching credits also had to meet financial institution requirements to open a deposit account before they could open a *learn*$ave account. This included identification requirements (under federal banking regulations) as well as a review of their financial history (determined by internal bank policy). Participants were informed of these conditions in advance of their enrolment and very few participants were ever turned down by participating financial institutions for their *learn*$ave deposit account.

As mentioned, the matching credits were never deposited into the *learn*$ave account but rather were an accumulated future entitlement until such time as the participant "cashed out" his or her savings for an eligible goal. Participants received regular account statements from their financial institution, as well as separate statements from their *learn*$ave delivery agency with the additional information on the value of the accumulated savings credits. These latter statements were generated by the local project staff using the Project Management Information System (PMIS, discussed in greater detail later in this chapter) and drawing on account information shared through secure data transfers from the financial institution.

When they exited the project, participants were able to convert their *learn*$ave accounts into regular deposit accounts within the host financial institution. In fact, for some participants, this may have been their first re-introduction or at least intensification of a relationship with a mainstream financial services provider. Chapter 6 of this report presents evidence of the degree to which participants' financial integration was enhanced under *learn*$ave.

Uses of the matched credits

The two main purposes for which the matched credits could be used or "cashed out" were in accordance with one's saving stream/goal: (1) education or training and (2) micro-enterprise (small business) start-up. The credits were generally expected to be used for the participant's own education or small business start-up, depending on the saving stream. However, given that only one eligible household member could take part and recognizing that households tend to pool resources, credits could also be transferred to another adult family member who had been eligible at the start of the project. Participants could use their earned credits in any number of smaller withdrawals or could withdraw a larger lump sum all at once.

Education stream participants had to use their credits for learning purposes only. The credits could be used to cover tuition costs. In addition to tuition, certain other costs related to adult learning could be covered by *learn*$ave funds. These were labelled "learning supports" and included books and computers, as well as child care services and disability supports unavailable from government programs. Participants could use up to 50 per cent of their accumulated *learn*$ave funds (deposits and credits), to a maximum of $1,500, for supports to learning. Cheques for supports to learning were made out to the vendor selling the good or service, just as the cheques to cover the tuition. Also, in the rural communities of Grey-Bruce and Digby-Annapolis, participants could use their credits to cover transportation costs to sometimes distant education institutions.

For micro-enterprise stream participants, small business activities eligible for matching credits had to be part of a new business venture that required no more than $10,000 in start-up capital from all sources. A business plan was needed prior to receipt of the match funds, which could also be used to cover the cost of developing a business plan.

Financial management training and case management services

Traditional IDA approaches offer a mix of financial incentives and social services that are thought to work together to improve participant outcomes. In focus groups and interviews, IDA program participants often cite the financial incentive as the major factor that drew them to apply but attribute much of their success to the attention and support received as part of the social services of the program (Sherraden, McBride, Johnson, Hanson, Ssewamala, & Shanks, 2005). Most *learn*$ave participants received financial management training and enhanced case management services, in addition to the matching savings credits. These included all clients in the *learn*$ave-plus program group at the experimental sites, all participants at the non-experimental sites and the subset of income assistance participants at the experimental sites.

Financial management training

The financial management training curricula was adapted from existing sources. Several financial education courses, programs and curricula are available (Social and Enterprise Development Initiatives, 2006; Orton, 2007) and several had in fact been developed for use in previous IDA programs. After reviewing some of these existing resources, SEDI elected to develop a new training program that could be standardized across the project sites. A 15-hour curriculum was developed with consultants from the Prior Learning and Assessment Centre (PLA Centre) in Halifax.

All experimental sites used the same curriculum, as did most non-experimental sites. The exceptions were the project delivery agencies in Fredericton, Grey-Bruce, Winnipeg and Calgary, which chose to use their own locally-designed curricula instead.[7]

Project staff from all sites were invited to take part in facilitator training on the new curriculum before it was delivered to project participants. The curriculum was also pilot-tested with very early groups of participants and adjustments were made to the facilitator and participant materials as required. The learn$ave curriculum was taught in five modules, the details of which are provided in Appendix B, with both in-class small group activities as well as individual assignments to be completed at home. In essence, the course had two main components:

- *Financial knowledge and skills:* Understanding compound interest, tracking expenses, setting and staying within a household budget, understanding credit rating systems and interacting with financial institutions.
- *Personal development:* Exploring personal attitudes and beliefs about money and consumption, documenting past applied financial learning experiences, setting goals and planning for associated costs.

The financial information and education offered in the curriculum were not unique to *learn*$ave and could also be obtained through a wide range of other curricula or financial information sources.[8] The use of the Prior Learning and Recognition (PLAR) pedagogical approach was unique to the *learn*$ave curriculum, however. Originally developed for use in facilitating the transition of mature students into PSE institutions, PLAR aims to offer a mechanism to document informal learning alongside formal credentials. PLA programs generally involve the creation of a personalized inventory of informal or experiential learning for participating clients as well as some effort to document the skills and knowledge that were gained and could be applied in an education or employment setting. According to a recent review commissioned by the Canadian Council on Learning, PLAR is now practiced in a variety of academic and employment contexts across Canada such as foreign credential recognition for skilled newcomers, recognition of employment-related learning in apprenticeship programs and employment planning for social assistance clients (Wihak, 2005). Given the context of adult learning, it was expected that the PLAR approach would enrich the financial management training experience for participants.

The primary objectives of the financial management training were to impart financial knowledge and to help participants set and meet their savings goals. It was also expected to reduce the chance of hardship and promote sustained improvements in financial practices beyond the life the project. However, as discussed in Box 2.2, there is widespread debate about whether financial education does lead to measurable behavioural change.

It is also worth noting that the financial training in the *learn*$ave curriculum was of a general nature and not necessarily specific to the either education or small-business financing. While participants were supported in developing a savings plan to meet their goals, they did not receive instruction on, for example, accounting practices for small-businesses or how to select an education or training course or program. This is worth noting because the leading IDA practitioners in the U.S. have long argued that the financial management training ought to be tailored to each savings goal to be most effective (see, for example, Connecticut Department of Labour, 2009). The U.S. context, however, is one in which homeownership is among the most common IDA uses, an asset with arguably greater potential risk to the participant where asset-specific information might be critical.

Case management services

The intensity of case management services provided by site staff varied by program group. All participants (except, of course, control group members) at all 10 sites received basic administrative assistance from project site staff, but the *learn*$ave-plus group received enhanced treatment, as will be described below. The basic administrative assistance provided to all participants included help in filling out the application and enrolment forms,

7 In most cases, this was largely comparable to the SEDI-PLAR program developed for *learn*$ave but may have placed emphasis on certain types of information over others, pursued different pedagogical approaches (for example Winnipeg's financial education program was designed to be sensitive to cultural differences among a predominantly urban Aboriginal population) or offered longer periods of training.

8 For a review of financial literacy resources in Canada, see Social and Enterprise Development Initiatives, 2006.

Box 2.2 Impacts of financial education on behaviour

In their review of available evidence, Hogarth, Beverly, and Hilgert, (2003) find that, while there is a positive link between financial knowledge and financial behaviour, financial education is not the only way of acquiring such knowledge. People may learn from family, friends and personal financial experiences: they can gain financial knowledge by merely saving and by learning from others.

Survey research in the United Kingdom has found that levels of financial literacy are very low across the population. As a result, at least one leading researcher has concluded that it is unreasonable to expect large changes in response to public investments in short-term financial literacy interventions (Kempson, 2007). In the U.S., Lyons (2006) has suggested that behavioural models adapted from mental health and addictions treatment might be helpful in understanding the link between financial education and financial behaviour. It is proposed that individuals move from being unaware that a change in behaviour is needed, to pondering such a change, to planning for such a change until they begin to take concrete steps in the fourth and fifth stages by practicing and finally solidifying new behaviours. Lyons suggests that financial education programs may move individual clients somewhere further along the five-stage continuum, for example from contemplating a change in financial behaviour to planning or even practicing. However, she cautions, it may be unrealistic to expect one program to turn the tide for someone unaware that new financial habits are even needed, transforming them into someone with exemplary and lasting financial skills.

issuing monthly account statements (mentioned above) and help in accessing matching credits as part of the process of cashing out. As part of the account statement and support in the cash out process, participants also received periodic letters from the local delivery agency alerting them when they were eligible to withdraw matching credits, reminding them of the number of months left to save and, later, the approaching time limit to cash out any remaining savings credits. This basic level of service was considered to be the minimum necessary to move participants through the stages of the project and could be considered comparable in most respects to services from a financial institution on self-managed savings or registered investment accounts.

A higher level of case management support was available to participants in the *learn*$ave-plus group and the non-experimental groups. This assistance consisted of encouragement to meet savings targets, assistance to identify and address problems in meeting those targets, more frequent written reminders regarding program targets and deadlines and referrals to appropriate agencies to deal with personal or household challenges that were disclosed to case management staff (for example addictions counselling, mental health challenges, basic literacy needs or employment counselling). Case managers maintained contact with an assigned pool of participants but the size of the caseload varied significantly from one site to another.

As part of this more intensive level of service, case managers were expected to undertake a quarterly review of participants' saving activities, attendance at financial management training sessions, and progress toward goals. If a participant was having difficulty in any of these areas (including if they had not made a deposit in the previous three months), the case manager contacted him or her by telephone or personal letter to discuss reasons for their inability to save as well as to explore possible solutions. As well, participants outside the control and *learn*$ave-only groups were free to contact their case manager on their own at any time on issues related to their *learn*$ave participation. As mentioned earlier, the Kitchener-Waterloo site opted, as part of their program design, to offer as well in-house mental health services (individual counselling) within the enhanced case management services.

Program operations

Recruitment, orientation and group assignment

According the project design, *learn*$ave was intended to be as inclusive as possible to the greatest numbers of low-income working-age adults who might benefit from returning to school. There was no sampling frame or master or administrative list from which to randomly or systematically draw prospective participants. The local delivery agencies were charged with the task of recruiting participants from the general population. Spaces were filled in the project as eligible persons learned about it, elected to apply and were accepted on a first-come-first-served basis. Those who were recruited not only had to match the participant eligibility criteria but also self-select for the project on the basis of whether or not they (a) were interested in furthering their formal learning and (b) were willing to save toward such a goal. This means that the program interventions were not tested on a random sample of low-income working age adults but rather on a convenience sample made up of a group of eligible volunteers who had some interest (at the time of applying) in either savings or education as a means to improve their well-being.

The recruitment and related processes are portrayed in Figure 2.1 and described below.

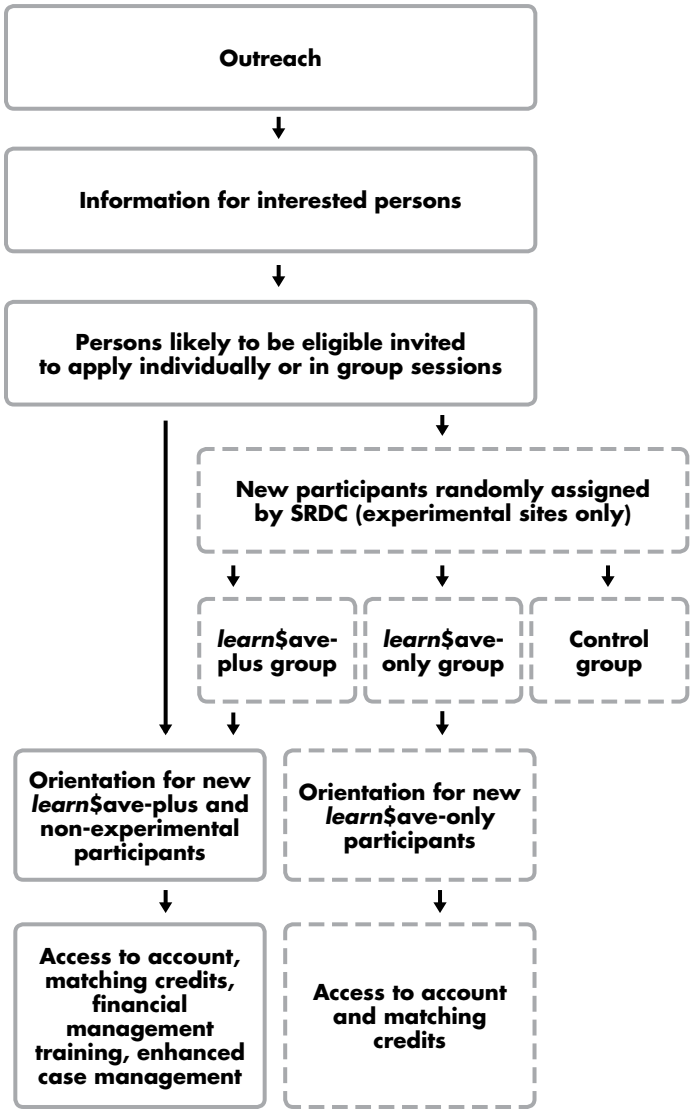
The recruitment conducted by local delivery agencies was through a variety of means but initially relied heavily on referrals from other agencies, brochures, posters and local government employment service offices. After several months into the recruitment period, the marketing approaches were expanded, with HRSDC's agreement, to emphasize print and electronic mass media, public posters and advertising on public transit. A more detailed discussion of the recruitment experience in *learn*$ave is available as part of the implementation discussion in Chapter 4.

The initial marketing messages directed interested people to call the local project office. During an initial telephone conversation (or more rarely an in-person inquiry), local project staff members provided additional basic information about *learn*$ave and generally reviewed eligibility criteria as a preliminary screening assessment. If the person appeared likely to be eligible and was still interested, she or he was invited to an application session, held in different locations throughout the community. In the smaller sites, this process was often compressed and participants may have applied to the project individually rather than waiting for a group application session.

In the experimental sites, application sessions included a standard slide presentation with information about the *learn*$ave project — including the IDA program parameters and the random assignment process.[9] Following the information presentation, interested persons were given an application package, including an informed consent form. The application form requested information to establish eligibility on each of the program criteria outlined earlier in this chapter. Most information was self-reported although applicants were required to provide proof of personal and spousal income for the current and previous year and project staff had discretion to request additional information or clarification as needed.

Once the application form was complete and all supporting documents were supplied, the site office conducted a final eligibility check. If the participant was deemed eligible for *learn*$ave in an experimental site, the completed application form was forwarded to POLLARA (the firm conducting the baseline and follow-up surveys), which entered the information contained on the form into a database and contacted the applicant for the baseline survey interview. No baseline surveys were conducted with participants outside of the experimental groups.

Figure 2.1 Overview of Program Intake



Once the enrolee completed the baseline interview, the participant's record was forwarded to SRDC for random assignment. If the enrolee was assigned to one of the two program groups, the project office contacted the person for an orientation session. This session gave the participant further details about the savings rules and opening a *learn*$ave account.

While recruitment was slow in most sites, the final project sample size was reached. The final sample of participants were, across the entire project, nearly 5,000 low-income, working-age adults with few liquid or housing assets who were not currently full-time students but who were willing to save to invest in their own further learning. A fuller description of participant characteristics is presented in Chapter 4.

9 An example of the kind of information shared with prospective applicants can be found in the electronic version of an information package developed by SEDI and available on-line at the organization's website (www.sedi.org).

Participation of income assistance recipients

Although U.S. IDA programs had shown that welfare recipients could and would save in matched savings accounts, there was some concern that income assistance (IA) recipients might have incomes so low that it would preclude savings or lead to increased risk of hardship if they did participate. Furthermore, HRSDC officials were clear from the outset that they viewed *learn*$ave as a test of a tool with the potential to fill a need for the working poor. In response to direction from HRSDC, the number of participants on provincial IA was capped at not more than 25 per cent of participants at the non-experimental site.[10]

In the experimental sites, all participants, including IA participants, had to have comparable treatments across the three sites as the original desire was to be able to pool all participants' results for analysis. In the early stages of *learn*$ave, therefore, SEDI with local delivery agencies consulted with provincial welfare authorities regarding the treatment of savings in *learn*$ave accounts under provincial welfare asset tests. All provinces and territories in Canada maintain regulations regarding the amount and kinds of assets that clients applying for IA or receiving IA can hold (for a more detailed discussion, see Robson, 2008). In BC and Nova Scotia, provincial authorities confirmed that current regulations and/or ministerial authority to issue exemptions would ensure that participant savings, as well as matched credits, would not be included in asset tests applied to new or ongoing IA cases. In Ontario, the exemption applied only to the matched credits while participants' own savings were considered to be part of the liquid assets scrutinized under the Ontario Works (OW) or Ontario Disability Support Program (ODSP) rules. A similar decision was made by New Brunswick welfare authorities. As a result of different treatment of *learn*$ave match funds by IA officials across the experimental sites, IA participants had to be excluded from the experimental sample and instead formed a small non-experimental group at each of these sites.

In practice, OW and ODSP participants in Toronto, as well as such participants in the non-experimental sites Grey-Bruce and Kitchener-Waterloo and IA participants in Fredericton, all had to monitor their personal savings to ensure they would not exceed provincial asset limits. If these participants reached the provincial limit and were eligible to cash out, participants were encouraged to do so and to restart their savings if time allowed. In all other provinces (BC, Alberta, Manitoba, Quebec and Nova Scotia) regulation changes or ministerial exemptions ensured that *learn*$ave accounts were excluded from provincial welfare asset tests. In the wake of *learn*$ave, however, many provinces have introduced exemptions in their welfare regulations to enable IA participants to take part in IDA programs.

Cash-out process

All credits originally had to be "cashed out" or used by month 48 following project enrolment. However, for situations where, for education stream participants, the end of the cash-out period did not coincide with the start of a school term, a six-month grace period was added. This enabled participants to use their credits up to month 54 provided they could demonstrate at month 48 the program for which they wanted to use credits would start within the next six months.

Well before participants become eligible to claim their matched credits (by having saved at least $10 net in 12 months since opening their account), local delivery agencies offered them detailed written information on the process as well as all of the necessary forms. Later on in the project, experimental sites began sharing this information and forms with participants as early as the orientation session (held shortly after a participant had enrolled).

Based on their experience with the earliest *learn*$ave participants who cashed out and in an attempt to make the cash-out process smoother for both participants and staff, the experimental sites held "cash-out orientation sessions" for participants in mid-2003. Given the smaller numbers of participants in other sites, project staff were able to respond to cash out requests, questions or concerns individually. Where numbers were sufficient in the experimental site, the local project staff invited participants to a group session before they become eligible for cashing out. These sessions reiterated the eligible uses of the funds and described the process, timelines and documents for cashing out. However, these sessions were not well attended — only a limited number of participants in Toronto and Vancouver and a few secondary sites attended.

When participants decided to withdraw matched credits, they submitted the completed request forms along with the necessary documentation to the site office. The office verified the forms to ensure eligibility and completeness and forwarded the information to SEDI. SEDI then produced the cheques and sent them to the project office by courier within one week after they received the requests. The credits were paid out by cheque to eligible

10 An exception was made in Winnipeg given that the local agency anticipated and did recruit significantly lower income participants than in other sites, many more of whom were dependant on IA at the time they applied. Note as well that this limit was exceeded in Grey-Bruce because the 25 per cent limit of 150 was reached before realization that the 150 per cent target would not be attained.

vendors of goods or services (an education or training provider or the small business, depending on the stream), not the participant. The cheques were prepared by SEDI after a careful review of participant claims forwarded from the project sites. The participants then picked up the cheques from the site office to cover no more than 75 per cent of the expenditure and, to cover the remaining portion (at least 25 per cent), withdrew the savings from their *learn*$ave accounts that corresponded to the matched credits.

To use their earned credits for education or training, participants had to first apply to and enrol in a course or program with an accredited education or training provider. Only education or training providers (including universities, community colleges, technical institutes, business schools, private academies and private career colleges) listed by the Canada Student Loans Program as a "Designated Educational Institution" were recognized as eligible learning institutions for the *learn*$ave project.[11] Even if participants were not requesting money for tuition, they had to provide proof that they were enrolled in a designated educational institution and a price quote from a reputable vendor. Restricting the education providers to this list was expected to protect against a major flaw in the design of a pilot of learning accounts in the United Kingdom. In the English locations of the pilot, concerns over fraud led to the suspension of the program as many participants were suspected of claiming Individual Learning Account benefits for questionable or non-existent training by unverified training providers (Haukka, Keating, and Lamb, 2004).

Cash-outs could be approved only for courses that would be taken in the future — participants could not be reimbursed for courses already underway or for courses they had completed. For both tuition and supports to learning, participants were required to provide original receipts within 30 days of cheque receipt. In addition, they were required to provide proof of course completion within 90 days of finishing the course.

The cash-out process for starting a new business was different from that for enrolling in education or training. To receive matched credits for start-up capital, micro-enterprise stream participants had to complete a business plan detailing the following: the nature of the business; a marketing and sales strategy; administrative and production processes; a human resources plan; and a financial plan. Participants' final business plans were then referred to a reputable business development agency in their local area that provided training and assistance for development of the business plan and was responsible for its approval. In addition, participants had to register their business with the provincial government and open a bank account in the name of the business. After the plan received approval, the matched credits were released.[12] The credits could not exceed the amount of the capital costs identified in the business plan. Although participants were not required to provide original receipts in all instances, they had to retain their receipts and produce them on request.

Participant Management Information System

The *learn*$ave Participant Management Information System (PMIS) was used at all sites to support both program operations and evaluation needs. This kind of computerized information system is considered an essential tool in any project involving large numbers of participants and financial transactions across many sites, such as in *learn*$ave. The PMIS was developed by SEDI to fit *learn*$ave's information needs, drawing on the example of MIS IDA, a management information system for U.S. IDA programs developed by the Center for Social Development. Site staff entered participant information into the PMIS at enrolment and also logged case management, financial management training and used the system to monitor client progress and account activities. The PMIS did not include information on control group members in the experimental sites as they received no benefits or services under *learn*$ave and therefore had no expected contact with the delivery agency.

Participating financial institutions sent monthly client account transaction data (including records of all *learn*$ave account deposits, withdrawals and balances) through secure on-line data channels to delivery agencies. The agencies used the PMIS to generate regular account statements and other communication with participants and shared the account information with SEDI. For monitoring and tracking services and credits for the participants with accounts, the PMIS proved invaluable to the sites, SEDI and eventually to the SRDC researchers.

Timeline for the project

The *learn*$ave project took place over a nine-year period. The project began in June 2000 when work began to design the demonstration project's operations and its evaluation. In the experimental sites, participant recruitment, enrolment and random assignment started in June 2001 and ended in February 2004. In the non-experimental sites, recruitment began slightly earlier and ended sooner, owing to the smaller numbers of

11 As part of the loan program, each province maintains a list of institutions at which students can receive federal and provincial student loans.

12 By providing an outline of their business plan, participants could withdraw a portion of their matched credits to complete the plan and conduct related activities such as market research, business training, and technical consulting.

participants to attract. A participant could be active in the program for up to four years from enrolment to the final opportunity to cash-out his or her savings. The last participant's saving period ended in February 2007 and his/her cash-out period in February 2008. Interim evaluation results have been released at intervals since 2005. The final stage of the project (its final evaluation) ends with this report.

Chapter 3 Research questions and evaluation methodology

The first section of the chapter discusses the main research questions that guided the evaluation of the project. The second section describes the methodology employed to address these questions.

Program logic and key research questions

Originally, the *learn*$ave project was conceived of and proposed by Social and Enterprise Development Innovations (SEDI) not as a test of a policy instrument for adult learning and small business start-up but as a national pilot project of the individual development account (IDA) model proposed by Sherraden (1991). SEDI strongly argued in favour of a pilot that would use IDAs to encourage the acquisition of the same wide range of assets originally suggested by Sherraden. However, as already mentioned in Chapter 1, HRSDC was specifically interested at the time in innovative approaches to encourage human capital acquisition among low-income adults. As finally implemented, the project was fundamentally a test of the suitability of an asset-building tool for encouraging adult learning and small business start-up among a low-income segment of the adult population.

As indicated in Figure 3.1, from the participant perspective, *learn*$ave involved four phases of activity: first, being recruited into and applying for the project; second, opening a *learn*$ave account and earning matched savings credits by making deposits, with or without the support of enhanced case management and financial management training; third, using or "cashing out" the *learn*$ave account balance for an eligible project goal of education, training or small-business start-up; and finally exiting the program and hopefully reaping benefits from the new learning acquired. Out of each of the first three phases, key questions to be addressed by the research were raised, which are laid out below.

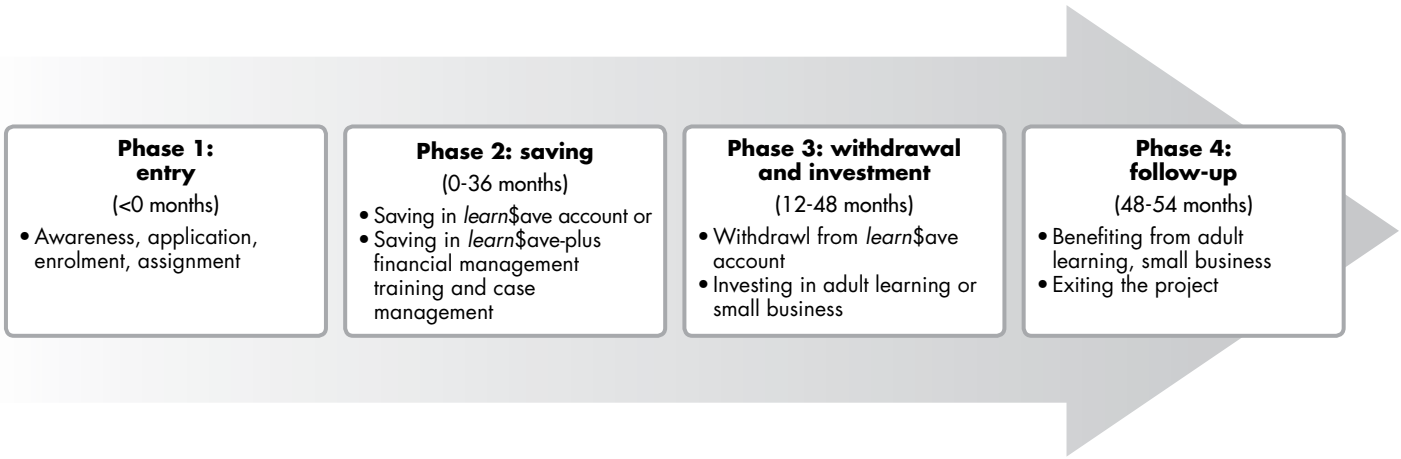
Phase 1 Entering *learn*$ave

The first stage involved attracting low-income people to a program that required them to put aside some of their scarce funds for education or small business start-up. It was not clear that low-income people would have been interested or willing to attend more education or even to save their own money towards it. Many prospective applicants could have been dissuaded by the eligible uses of the project funds (for education or small business only). Other prospective applicants could have been dissuaded by concerns regarding their capacity to make regular deposits into their *learn*$ave account. Finally, others may have been deterred by the fact that *learn*$ave was a demonstration project with no track record or the fact that they could end up in a control group without benefits. The key question arising from the recruitment stage of the project, then, is:

- Will the offer of a savings incentive for education or small business start-up attract large numbers of low-income Canadians? Will the participants have characteristics that distinguish them from the broader low-income population?

The recruitment process can tell us about the potential broader interest for assisted savings instruments in support of adult learning and education among low-wage individuals. Implementation research can also tell us about the relative attractiveness of the financial incentive and the offer of support for adult learning. Those participants who are attracted to the project and decide to take

Figure 3.1 Stages of *learn*$ave Program Participation



part may not be representative of the broader low-income population in Canada. The project results can reveal who is most likely to sign-up. The results can also be helpful in identifying other ways of attracting and serving clients.

Phase 2 Accumulating *learn*$ave savings

The second phase of the project was expected to yield increases in saving. Program participants were invited to open *learn*$ave accounts and make deposits that would be matched.

- Will *learn*$ave participants' attitudes to saving and education measurably change and will the change be durable? Will they be encouraged to set household budgets?

  Increases in savings toward an education goal are expected to lead to both improved financial behaviours, as participants need to budget and "learn to save," as well as increases in the value that participants ascribe to higher education and lifelong learning as they "save to learn." Looking out beyond the savings period of the program model, it is possible that the project will have lasting impacts on participants' attitudes towards education, though it will be impossible to verify this beyond the last data point in the current research design.

- Will *learn*$ave participants save more of their income than they would have saved without the financial incentive of the program?

  If the program model is to be deemed successful then it should encourage participants to make deposits into their IDA account and lead to an increase in savings.

- Will *learn*$ave participants accumulate new savings or will the program simply crowd out other assets? Will overall net worth (assets less debts) increase as a result of *learn*$ave?

  More than just leading to increased financial capital (at least in the short or medium-term) in the *learn*$ave account, the project should show that the matching grant incentives created real increases in savings, rather than creating an incentive for participants to simply shift savings from other accounts or saving vehicles into the IDA. Even with rules in place to guard against dumping existing capital into *learn*$ave accounts — such as the requirement to save $10 or more in each of 12 months to qualify for credits and a monthly cap of $250 in *learn*$ave deposits that earned matched credits — it is possible that some participants would meet their *learn*$ave deposit goals by reducing or eliminating other forms of saving, making the IDA a substitute rather than complement to other means of building financial capital. Participants could also choose to adjust other physical assets such as retirement income or even household goods or to increase their personal debts to invest in the IDA account.

  It should be pointed out that changes in net worth may not all happen within the project period. For instance, it is quite possible that, by encouraging education or small business start-ups with own or borrowed funds, *learn*$ave could lead to lower net worth during the project period. However, in the longer term, net worth could rise as the acquired skills and human capital translate into well-paying jobs or that the small business grows into a successful enterprise.

- Will *learn*$ave participants save without incurring hardship?

  Increases in saving and new savings are of no real value if they lead to a decrease in overall well-being. For example, if participants forego essential consumption (such as food, clothing or shelter) then the program model would have had a perverse effect. Sherraden's model proposes that it is possible and even likely for low-income households to reorganize their household expenditure patterns to increase saving while maintaining adequate consumption levels. The financial management training and enhanced case management support were thought to offer some added protection to participants by increasing their access to information and providing support in planning household resources.

- Will the additional financial management training and enhanced case management services contribute to increase the impact of the matched saving credit on saving and related behaviour? Will there be important differences in saving outcomes between participants in the "*learn*$ave-only" and "*learn*$ave-plus" groups?

  The only experimental research on IDAs before *learn*$ave was done in the U.S. as part of the Downpayments on the American Dream Demonstration (ADD), a collaboration between Michael Sherraden's own Center for Social Development and the Corporation for Enterprise Development (CFED). While a random assignment method was used, the results were not able to cast much light on the relative importance of the financial incentive and the ancillary services offered to participants. Since ADD was launched, there has been growing awareness among IDA practitioners that increasing the size of their programs is very challenging given the labour-intensive and high cost nature of the ancillary services in the traditional IDA package. Several leaders in the U.S. asset-building field at the Center for

Box 3.1 What can be learned about financial training from *learn*$ave?

As will be discussed in greater detail later in this chapter, the research design of ***learn***$ave does not permit measurement of the independent effects of the financial management training on participant savings, attitudes and other outcomes. Moreover, ***learn***$ave used only one particular financial literacy training model — the five-module curriculum developed adapted and developed for this project some nine years ago. In the intervening years, there has been an explosion of activity in the financial literacy field, but there has been little solid research about what works.

The Government of Canada has recently committed to developing a national strategy on financial literacy. Financial literacy programs targeting low-income and low-wealth populations like the ***learn***$ave sample are already offered by several community-based organizations across the country. Informed by their experience in asset-building programs, SEDI has launched a Centre for Financial Literacy to offer capacity building and advocacy on the topic. Private sector donors and foundations, such as the Investors Education Fund in Ontario have also begun to demonstrate support and at least one province (British Columbia) has made financial literacy a mandatory component of the secondary school curriculum. Outside Canada, the OECD, World Bank, EU, Australia, New Zealand, the U.S., Ireland, the UK and other nations and international bodies are similarly engaged in financial literacy or financial capability efforts.

The ***learn***$ave demonstration project was not primarily a test of financial literacy training for low-income adults and must not be interpreted as such. However, its results can provide several policy insights. For example, the implementation research can tell us what worked well in the delivery of the curriculum and whether participants fully took advantage of the financial training offered to them (see Chapter 4). Also, the impact analysis can shed light on whether addition of financial education and enhanced case management services can enhance the impact of the matched saving incentive on financial behaviours such as budgeting, avoiding hardship and planning ahead financially, in addition to saving more (see Chapter 5 and Chapter 6).

Social Development, New America Foundation, Brookings Institute and CFED have proposed that the solution to the question of scale might be to strip the IDA model down to its core as a matched savings account, or at least reduce the role played by the services (e.g., Sherraden, 2005). Like other assisted savings instruments, accounts could be publicly subsidized but administered through the income tax system and delivered through financial institutions that have the capacity to market and deliver products at scale. This has been the approach taken in the UK with both the Child Trust Fund and the Savings Gateway.

In designing the project, it was hoped that *learn*$ave might be able to make an important contribution to this policy debate. Thus, the decision was made by project leads to create two separate experimental groups who would receive either the financial incentive (the matched saving credits) alone or the financial incentive with the services (enhanced case management and financial management training). The final results from *learn*$ave, which include assessment of the incremental impacts of the services as well as their cost (see Box 3.1 for more on this), should shed a great deal of light on whether more easily scaled-up models might be effective.

Phase 3 Investing in human capital

In the third stage of the program model, it was expected that participants would stop building their account balances and draw them down to invest in one or more forms of adult learning or starting a small business. At this stage, then, it was expected that the increased financial capital would lead directly to increases in human capital investments and small business start-up. Over the longer term, perhaps well beyond the end of the project timeframe of nine years, these investments were expected to yield sufficient returns on investment to improve the labour market outcomes (both in terms of participation and earnings) for participants, helping more of them exit poverty than would have been the case without the project. In other words, more saving was expected to yield more learning which was expected to yield better employment and a measurable increase in household resources. Key questions regarding this phase of the program model include:

- Will *learn*$ave participants invest in more formal learning by taking more courses or programs than they would have otherwise?

Although some interest in adult learning opportunities was nearly a pre-requisite for project enrolment since participants self-selected, the primary research aim was to see whether working age adults would be responsive to an incentive to upgrade their training or return to school. This means that to be considered successful, *learn*$ave would have to show some measurable increases in the participation and/or persistence in adult learning over and above what would have been the case without the project. Given that the project attracted only participants who already had some interest in adult learning, it is reasonable to expect that many of these would have found other means to pursue that goal (for example by

borrowing from a student loans program). If the evaluation results showed a real increase in adult learning (both in short-term courses and longer-term programs) within the project sample, then it would be reasonable to expect it could have some impact on the broader population of low-income working age adults (interested in learning) if it were to be rolled out as a policy instrument.

- Will *learn*$ave participants invest in starting more small businesses than they would have otherwise?

It was expected that *learn*$ave would have a measurable effect on the investments in small business start-ups. To be successful in this regard, *learn*$ave would have to show that the number of small businesses created was greater with the project than without it.

- Can projections be made regarding the longer-term returns to participants' investments in adult education and small business start-ups? Can the program attain its goals of increased human capital in a cost-effective fashion?

In its later stages, the project may begin to show whether there are measurable improvements to the employment, earnings and net worth of program participants beyond what would otherwise have been the case. While the limited time period of the study may now allow for a complete cost–benefit analysis, it should be possible to observe the costs of delivering the credits and services and to compare them to the costs of other similar social programs.

Figure 3.2 provides an overview of the *learn*$ave IDA program logic including the expected impacts corresponding to the research questions described above.

Methodology

To address the research questions, the evaluation design called for multiple lines of evidence to be used. There were six main components of the *learn*$ave evaluation, briefly outlined below, along with details on the sources of the data used for each component.

- *Implementation research:* This research covered information on recruitment and enrolment, participant characteristics and the target population, financial management training and case management. It drew lessons learned on the implementation of an IDA program and set the context for interpreting subsequent impact results. This research was based on interviews with site representatives, focus groups with participants and non-participants, a 10-month mini follow-up survey of participants, and a market research survey of eligible individuals at some of the sites. The results are provided in this report and presented in detail in Kingwell et al. (2005).

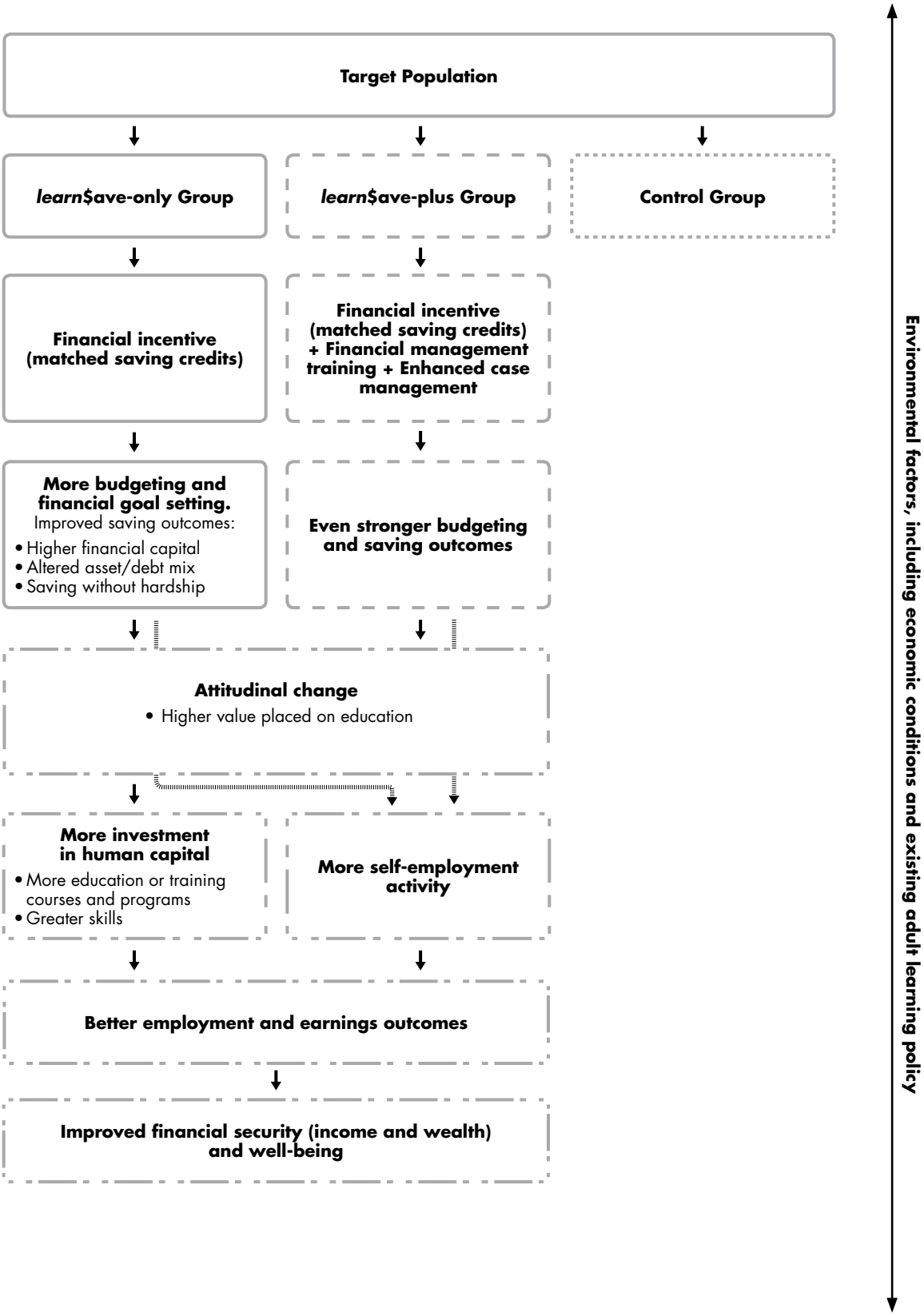
- *Service delivery case study of non-profit organizations:* Case studies of the 10 community-based delivery organizations participating in *learn*$ave were conducted to identify organizational characteristics that contributed to and hampered delivery of the *learn*$ave IDA. The results of this study contribute to the discussion of how best to deliver an IDA, taking into account the actual and potential roles played by financial institutions and the government. The source of data for this study was a series of interviews with representatives of sites, SEDI and the financial institutions.

- *Analysis of* learn*$ave account activity:* Measures of participants *learn*$ave deposit and withdrawal activity were generated to determine to what degree credits were earned and cashed out as well as potential reasons for not saving to the maximum and/or cashing out all credits earned. Incidences, means, and patterns over time were examined, as well as the use and impacts of the financial management training and case management services. The Participant Management Information System (PMIS) was the source of data for this analysis.

- *Measuring the role played by program parameters and income assistance:* This study took advantage of the variations in program parameters across the seven non-experimental sites and the three experimental sites to measure, using Ordinary Least Squares regression, the role played by program parameters and Income Assistance (IA) status in *learn*$ave saving activity among participants. Included in this study are *learn*$ave-plus and IA recipients at the experimental sites and all participants at the non-experimental sites. The Participant Management Information System (PMIS) was the main source of information for this study.

- *Estimating the impacts of* learn*$ave:* The impacts of *learn*$ave were estimated by comparing outcomes of randomly assigned program and control groups. The main advantage of random assignment experiments to estimate impacts of a policy intervention is that it permits consideration of the "counterfactual," i.e., what would have occurred without a particular intervention, or, in this case, what individuals would have done had they not participated in *learn*$ave. It is quite possible that some low-income individuals, on their own, without *learn*$ave, would have decided to save and to use their savings to continue their education or start a new business, and in many cases, their employment situation and earnings would have improved over time as a result. Therefore,

Figure 3.2 *learn*$ave Program Logic



to capture what would have happened to the program group in absence of the program, one needs to observe the experiences and activities of a control group of individuals who were similar in every way to participants in the *learn*$ave IDA but who did not receive *learn*$ave incentives or services.

The baseline and follow-up surveys of participants were the main sources of data for estimating the program impacts on outcomes described earlier as research questions regarding budgeting, savings, net worth, hardship, education and training enrolment, small business start-up, and employment. Early and intermediate impact estimates appeared in earlier reports in the *learn*$ave series (Leckie et al., 2008 and 2009).

- *Cost-effectiveness study:* A cost-effectiveness study was conducted to derive an estimate of the cost of delivering an asset-based program like the *learn*$ave IDA. One source for this study was the administrative cost data provided by the site organizations and SEDI on various activities conducted under *learn*$ave, including recruiting, training, case management, and so on. The other sources of data were a time/cost study of the experimental sites, as well as the PMIS, which provided data on the amount spent on cash-outs. Together, these data generated measures of cost-economy (per unit of input, e.g., per participant); cost-efficiency (per unit of output, e.g., per dollar saved); and cost-effectiveness (per unit of incremental impact, e.g., per participant who was enrolled in an education program).

Sample size

In total, there were data collected on 4,802 participants for the various *learn*$ave studies across the experimental and non-experimental sites.

At each experimental site, the original *learn*$ave design plan called for 1,200 enrolees, for a total of 3,600 participants. However, as a result of low response in Halifax (due in part to the relatively small population base of that city), the unused Halifax places were allocated to Toronto and Vancouver. In the end, 3,601 were recruited, with 254 in Halifax, 1,697 in Toronto, and 1,650 in Vancouver, equally distributed across the three research groups.

However, the actual sample size for the experimental study was somewhat lower. In a subsequent review of the application process, 14 enrolees were found to not meet the eligibility criteria and 2 enrolees withdrew their application. Two other enrolees were excluded for administrative reasons. This left 3,583 in the sample for the experimental study, as shown in Table 3.1, with 1,648 in Vancouver, 1,681 in Toronto, and 254 in Halifax. Across the three research groups, participants were distributed as follows: 1,195 in the *learn*$ave-only group; 1,193 in the *learn*$ave-plus group; and 1,195 in the control group. As well at the experimental sites, there were 225 Income

Table 3.1 Number of *learn*$ave Participants, by Study Type, Research Group, Site, and Income Assistance (IA) Status

	Experimental Study, by Research Group				Non-Experimental Study, by IA Status		
Site	**Control**	***learn*$ave-only**	***learn*$ave-plus**	**Total**	**IA Recipients**	**Non-IA Recipients**	**Total**
Experimental	**1,195**	**1,195**	**1,193**	**3,583**	**225**	…	**3,808**
Halifax, NS	85	85	84	254	75	…	329
Toronto, ON	560	560	561	1,681	75	…	1,756
Vancouver, BC	550	550	548	1,648	75	…	1,723
Non-Experimental	…	…	…	…	**241**	**753**	**994**
Digby-Annapolis, NS	…	…	…	…	38	112	150
Fredericton, NB	…	…	…	…	37	112	149
Montreal, QC	…	…	…	…	25	124	149
Grey-Bruce, ON	…	…	…	…	37	64	101
Kitchener-Waterloo, ON	…	…	…	…	39	110	149
Winnipeg, MB	…	…	…	…	48	102	150
Calgary, AB	…	…	…	…	17	129	146
Total	**1,195**	**1,195**	**1,193**	**3,583**	**466**	**753**	**4,802**

Source: Participant Management Information System.

Note: … not applicable. Non-IA recipients were not recruited for the non-experimental study at the experimental sites, nor were there experimental participants at the non-experimental sites.

Assistance (IA) recipients who received *learn*$ave-plus services and the matched credits but were not part of the experimental study.

At the non-experimental sites, the target of 150 participants, who received *learn*$ave-plus services and credits, was attained for the most part. The main exception was in the large rural area of Grey-Bruce where just 101 participants could be enrolled (Table 3.1). This made for a total of 1,001 participants at these sites, of whom 466 were IA recipients. The number of IA recipients was originally limited to 25 per cent of a site's participants. However, Winnipeg received special dispensation to exceed the limit owing to the nature of its clientele, and Grey-Bruce reached its IA target of 25 per cent of 150 participants before realizing it could not reach its total target of 150.

Survey data

Baseline and follow-up telephone surveys conducted by POLLARA Inc. (under contract with SRDC) were the source of the outcome data used in the experimental study for the estimation of impacts.[1] At the experimental sites, shortly after meeting the eligibility criteria and before random assignment to one of the three research groups, applicants were surveyed to gather baseline information on personal and family characteristics. Participants were then re-contacted in follow-up surveys at 18, 40, and 54 months from the date of their last interview to gather outcome data in order to measure impacts. Data from the final 54-month follow-up survey were the source for impact estimates presented later in this report, with comparisons to published results from earlier 18- and 40-month surveys.

A total of 2,269 participants responded to the 54-month survey, out of the original individuals randomly assigned in the project (Table 3.2). This translates into a response rate of 63.3 per cent. The 54-month survey occurred between September 2006 and September 2008, with an average survey interview length of about 34 minutes. This is considerably lower than the 55-minute average for the 40-month survey, for at least three reasons: first, only a 14-month period over which to ask respondents to recall their education activities, compared to a 22-month period in the 40-month survey (40 less 18 months); second, familiarity on the part of interviewers and interviewees with the routine and the questions; and third, a lack of contact module, which was used in prior surveys to update participants' contact information. Owing to the project time constraints, the 102 participants in the last three month-cohorts were called in the 54-month survey (as with the 40-month survey) 3, 2, and 1 month(s) early, respectively, as part of the fourth last month-cohort in June 2008. This meant that there was somewhat less time for these individuals than earlier cohorts (1) to be contacted (though there were only 10 who still were being called when the survey closed) and (2) to have saved and used their credits for education or small business.

Between the 40- and 54-month surveys, survey response actually rose as attrition appears to have levelled off. One reason is that many of the participants who were going to "drop out" had already done so prior to the later surveys. The plateauing of attrition was also due to two measures introduced by SRDC:

- the introduction, after the first months of the 54-month survey, of a $50 response incentive for all respondents.[2] (In the 40-month survey, the response incentive had been $20 or $50 upon first refusal.); and
- the use of the PMIS to obtain contact information on the program group participants to bolster survey response. The PMIS was felt to have more up-to-date contact information than the survey data. Indeed, this appeared to have worked somewhat, as the untraceable rate fell from the 40-month to the 54-month survey.[3]

As in all follow-up surveys, the 54-month survey response rate for the control group was lower than it was for the *learn*$ave-only and *learn*$ave-plus program groups, with the gap widening somewhat between groups since the 18-month survey (Table 3.2). This is not surprising given that control group program members likely felt less engaged in the project and thus less inclined to participate in the surveys. Most of the differences in response rates can be attributed to the higher incidence of refusals and untraceable participants in the control group than in the program groups, as the table indicates.

The unbalanced attrition (difference in response rates between program and control groups) did not negatively affect the socio-demographic comparability of the groups to a significant extent and therefore the ability to measure impacts by comparing the outcomes of the groups. If significant differences had appeared, then differences in

1 Data sources used in other aspects of this study are described in Appendix C.

2 As there was some fear that the increased incentive for everyone would mean an overall lower income respondent pool, comparisons were made between those responding before and after the incentive was introduced. However, there were few differences in baseline characteristics between these groups.

3 There was some concern that, as the PMIS contained information on the program groups but not on the control group, this would create systematic bias between research groups. However, respondents found through PMIS contact information were found to be no different from those who were contacted right away or through contact information found in regular database searches.

Table 3.2 *learn*$ave Survey Response, by Survey and Research Group

Survey and Response Disposition	Total	*learn*$ave-only Group	*learn*$ave-plus Group	Control Group
Baseline Survey (n)	3,584[1]	1,195	1,194[1]	1,195
18-month Survey				
No. of completions (n)	2,583	920	915	748
Completion rate (%)	**72.1**	**77.0**	**76.6**	**62.6**
Non-completions (%):				
▪ Refusals and withdrawal requests	5.5	3.3	2.6	10.5
▪ Untraceables	17.6	15.6	16.3	21.0
▪ Other incompletes[2]	4.9	4.1	4.5	5.8
40-month Survey				
No. of completions (n)	2,260	837	815	608
Completion rate (%)	**63.1**	**70.0**	**68.3**	**50.9**
Non-completions (%):				
▪ Refusals and withdrawal requests	7.1	5.2	4.2	12.0
▪ Untraceables	27.3	23.1	25.6	33.3
▪ Other incompletes[2]	2.5	1.7	1.9	3.8
54-month Survey				
No. of completions (n)	2,269	842	859	568
Completion rate (%)	**63.3**	**70.5**	**71.9**	**47.5**
Non-completions (%):				
▪ Refusals and withdrawal requests	6.1	4.2	3.9	10.1
▪ Untraceables	25.1	20.8	18.8	35.6
▪ Other incompletes[2]	5.5	4.5	5.4	6.7

Source: Reports provided by POLLARA to SRDC and calculations by SRDC.

Note: [1] One *learn*$ave-plus participant who should not have been was included in the survey sample, but that person's data were excluded from the analysis.
[2] Includes those who were "retired" because they were called 15 times without success or who had not been reached by the time the survey ended, plus those who withdrew from the research prior to the survey or who could not respond because of illness or death or for some other reason.

outcomes between program and control groups could no longer be considered reliable estimates of impacts, which was the original reason for random assignment of enrolees among research groups. However, the profiles of the respondents to the 54-month survey, based on their socio-demographic characteristics at baseline, were found to be fairly similar across research groups (see Table 3.3). The differences in baseline variables that arose along the way[4] include the following: marital status, level of higher education of mother, being unemployed, and total household income. The next section explains how this response bias was controlled using regression adjustments.
Other response bias tests, including an examination of the characteristics of non-respondents, indicated that unbalanced survey attrition did not affect the integrity of the research groups. For details see Appendix D.

Survey data on monetary values such as assets are subject to fairly high incidence of missing values and variation. In the case of *learn*$ave, this occurred despite follow-up range questions for those who did not a supply an answer and follow-up "re-ask" questions for responses that exceeded certain limits. Owing to the potential compound effect of missing survey values in the large number of assets and liabilities variables making up net worth, it was decided that missing values for these variables should be imputed. The imputation procedure used here, called "Multiple Imputation" in the statistics literature, involved the replacement of missing value with several randomly selected values of the responses to the respective question provided by similar participants who did supply a response to the respective question. See Appendix D for details of the imputation procedure,

4 Indeed, an examination of the longitudinal subsample of those who responded to all surveys indicated several significant differences from the baseline sample and it was decided, therefore, that it could not be used to measure impacts representative of those of the would-be participants. The smaller sample size of the longitudinal sample was also a barrier to using it in the analysis.

Table 3.3 Baseline Characteristics (%) and Differences between Research Groups, for 54-month Respondents

	Means, by Research Group			*learn*$ave-only vs Control		*learn*$ave-only vs *learn*$ave-plus		*learn*$ave-plus vs Control	
Characteristic at Baseline	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff.**	**S.E.**	**Diff.**	**S.E.**	**Diff**	**S.E.**
Gender									
Male	44.5	45.8	43.8	0.7	(2.7)	1.2	(2.4)	1.9	(2.7)
Female	55.5	54.2	56.2	-0.7	(2.7)	-1.2	(2.4)	-1.9	(2.7)
Age									
Under 21 years	0.4	0.7	1.2	-0.9*	(0.5)	0.3	(0.4)	-0.5	(0.5)
21-30 years	40.5	40.4	39.3	1.2	(2.7)	-0.1	(2.4)	1.1	(2.7)
31-40 years	43.7	44.0	43.3	0.4	(2.7)	0.3	(2.4)	0.7	(2.7)
41-50 years	12.8	11.9	13.9	-1.1	(1.8)	-1.0	(1.6)	-2.0	(1.8)
51-65 years	2.5	3.0	2.3	0.2	(0.9)	0.5	(0.8)	0.7	(0.9)
Average age (years)	33.5	33.5	33.7	-0.2	(0.4)	0.1	(0.4)	-0.1	(0.4)
Marital Status									
Single	44.7	43.4	42.4	2.2	(2.7)	-1.2	(2.4)	1.0	(2.7)
Married	44.2	44.2	44.7	-0.5	(2.7)	0.1	(2.4)	-0.5	(2.7)
Separated, Divorced or Widowed	11.2	12.3	12.9	-1.7	(1.8)	1.2	(1.6)	-0.5	(1.8)
Equity Groups									
Visible minority	63.9	64.8	62.0	1.9	(2.6)	0.9	(2.3)	2.8	(2.6)
Aboriginal	1.0	0.9	1.4	-0.5	(0.6)	0.0	(0.5)	-0.5	(0.6)
Activity limitation (disability)	5.8	8.0	8.3	-2.5*	(1.4)	2.2*	(1.3)	-0.2	(1.4)
Year of Entry into Canada									
Before 1993	16.7	14.0	13.2	3.5	(2.4)	-2.7	(2.1)	0.8	(2.4)
1993 - 1997	6.4	7.6	7.6	-1.2	(1.7)	1.2	(1.5)	0.0	(1.7)
After 1997	77.0	78.4	79.2	-2.3	(2.8)	1.5	(2.4)	-0.8	(2.8)
Highest Level of Formal Education									
Less than high school graduation certificate	1.7	2.3	1.9	-0.3	(0.8)	0.7	(0.7)	0.4	(0.8)
High school graduation certificate	5.9	6.9	9.0	-3.0**	(1.4)	0.9	(1.2)	-2.1	(1.4)
Some post-secondary education	15.3	15.3	17.1	-1.8	(2.0)	-0.1	(1.8)	-1.8	(2.0)
Non-university certificate or diploma	21.6	19.6	21.5	0.1	(2.2)	-2.1	(2.0)	-1.9	(2.2)
University degree	55.5	56.0	50.5	4.9*	(2.7)	0.5	(2.4)	5.5**	(2.7)
Labour Force Status									
Work for pay	54.9	56.5	55.2	-0.3	(2.7)	1.5	(2.4)	1.3	(2.7)
Self-employed	10.9	12.0	12.7	-1.8	(1.8)	1.1	(1.6)	-0.7	(1.7)
Unemployed	24.1	22.7	24.7	-0.6	(2.3)	-1.4	(2.1)	-2.0	(2.3)
Out of labour force (student, at home, retired, and not working for pay)	10.0	8.8	7.4	2.6*	(1.5)	-1.1	(1.4)	1.4	(1.5)

Table 3.3 Baseline Characteristics (%) and Differences between Research Groups, for 54-month Respondents (Continued)

	Means, by Research Group			*learn*$ave-only vs Control		*learn*$ave-only vs *learn*$ave-plus		*learn*$ave-plus vs Control	
Characteristic at Baseline	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff.**	**S.E.**	**Diff.**	**S.E.**	**Diff**	**S.E.**
Household Income									
Under $5,000	14.4	14.4	10.2	4.1**	(1.9)	0.0	(1.7)	4.2**	(1.9)
Between $5,000 and $9,999	18.8	19.4	19.7	-0.9	(2.2)	0.7	(2.0)	-0.3	(2.2)
Between $10,000 and $14,999	23.9	22.3	22.0	1.9	(2.4)	-1.6	(2.1)	0.3	(2.3)
Between $15,000 and $19,999	19.1	22.0	20.5	-1.3	(2.3)	2.9	(2.0)	1.6	(2.3)
Between $20,000 and $24,999	12.4	11.3	14.2	-1.8	(1.8)	-1.1	(1.6)	-2.9	(1.8)
Between $25,000 and $29,999	5.4	4.8	7.0	-1.6	1.3	-0.6	(1.1)	-2.2*	(1.3)
$30,000 and more	6.1	5.8	6.4	-0.3	1.3	-0.3	(1.2)	-0.7	(1.3)

Source: Calcuations based on the 54-month survey.

Note: Overall, there were 842 *learn*$ave-only respondents, 859 *learn*$ave-plus respondents, and 568 control group respondents to the 54-month survey.
Sample sizes vary by individual measures because of missing values.
Two-tailed t-tests were applied to the differences between research groups.
Statistical significance levels are indicated as * = 10 per cent, ** = 5 per cent, and *** = 1 per cent.
Rounding may cause slight discrepancies in sums.

including information on how outliers were dealt with, as well as the results of sensitivity testing.

Measuring impacts

To measure *learn*$ave's impacts, the first step was to compute differences in outcomes between research groups covering the 54-month period.[5] For each outcome variable, three impact estimates are presented, as follows:

- To measure the pure impact of the matched credits alone, the outcomes of the *learn*$ave-only group were compared to those of the control group.
- To measure the added impact of the financial management training and enhanced case management services over and above the credits, the outcomes of the *learn*$ave-plus and *learn*$ave-only program groups were compared. Note that when comparisons are made between these groups as reported in later chapters, it is critical that the reader not draw the inference that the difference between the groups represents the pure effect of the services. Instead, the difference should be interpreted as the incremental effect associated with adding the services to the matched credit. As there was no program group receiving only the services, the effects of the services cannot be examined in isolation from their interaction with the effects of the matching credit.
- Finally, the total combined impact of the incentive and the services is measured by the differences in outcomes between the *learn*$ave-plus and control groups.

These comparisons generate reliable estimates of impacts because of the similarity of the groups, as per the experimental design. Thus, statistically significant differences in outcomes can be attributed to *learn*$ave since other factors, such as socio-demographic characteristics and attitudes to the future, have been controlled for through random assignment.

Finally, to strengthen the estimates, they were regression-adjusted. All impact estimates presented in Chapters 6 and 7 and the corresponding tables in the appendix have been regression-adjusted. As per Mohr (1995) and Orr (1999), while the unadjusted estimates are reliable estimates based as they are on randomly assigned program and control groups, regression adjustment improves the impact estimates by increasing their statistical precision and controlling for the effect of small differences in socio-demographic variables that existed at baseline or that arose in the follow-up surveys (although, as noted, the research groups remained fairly comparable throughout). In the adjustment model, the outcome is "explained" in terms of (or controlled for) a set of variables indicating the participants' research group, their *learn*$ave site (Halifax, Toronto, or Vancouver), their socio-demographic characteristics at baseline, and their attitude toward the future at baseline. See Appendix D for further details on the regression adjustment process.

5 Measurement of monetary value outcomes was done in nominal terms and not adjusted for inflation, for two reasons. First, program asset eligibility rules were not inflation-adjusted over the approximately two-year recruitment period. Second, inflation during the study period was low and the time between two surveys was less than two years.

Chapter 4 Implementation and service delivery: Lessons learned

The process of developing and delivering a demonstration project of the size of *learn*$ave can provide a rich source of information for policy-makers and practitioners involved in the implementation of similar programs. This chapter first discusses lessons learned regarding recruitment, the delivery of ancillary services (case management and financial management training) and the process of claiming or "cashing out" earned matched credits. The last part of the chapter discusses the role of community organizations in the delivery of *learn*$ave within the context of on-going policy discussions on the relative merits of using the tax system versus community organizations for the delivery of such programs. The research findings are drawn from several lines of evidence. They are listed below and described in greater detail in Appendix C of this report, Kingwell et al. (2005) and Ritch (2008):

- focus groups with participants and non-participants at a number of sites and with participants with regular saving patterns and those with patterns of low or no savings;
- interviews with representatives of all 10 project delivery sites, on-site observations, and exit interviews with 39 participants at the early stages of the project;
- a mini 10-month follow-up survey of participants;
- a Market Research Survey (MRS) in the three experimental sites; and
- interviews with approximately 45 representatives of all project partner organization conducted during the later stages of the project.

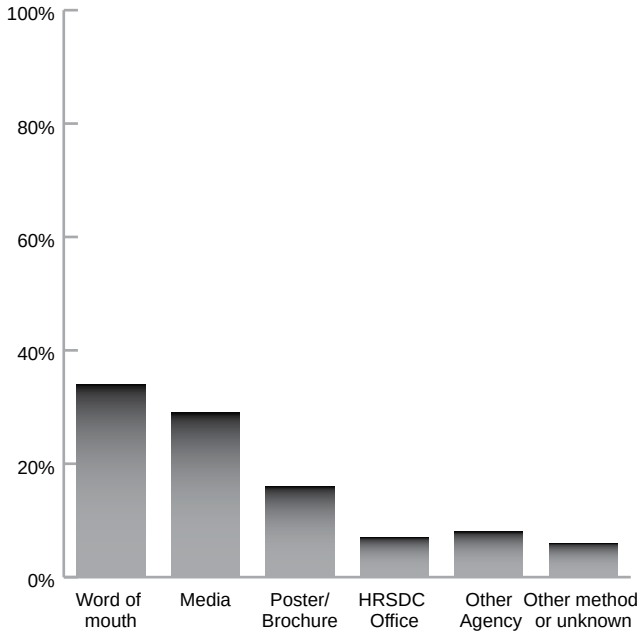
Recruitment

Recruitment was conducted through a variety of means, each with varying degrees of success. Across the sites, word of mouth proved to be the most effective way to attract new participants to the project but it took time to first build awareness of the project, and second to enrol a critical mass of participants who could help disseminate interest for the project within their community and social networks.

At the three experimental sites, outreach began in August 2001 and as early as May 2001 in the first of the non-experimental sites. Early in the recruitment period all three experimental sites (but especially Halifax and Toronto), drawing from past experiences in recruiting participants to smaller scale programs, relied heavily on outreach through networking with other local agencies. Within a short period of time, these sites found this method to be fairly time-consuming and relatively ineffective in attracting applicants to the project. For any number of reasons, other agencies did not serve as a significant source of referrals for participants in the experimental sites. In fact, just eight per cent of all participants who enrolled in the experimental sites had learned about the project through a referral from another local agency (see Figure 4.1).

Initially, *learn*$ave achieved very low recruitment results (see Figure 4.2). This is not inconsistent with what happened in the ADD project in the U.S. (Sherraden, Johnson, Clancy, Beverly, Schreiner, Zhan and Curley, 2000). It is also similar to initial patterns in uptake of other savings instruments aimed at lower income populations, such as the Canada Learning Bond. Several months into the recruitment period, and with coordination from SEDI, the project offices at the experimental sites began to conduct an outreach campaign for the project aimed directly at target clients, rather than at third-party agencies. Their second-wave efforts included advertizing on public transit, in local newspapers, distributing brochures and posters and arranging interviews with local media to promote awareness of the project among

Figure 4.1 Primary Method by which Participants Heard about learn$ave, Program Group Participants



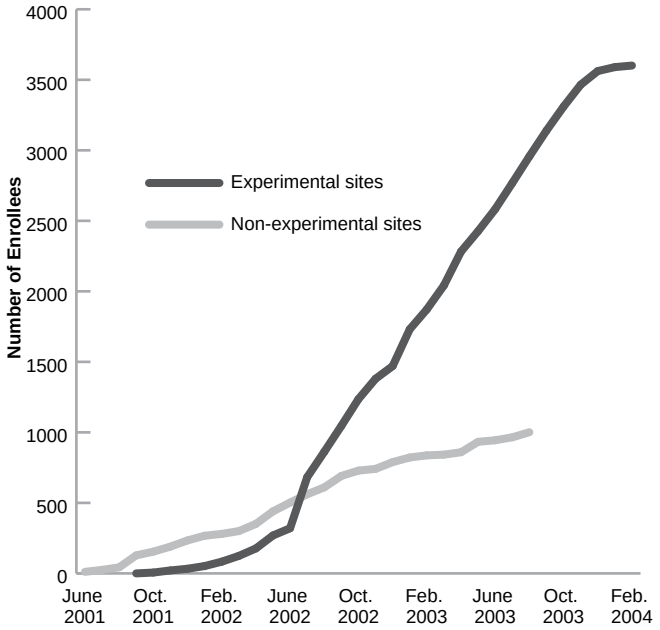
Source: Participant Management Information System.
Notes: For this figure, data for the experimental study includes only participants in the *learn*$ave-only and *learn*$ave-plus groups.
Total sample size is 2,383.

the target population. The non-experimental sites used similar methods to promote the project and attract eligible participants but at one or more sites (including the experimental Halifax site), advertizing on local cable television channels and direct mail targeted by postal code were additional methods used; two sites (Winnipeg and Calgary) were able to benefit from awareness of their own IDA programs. The larger urban centres of Montreal, Winnipeg and Calgary found that a multi-faceted advertising campaign was not necessary. However, many of the smaller-market sites discovered at some point that they had to expand their range of marketing methods at least to some degree.

Nearly one third (29 per cent) of enrolees in the project reported hearing about *learn*$ave through local media. Advertizing or interviews with local ethnic media were particularly effective in Toronto and may have contributed to the large proportion of the final sample who were newcomers to Canada at the time they applied to *learn*$ave (see the discussion later in this chapter).

When participants were asked how they had heard about the project, the single most common method was "word of mouth," at 35 per cent of participants. This is again consistent with previous experience with IDAs in the U.S. The marketing directly to target clients may have raised their awareness of the project, but the decision to apply and enrol may have required some communication with a trusted personal contact such as a friend, family member or community member. In fact, research on

Figure 4.2 Cumulative Recruitment by Type of Site and Date



Source: Participant Management Information System and Baseline Survey.

marketing techniques suggests that external marketing efforts (such as advertising, direct mail, etc.) are primarily effective only in the early stages when a new product or service is being introduced (Goldenberg et al., 2001) and are quickly outpaced by the effects of word of mouth from strong or even weakly connected community members. This suggests that building a critical mass of participants already in the project may have been important to recruiting the remainder, which would be consistent with the observed patterns of enrolment. Through their social networks, existing participants can serve as examples or even informal ambassadors for the IDA project, demonstrating that the project is real and achievable for others who meet the selection criteria.

Recruitment was completed in Halifax in July 2003, in Toronto in August 2003, and in Vancouver in December 2003. Recruitment in the last of the non-experimental sites ended by August 2003.

Potential reasons for recruitment difficulties and slow/low take-up

The focus group and other evidence points toward a number of reasons why recruitment was difficult. The evidence suggested that recruitment challenges were related both to certain features of the project design and to information or attitudinal barriers. The above-mentioned pattern of word of mouth may be most effective in addressing these attitudinal challenges to attracting new participants.

Barriers related to project design

- *Constrained uses of matched credits:* More people might have applied if the credits could have been used for other "assets" as not all eligible low-income adults saw education or small business as the best route to improving their well-being. Several focus group members suggested that savings and credits should have been available for other purposes beyond education or starting a small business, placing a higher value on other goals such as home ownership to "get ahead." Similarly, several non-participants indicated a lack of interest in *learn*$ave's savings goals (education or small business) and expressed uncertainty about their own goals. Although the non-participants were eligible, they did not see enough personal benefit in the project to decide to participate.

- *Research component:* More individuals may have been attracted and signed up for *learn*$ave if it had not had been a temporary/demonstration project. The time limits may have made it less attractive than, for example, on-going RRSP savings that allow accountholders to opt in and out as their circumstances permit. According to the focus groups, many who decided not to enrol in *learn*$ave

were discouraged by the possibility of being assigned to the control group at the random assignment sites. On the other hand, focus groups with participants who had enrolled in the project found that, while they did not want to be put in the control group, they regarded *learn*$ave as an opportunity and felt they had nothing to lose in taking a chance an applying despite the random assignment process. Also, results from the Market Research Survey indicated that only about 5 per cent of the small number of respondents who had negative views of *learn*$ave specifically identified the risk of being assigned to the control group as the reason for their poor impression of the project (Kingwell, 2005).

- *No master list:* The fact that recruitment had to be done from a convenience sample contributed to the recruitment challenges. For ease in filling project spaces, as well as for targeting, checking against eligibility criteria and intake, it would have been preferable to have selected participants (including those for the control group) randomly from a list of eligible people. Other demonstrations and even ongoing programs frequently rely on some administrative rule for program participation as a pre-condition for eligibility. For example, the Canada Learning Bond is aimed only at families receiving the National Child Benefit Supplement. Similarly, most EI Part II-funded training is limited to clients with recent or current eligibility for or receipt of EI Part I income-benefits. In these cases, a master list can be used to randomly or systematically select participants and their outcomes can then be compared.

- *Paper burden:* Most non-participants said they were overwhelmed and discouraged when faced with the *learn*$ave application form. This was particularly true for those who took part in application sessions attended by a large number of people and those living in rural areas who had received forms in the mail. A few non-participants expressed irritation with the type of information requested on the *learn*$ave application form. Several participants were reluctant to share income tax information to prove their income as part of the screening for eligibility.

Barriers related to information or attitudes

- *Fears and lack of confidence:* For some non-participants, personal problems and fears were an obstacle to applying. For a few, the *learn*$ave offer came at a time when they were experiencing family crisis or other pressing problems that led them to conclude the project was outside of their reach for the time being. A few non-participants expressed serious reservations about their ability to save the required minimum amount of 10 dollars per month for 12 months to receive matching funds. Several non-participants saw the need to open a bank account as a barrier to enrolling in the project and expressed mistrust in financial institutions or feeling intimidated by mainstream banks. Some believed that past financial difficulties would make it difficult or impossible to open an account. Others were concerned that creditors would seize their *learn*$ave savings to cover outstanding debts or that they might lose other means-tested government benefits (such as daycare subsidies, rental subsidies and social assistance). These concerns have also been noted in the American Dream Demonstration IDA (Adams, 2005) and in other reviews of asset-tested benefits (Robson, 2008).

- *Skepticism:* Without a track record of success and proven effectiveness, it would be understandable that many prospective applicants might view the project with some skepticism. Also, at the time *learn*$ave was implemented, there were few if any other savings and asset-building initiatives for low-income households, perhaps reflecting or even shaping a less supportive climate for such measures. Both participants and non-participants said they were initially skeptical about the offer and described a $3 to $1 match rate as "too good to be true." This response is consistent with the experience in matched saving programs like IDAs in other countries (Rohe et al., 2005; ECOTEC, 2004; Russell, 2006). It is worth noting that low-income adults may face greater exposure than other adults to misleading advertisements promising quick and easy access to income. Those who eventually enrolled in *learn*$ave initially seemed to be actually more skeptical than non-participants about the legitimacy of the project; but they were more inclined to make the effort and make inquiries, which eventually satisfied their doubts. To them, gaining access to the matched credits had been worth the effort.

- *Insufficient proceeds:* Some non-participants in the project reported in focus groups that they felt the amount of credits available from the *learn*$ave IDA (a maximum of $4,500 in cashed out credits at the experimental sites plus $1,500 in one's own savings) might not have been enough for the education they wanted to take. However, this may be linked to misperceptions about the real cost of education (see Box 4.1).

Lessons learned

The initial recruitment difficulties and the qualitative evidence suggest that significant efforts would be needed to market a *learn*$ave program at scale. A national marketing campaign (using mass media) on the benefits of saving, alongside public information about the new program, would need to be considered to raise awareness and addressing skeptical attitudes or low confidence of

Box 4.1 Were the maximum funds high enough?

One way to evaluate the value of the funds is to consider what kind of education or training participants might have been able to purchase with ***learn***$ave funds. Under ***learn***$ave, up to $6,000 could be used towards the costs of adult learning (a maximum of $4,500 in matching credits plus $1,500 of the participants' own savings). In 2006-07, survey results presented by the Millennium Scholarship Foundation indicated that in Canada the average annual tuition of a full-time undergraduate university program was $4,400 while the average college tuition was about $1,500 overall, or $2,350 excluding Quebec where a large proportion of students pay nothing for college (Berger, Motte, and Parkin, 2009). This suggests the funds offered were sufficient for tuition-only costs of about 1.5 years of a university program and 3-4 years of a college program. However, tuition fees represent only about a fifth per cent of the annual costs for full-time college studies and a third of the annual costs of full-time university studies (Berger, Motte, and Parkin, 2007). On the other hand, the funds available would have been sufficient for a large number of shorter courses.

It is possible that some non-applicants to ***learn***$ave may have felt that what it would have cost them to increase their education was too high relative to the benefit they expected to reap, particularly when including the opportunity costs of foregone income from leaving a job and/or added costs of childcare, supplies and books and transportation. In fact, previous research suggests that Canadians in general overestimate university tuition fees and underestimate expected earnings of university graduates (Usher, 2005). The misperception is most pronounced among low-income Canadians who, in that study, gave an average estimate of tuition fees nearly three times the actual national average and an average estimate of the earnings of university grads that was less than one fifth the actual figure. If these estimates were used in a simplified cost-benefit analysis, argues Usher, low-income Canadians would rationally conclude that university education actually has a negative cost-benefit ratio.

being able to save. In *learn*$ave's case, because there was little national federal assistance in this regard, recruitment came down to a piecemeal approach whereby marketing was carried out by individual non-profit organizations in different ways in each community. There were real differences in potential clientele across communities and in what was needed to attract them. For example, the high immigrant population in large cities like Toronto and Vancouver are best approached and recruited for a program like the *learn*$ave IDA using ethnic media, while a more diffuse rural population in smaller communities might mean that media marketing alone is insufficient.

Intensive information sessions might be needed to overcome information or attitudinal barriers to participation. In addition to trying to convince potential participants that there are benefits to saving and that they can meet modest savings goals, information would be needed on how the proceeds from the IDA (cashed out earned credits) could meet their needs and reassurances regarding creditors and means-tested benefits.

The importance of word of mouth as a recruitment method suggests that these and other external marketing methods should be used most heavily in the early stages of recruitment. Wherever possible, outreach efforts should encourage more "viral" forms of marketing that might profile existing participants and foster discussion in existing social networks and communities.

Targeting and screening

As in nearly all other IDA programs (e.g., Sherraden, 2009), *learn*$ave participants were different from the general population of low-income adult Canadians. According to the latest data, 11.3 per cent of all working-age adult Canadians lived at or below the low-income cut-off (Statistics Canada, 2008). As a group, low-income Canadians are likely to be younger working-age adults, and lone parents and many (about 13 per cent) work full-time full year (National Council of Welfare, 2007). However, it is important to recall that the eligibility criteria for *learn*$ave included an income threshold of 20 per cent above the official Low Income Cut-Off and added age, wealth and labour force status as selection criteria. This created, by definition, a population of *learn*$ave-eligible adults with low or modest incomes and low or modest assets that overlaps with, but is distinct from, the general low-income adult population in Canada. Calculations by SRDC using Statistics Canada data for the three random assignment sites suggest that, if *learn*$ave's eligibility criteria were retained, even in a large-scale roll-out, the project would target roughly 10 per cent of the total population (see Table 4.1), a population of similar size to the low income adult population but not identical.

However, within an eligible target population, not all eligible adults will apply to, or enrol in, a program like *learn*$ave. In some cases, eligible and otherwise interested adults may not be aware of the opportunity to open a *learn*$ave-type of account. In other cases, eligible and aware adults may conclude that they are not interested or do not feel able to enrol. This was clear from the challenges the project faced during recruitment and opinions expressed in the focus groups, and was confirmed in the

Market Research Survey. After being offered information about the *learn*$ave project, eligible adults were asked if they would take part in such a program. The results suggested an estimated take-up rate of approximately three per cent among the eligible population, at any given point in time. This does not necessarily imply that IDAs are not an attractive instrument to this target group. Nor can it be concluded that adult learning opportunities are not of interest to this target group. What can be said is that the combination of a matched savings incentive and financial education with a longer-term goal of promoting adult learning is likely of more limited appeal. In the end, the *learn*$ave sample is representative of the population of adults who are *learn*$ave-eligible, aware and interested, without any efforts to influence their attitudes or preferences, in an opportunity to save towards their own higher education. These attitudes and preferences regarding adult learning are by no means constant and can be influenced through, for example, social marketing or social norms.

Who would enrol in full scale *learn*$ave program?

In addition to offering some insight into potential uptake should an initiative like *learn*$ave be rolled out at scale, the implementation research can also help policy-makers and practitioners predict which segments of the eligible population would be most likely to avail themselves of an IDA for adult learning. Because participants voluntarily self-selected for the project, they may be different from the rest of the eligible population in terms of their interest in education, their willingness to save and/or socio-demographic characteristics (within the limits of the project eligibility criteria such as the restrictions on age, income level, social assistance dependency, etc.). Again, this analysis does not and cannot project how participant characteristics may differ if public opinion can be swayed using social marketing or other means.

The socio-demographic characteristics of *learn*$ave participants were compared to those of the total eligible population to uncover any characteristics of the adults likely to first voluntarily open an IDA for adult education. Data on the characteristics of the target population group were obtained from Statistics Canada's Survey of Labour and Income Dynamics (SLID).[1] The table below (Table 4.2) presents the comparisons between the recruited sample and the SLID data for the three experimental sites. This comparison identified a number of important differences between those who enrolled in *learn*$ave and the broader eligible population. As shown in Table 4.2, *learn*$ave participants were more likely to be younger adults, unattached, recent immigrants, university-educated and employed than the broader *learn*$ave eligible population.

Table 4.1 Proportion of Population Likely to be Eligible for *learn*$ave (Experimental Study Sites only)

Total population (2001 Census)	4,827,642
learn$ave-eligible population (estimated from 2002 SLID)	488,564
learn$ave-eligible population as a % of total population	10.1

Source: Statistics Canada data, census and survey of labour income dynamics, and Kingwell et al. (2005).

Recall that the SLID data describes a population who would meet the eligibility criteria for *learn*$ave but who may or may not be willing to enrol in a similar program.

The fact that there were many more highly educated and employed individuals than the low-income population generally is not surprising. This finding may reflect the general association between previous education and future investments in human capital (see discussion in Chapter 1) and echo other survey data that show that younger Canadians and those who have already completed some post-secondary education are more likely to have participated in further skills development (Rubenson, Desjardins, and Yoon, 2007). The results also match the experience of the American Dream Demonstration (ADD) whose participants were found to be better educated and more likely to be employed than the low-income American population at large (Schreiner and Sherraden, 2005). The ADD researchers further noted that participants were not a random sample of "poor people," but were a program-selected and self-selected sample of low-income people who volunteered to be part of the project, expecting to see some net benefit from their participation in the IDA. This could be said of the *learn*$ave participants as well.

The higher levels of educational attainment among *learn*$ave participants (discussed below) are also related to the very large proportion of the sample who were new immigrants to Canada. In the end, a little more than half of the sample (55.4 per cent) in the experimental sites were in fact recent immigrants at the time they applied to the project.[2] Consistent with the general population of recent immigrants to Canada, these applicants tended to have higher levels of prior education and training, despite meeting the project requirements of having low income and few assets. In fact, recent immigrants to Canada are nearly twice as likely to have a university degree as their Canadian-born counterparts (Statistics Canada, 2008a). The higher than expected representation of newcomers to Canada almost certainly pulled the average prior education level of *learn*$ave participants upwards. In fact, the overlap between the participants with university degrees

1 See Kingwell et al. (2005) for details.

2 See the note in Table 4.2 for what is meant by "recent" in this context.

Table 4.2 Comparison between *learn*$ave Sample at Baseline and Eligible Population for the Experimental Study Sites (%), 2002

Characteristic	*learn*$ave Sample	Eligible Population
Gender		
Male	49.0	51.1
Female	51.0	48.9
Mean age (in years)	33.5	41.0
Household Type		
Unattached individuals	45.5	23.1
Couples with children under 18 years of age	13.7	23.1
Couples without children under 18 years of age	27.8	31.5
Lone parents with children under 18 years of age	7.4	4.2
Other	5.6	18.1
Recent Immigrant[1]	55.4	25.4
Highest Level of Education		
Less than high school	2.5	11.0
High school graduate	6.9	14.3
Some post-secondary education	15.7	10.3
Non-university certificate or diploma	19.8	21.0
University degree	55.1	19.3
Don't know or refused	0.0	24.1
In School Part-time	12.7	8.4
Dwelling Owned by Household	4.4	44.4
Employed	65.8	54.5
Sample size	3,584	488,564

Source: *learn*$ave application form, baseline survey, and 2004 custom tabulations from Statistics Canada from the Survey of Labour and Income Dynamics (SLID) reference year 2002.

Note: The sample profile shown represents the characteristics of the *learn*$ave sample that would have existed at baseline if each of the three experimental sites had enrolled the same proportion of participants as the sites represented of the eligible population.

[1] The eligible populations include respondents who immigrated in the years 1998–2002. The *learn*$ave sample includes enrollees who immigrated in 1998 or later.

and participants who were newcomers was roughly 80 per cent in the *learn*$ave sample.

The key question that then arises is whether or not these low-income, highly skilled immigrants who enrolled in *learn*$ave would have taken part in adult education or training even without the financial incentive to save towards this goal. A review of past research yielded no reliable estimates of the rates of participation in education upgrading or training (outside language training) among newcomers to Canada and therefore the extent of the demand for education and training among immigrants. However, analysis of data from the 2001 Longitudinal Survey of Immigrants to Canada found that 67 per cent of immigrants reported making efforts to take some training within the 6 months after they had landed in Canada and 40 per cent of these had experienced some difficulty in trying to get the training they desired (Chui, 2003). Much of the training newcomers seek out may in fact be language or basic literacy training as these two have been repeatedly identified in the literature as pressing needs for Canada's new immigrants (Canadian Council on Learning, 2008). A review by Kunz (2002) suggests that employed newcomers to Canada have a low incidence training, in stark contrast to the overall pattern of better access for those with higher education. She further suggests that because they are more likely to be in lower wage and precarious employment, newcomers to Canada may be limited in their ability to bear the costs themselves of a return to education even when the return on the investment could be significant. Taken together, this suggests that newcomers to Canada may have higher levels of demand for adult education but it is unclear whether they also have higher participation rates, all things being equal, compared to their Canadian-born counterparts. In that respect, the impact results from *learn*$ave (presented in Chapter 8) should be most interesting.

Screening for eligibility

The *learn*$ave project was not unique in struggling with the tradeoffs between administrative efficiency and accuracy in targeting or in finding an approach to means-testing that was fair but responsive. Within the context of asset-building programs more generally, particularly when these are delivered by smaller community-based organizations, the issues encountered in the selection of participants for *learn*$ave were similar to those long associated with other social policy programs such as social assistance and other means-tested programs (U.S. Government Accountability Office, 2005).

The interviews with site staff identified a number of practitioner concerns with the implementation of the *learn*$ave selection criteria.

- *Checking income:* Staff reported that they spent a considerable amount of time obtaining complete income documentation from applicants to the project. While the documents required to prove current and past years' income (income tax T4 slips) were viewed as necessary to verify eligibility through a reliable source, the documents were not always easy for participants to provide, either because they had not retained a copy or had not filed a tax return. Site offices frequently had to follow up on missing documents and remind applicants to provide the missing information. This approach to administering an income-test is in contrast with other savings instruments such as RESPs where income tax data can be used to determine eligibility for benefits.

- *Checking other criteria:* Outside of income, other eligibility criteria were generally verified only through information reported by applicants. While project staff reserved the option to request additional information and even documentation, it would have been difficult to offer reasonable proof of many of the other criteria (for example proving that one is not in school). Site staff reported that they felt applicants were generally forthcoming about declaring assets, educational participation or other unmet criteria that might have rendered them ineligible. It is also worth noting that *learn*$ave was not unique in relying on self-reported information. For example, provincial income assistance and student loans programs generally rely on self-reported information on assets.

- *Liquid asset criterion:* The exemption of chequing account balances in the liquid asset test may have created the potential for applicants to shift funds into chequing accounts to avoid rejection due to the asset limit. This exemption was implemented to minimize situations in which applicants could have been rejected because of a temporary lump sum of income (such as depositing a pay cheque on the day before applying). Indeed, evidence from the baseline survey suggests that as a result of this loophole a small proportion of enrolees with high bank account balances were accepted into *learn*$ave: 9.8 per cent of the respondents to the baseline survey indicated that they had a total minimum balance of more than $5,000 in all their bank accounts, including chequing accounts, when they entered *learn*$ave (Kingwell et al., 2005).

- *Debts not considered:* Debts were not considered as part of the eligibility criteria for *learn*$ave. This may have meant that some high-asset individuals with low or even negative net worth (all assets minus all debts) were excluded from the eligible population. According to site staff, participants with low net worth may have benefited significantly from the project, perhaps improving their ability to manage or repay debt in the short-term and gaining access to financial savings for education which they would not otherwise have been able to access. Again, it is worth noting, by comparison, that debt is not necessarily considered in needs- and means-testing for income assistance or student loans.

Lessons learned

The program model was attractive only to a small portion of the eligible population. The incentive in *learn*$ave may not turn the tide for adults who were not already inclined to invest in their own education and who were willing to save towards it. The take-up might be increased by either adjusting the criteria that distinguish the eligible population, or perhaps by reconsidering the program model itself.

But perhaps the single greatest lesson from the implementation research on targeting and screening is that simplicity is essential. Self-reported information on many of the eligibility criteria appears to have been reasonable in most cases but the income assessment alone was a time-consuming endeavour for applicants and staff alike. If eligibility could be tied to some other existing income-tested benefit, such as the Working Income Tax Benefit or similar provincial income supplements, there may be important administrative efficiency gains without sacrificing accuracy in targeting the program. The theme of simplifying the enrolment process is discussed again in Chapter 8 as a way of reducing the cost of delivery.

Financial management training

Participants in the *learn*$ave-plus group were expected to attend 15 hours of financial management training prior to making matched withdrawals (cashing out earned credits). Table 4.3 reveals that about 91 per cent

of participants received some training over the entire program period and that the proportion of *learn*$ave-plus participants who received 15 or more hours of financial management training was about 81 per cent.

The implementation research provides insight into how the financial training was delivered in *learn*$ave and shed light on what appeared to work well (and less well) from the perspectives of staff and participants:

- *Good impressions of the delivery:* The delivery of the financial management training received positive reviews from *learn*$ave-plus participants. The facilitation style of site staff was the most positive feature of the training and received the most positive responses — 33 per cent strongly agreed and 62 per cent agreed that staff taught the material well. Most *learn*$ave-plus participants also spoke positively about the financial management training during focus group sessions. The majority stated that the sessions were facilitated well and professionally by *learn*$ave site staff, and that the facilitators were respectful and understanding.

Table 4.3 Incidence and Intensity of Financial Management Training, Over 48 Months, Program Group Participants

Proportion who received any financial management training (%)	90.7
Proportion who received 9 or more hours (%)	86.2
Proportion who received 15 or more hours (%)	80.6
Average number of hours spent in training[1]	14.4
Sample size	1,193

Source: Participant Management Information System
Note: [1] Among *learn*$ave-plus participants who took some financial management training.

- *Attributing impact on their savings:* In addition to enjoying the facilitation style of the training, participants also credited the training with having a positive impact on their savings outcomes. Respondents to the 10-month follow-up mini survey generally agreed that the classes helped them to save and meet their goals — 19 per cent strongly agreed and 64 per cent agreed that the training helped them to save, while 23 per cent strongly agreed and 63 per cent agreed that it helped them to set goals. This sentiment was echoed by focus group participants who indicated that the training sessions provided them with the opportunity to share their experiences with other participants and helped them in their efforts to achieve their savings goals.

- *Mixed views on content by saver profile:* As for the content of the training (described briefly in Chapter 2 and Appendix B), there were mixed views among participants on the balance between the traditional financial management training segment and the prior learning assessment and goal-setting segment of the curriculum. Regular savers and new immigrants (most of whom were also regular savers) saw the financial component as a good review of financial knowledge they already had but did not report that it offered new information or skills. These participants were more likely than irregular savers to question the usefulness of the personal development, prior-learning and goal setting portions of the curriculum. The same participants reported that they would have preferred to receive more advanced "nuts and bolts" financial information on items such as budgeting, investments and credit management.

  On the other hand, many irregular savers, reported they found helpful the opportunity to engage in self-reflection, to develop a personal skills portfolio and to share experiences with other *learn*$ave participants.

- *Mixed views on content among staff:* Similarly, there were opposing views among site staff as to whether the best balance between the financial and the prior learning self-assessment components had been achieved in the final curriculum design. Some staff members would have preferred the curriculum to focus more heavily on self-assessment and goal setting while others would have liked more time to be spent on financial management topics such as budgeting and ways of spending wisely. Others expressed concern that the exercises specified in the curriculum were too simple for many of the participants and that certain exercises appeared to be redundant. Some site staff voiced a concern that 15 hours of financial management training was too little time to have an impact on established financial attitudes and behaviour patterns. However, there was some suggestion that 15 hours may have been more than many participants were willing or able to commit to. In more than one site, staff also voiced a strong desire to offer a financial management curriculum more specific to planning for the costs of education and training.

- *Operational challenges reported by staff:* Although participating in the financial education workshops was clearly expressed to participants in the *learn*$ave-plus and case study sites as an expected part of earning their matched credits, staff in several sites noted difficulties with attendance in the sessions. Best efforts were made to schedule sessions outside of working hours for the majority of participants and extra measures were taken to overcome transportation and childcare issues. In at least one case study site, staff went so far as offering the training to certain participants through individual

sessions scheduled at the participants' convenience. With these efforts, attendance in the workshops was high but not universal. Among participants in the experimental group, 91 per cent attended some of the training and 81 per cent attended all 15 hours. Within the case study of income assistance participants, perhaps the participants with the greatest barriers, roughly 78 per cent attended some of the financial education but only half attended all 15 hours (Kingwell et al., 2005).

Lessons learned

Within the IDA field in the U.S., "asset-specific" training (e.g., tailored to homeownership, education or small business planning) has been recommended as a best practice in implementation (see for example, Corporation for Enterprise Development, 2000). This type of training may be useful in enhancing the financial management training that was offered in *learn*$ave. For example, "asset-specific" training in *learn*$ave might have covered topics such as the returns to education, finding other sources of education funding, and understanding the interactions between funding sources for higher education.

Another finding from the assessment of the financial training is that it may be difficult to have one curriculum to fit the needs of all communities and clients. As observed in Chapter 2, some sites chose to adapt the curriculum to address the needs of their particular clientele or organizational priorities. But within sites, the implementation research suggested that not all clients have the same needs for financial knowledge and skills. IDA programs like *learn*$ave may in fact "cream" some of the more financially astute participants within the eligible population. In designing curricula for low-income populations, it would be important not to presume that all participants will have a low level of financial awareness and skill coming into the training. If some measure of participants' financial literacy could be taken at the outset, it might be possible to personalize the training by suggesting which workshops or modules might be of greatest value or referring participants to other resources to meet their own knowledge and skills needs. Repeating the measure following the training would also provide some mechanism for assessing the training's effectiveness.

Case management services

Case management was seen by the majority of *learn*$ave-plus participants as a very important and positive component of their experience with the project. There was an overwhelmingly positive response from the majority of participants in the focus groups about the support the case managers provided. Particularly for participants who were saving on an irregular basis, the case managers seemed to have made participation easier. The needs of these participants were more centered on overcoming barriers to saving and not losing sight of their savings goals. Many of those participants stated that the support they received from the case managers was instrumental in keeping them focused on meeting their savings goals as well as their broader personal development goals.

Project staff spent about four hours (about 238 minutes) with participants on average during the full program period (see Table 4.4), which is almost an hour longer than the time spent over the first 40 months (Leckie et al., 2009). This is indicative of the increased assistance needed for cash-outs, which was the main if not only activity taking place at this stage in the project.

Use of services was similar between program groups with respect to incidence but higher in intensity in the *learn*$ave-plus group. Of *learn*$ave-only participants, 93.3 per cent received some services in the program period compared to about 98.6 per cent of the *learn*$ave-plus group. The same is true for project-related contacts. However, the average amount of time spent with the *learn*$ave-plus group was higher, both substantively and statistically, than that of the *learn*$ave-only group (277 versus 199 minutes) because of the enhanced case management services provided to the former as per the original program design.

Table 4.4 Provision of *learn*$ave Services[1], by Program Group, Over 48 Months

	Total	*learn*$ave-only	*learn*$ave-plus
Proportion who received any referrals (%)	5.9	2.5	9.2***
Average number of referrals	7.8	2.9	12.7***
Proportion who received any project-related contact (%)	95.9	93.3	98.6***
Average number of project-related contacts	21.3	18.2	24.5***
Proportion who received any services (%)	96.2	93.7	98.7***
Average number of contacts	21.7	18.3	25.1***
Average number of minutes spent with participants	238	199	277***
Sample size (program group participants)	2,388	1,195	1,193

Source: Participant Management Information System

Note: [1] Due to correction of records in the PMIS, some figures may differ slightly from those previously published.

Two-tailed t-tests were applied to differences in measures between the program groups.

Statistical significance levels are indicated as: * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Cash-out procedures and overall program satisfaction

As noted in Chapter 2, many steps were involved in the procedures established for the withdrawal of matched credits and several verifications were included to ensure that public funds were being used accountably and transparently. All matching funds were disbursed by SEDI in the form of paper cheques payable directly to education providers or other vendors of eligible goods or services. The main finding from this process is that the cash-out procedures were found to have generally worked as intended and no incidents of fraud or misuse of project funds were ever identified among any project participants.

Interviews with project staff suggested that there were some operational challenges with the cash-out procedures largely related to convenience for the participant and the administrative burden on project staff:

- *Speediness in issuing cheques:* Some participants wanted their cheques on short notice and SEDI was able to provide the sites with cheques within one week after receiving the request. Nevertheless, project staff reported that participants often expected an even faster turnaround time and they had often pressured project staff to expedite the request.

- *Inconvenience of paper cheques:* The use of paper cheques for payment for the cashed out credits proved to be inconvenient, as many training and education vendors did not accept cheques from third parties, requiring SEDI to certify the cheques and resulting in further delays.

- *Eligible education providers*: A third difficulty for some participants arose from the list of designated institutions. Although it contained a wide variety of both public and private institutions, project staff reported several instances where participants requested funds for institutions that were not on the list. At one time, participants were allowed to seek an exemption from SEDI for courses not on the list. As of December 2002, however, exemptions were no longer permitted. This change was necessary to ensure that *learn*$ave remained a test of an initiative that could become a national program — exceptions could not be made in that case.

- *Administrative challenges:* Another source of difficulty was incomplete forms and missing documentation. In an attempt to minimize this problem, SEDI updated the forms several times to make them clearer and easier to complete. Also, while other IDA programs often administer matching funds through a standalone pool, in the case of *learn*$ave these were advanced on a monthly basis to SEDI, thus making accurate forecasting of expected cash out demands from participants important, if difficult.

There were mixed reviews of the cash-out procedures among participants as well. Through interviews with project staff, SRDC found that the cash-out process was recognized by participants as necessary but that the verification procedures were seen as cumbersome, occasionally difficult and time-consuming. The amount of documentation that participants had to provide before and after receiving their cheque for the credits was a source of dissatisfaction. Some participants also complained that the list of designated learning institutions did not include the institution of their choice. Further, some participants did not obtain their funds before tuition payment cut-offs dates of the educational institutions. Others experienced "chicken-and-egg" problems: some educational institutions would not allow participants to register until they had the money in hand, but *learn*$ave rules dictated that participants could not obtain *learn*$ave funds until they registered.

Despite the anecdotal evidence suggesting difficulties with the cash-out procedures, the survey evidence indicates that a majority of participants felt that the process of making matched withdrawals was easy. About two-thirds (67 per cent) of education stream participants and 61 per cent of micro-enterprise stream participants agreed or strongly agreed that "it was easy to use the match money." However, other survey results suggest that the cash-out procedures may have had a dampening effect on cash-outs, as cash-out incidence was lower among those who had a negative view of the ease of use of the cash-out procedures.

The survey evidence also indicates that, on the whole, a large majority of participants were satisfied with *learn*$ave. Only 6.3 per cent of education stream and 16.4 per cent of micro-enterprise stream participants were not very or at all satisfied with *learn*$ave.

Lessons learned

Given the mixed views of the cash-out procedures, the use of electronic funds transfers might significantly hasten and streamline the administrative procedures and convenience to participants in using their matched savings credits. Recognizing the attendant trade-offs in accountability, another option would be to simply transfer matching funds directly into participants' accounts when cash-out criteria are met as is the case with the current administration of Registered Education Savings Plans in Canada and the United Kingdom's Saving Gateway program.

Finally, the generally high levels of participant satisfaction in the project suggest that similar programs operating with similar approaches would be well received by the target population who takes part. Research on IDAs in the U.S. (e.g., Sherraden et al., 2000) and project reports on smaller IDAs in Canada generally share *learn*$ave's findings that participants like the program and feel their experience was a positive one.[3]

The role of delivery agents

The focus of this last section is on the role of the various delivery agents involved in the project. Until now, there has been almost no research regarding the characteristics of service delivery approaches in IDA programs in Canada or the United States (U.S.).

Before we look at the role community-based organizations, non-profit agencies and financial institutions played in the delivery of *learn*$ave, it is useful to review briefly some of the existing proposals and models for the delivery of IDAs.

Delivery models for IDAs

Non-profit community-based agencies are typically the primary point of contact for IDA participants in the U.S. and in Canada. These organizations are generally responsible for promoting the program among the target population; recruiting, screening and enrolling participants; providing case management and financial training; and supporting participants when dealing with bank statements and administering matched withdrawals. The agencies usually establish partnerships with financial institutions, but the role played by the latter has been essentially limited to opening the IDA saving accounts and accepting deposits and withdrawals from these accounts. Those in the IDA field who argue for a strong role for community agencies argue that these agencies are needed to including hard-to-reach disadvantaged clients in the program.

While the U.S. has a long history of delivering IDAs via mainly community agencies, programs remain small in scale. One reason often cited for the small scale of IDA programs is their high delivery costs, largely attributed to the intensive nature of ancillary services provided by sponsoring community organizations (Schreiner, 2005). A cost study of the ADD project site in Tulsa, Oklahoma found that the cost was about $60 for every month that a participant was enrolled in the project, which represents about $3 for every dollar saved by the participant (Schreiner, 2005). However, the "human factor" associated to this mode of delivery, while costly, was highly valued by participants in the project, who felt they needed personalized interactions and assistance to be successful savers (Schreiner, 2005).

It is argued that financial institutions are best placed to carry out all of the financial transactions of opening accounts, issuing statements, accepting deposits, and transferring matched withdrawals (Schreiner, 2005). The key tension is between more efficient program delivery that can be scaled-up through financial institutions, versus more responsive, higher-touch and higher-cost services offered by community organizations. Some authors (e.g., Sherraden, 2000 and Schreiner, 2005) have suggested a hybrid system that would combine a universal, statutory and publicly-funded system of matched savings accounts available through financial institutions, as well as a network of "high-touch," intensive and community-based IDA programs supported by multiple funding organizations (including foundations and lower levels of governments), providing greater, though shorter-term services, and reaching fewer participants in greater need.

One model being considered in the U.S. is to use the tax system to promote private sector investments in IDAs and to expand access and take-up of IDAs among a larger segment of the low-income population. One such approach has been codified in the draft Savings for Working Families Act (SWFA),[4] a bill that has repeatedly (since 2000) been introduced in the U.S. Congress, and with bipartisan support, but has never succeeded in being passed into law. The SWFA model would offer tax credits to financial institutions to encourage their participation in IDAs by reimbursing them for the costs of matching a tax-free IDA at $1 for each $1 saved (see Chapter 8 for more discussion of this approach in the context of the cost-effectiveness analysis). Participating financial institutions would be expected to seek out and form partnerships with non-profit organizations, but these would assume a lesser role than under current IDA models and presumably would focus their services on the higher-needs segment of the low-income population. If enacted, the recent version SWFA has been touted to expand the number of IDA accountholders in the U.S. to about 2.7 million. In contrast with the Canadian context, it is important to note that U.S. financial institutions are subject to annual targets under the *Community Reinvestment Act*. Part of the rationale behind the SWFA model is that it would provide a tax-based incentive to shape the community reinvestment choices of financial institutions towards IDAs over other alternative community giving activities. It is not clear whether a

3 See, for example, SEED Winnipeg Inc. (www.seedwinnipeg.ca/programs.htm) and Momentum (www.momentum.org/home) on their local IDA programs.

4 See description at: http://cfed.org/policy/federal_policy_advocacy/policy_highlights/ida_tax_credit_swfa/ (accessed August 23, 2010)

dollar-for-dollar credit would be sufficient incentive to promote financial institutions engagement in IDAs without this external legislative factor.

Another delivery model for asset-building is to use the income tax system to direct the credit or tax benefit directly to individual savers. Chapter 1 has already reviewed several Canadian examples of tax-benefitted savings such as Registered Education Savings Plans and the associated Canada Education Savings Grant and Canada Learning Bond. As discussed in Chapter 1, these delivery models tend to have very low administrative costs, to have simplified eligibility requirements and to rely heavily on financial service providers for promotion and marketing. It is, however, worth adding here that in 2006, the federal government introduced the Education Savings Incentives Pan-Canadian Community Outreach Initiative to fund community-based organizations that develop and deliver projects that reach those at risk of not benefiting from the Learning Bond, Canada Education Savings Grant and other related programs.

The U.S. also has several tax-based savings incentives outside IDAs. U.S. asset-building advocates have begun to look at ways to expand existing federal income tax measures such as the Savers Credit and 401K retirement accounts. For example, authors at the New America Foundation and Brookings Institute have recommended that the Savers Credit be made available for savings in IDAs and other sanctioned accounts and that it be made a refundable credit to offer some benefit to households with little or no tax liability (Cramer and Lopez-Fernandini, 2008).

Perhaps the most notable example of a large-scale policy for matched savings for low-income households is the United Kingdom's Savings Gateway that was to be launched as a statutory income-tested program in 2010 but was not implemented with the change in government. During the first pilot, the Savings Gateway relied on co-operation between the participating financial institution and community organizations, partly through a related pilot of financial education. However in the second pilot, recruitment relied solely on government administrative systems and a very minimal amount of financial literacy training was offered, relying largely on CD-ROM and leaflets for accountholders, though some face-to-face training was offered to Savings Gateway participants and non-participants in a smaller number of pilot sites. Rather than encouraging accountholders to save and invest in particular forms of capital, the Savings Gateway was intended to "kick-start" a savings habit by matching savings at variable rates over the 18-month pilot. The final evaluation of the second pilot found very low uptake for the financial literacy training but an increase in new savings without hardship (Harvey et al., 2007). At first glance, this suggests that a more efficient and low-touch approach to asset-building can be implemented without the higher cost services offered by community agencies. However, the UK is distinct from Canada in that it has a large network of community agencies (namely Citizens Advice Bureaus and similar agencies across the country) that regularly provide advice, referrals and support with administrative forms or processes for disadvantaged clients. While these agencies are not explicitly part of the current design of the Savings Gateway, their regular and ongoing activities may in fact be important for its implementation. Moreover, an early assessment of the program concluded that significant staff time within sponsoring agencies was needed to monitor the use of the accounts.

What the preceding suggests is that, whatever the delivery model used for asset-building programs, there remains a need for community-based organizations to engage and increase access for the economically disadvantaged. It is this role that was explored in the implementation and service delivery research and for which policy and program lessons will be drawn at the end of the chapter.

Findings from *learn*$ave service delivery

The qualitative evidence suggests that *learn*$ave non-profit organizations played their most critical role in the recruitment and enrolment of participants and again in assisting participants to access their earned matched credits.[5] Without the support of the non-profit organizations to overcome the paperwork and administrative hurdles, it is likely that fewer people would have heard about, enrolled in or benefited from this program.

Not all services provided by non-profit organizations were of equal importance. The qualitative evidence and impact data together tell an interesting story about participants' use of services. On the one hand, the demand for the enhanced case management services among *learn*$ave-plus participants was fairly low. On the other hand, a higher level of support than expected was needed by participants across the experimental groups for aspects of the project requiring paperwork such as enrolment and registration at educational institutions. This finding is helpful in knowing when and how much to invest in both high- and low-touch service delivery to

5 Evidence presented in this section is based principally on some 45 interviews conducted in the latter stages of the project with representatives of the organizations delivering *learn*$ave in each of the 10 sites delivering *learn*$ave, representatives of SEDI, and representatives of the financial institutions participating in *learn*$ave. This evidence was gathered as part of a service delivery case study of the 10 sites, the results of which appear in an internal working paper by Ritch (2008). Some evidence gathered earlier at the implementation stage of this project was also considered in this study.

ensure successful outcomes for participants. It suggests that it may be advisable to make greater use of non-profit organizations for recruitment and enrolment, and less so for ongoing case management.

The style or quality of the *learn*$ave service delivery approach was similar across community organizations in the project. The organizations reported that they brought the same principles or values of their usual service delivery approaches to their role in *learn*$ave. These principles appear to have been strongly related to the positive experience of participants in the program groups. Key among these was the important role of the quality of "caring" in how service was delivered to *learn*$ave participants. This caring attitude influenced how staff interacted with individual participants as well as their flexibility and responsiveness in the implementation of *learn*$ave services.

An important element that influenced the nature of the service delivery was the type of person who was hired to work on *learn*$ave. When hiring new staff or assigning existing staff to *learn*$ave, organizations favoured those with strong interpersonal skills over those with administrative abilities and experience with enhancing financial literacy. This finding is consistent with implementation research conducted by the Centre for Social Development for the American Dream Demonstration. In her report, Adams (2005) emphasizes the central role of staff "enthusiasm, creativity and rapport with participants" in the successful implementation and outcomes of that program.

The evidence also indicated that *learn*$ave project staff placed a high value on service delivery that was focused on participants' meeting of program requirements such as enrolment, completion of the financial management training and cashing out matched savings. Throughout the program, staff at all project sites went beyond project expectations when participants required help to complete these requirements and developed local approaches to respond to differences in support needed by participants. This was particularly true in the smaller non-experimental sites where there were fewer requirements to maintain comparable approaches compared to the experimental sites. Staff spent considerable time working one-on-one with participants to complete the necessary paperwork to enrol in *learn*$ave, recognizing early on that many participants experienced challenges completing written forms. In rural sites or those sites with limited public transportation, staff even went to people's homes to offer help in filling in enrolment forms.

The need for such hands-on and focused support to help participants with the paperwork is an unexpected finding from *learn*$ave. The task of completing paperwork, while seemingly mundane, may in fact be a significant factor in the recruitment and retention of program participants in this target group, as was pointed out earlier in the findings from the implementation research. Some staff suggested that it is likely that many participants in this target group had low literacy skills and therefore would likely experience challenges with the completion of written forms. Given the high proportion who were newcomers to Canada, language may also have played a significant factor.

> At the beginning, it was up to the participants to take the forms, fill them out themselves and bring them back. And that wasn't happening. People were confused. They didn't know how to fill it out properly. So in the end, we ended up doing one-on-one with them. (Project staff)

> They don't understand the forms, so we put a lot of little sticky arrows, "Sign here. Fill this space. Sign here." People aren't particularly good at reading forms and literacy is an issue. (Project staff)

Many participants also needed assistance during the process of claiming their matched credits and registering with educational institutions. These processes involved a series of steps that may have seemed daunting to participants with limited experience with educational institutions or who were new to the Canadian educational system. In some cases, staff made phone calls to educational institutions on behalf of participants. In other cases, they helped participants read brochures and websites to assist them in selecting appropriate programs and courses. In some cases, staff negotiated payment schedules with educational institutions to allow participants to finalize their registration in advance of matched credits becoming available.

> Sometimes the logistics were just little awkward: letting the community college know that, okay this person will have money coming from this program, but we need your letter of acceptance. Or there was just a little bit of red tape, or a little bit of the stuff that you just had to work through. (Project staff)

> We'd send letters to the school saying, "Can you just hold off and don't charge the person late fees? The money is on its way." I found that worked out pretty well too. (Project staff)

Staff routinely revised the delivery model for the *learn*$ave financial management training that they

were required to deliver. It was apparent early on that program participants had very busy lives and that the training workshop schedule needed to be adapted to give participants the best chance to meet this program requirement. Staff scheduled sessions at a variety of times and locations to accommodate participants schedules. For example, in addition to holding large and small group sessions, some staff made visits to people's homes and lesson modules were even sent by mail to people's homes. Alternatively, workshops were offered at the organization's offices and in public places such as coffee shops, and even by telephone.

The goodness of fit for the organizations between *learn*$ave and their broader mission also appears to have been important. Project staff in the *learn*$ave delivery agencies viewed the financial literacy component of the project as most compatible with their pre-existing organizational priorities. The focus on asset-building, savings and adult education was less frequently viewed as such. Some organizational representatives went further, stating they favoured the financial literacy component of *learn*$ave as they felt it offered considerable promise of benefit to participants both during *learn*$ave and into the future.

> There was some tension as to how much financial literacy is really required. At some point the question was asked, couldn't we just do the matched savings thing? For us, it almost came to a point of integrity. We would not deliver the program without accompanying financial literacy training because we know that unless the knowledge is increased for people when they are in the savings program, they're going right back to where they were when they're finished. (Project staff)

As discussed in Chapter 2, a financial management training curriculum was developed specifically for *learn*$ave so that participants across all sites would have access to the same information about financial literacy. The delivery organizations voiced strong opinions about the content of the *learn*$ave curriculum. This was perhaps indicative of the importance organizations gave to financial literacy, in terms of what it could offer participants. Sites with existing curriculum held a strong attachment to their own organization's curricula and wanted to retain local autonomy over this aspect of service delivery. They argued that the organization's experience and knowledge in this area ought to have been better "valued" by *learn*$ave program planners.

Working with financial institutions

Several site staff members believed that an important aspect of *learn*$ave was the opportunity for low-income participants to become connected with financial institutions as a way of enhancing long term self-sufficiency and social inclusion. Although financial inclusion was not a primary goal of *learn*$ave, it was interesting to hear this perspective strongly expressed by staff in participating organizations. Most staff thought that involvement with financial institutions was a critical element of *learn*$ave. Staff argued that the use of banking services rather than those of cheque-cashing outlets was a positive outcome and served as a connection to one aspect of society from which participants may have been alienated and excluded.

There is qualitative evidence that *learn*$ave increased attachment to financial institutions among *learn*$ave participants. While almost all participants had a bank account at the time they were enrolled in the *learn*$ave, staff observed that encouraging participants to deposit money in their *learn*$ave accounts, which were located in mainstream financial institutions, enabled some participants to establish a "more positive association" with these financial institutions and be more comfortable in using them. Also, as will be discussed in the chapter on savings and net worth impacts (Chapter 6), participants did feel more comfortable with banks as a result of their *learn*$ave experience.

While acknowledging the important role the non-profit organizations played in engaging the low-income base in the project, representatives of the financial institutions interviewed for this study identified some real challenges in being a partner in *learn*$ave. One set of challenges concerned the "onerous" security measures and technological and administrative procedures around maintaining the database and writing cheques from the accounts. Another set involved the long-term research nature of the project and the amount of resources required to ensure it was run well. As compared with other projects undertaken by financial institutions, *learn*$ave had a very small client base and uncertain returns over a much longer timeframe. It is unlikely that a financial institution would be able to make a direct business case for engaging in similar pilot initiatives in the future, but rather would make the investment as a philanthropic endeavour or as a corporate social responsibility. For example, representatives of one participating financial institution noted that, as compared with RESPs, *learn*$ave had much larger administrative requirements, a very small client base and a predetermined end as a demonstration project. To engage financial institutions, asset-building programs need to consider administrative burdens as well as business models and practices of mainstream banking service providers. By giving consideration to these at the outset of program design, it may be possible to find more

sustainable, effective and mutually rewarding models for service delivery.

SEDI's role: The national coordinator

It is important to consider the perspective and role of the operational lead in *learn*$ave, namely Social and Enterprise Development Innovations. SEDI had first proposed a pilot of IDAs to the federal funder (although, as noted earlier, the final design differed from the original proposal), consulted with stakeholders to build support for the project, recruited delivery partners in the 10 selected communities, and coordinated operations among the delivery, financial and research partners. The funding agreement with HRSDC for the implementation of the project was also under SEDI's responsibility. As such, SEDI also took on the role of negotiating and monitoring sub-contracts with the delivery agencies (and even took over responsibilities in actual delivery when some partners were unable to), and directing funds to delivery partners for project costs and to third parties for the matched credits as part of the savings cash out process. Other studies of the voluntary sector concluded that organizations face significant and rising costs and challenges in meeting funder accountability and reporting challenges.[6] In this light, SEDI's administrative and reporting function was more complex (as both a funding recipient and provider) and should be acknowledged. This may have alleviated some of the administrative burden on the delivery partners, freeing them to invest more staff time and resources in service delivery within the project. However, given the time-consuming administrative processes used (for example, to verify a single cash-out), it is not clear whether there was any real transfer of organizational responsibilities or instead just a higher level of administrative demand across all project organizations.

SEDI also played a role in offering technical assistance, staff training, ongoing advice to project staff and other investments in the organizational capacity of the delivery partners. For example, SEDI offered suggestions to the delivery agencies about key messages for project outreach and marketing activities, approved advertisements and promoted the sharing of information among the sites. SEDI encouraged and facilitated opportunities for the sites to share effective service delivery practices through email, on-line bulletin boards, teleconferences, participation in conferences, bi-annual meetings of project staff from across delivery sites, and regular meetings of representatives from the primary sites.

6 See for example Hall, Andrukow, Barr, Brock, de Wit, Embuldeniya, Jolin, Lasby, Lévesque, Malinsky, Stowe, and Vaillancourt. (2003) and Lara-Cinisomo and Steinberg (2006). The latter estimated the annual costs of funder-directed compliance activities to be on average 11 per cent of total organizational budgets.

SEDI staff reported that they were guided in part by the organization's previous role in the delivery of self-employment services to Employment Insurance clients in the Toronto area using what they described as a "coordinator model." SEDI also drew heavily on the experience and expertise of the U.S. Corporation for Enterprise Development, a non-profit organization that had played a role similar to SEDI's in the ADD project.

For program models like *learn*$ave with little or no local track record and where front-line agencies may need significant and on-going investments in capacity, the coordinator model may offer certain benefits in the implementation. As the asset-building field gains maturity and organizations develop internal expertise, the value-added of the coordinator function may change over time. However, given the still nascent quality of IDA practice in Canada and the federal government's desire to depend on third-party delivery, there is likely to be continuing need for organizational capacity building, technical advice, staff training and other information such as that which was provided by SEDI if new IDA projects are launched. Given SEDI's role in initiating the *learn*$ave project, the organization may also continue to be a source of new ideas for exploration in asset-building practice and the related field of financial literacy. Whether and how the so-called coordinator role can be reconciled with a clear pressure on the field to offer more efficient program models, however, is unclear.

Lessons learned

- In looking for ways to gain efficiency by streamlining IDA delivery models, consideration should be given to more effectively targeting the high-touch and high-cost services where they matter most. Based on the evidence from *learn*$ave, help in navigating administrative processes and completing forms appears to have made the most difference to participants and there was little evidence of demand for other forms of enhanced case management such as referrals to other services or personalized counselling.

- Organizational missions matter and must be compatible with the goals of IDA or similar programs. Successful implementation requires that organizations are able to identify at least some overlap between their organizational priorities and the stated objectives of the program. At the time *learn*$ave was delivered, delivery organizations were most able to identify with the financial training component rather than the savings or adult education elements. It may matter less which program component organizations embrace and more that they do so at all. Given the labour-intensive and long-term nature of the project, the fit with the organizational

mission may have helped to maintain the necessary enthusiasm and motivation among staff.

- Organizational values and approaches to service are important. The theme of caring about client success and being willing to go above and beyond to help clients was repeated throughout the review of the service delivery. This type of individual and compassionate attention was heavily valued by participants and appears to have influenced their overall experience in the program model. It is not clear how this demand could be fulfilled through delivery mechanisms that rely only on tax systems.

- Opportunities for public-private-non-profit partnerships appear to benefit the end users of IDA projects. While the organizational mission and values of the non-profit partners were key, there is evidence from *learn*$ave that the involvement of mainstream financial service providers also had a positive benefit and may, over the longer term, provide a model for reducing financial exclusion.

- Delivery organizations bring expertise but also need on-going support and capacity building. In the case of *learn*$ave, the delivery agencies had and offered important insights into their clientele and best practices from comparable organizational initiatives. This expertise can and should be harnessed in developing and implementing the program model, from targeting, through training, through cash-out. At the same time, the very limited number of organizations in Canada with any experience in delivering asset-building programs, particularly IDAs for adult learning, combined with the sometimes complex nature of IDA program delivery, suggests an ongoing need for some support in the form of staff training, curriculum assistance and technical advice (among other forms of capacity-building). This type of support may best be delivered by a fellow non-profit organization with a better understanding of the context, limitations and strengths of non-profit delivery agencies.

Taken as a whole, the implementation and service delivery research suggests the ongoing need for some hybridization in IDA and related asset-building programs models aimed at low-income populations. There appears to be an irreducible need for a basic level of support and personalized service that cannot be delivered through the income tax system alone and would be challenging to deliver through mainstream financial institutions to those who have little or no association with such institutions ("the under-banked") or are mistrustful clients. At the same time, the financial service providers bring a crucial infrastructure, expertise and a suite of financial products. The future policy debate regarding IDAs or similar account-based mechanisms, should be less about whether there are roles for each of the non-profit and financial sectors, and instead about how to achieve the optimal level of collaboration between these two in asset-building programs.

Chapter 5 Savings and investment outcomes

Because *learn*$ave is among only a handful of savings-based initiatives ever developed for low-income Canadians, the project can provide important information on how low-income participants responded to an asset-based and matched savings incentive. This chapter aims to shed light on the following questions: Did participants save in their *learn*$ave accounts as they were expected to, during the relevant portion of the program? Did participants withdraw the matched credits as they were expected to during the relevant portion of the program? Did the addition of the financial management and case management services have any measurable incremental impacts on the savings outcomes of participants in the *learn*$ave plus groups over and above the outcomes of the *learn*$ave only groups? What can be learned about the saving and account use of the project's lowest income participants, those on social assistance? Did program parameters, which varied across sites, have an effect on saving outcomes for participants?

Before turning to the results, it is important to recall the participants had the opportunity to accumulate savings in the *learn*$ave project during a time-limited period that began on the date they were enrolled in the project and expired three years later. This is the outlined box of the program model depicted in Figure 5.1 and is the focus of the next few sections of this chapter. When participants had accumulated a level of savings they determined to be sufficient, they were eligible to withdraw savings and use their matching credits worth $3 for each $1 of their contribution to invest in eligible project uses.

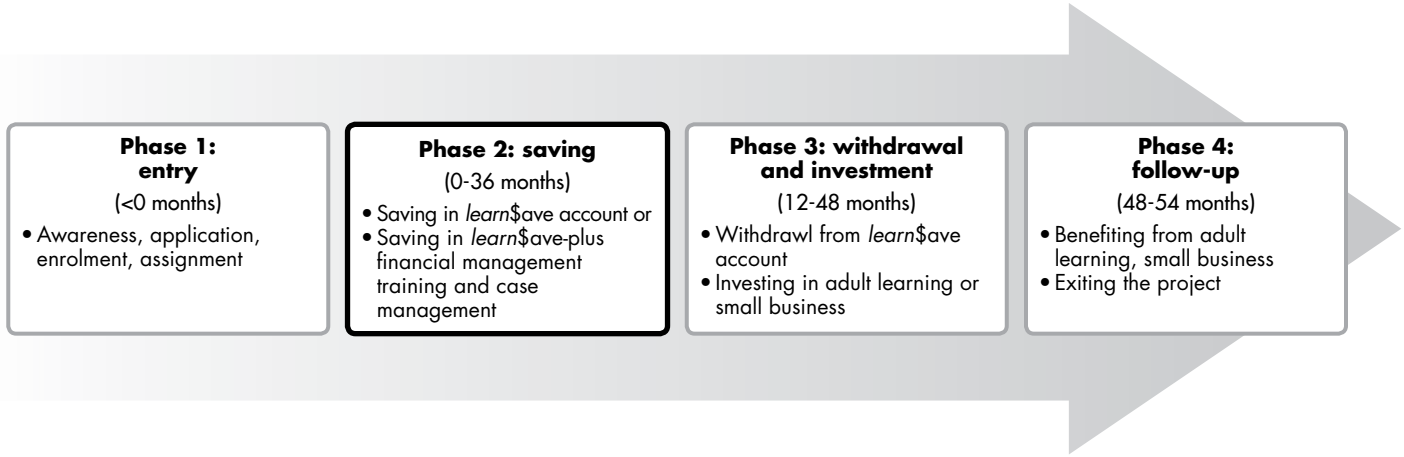
By design, participants' savings in their *learn*$ave account would have been expected to first rise and then fall to as low as $0 as participants exhausted their IDA savings for education and other eligible project uses. For a definition of IDA terms used in this chapter and elsewhere, see the glossary near the end of this report.

Evidence on the use of the *learn*$ave IDA

Mean savings

Across all sites and all program groups, the 3,607 participants in *learn*$ave (excluding the control group) saved nearly $3.5 million in their Individual Development Accounts, with a mean of $959 per participant, or about $320[1] a year during the saving period. This is a substantially higher than the average annual net deposits of U.S.$229 (Sherraden, 2002) observed in the Downpayments in the American Dream Demonstration (ADD), even accounting for the differences in years and in the value of the currencies, but acknowledging the lower ADD match rate. There was some variation in average *learn*$ave savings across project sites. Participants in the experimental sites saved more than participants in the non-experimental sites (Table 5.1). Similar to the ADD results, *learn*$ave participants receiving income assistance (IA) at the time they enrolled in the project saved substantially less than the project-wide average, with mean net savings of $553 (not shown) across the project sites. Thus, the mean amount saved in the *learn*$ave account among non-IA recipients is higher (last column of Table 5.1). Results for participants receiving IA are discussed in greater detail later in this chapter.

Figure 5.1 Stages of *learn*$ave Program Participation



1 This amount was obtained by simply dividing the $959 overall average in the table by 3, which is the length in years of the *learn*$ave saving period in all but one site. As Calgary's saving period was just two years, the overall annual average is in fact somewhat higher.

Table 5.1 Mean Participant Net Savings* in *learn*$ave Account ($), by Project Site, at 36 Months

Project Site	Mean Savings among all Participants in Project Sites	Mean Savings among Participants Excluding IA Recipients
Experimental Sites	**1,045**	**1,089**
Halifax, NS	507	591
Toronto, ON	1,173	1,209
Vancouver, BC	1,027	1,043
Non-experimental Sites	**733**	**800**
Digby-Annapolis, NS	526	572
Fredericton, NB	887	1,022
Montreal, QC	609	612
Grey-Bruce, ON	699	842
Kitchener-Waterloo, ON	813	934
Winnipeg, MB	823	863
Calgary, AB	767	801
Total Across all Sites	**959**	**1,020**
Sample Size	3,607	3,141

Source: Participant Management Information System
Note: * Net savings do not exclude matched withdrawals, but do exclude unmatched withdrawals.

While the above figures on average participant savings suggest that participants did in fact make use of the *learn*$ave IDA as intended, a full understanding of the use of the savings account also demands that we look at a range of indicators including account opening, deposit patterns and withdrawals. These other indicators are drawn from the Project Management Information System (PMIS) and only for the participants in the experimental program groups (both the *learn*$ave and *learn*$ave-plus). Income Assistance (IA) recipients at the experimental sites and all participants at the non-experimental sites were excluded to maintain a similar participant group and program parameters (parameters differed across the non-experimental sites).

Take-up of accounts

Nearly all participants (93.4 per cent) opened the *learn*$ave account offered to them. All participants with an account made at least one deposit and the vast majority (82.3 per cent) met the minimum savings threshold to qualify for matched credits by saving at least $10 in their account for 12 or more months (Table 5.2). Taken together, these indicators suggest a very high degree of take-up by participants. Few participants in the experimental sites were unable to open an account or make at least one deposit. This outcome increases the likelihood of that the full expected impacts could be measured across nearly the entire research sample.

Table 5.2 Participation in *learn*$ave Saving Activities, Over 36 Months, Program Group Participants

Proportion who opened a *learn*$ave account (%)	93.4
Proportion who did not open a *learn*$ave account (%)	6.6
Proportion who saved at all (deposited any money in *learn*$ave account) (%)	93.0
Average number of active saving months (deposited >$10 in a month)	13.8
Proportion with 12 active saving months (met savings requirement to earn credits) (%)	82.3
Average peak matchable savings in *learn*$ave account ($)	1,194
Average number of months to reach peak matchable savings (among savers)	20.0
Distribution of participants by peak matchable savings (%):	
▪ $0	7.0
▪ $1 to less than $120	3.6
▪ $120 to less than $500	6.2
▪ $500 to less than $1,000	6.5
▪ $1,000 to $1,483.33	11.8
▪ $1,483.34 to $1,500 (the maximum)[1]	64.9
Proportion who saved maximum matchable amount[1] (%):	
▪ by month 18 ("early savers")	43.0
▪ after month 18	21.9
Sample size	2,388

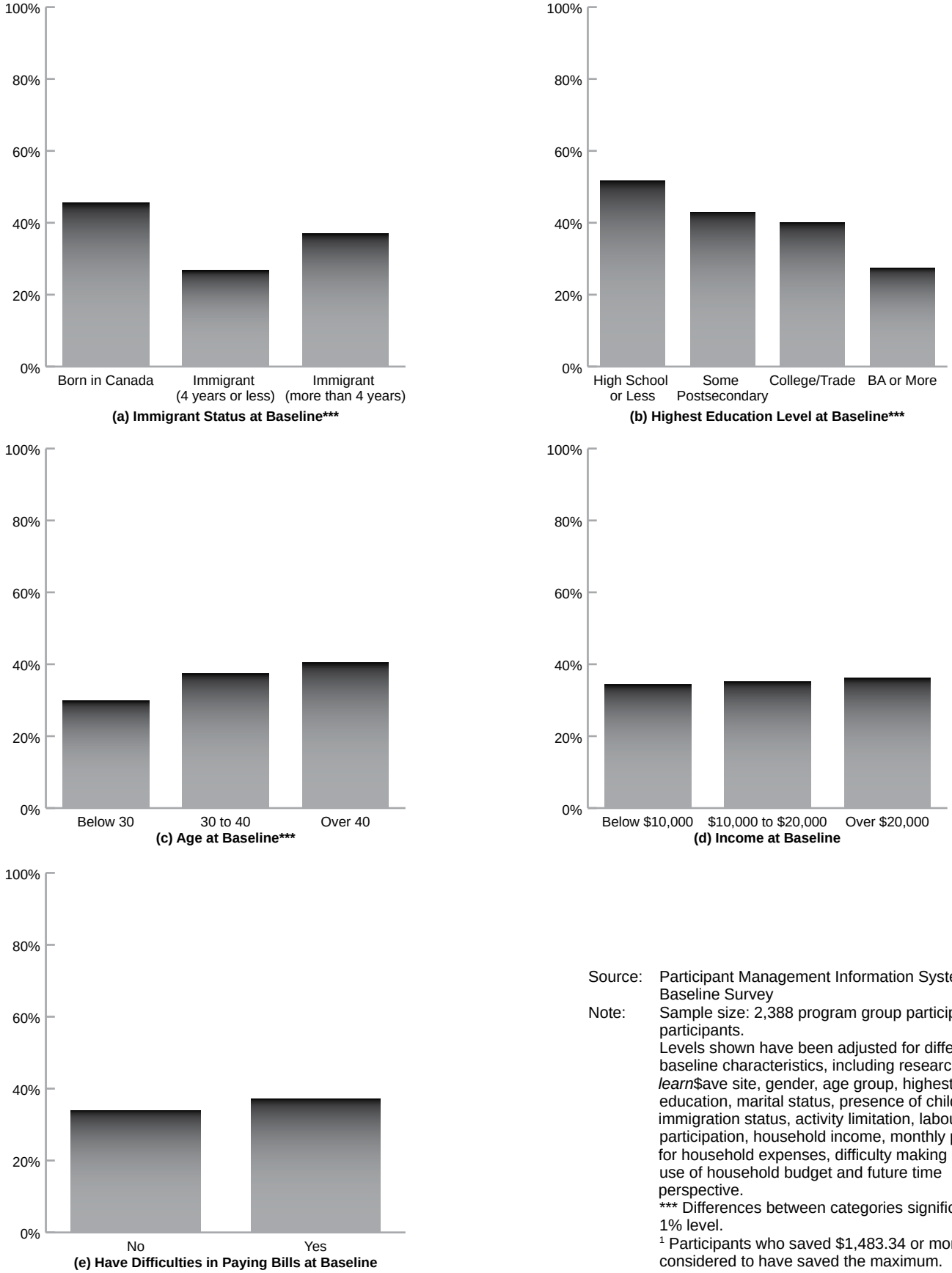
Source: Participant Management Information System.
Note: [1] Participants who saved $1,483.34 or more are considered to have saved the maximum.

Saving patterns: Diversity in the sample

Three patterns of saving emerge: unsuccessful savers who did not reach even the minimum savings to trigger matched credits; modest savers who saved less than the maximum on which they were eligible to receive a match; and high savers who saved the maximum $1,500 or perhaps even more.

As illustrated in Table 5.2, almost two-thirds (64.9 per cent) saved the $1,500 maximum during their 36 month saving period and could be described as "high savers." The remaining participants include those "unsuccessful savers" whose savings were too small to be eligible for any matched savings credits (10.6 per cent saving $120 or less) and nearly one-quarter of participants (24.5 per cent) who had some savings, earned matched credits but less than the maximum (i.e., saving between $120 and $1,483.33). It is unknown whether the actual savings level of those who saved less than the maximum reflects personal preferences or budget constraints. The evidence from Figure 5.2 suggests these "modest/unsuccessful savers" were more likely to be Canadian-born and to have lower levels of education at baseline but they were equally likely to fall into the lowest, middle and highest

Figure 5.2 Proportion of Program Group Participants who Did Not Save to the Maximum, by Baseline Participant Characteristics



Source: Participant Management Information System and Baseline Survey

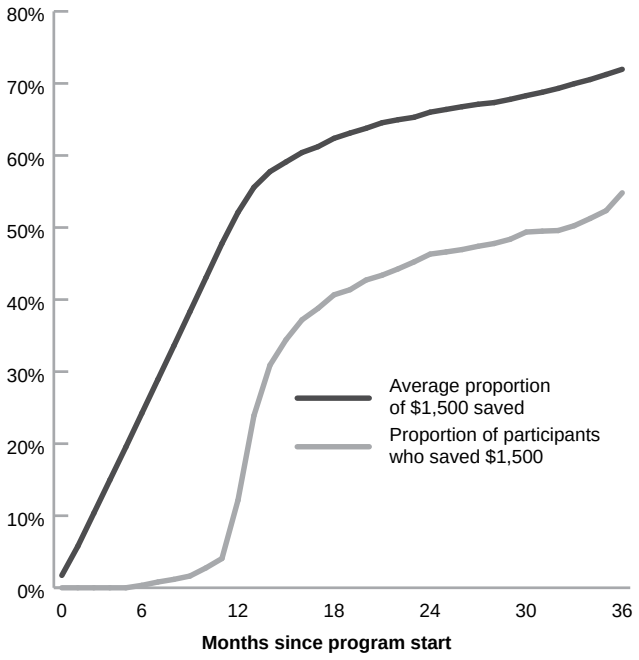
Note: Sample size: 2,388 program group participants participants.
Levels shown have been adjusted for differences in baseline characteristics, including research group, *learn*$ave site, gender, age group, highest level of education, marital status, presence of children, immigration status, activity limitation, labour force participation, household income, monthly payments for household expenses, difficulty making payments, use of household budget and future time perspective.
*** Differences between categories significant at the 1% level.
[1] Participants who saved $1,483.34 or more are considered to have saved the maximum.

income ranges in the project. If *learn*$ave savings were lower among these participants because they faced tighter budget constraints, then we might expect to see a clustering in the lower end of the baseline income range but this was not the case. Similarly we might expect these participants to be more likely to report difficulties in meeting bill payments at baseline, but the difference was small. The financial circumstances of participants at baseline may not predict their eventual savings outcomes.

Many participants did not need the full 36 months to save to the project maximum of $1,500. The average number of active savings months (that is the number of months out of 36 in which at least $10 net was deposited in the account) was just 13.8 months (Table 5.2). This average was likely pulled downward by the sub-group of participants who did nearly all their saving early in the project. In fact a small number of participants were even able to reach an account balance of $1,500 before the 12 month minimum saving period (Figure 5.3), suggesting they were over-saving in their *learn*$ave account.

Of the participants who saved $1,500, many (43 per cent), did so within the first 18 months after opening their account and, of these, over one-half (23.9 per cent within the 43 per cent) did so by the minimum saving period of 12 months (Table 5.3). While the additional time after 18 months did allow another 21.9 per cent of participants to reach the maximum matchable savings, the pace at which this took place was very slow compared to the first 12 to 18 months (Figure 5.3 above). In addition to pulling down the average active savings months, these fast-saving participants also had a disproportionate effect on the aggregate measure of participant savings (Table 5.3):

Figure 5.3 Reaching the Maximum Matchable Savings, by Month, Program Group Participants



Source: Participant Management Information System
Note: Sample size: 2,388 program group participants.
[1] Participants who saved $1,483.34 or more are considered to have saved the maximum.

Because of their influence on the project savings results, our discussion now turns to the sub-set of participants who saved the maximum and did so early in the project. We have termed this sub-set "early savers" and conducted an analysis to identify patterns in their baseline characteristics (shown in Figure 5.4).

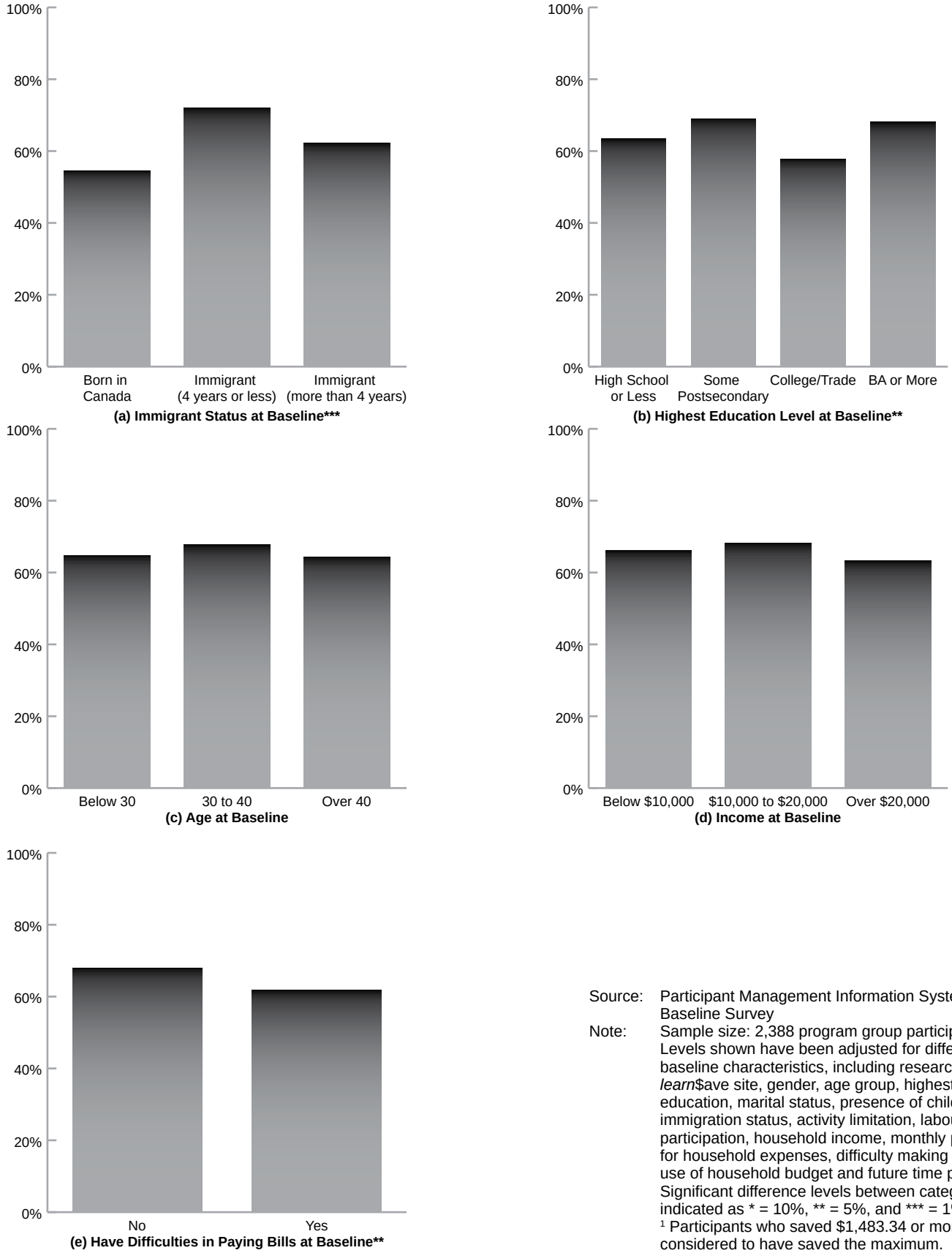
At baseline, these participants were more likely to have certain characteristics: being a recent immigrant to Canada and having a higher level of prior education. These participants were somewhat less likely to report difficulties at baseline in paying bills but were not more likely to have incomes in the higher range so again the role of baseline financial circumstances on savings outcomes is not clear. However, these participants may have had different or even stronger motivations to maximize their *learn*$ave savings and to do so quickly. The finding that higher education was related to higher savings is not surprising if the higher savings are viewed as a proxy indicator of the value these participants gave to the education they could purchase with their *learn*$ave savings and matched credits. Other research (e.g., de Broucker and Myers, 2006 and Hui and Smith, 2002) has found that those adults with higher levels of educational attainment are more likely to take part in adult education. Similarly, newcomers to Canada may have joined *learn*$ave to take part in some recertification, retraining or upgrading of their education once in Canada to be able to participate more fully in the Canadian labour market. Taken together, these two types of motivations are consistent with the observed baseline characteristics of the "early savers." Motivation or instrumental goals when participants enter a project like *learn*$ave may play

Table 5.3 Early Savings Incidence and Share of Total Savings, in *learn*$ave Account (%), Program Group Participants

	Proportion of Participants	Share of Total Participant Savings
Reached maximum savings within 12 months	23.9	43.0
Reached maximum savings within 18 months	43.0	65.6

Source: Participant Management Information System
Note: Sample size is 2,388
Maximum savings is $1,483.34 or greater

Figure 5.4 Proportion of Participants who Saved (Reached the Maximum)[1] Early, by Baseline Participant Characteristics



Source: Participant Management Information System and Baseline Survey

Note: Sample size: 2,388 program group participants. Levels shown have been adjusted for differences in baseline characteristics, including research group, *learn*$ave site, gender, age group, highest level of education, marital status, presence of children, immigration status, activity limitation, labour force participation, household income, monthly payments for household expenses, difficulty making payments, use of household budget and future time perspective. Significant difference levels between categories are indicated as * = 10%, ** = 5%, and *** = 1%.

[1] Participants who saved $1,483.34 or more are considered to have saved the maximum.

a greater role in predicting savings outcomes than do financial characteristics at baseline.

Dissaving patterns

During the saving period, participants had full access to their own deposits in their *learn*$ave account and could withdraw their own funds at any time and for any reason, while forfeiting a proportional amount of matched credits if it was done for non-accredited purposes. In fact, more than a quarter of participants "dissaved," that is, they withdrew their own funds for a purpose other than eligible goals in the project.

The incidence of dissaving was calculated by comparing the highest value of savings eligible for matched credits at any time during the saving period with the value of savings eligible for matched credits at the end of the program. If participants savings declined from their highest or "peak" level this would reflect an un-matched withdrawal or dissaving. The occurrence of dissaving is not unusual in a savings instrument and even widely available registered instruments such as Registered Retirement Savings Plans may dissuade dissaving but show patterns suggesting it is not infrequent. Dissaving is also not necessarily negative. Because money is fungible, *learn*$ave participants had to make decisions each month about how much to deposit into their *learn*$ave account and dissaving may have been a very rational attempt to reallocate money across the *learn*$ave and other financial commitments.

Just as baseline financial characteristics did not neatly predict less than maximum or "early" saving, neither did they predict dissaving. As illustrated in Figure 5.5, those with lower incomes at baseline, those who reported difficulties in paying bills at baseline and those who were not working when they entered the project were not more likely to dissave.

As noted, dissaving may have been used as a strategy for participants to reallocate money because of a change in circumstances from one month to the next. But dissaving may also have reflected changes in what participants felt they needed to save to afford (with matched credits) their planned investment in adult learning. For example, if a participant changed his or her goal from course A to course B costing less, then withdrawing the excess funds from the *learn*$ave account would make sense to avoid holding up funds in the *learn*$ave account that would not be used for a matched withdrawal and could be used in other ways. To differentiate between these two motives for dissaving, the incidence of dissaving was compared across participants based on their total savings. These results are presented at Figure 5.6.

Dissaving was most common among participants who saved less than the maximum, and specifically those with savings of $120 to $999. Interestingly, participants who were unsuccessful savers, i.e., those with less than $120 in net *learn*$ave savings were not as likely to draw down their *learn*$ave account and dissave. This finding suggests that not only were these participants very low savers, they were largely inactive or even disengaged accountholders.

Dissaving was even less common among participants who reached the maximum savings during the saving period and very uncommon among the subset who were the "early savers" who reached the maximum quickly. This suggests that participants made unmatched withdrawals from their account for reasons other than changing their minds about how much money they wanted to invest in adult education. Most likely then is that dissaving was done to manage changes in circumstance and rebalance financial commitments. However, this hypothesis should predict that those with the lowest incomes and the lowest savings were more likely to dissave, which is not consistent with the findings either (figures 5.5 and 5.6).

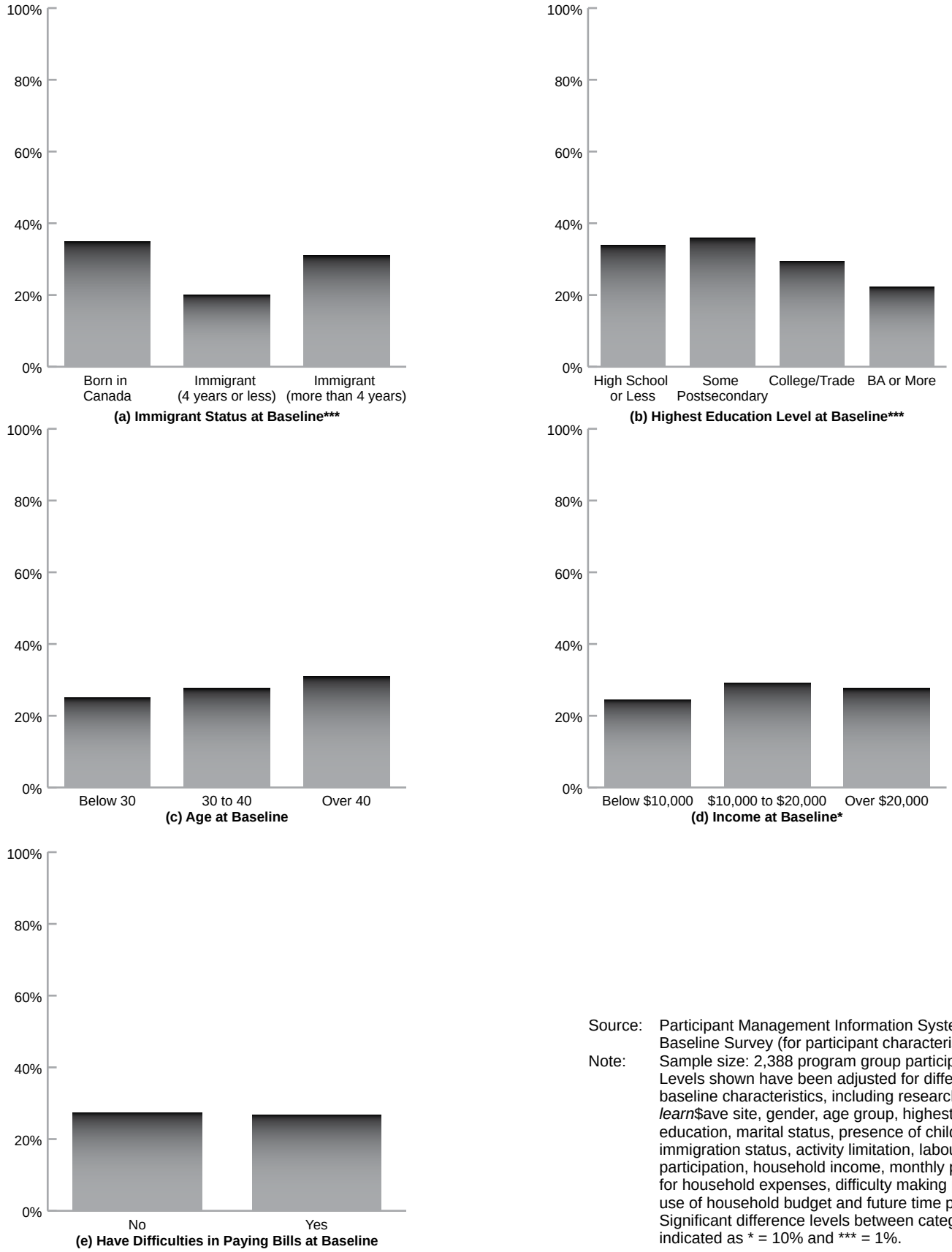
Interaction of saving and dissaving patterns

We suggest that the combined savings and dissaving results reveal at least five different patterns in the use of the *learn*$ave account, offering more nuance than our original three typologies using savings data alone.

As summarized in Table 5.4, once both saving and dissaving are considered, there was some diversity in the observable patterns among participants in *learn*$ave. "Unsuccessful" savers, who saved little if anything, did not qualify for even the first dollar of matched credits and saw little, if any, real financial benefit from the project. But it is also unclear if their financial circumstances prevented them from saving more since they neither withdrew funds for non-project purposes nor did they have greater financial constraints at the time they entered the project. It is likely that they lost interest in the project shortly after enrolling and then paid little attention to their *learn*$ave account for either deposits or withdrawals. Their behaviour would suggest they essentially dropped out of the project. Nevertheless, for the purpose of the analysis, they remain in the research sample as our research design uses an "intent to treat" model.

"Struggling" and "moderate" savers are distinguishable from each other only in terms of the amounts finally saved and the likelihood that withdrawals for purposes outside the project were made during the saving period. Both groups showed similar baseline characteristics as predominantly Canadian-born and having low levels of

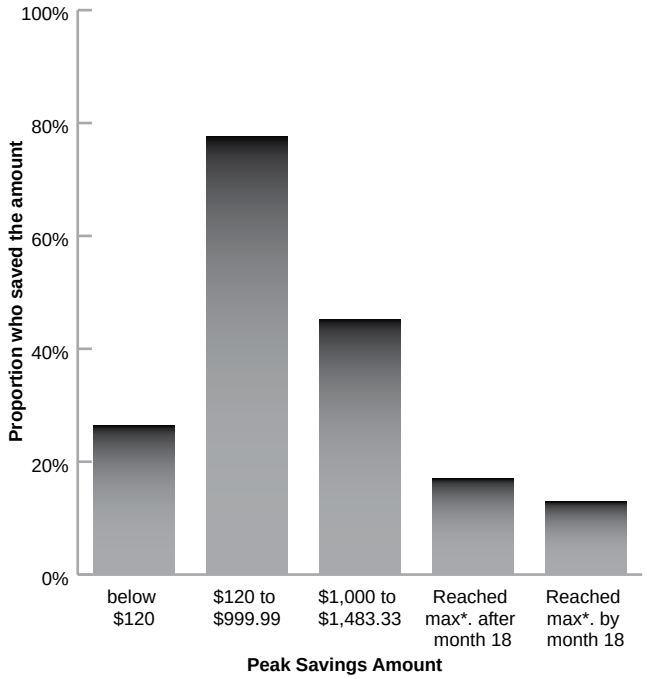
Figure 5.5 Proportion of Program Group Participants with Savings Lower than their Peak, by Baseline Participant Characteristics



Source: Participant Management Information System and Baseline Survey (for participant characteristics)

Note: Sample size: 2,388 program group participants. Levels shown have been adjusted for differences in baseline characteristics, including research group, *learn*$ave site, gender, age group, highest level of education, marital status, presence of children, immigration status, activity limitation, labour force participation, household income, monthly payments for household expenses, difficulty making payments, use of household budget and future time perspective. Significant difference levels between categories are indicated as * = 10% and *** = 1%.

Figure 5.6 Proportion of Participants with Final Savings Lower than Peak Savings, by Peak Savings Amount, Program Group Participants



Source: Participant Management Information System.
Note: Sample size: 2,388 program group participants.
* Max. = $1,483.34 or greater in savings.

education but no clear observable likelihood of greater financial constraints. By looking at the total savings and dissaving activity, it appears that the "struggling" savers may have been otherwise similar to the moderate savers but used their *learn*$ave accounts almost as operating accounts, where money was nearly as likely to be deposited as it was to be withdrawn (for non-project purposes), suggesting they were in fact struggling with the intended goal of ever-increasing savings balances. The "moderate" savers were able to accumulate more and withdraw less, suggesting they were more oriented towards treating the *learn*$ave account as a true savings account and less as an operating account.

The "early" savers were, as discussed earlier, the least likely to withdraw any funds from the account for purposes other than their intended learning goal. Given that these participants were largely newcomers to Canada, we strongly suspect that the special treatment of stored income for recent immigrants at the participant selection stage may have inadvertently countered any attempts to guard against transfers of existing lump-sum amounts into the *learn*$ave account to benefit fully and quickly from the financial incentive in the project. All Economic Class immigrants to Canada are expected to arrive in this country with stored income adequate to support themselves and their dependants for one year above the applicable Low-income Cut-off. Under that policy regime, the lump sum is clearly a stored reserve of income to be drawn down during the first year of settlement in Canada. From one perspective, this could be assumed to have encouraged windfall gains as newcomers with stored financial resources simply transferred money into their *learn*$ave account, moving quickly towards the goal of adult education which they would likely have invested in even in the absence of *learn*$ave. From another perspective, the project designers and immigrant participants both behaved consistently with an external immigration policy that says financial resources brought into Canada on landing are income and should be spent down on purposes that help with settlement and adaptation. The response of this sub-set of participants highlights very clearly the problem of different approaches to deciding when an amount of money is a flow of income and when it is a stock of assets. This is a much broader question for policy-makers that requires significantly more attention than can be offered in this report. In any case, because the study of *learn*$ave's impact was based on random assignment in which newcomers were roughly equally distributed between the program and control groups, the assets brought in by newcomers to the country had no net effect on the impact estimates of the project.

Finally the "determined" savers are so labelled because they were able to meet the maximum savings goal but needed more time than their "early" saving colleagues to do so. They were also much less likely than their "modest" or "struggling" saving counterparts to make any withdrawals from their *learn*$ave account for purposes outside the project. Like the tortoise in the parable of the tortoise and the hare, they made it to the finish line handily but at their own pace. This group, representing a little over a fifth of participants (21.9 per cent), may demonstrate the behaviour most closely resembling the hoped-for results of the original design of the program savings instrument. In the context of the *learn*$ave sample of low-income adults, this suggests that flexibility (discouraging but not forbidding withdrawals) and patience (allowing short as well as medium-term saving terms) may be important virtues in any savings vehicle designed for vulnerable populations.

Making matched withdrawals

In this next section we move from examining whether and how participants used their IDA accounts for building savings to examining how participants used their accounts to withdraw (or use) matched credits for eligible

Table 5.4 Initial Typology of *learn*$ave Savers According to Saving/Dissaving Patterns

Pattern of Account Use	Description	Associated Baseline Characteristics	Proportion of Participants
Unsuccessful /disengaged	No savings or very low savings (less than $120) and little/no dissaving	None identified; not related to lower baseline incomes or financial constraints	10.6%
Struggling	Modest savings ($120-$999.99) and very high rates of dissaving May have used learn$ave as an inadvertent operating account rather than strict savings account	More predominant among Canadian-born, participants with lower level of prior education; not related to lower baseline incomes or observable financial constraints	12.7%
Moderate	More savings but less than maximum ($1,000 to $1,483.33 and moderate rates of dissaving May have had changes in circumstance that precluded additional savings	More predominant among canadian-born, participants with lower level of prior education; not related to lower baseline incomes or observable financial constraints	11.8%
Early	Quickly saved maximum ($1,500) within 18 month or less, little dissaving May have been highly motivated by *learn*$ave incentive; may have had higher financial flexibility to meet savings goals so early	More predominant among recent immigrants to Canada and participants with higher levels of prior education	43.0%
Determined	Saved maximum but did so making use of longer saving period, little dissaving	None identified	21.9%

project purposes. This stage of the program model is the outlined box in Figure 5.7. As with any defined purpose savings account, it is important to emphasize that the *learn*$ave accounts had a clear expected accumulation/saving phase and a clear expected investment phase during which the financial capital would be depleted. This period began no earlier than 12 months after entering the program (the earliest date that a participant could qualify for matched savings) and ended no later than 48 months after entering the program (the time 12 months after the saving period when *learn*$ave matched credits had to be used or forfeited).

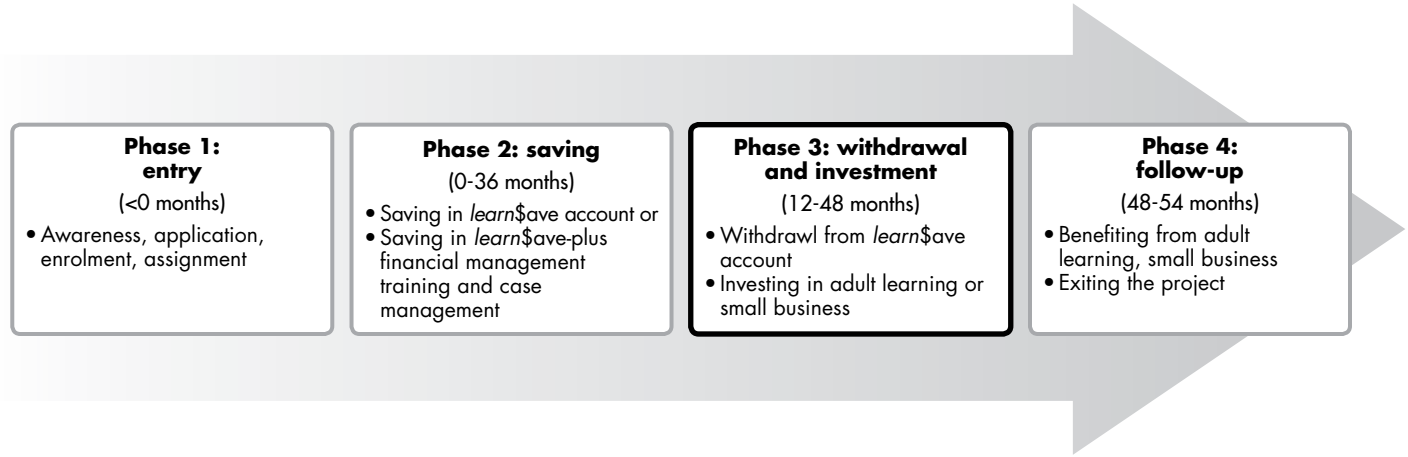
On average (Figure 5.8), at month 48 — the end of the saving period — participants had earned $3,077 in matched credits, or about two-thirds (68.3 per cent) of the potential maximum of $4,500. It is important to note that these earned credits are an average across all participants including the "early," "struggling," and all other types of savers in the project. It is also worth noting that this figure is lower than the average credits earned observed at the end of the saving period ($3,267 at month 36) because of dissaving (unmatched withdrawals) between months 36 and 48.

Turning to credits use, by the end of the investment period at month 48, participants spent $2,435, on average, for eligible project purposes (Table 5.5) or roughly 80 per cent of average available matched funds ($3,077). An analysis of the use of *learn*$ave account usage during the investment phase, however, should take into consideration use of own funds, as participants had to use at least $1 of their own *learn*$ave savings for every $3 of matched credits used for approved purchases. When individual participant own savings are imputed using the 3:1 match rate, we estimate an average participant at month 48 had $4,103 ($3,077 + $3,077/3) in available funds (disregarding approved purchases made) and invested a minimum of $3,247 ($2,435 + $2,435/3) in eligible project goals.

The trend line in earning matched credits was heavily influenced by the "early" and then "determined" savers, rising steeply from 0 to 12 months and then along a flatter slope from 12 to 36 months. The trend line in matched credits used did rise more consistently over time after 12 months, although perhaps less steeply and at a lower overall level than the project designers originally expected.[2] The sudden upturn in the average use of matched credits after month 46 almost certainly reflects the last-minute efforts of participants to invest available *learn*$ave funds before they expired, either of their own initiative or in response to reminders from case managers.

2 Based on projections by SEDI for HRSDC's project financial forecasting.

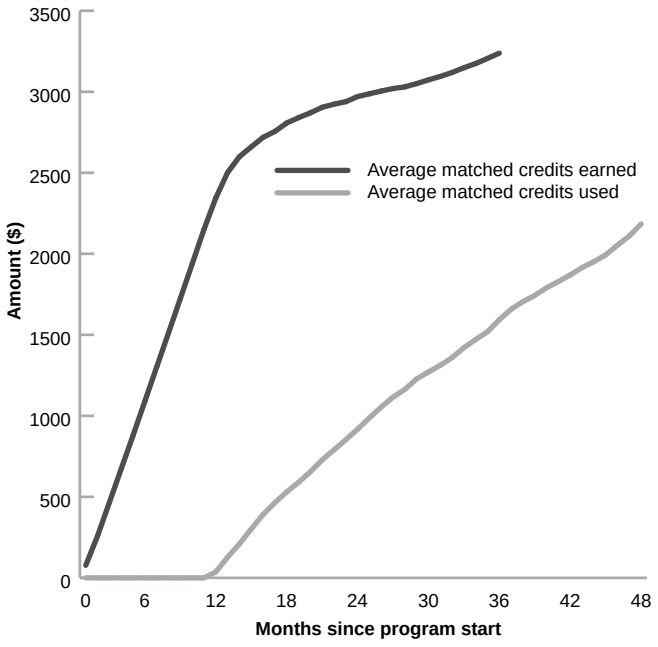
Figure 5.7 Stages of *learn*$ave Program Participation



Take-up of matched credits earned

Another way to examine the use of matched credits during the investing/learning phase is to determine how many participants used the full amount of the matched credits they had accumulated. In this case the reference is to their own individual savings rather than the pooled average of all participant savings. This avenue of inquiry leads to a slightly different picture: just over a third (37.2 per cent) of participants who saved used all of their matched savings credits in their *learn*$ave IDA (last row of Table 5.5).

Figure 5.8 Average Matched Saving Credits Earned and Used, by Month, Program Group Participants



Source: Participant Management Information System
Note: Sample size: 2,388 program group participants.

Table 5.5 provides some additional information behind this figure. When all project participants are considered, two-thirds (66.1 per cent) were able to draw some benefit from the matched credits. Among those who did access matched credits during the investment phase, about half (49.3 per cent = 32.6 / 66.1) used the maximum amount of credits ($4,450 or greater). However, these figures do not adjust for the 17.7 per cent of participants who did not meet the eligibility criteria for withdrawing matched credits.[3] If these participants are taken out of the calculation since they could not have withdrawn any matched credits in any case, then just over 80 per cent of participants who qualified for matched credits (66.1 / 82.3) saw some benefit from the matched credits they had accumulated in the project. The 80 per cent of participants who made use of the matched credits tended to make multiple withdrawals (more than 4 on average) but in relatively large sums ($1,988 on average).

Similar to the different savings patterns among participants, the withdrawal of matched credits shows some variation (Table 5.5). Among all participants, about one-third (32.9 per cent) made no withdrawals of matched credits. It appears that many of these participants had earned matched credits but, for unknown reasons, did not use any of them. As indicated above, 17.7 per cent of participants did not qualify for credits, which means that, of the 32.9 per cent who made no withdrawals, almost half (46.2 per cent = 100 – (17.7 / 32.9)) withdrew no matched credits even though they had earned them during the saving period.

A handful of participants (0.8 per cent and 4.6 per cent, respectively) withdrew a very small amount (between $1

3 Recall from Table 5.2 that 82.3 per cent of participants were eligible for matched credits because they saved at least $10 in each of 12 months.

and $359) or a small amount (between $360 and $1,499) in matched credits. A little more than one-quarter (28.2 per cent) withdrew a moderate amount of matched credits ($1,500 or more but less than the maximum). Finally about a third (32.6 per cent) of all participants withdrew the maximum matched credits available within the project rules.

An analysis of the characteristics of participants who made use of the matched withdrawals they had accumulated (Figure 5.9) suggests that those with higher levels of prior education when they entered the project were more likely to make use of credits they were entitled to. This is not surprising given that education was also associated with higher rates of saving during the saving period (recall Figure 5.4) and those with higher savings should be expected to have better access to matched credits as per the design of the matching accounts. Interestingly, participants with matched credits were as likely to have incomes at the very lowest as the very highest ends of the range in the project.

Patterns of saving and matched withdrawals

The average amount of matched credits used rose directly with the dollar amount of savings during the saving period and the likelihood of not using matched credits earned declined. Figure 5.10 compares participants with different peak savings levels by the average amount of their own earned matched credits used. This analysis makes the results less sensitive to the impact of the dollar value of the credits earned since there is no clear reason to expect a priori that participants who earned $360 dollars in matched credits (the lowest amount that could be earned at the lowest eligible savings level) should be any more or less likely to use up all credits than participants who earned $4,500 in matched credits. This analysis also excludes the "unsuccessful" savers who saved less than $120 and did not qualify for any matched credits.

Those participants identified in the previous section as "early savers" (reaching the maximum by month 18) were the most likely to use their matched credits using an average of $0.76 for each $1 in matched credits earned (as shown in Figure 5.10(a)) and were the least likely to leave earned matched credits unused (11.7 per cent) (as shown in Figure 5.10(b)). This is consistent with the suggestion that these participants may have had a particularly strong motivation to make the most use of the *learn*$ave financial incentive. The "determined savers" (reaching the maximum past month 18) were, like the "early savers," also "high investors" and were the next most likely to make maximum use of their matched credits (using an average of $0.71 for each $1 in matched credits earned) but over one in six of these participants (17.2 per cent) left all of their matched credits unused. At the other end, "struggling" participants (with peak savings of $120-999.99), who saved enough to qualify for matched credits but in relatively low amounts, were least likely to use the own matched credits available to them (using on average just $0.12 for each $1 saved) and almost two-thirds of these participants (64.2 per cent) did not use any of their earned matched credits at all.

Putting together the information on saving, dissaving, and matched credit use suggests the typology of *learn*$ave account use (presented in Table 5.6), which is an expansion of the patterns described earlier in this

Table 5.5 *learn*$ave Matched Withdrawals[1], Over 48 Months, Program Group Participants

Among all participants	
Proportion of all participants who used any matched credits at all (%)	66.1
Proportion of all participants who used no matched credits at all (%)	33.9
Distribution of the matched credits used (%):	
▪ $0	33.7
▪ $1 to $359.99	0.8
▪ $360 to less than $1,500	4.6
▪ $1,500 to less than $3,000	11.2
▪ $3,000 to less than $4,450	17.0
▪ $4,450 to $4,500 (the maximum[2])	32.6
Average number of matched withdrawals	2.9
Average matched credits used per participant ($)	2,435
Proportion who used all matched credits earned (%)[3]	37.2
Among those eligible for matched credits	
Proportion who were eligible for matched credits and used any at all (%)	80.2
Proportion who were eligible for matched credits but used $0	19.8
Among those who withdrew credits	
Average number of months it took to make first matched withdrawal	9.8
Average number of matched withdrawals	4.3
Average number of months with matched withdrawals	3.3
Average amount withdrawn per matched withdrawal ($)	1,988
Average amount withdrawn per person over the period ($)	3,678
Sample size	2,388

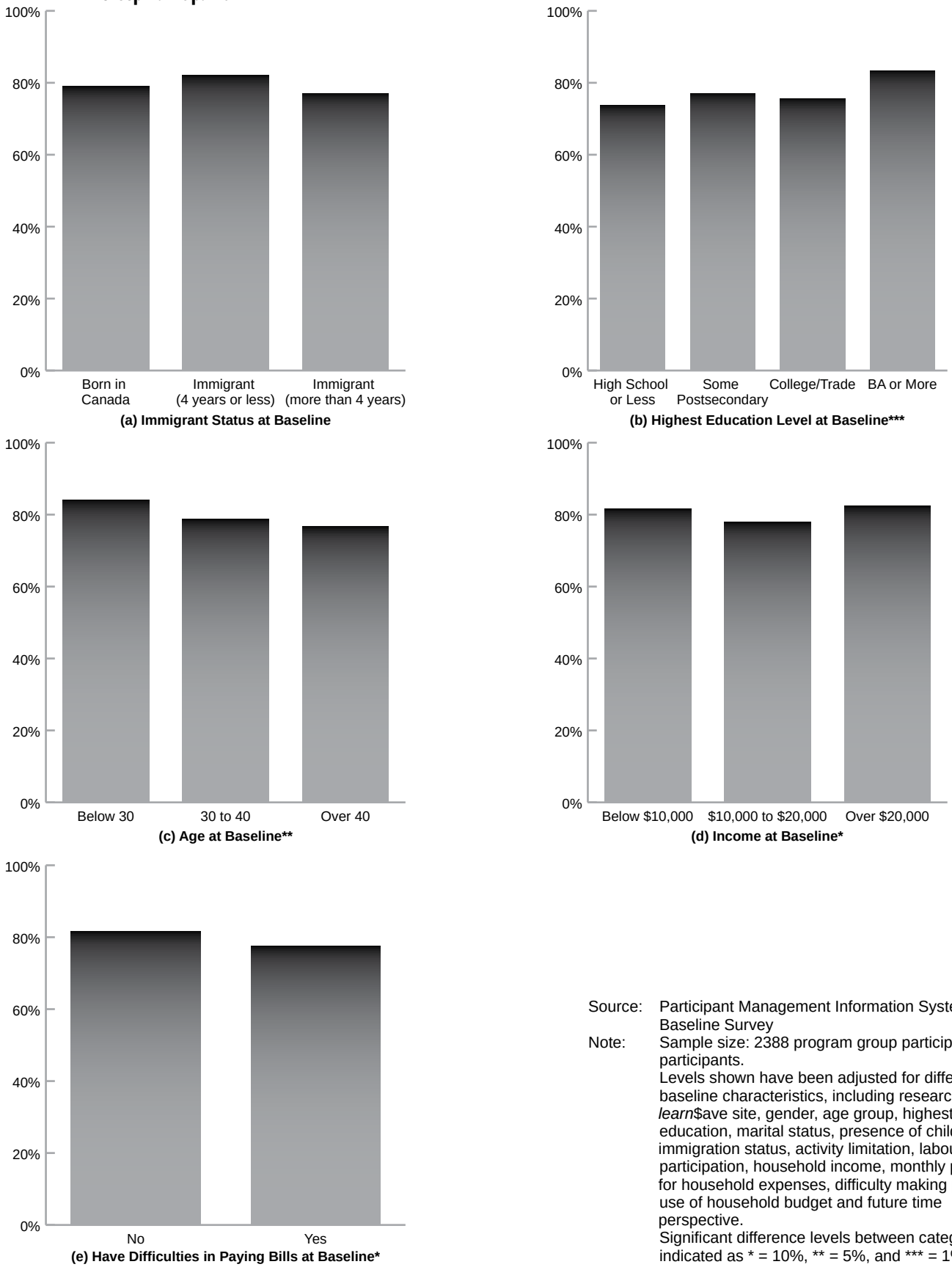
Source: Participant Management Information System and Baseline Survey

Note: [1] Due to correction of records in the PMIS, some figures may differ slightly from those previously published.

[2] Participants who used $4,450 or more matched credits are considered to have used the maximum amount.

[3] Among those who saved.

Figure 5.9 Proportion of Participants Eligibile for Matched Credits with Matched Withdrawals, by Baseline Participant Characteristics, Program Group Participants



Source: Participant Management Information System and Baseline Survey

Note: Sample size: 2388 program group participants participants.
Levels shown have been adjusted for differences in baseline characteristics, including research group, *learn*$ave site, gender, age group, highest level of education, marital status, presence of children, immigration status, activity limitation, labour force participation, household income, monthly payments for household expenses, difficulty making payments, use of household budget and future time perspective.
Significant difference levels between categories are indicated as * = 10%, ** = 5%, and *** = 1%.

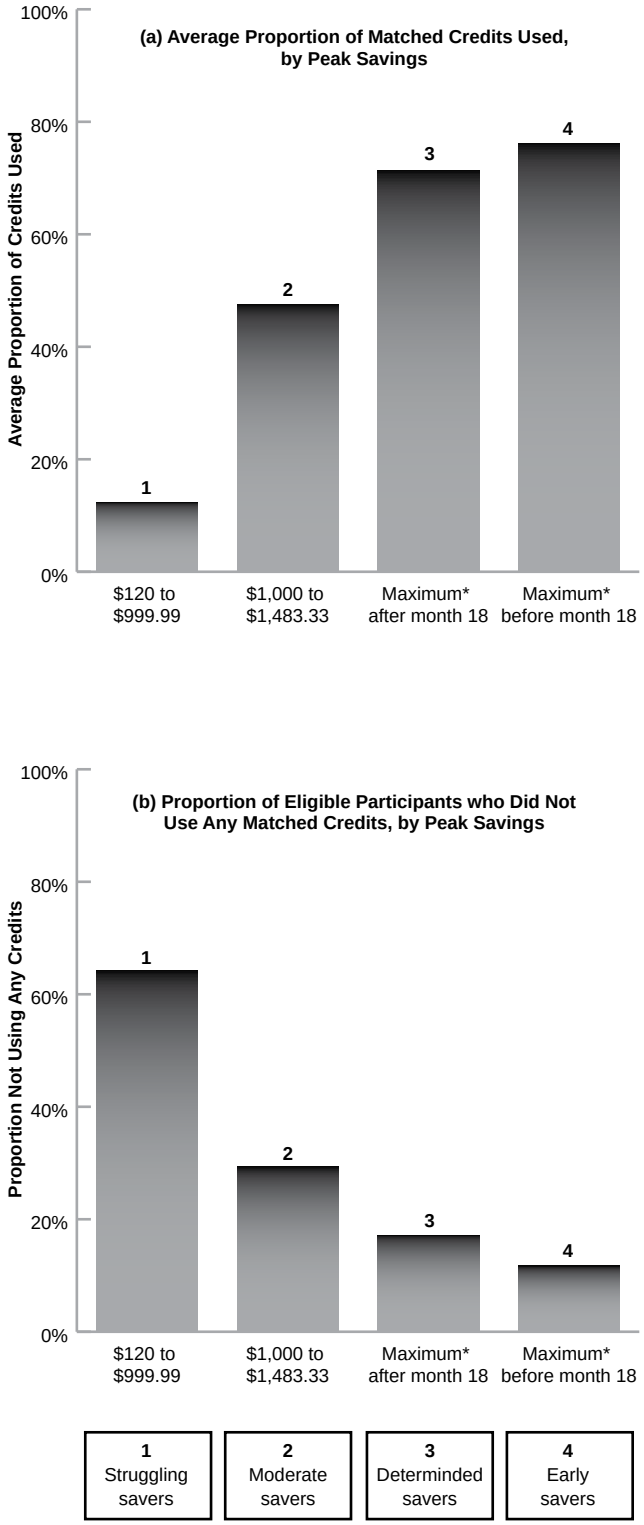
chapter in Table 5.4. The evidence suggests a typology of five different low-income IDA users. These may or may not be consistent outside of the *learn*$ave context but we have, as yet, no available data to verify this. Future research on low-income savers, both in IDAs and in related account-based mechanisms, may confirm our proposed typology above.

Some observations are worth making before turning to the next section of this chapter: First, while the overall use of matched credits among participants was high as an aggregate (about two-thirds used credits at least once), it was almost certainly pulled upwards by a small group of participants who saved the most, earned the most credits and cashed out the most credits during the project. However, using the aggregate measure alone conceals the non-negligible number of participants who earned matched credits but then abandoned at least some of the credits to which they were entitled. A non-negligible proportion of participants who had accumulated matched credits did not use even $1 in matched credits when the time came to cash out — 32.9 per cent of all participants did not use any credits. Certainly, this risk fell as participant savings rose, consistent with the idea of stakeholding that is a key part of the asset-building model that informed *learn*$ave. However, the combined trends of less than full use and also zero use of earned credits and the pattern of using no credits at all suggests that some *learn*$ave participants may have faced certain barriers during the part of the program period (month 12 to month 48) when they were expected to invest their savings in eligible project goals. While motivation and then current circumstances may have supported the idea of saving and investing in adult learning when participants enrolled in *learn*$ave, changes in employment, health, family, income or other conditions may have forced a re-evaluation of the *learn*$ave goals.

The role of ancillary services: Experimental results

During the saving period (months 0–36) participants in the *learn*$ave-plus group were expected to take part in 15 hours of financial management training (FMT) and participants in this group had access to case management services through to the end of the cash-out or investment period (up to month 48). Some case management was also made available to *learn*$ave-only participants, including account and project information, updates, reminders and help with the cash-out process. However, *learn*$ave-plus participants were offered a more intensive level of case management services including referrals to other community programs and services and a more proactive approach to providing account and project updates and reminders. The design of *learn*$ave suggested that participants in the *learn*$ave-plus group who received the

Figure 5.10 Matched Credits Used and Unused, by Peak Savings Amount, Program Group Participants



Source: Participant Management Information System.
Note: Sample size: 2,388 program group participants.
* Savings of more than $1,483.33 were considered as the maximum.

Table 5.6 Final Typology of *learn*$ave Participants According to their Account Use Patterns

Pattern of Account Use	Description of Saving/Dissaving	Description of Matched Credit Use
Unsuccessful savers/ disengaged participants	No savings or very low savings (less than $120) and little/no dissaving	No benefit from matched credits. Not eligible for matched withdrawals.
Struggling savers, low investors	Modest savings ($120-$999.99) and very high rates of dissaving May have used *learn*$ave as an inadvertent opperating account rather than strict savings account	Very low use of matched credits earned ($0.12 per $1). Likely to not use any matched credits at all.
Moderate savers and investors	More savings but less than maximum ($1,000 to $1,483.33) and moderate rates of dissaving May have had changes in circumstance that precluded additional savings	Modest use of matched credits earned ($0.47 per $1). Some risk of not using any matched credits at all. May have had changes in circumstance that precluded additional use of credits.
Early savers, high investors	Quickly saved maximum ($1,500) within 18 month or less, little dissaving May have been highly motivated by *learn*$ave incentive; may have had higher financial flexibility to meet savings goals so early	Highest use of matched credits earned ($0.76 per $1). Least likely to not use any matched credits.
Determined savers and high investors	Saved maximum but did so making use of longer saving period, little dissaving	High use of matched credits earned ($0.71 per $1). Low but not negligible likelihood of not using any matched credits.

two ancillary services might have better outcomes during the saving and cash-out period. In this section we review evidence for any incremental impact of these ancillary services on *learn*$ave participant savings and use of the matched credits they earned. It is important to repeat our report's earlier emphasis that these results do not show the impact of the ancillary services alone but instead are reflective of the change in the impact when the ancillary services are combined with the financial incentive of the *learn*$ave IDA.

The impact of the *learn*$ave services on saving and withdrawal outcomes was small in substantive terms but given that the impacts are in an experimental context, they are notable. As indicated in Table 5.7, participants in the *learn*$ave-plus group were more likely than the *learn*$ave-only group to save at all, to meet the minimum active savings months threshold to qualify for matched credits, to have higher savings at their peak, and were more likely to save the maximum. Each of these findings is discussed in greater detail below.

As the first section of Table 5.7 indicates, *learn*$ave-plus participants were more active in saving than the *learn*$ave-only group: the proportion eligible for matched withdrawals was higher for the *learn*$ave-plus group (by 3.8 percentage points); and *learn*$ave-plus participants had a higher average number of active savings months than *learn*$ave-only participants, by close to 1 month. The *learn*$ave-plus participants also had higher peak savings by $54 and more of them saved the maximum matchable savings (by 5.5 percentage points overall and by 3.6 percentage points by the 18-month mark). At the end of the saving period, the *learn*$ave-plus group had, on average, $72 more in their account balance and $65 more in matchable deposits.

Though statistically significant and positive, these impacts of *learn*$ave services remained small in substantive and relative terms. For example, the $65 difference in average matchable deposits between program groups represents only 6 per cent of the *learn*$ave-only group's average deposits ($65 / $1,056). The same could be said of peak deposits ($54 / $1,167 = 6.2 per cent).

The services made an even smaller difference in matched withdrawals than they did for matched deposits. The bottom half of Table 5.7 reveals that the only statistically significant impacts the services had on withdrawals were in the likelihood that all participants in the group withdrew any matched credits at all (4.7 percentage points greater) and the proportion who used the maximum amount (3.7 percentage points higher). However, these are small differences which correspond to the patterns of impacts on matched credits earned. There is virtually no difference between groups in the likelihood that the subset of participants who qualified for matched credits would make any use of them or the average proportion of matched credits used between the two program groups. This analysis from the experimental data strongly suggests that the key incentive for savings and matched credit use came from the *learn*$ave account and matched credits rather than the financial management training and case management services.

The difference that the services made to the accumulation and use of matched credits grew at a modest pace over time. As the top two lines of Figure 5.11 indicate, from month 29 onward, the difference in average

Table 5.7 Impact of Financial Management Training and Case Management Services on *learn*$ave Savings and Withdrawals, Over 48 Months, Program Group Participants

	***learn*$ave-only**	***learn*$ave-plus**	**Impact of Services when Offered with Matched Credits**	**Standard Error**
***learn*$ave Saving Activity**				
Proportion who saved at all (%)	91.3	94.6	3.3***	(1.0)
Proportion with 12 active saving months (met savings requirement to earn credits) (%)	80.4	84.2	3.8**	(1.6)
Average number of active saving months (deposited >$10 in a month)	13.4	14.2	0.8***	(0.3)
Average highest balance in *learn*$ave account (peak savings) ($)	1,167	1,221	54**	(22)
Distribution of the peak matchable savings (%):				
▪ $0	8.7	5.4	-3.3***	(1.0)
▪ $1 to less than $120	3.8	3.4	-0.3	(0.8)
▪ $120 to less than $500	6.1	6.3	0.2	(1.0)
▪ $500 to less than $1,000	6.3	6.7	0.4	(1.0)
▪ $1,000 to $1,483.33	13.0	10.6	-2.4*	(1.3)
▪ $1,483.34 to $1,500 (the maximum)	62.2	67.6	5.5***	(2.0)
Proportion who saved maximum matchable amount (%):				
▪ at least $1,483.34 by month 18 ("early savers")	41.2	44.8	3.6*	(2.0)
▪ at least $1,483.34 after month 18 ("determined savers")	21.0	22.9	1.9	(1.7)
***learn*$ave Matched Withdrawals**				
Proportion who used any matched credits at all (%)	63.8	68.5	4.7**	(1.9)
Proportion who were eligible and used any matched credits at all (%)	79.2	81.2	2.0	(1.8)
Distribution of the matched credits used (%):				
▪ $0	35.9	31.5	-4.4**	(1.9)
▪ $1 to less than $360	0.9	0.7	-0.3	(0.4)
▪ $360 to less than $1,500	4.6	4.6	0.0	(0.9)
▪ $1,500 to less than $3,000	11.1	11.4	0.4	(1.3)
▪ $3,000 to less than $4,500	16.7	17.4	0.6	(1.5)
▪ $4,450 to $4,500 (the maximum)	30.8	34.5	3.7*	(1.9)
Average proportion of earned matched credits used (%)[1]	58.6	60.9	2.4	(1.8)
Proportion who used all matched credits earned (%)[1]	35.8	38.5	2.7	(2.1)
Sample size	1,195	1,193		

Source: Participant Management Information System
Note: Two-tailed t-tests were applied to impact estimated by differences in measures between the program groups.
Statistical significance levels are indicated as: * = 10 per cent; ** = 5 per cent; *** = 1 per cent.
Rounding may cause slight discrepancies in sums and differences.
[1] Among those who saved.

accumulated matched savings credits was at least $164 (statistically significant at the 5 per cent level) and grew slowly from month to month to the end of the saving period, indicating the impact of the services on saving was also increasing gradually. The bottom two lines of Figure 5.11 indicate that the difference in average credits used between program groups also increased over time to about 187 at the end of the program. The reason for the cumulative effect of the services is not entirely clear but may be worth further exploration in future research.

The role of program design in saving performance

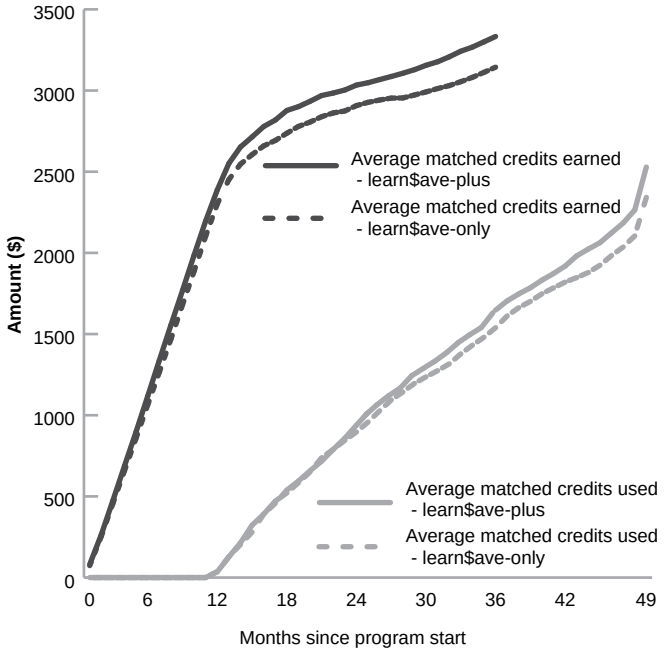
The non-experimental sites in *learn*$ave explored different variations in the design of the basic *learn*$ave model. Some tried higher or lower match rates, one tried a higher amount of participant savings that could

Box 5.1 Did the intensity of services matter?

While the presence of case management services and financial management education played a small role in saving and withdrawal activity, results from additional analysis using non-experimental data suggest an association between the intensity of case management services and account activity.

While Chapter 4 indicated little variation in hours of financial management training, it did show considerable variation in the number of hours of case management services received by participants. To illustrate, over a fifth of all participants received less than 60 minutes of case management and another 30 per cent received almost five times that amount or more. Even within either program group, there was a wide range in case management intensity. Results from further analysis suggests that the greater the case management hours received, the greater the savings and matched withdrawals: participants who received 250 minutes or more of case management services saved, on average, $1,406 and withdrew 5.5 times for $3,584 in credits. This is substantially greater than the project averages and substantially greater than the savings and matched credit use of participants who received very little case management service.

Figure 5.11 Average Matched Saving Credits Earned and Used, by Month and Program Group



Source: Participant Management Information System
Note: Sample size: 2,388 program group participants.

earn matched credits (match cap) while another tried a more compressed saving period (for a full discussion of these program design elements see Chapter 2). In this section, we present regression results from the analysis of the effects of these program design elements on two saving outcomes: savings level and regularity in saving. The basic data on these saving outcomes across all sites (including the experimental ones) are shown in the last two variables in Appendix E, Table E.1. Also shown are cross-site data on other variables used in the regression model thought to influence saving outcomes, including participant socio-demographic characteristics, IA recipient status, saving stream and local unemployment rate. For an explanation of why these variables were added to the regression, see Box 5.2. The data for this analysis came primarily from the PMIS.

The first saving outcome modeled in the regressions, savings level, is defined as the average matchable savings in the *learn*$ave account, a cumulative measure of *learn*$ave deposits, including any money that was taken out as a matched withdrawal (i.e., funds withdrawn from the account for eligible purposes did not decrease this amount). This measure excludes: unmatched withdrawals (i.e., funds withdrawn for purposes outside the project goals); deposits that exceeded $250 per month or deposited after the saving period ended (i.e., funds that could not be matched); and any other deposits that exceeded the local *learn*$ave match cap.

The second variable modeled, regularity in saving, is defined as the average proportion of eligible months with eligible savings. It was the (implicit) hope that *learn*$ave would promote regularity in savings — in an effort to encourage a saving habit. However, it should be noted that the expected results are ambiguous. Some savers might contribute large amounts to their *learn*$ave accounts for relatively few months (for example in each of the months when they receive their GST credit payments) while other savers might contribute small amounts for a continuous period. It is difficult to state a priori which pattern is more indicative of regularity in or a commitment to saving.

To ensure comparability of treatments across all sites for this analysis, the sample was limited to participants who received *learn*$ave financial management training and enhanced case management services in addition to the matched saving credits. Thus, the analysis sample (n=2,412 participants) was composed of those in the *learn*$ave-plus group at the three experimental sites (n=1,193), IA recipients at the experimental sites (n=225), and all participants at the non-experimental sites (n=994), with the latter two groups receiving

services very similar to those received by those in the *learn*$ave-plus group. Participants in the *learn*$ave-only and control groups at the experimental sites were excluded from this analysis.

Table 5.8 shows the results from the regression analysis of the effects of program design features and other variables on the two saving outcomes. Starting with the saving match rate (the first two coefficients in the first column of results in Table 5.8), the results indicate that the effect of the rate is positive while the square of the rate is negative. This suggests that raising the match rate will increase cumulative savings but at a decreasing rate of effectiveness. In other words, increasing the match rate from 2:1 to 3:1 and increasing the match rate from 3:1 to 4:1 will both increase cumulative savings, but the first rate increase will boost savings much more than the second rate increase. As for the match cap (the maximum savings eligible for credits), the results indicate that raising it by $100 increases cumulative savings by an average of about $34. Reducing the length of the saving period from three years to two (as was done at Calgary site) did not have a statistically significant effect on cumulative savings. This may reflect the fact that much of the saving for *learn*$ave accounts occurred in the first 18 months, as observed earlier in this chapter.

Turning to the second saving outcome, the results in the second column of Table 5.8 indicate that increasing the match rate by $1 for every dollar saved results in a 12-percentage-point increase in saving regularity. Decreasing the saving period from three to two years also increases regularity in saving during the shorter saving period. Increasing the savings limit (match cap) by $100 increases the proportion of months saved in that period by 1 percentage point.

While these results are not based on experimental techniques owing to a lack of control group, they are nonetheless striking and consistent with the view in behavioural economics that often small changes in parameters can sometimes have substantial effects on individual economic behaviour (see for example Thaler and Sunstein, 2009).

The role of social assistance status in saving outcomes

No results have been presented so far for the most vulnerable *learn*$ave participants: those on income support from social assistance. These participants had among the lowest incomes at the time they enrolled in the project and in many jurisdictions faced the added challenge of participating in *learn*$ave within a regulated structure meant to discourage savings and asset-accumulation among IA recipients.

Box 5.2 How do local economic conditions and participants' socio-demographic characteristics affect savings?

In the regression analysis conducted to examine the role of program parameters and IA status on ***learn***$ave savings outcomes, certain additional variables were introduced to control for other factors that could influence saving outcomes but which were not the main focus of attention.

One of these was the local unemployment rate at the time of the baseline survey. Drawing on data from Statistics Canada for data on site-level unemployment rates, we find that higher local unemployment did have a statistically significant and negative impact on saving regularity. However, the magnitude of the impact was small. Poorer local economic conditions may, as expected, lead to less overall and less frequent savings, perhaps reflecting few opportunities to earn income to save, but the variable's explanatory power is weak at best compared to the effects from program parameters and participant characteristics.

Socio-demographic characteristics also had an influence on savings. Participants who were separated, widowed or divorced at baseline were more likely to save less and less likely to be regular savers. Similarly, lower savings and lower regularity in savings were also observed among male participants and participants with one or more children in the household. However, none of these characteristics at baseline proved as negative as IA status on savings outcomes used in this analysis (see next section). Interestingly income level (among this group of low-income earners) played no role, confirming earlier observations. On the other hand, being married, being an immigrant and having an education beyond just a high-school certificate (particularly a university degree) had significant positive effects on savings level and regularity, also as demonstrated earlier.

In this section, we present results on the role of baseline IA recipient status on savings. These results were generated by the regression analysis described above to explain saving outcomes on the basis of program parameters, controlling for participant characteristics of which IA status was one. To repeat, the analysis was limited to participants who received *learn*$ave financial management training and enhanced case management services in order to ensure comparability of treatments across sites. In the analysis sample of 2,412 participants, there were approximately 465 IA recipients across the 10 sites. The comparison of interest here is between the saving outcomes of those who had IA and those who did not have IA as their primary income source at baseline. IA status is expected to have a negative effect on savings.

Table 5.8 Effects of Program Design Parameters, Income Assistance Status, Participant Characteristics, and Unemployment Rate on Cummulative Savings and Proportion of Months Saved: Regression Coefficients, *learn*$ave-plus and Non-experimental Particpants

Explanatory Variables	Cumulative Eligible Savings	Proportion of Eligible Months with Eligible Savings
Program Parameters		
Match rate (no. of $ matched per $ saved)	310.3**	0.117**
Match rate squared	-42.4**	-0.006
Saving period 24 months	4.9	0.063***
Match cap (in $100s)	34.1***	0.010***
Participant/Site Characteristics (at baseline)		
Micro-enterprise stream (vs Education)	102.4***	0.004
Received IA (vs not)	-206.8***	-0.043***
Male (vs female)	-82.4***	-0.055***
Immigrant (vs non-immigrant)	311.2***	0.059***
Aged 26 to 40 (vs <25 years)	31.8	0.035***
Aged 41 or more (vs <25 years)	-0.3	0.064***
Married (vs single)	152.3***	0.021
Separated/Divorced/Widowed (vs single)	-66.5*	-0.034**
One child in household (vs no children)	-97.8***	-0.031**
Two or more children in household (vs no children)	-101.7***	-0.024*
Some post-secondary education (vs highschool certificate only or less)	159.3***	0.040***
College/vocational certiticate/degree/diploma (vs highschool certificate only or less)	216.7***	0.064***
University - Bachelors degree (vs highschool certificate only or less)	398.5***	0.071***
University - Post-graduate degree (vs highschool certificate only or less)	362.2***	0.054**
Household income $12,500 or more (vs <$12,500)	-15.4	-0.004
Unemployment rate at start of program (in %)	-13.0	-0.018***
Intercept	-353.2*	-0.010
Sample Size	2,412	2,412
R-Squared	0.217	0.085

Source: Regression equations using data from the Participant Information Management System.
Note: Significance level of regression estimates (based on two-tailed t-tests): * = 10 per cent; ** = 5 per cent; and *** = 1 per cent.

IA recipient status at baseline did play a role in savings levels (see Table 5.8 above). Those who were IA recipients at baseline saved substantial amounts but $207 less on average than those who were not IA recipients at baseline, controlling for program design and socio-demographic factors. Other research based on only descriptive analysis of PMIS data indicates that account opening was also lower among IA recipients (Kingwell, Dowie, and Holler, 2004) as was the proportion of income saved, though the difference in proportions between IA recipients and non-recipients was smaller than the difference in average saving levels (Robson, 2008). Those who received IA at baseline also saved in fewer months during the project saving period (by 4.3 per cent) than those who had other income sources at baseline.

The results of this analysis cannot tell us whether IA recipients' lower and less frequent savings were due to a more restricted capacity to save (related to lower income levels) or to behavioural adjustments in response

to social assistance regulations in place on participant savings. As noted in Robson (2008), provinces in Canada all have some but varied rules on how much, when and what kinds of savings IA recipients can have or accumulate without penalty to their income assistance benefits. While an increasing number of provinces have, since *learn*$ave was launched, moved to exempt, fully or partially, savings held in IDA accounts, these exemptions were rare and not universal across the project when *learn*$ave was implemented. In Ontario, for example, IA participants were cautioned to manage their *learn*$ave participation within their applicable Ontario Works asset limit. For a single employable person, this would have meant saving no more than $520 before stopping, either permanently or to cash out their savings and then save again. This may have created a disincentive to save too much in the *learn*$ave account and in fact research outside of *learn*$ave (see Robson, 2008 for a review) does suggest that low-income populations may manage their assets and dissave in response to real or perceived asset limits for programs of last resort even when they are not currently dependent on social assistance income benefits. Since *learn*$ave was launched in 2000, at least six jurisdictions (BC, Alberta, Saskatchewan, Manitoba, Quebec and Nova Scotia) have moved to exempt personal and matched savings in recognized IDA programs for their IA clients (Robson, 2008).

Nevertheless, despite the lower than project average savings levels and regularity, the IA participants in *learn*$ave did demonstrate that even the very lowest-income participants in IDA programs may respond to the incentive of a matched savings program. Given the tightly constrained budgets they face, it is remarkable using a traditional lens that any IA participants were able to save at all. Robson (2008) proposes that higher and more flexible asset limits for IA participants, combined with the kind of financial incentives offered in *learn*$ave, may actually be associated with increases in welfare exits when the accumulated savings are used as investments for productive forms of capital. The current study cannot verify this claim and it is unknown from the data available through the PMIS whether *learn*$ave participants on IA at baseline were still dependent on IA after cashing out their savings. Just as other participants were expected to see long-term returns on their investment in adult education through *learn*$ave, IA participants might see an improvement in their well-being over the longer run which we will not be able to capture in this study.

In summary

Low-income Canadians recruited into *learn*$ave did, on average, make use of the accounts and financial incentives offered by the project. Nearly all opened an account, most saved something, and most used the matched credits. The analysis showed that income levels and financial constraints at baseline could not predict outcomes in savings and matched credit use. This finding contrasts with concerns expressed by critics of asset-building approaches that low-income populations do not have resources to allow any savings at all.

There was considerable variation in the ways that participants responded to the incentives of the accounts. Five distinguishable patterns of account use were identified:

- *Unsuccessful or disengaged participants:* who seem to have simply lost interest in the project.
- *Struggling savers, low investors:* whose patterns of saving and dissaving for non-project purposes suggest the presence of significant barriers (financial, time, opportunity costs, attitudinal or other) in accumulating and maintaining savings.
- *Moderate savers and investors:* who saved a fair amount but used, on average, about only a half of the matched credits they had accumulated, suggesting that investing in education may have proved to be more challenging because of changing circumstances or a change of heart about their interest in the project and returning to school.
- *Early savers, high investors:* who saved up all they could in *learn*$ave, saved it fast and used as much of the matched credits as they could within a short period of time. Because this pattern was more predominant among recent immigrants to Canada, it appears that the eligibility rules developed for newcomers applying to the project may have had an important influence on the project outcomes.
- *Determined savers, high investors:* who took longer but did save the maximum and also made substantial use of the credits they had accumulated. While the evidence from the early savers and the results at the non experimental site with a shorter saving period suggest that participants save most and most regularly over a shorter period of time, these participants remind us of the value of allowing flexibility and not imposing sunset clauses too early whenever possible.

In general, investing the matched credits appears to have been more challenging for many participants than was saving their own money. A significant number of participants did not use all of the matched credits they had earned and a significant number used none of their matched credits at all. We cannot do much

more than speculate as to why this was the case, but all of the reasons seem to point towards the possibility that the delay between saving and then investing may have eroded, for all but the "determined" savers, the attractiveness of the credits for adult education purposes. For others, this delay proved necessary to achieve their maximum saving goal and to proceed with their planned investment.

The design of IDA program models can have an important influence on participant outcomes. We found declining marginal returns on increases to the match rate so the most effective and efficient rate may not be the highest. Raising the ceiling on the amount of savings that will be matched does increase the amount saved. Decreasing the saving period did not seem to affect the amount of saving achieved, but increased saving regularity. Taken together, these types of variables seem to provide cues or references for participants, signalling how much and how fast to aim to save, how much to invest in eligible goals and by when. Even if participants do not plan to or actually meet the outer limit of an IDA program design, these features may nudge participants towards more or less, faster or slower savings.

The ancillary services of financial management training and enhanced case management services, when combined with the matched saving incentive, had a significant but substantively small incremental impact on savings and use of the matched credits, compared to the saving incentive alone. While *learn*$ave-plus participants may have been expected to save more and more regularly than *learn*$ave-only participants who received only the matched credits owing to the additional assistance the former received, this did not happen in a substantial enough way to suggest there is a major benefit to adding the services on top of the matched credits in an IDA account. Most of the savings behaviour can and should be understood as a response to the incentive of the matching credit and the (unobserved) value ascribed to the eligible savings goals. This is investigated further in subsequent chapters when we look at the role of the services on participation in adult education and training.

The role of asset tests and social assistance regulations is also worth noting. Though IA recipients did have lower than project average savings levels and regularity, they did demonstrate that even the very lowest income participants in IDA programs may respond to the incentive of a matched savings program. Also, noteworthy is the variation in IA asset tests across jurisdictions and how, since *learn*$ave was launched in 2000, at least six provinces have moved to exempt personal and matched savings in recognized IDA programs for their IA clients.

Chapter 6 Impacts on budgeting, savings, net worth, and hardship

The previous chapter discussed the ways in which participants used their *learn*$ave accounts to make deposits and withdrawals of their own funds and matched credits. The program design assumes that the deposits would be made out of the income flows of participants who otherwise would have had modest incomes and low assets. However, if participants in fact only shifted money from other assets into their *learn*$ave account, then the deposits into the account would not be real "saving" out of income, as the project design intended. This chapter examines changes in the overall financial wealth and net worth (all wealth less all debts) of participants to determine whether or not in fact any growth in *learn*$ave accounts was from new savings.

In addition, because the project was also heavily informed by the theoretical constructs of asset-building, the project offers an important opportunity to examine whether or not there were any persistent changes in the financial behaviour of participants. Specifically, this chapter presents evidence on whether or not the project led to any changes in budgeting and savings, and whether or not participants had to incur hardship to meet their *learn*$ave goal.

Impacts on financial goal setting and budgeting

As described in Chapter 2 (see Box 2.2), much of the prior research on changes in financial attitudes and behaviours suggests that people often learn best about money by doing. Consistent with this finding and with the theory of asset-building that informed much of *learn*$ave, we should expect the offer of matched saving credits to increase financial goal setting and budgeting because participants would need to deposit money regularly into their *learn*$ave accounts to qualify for and earn the matched credits. Furthermore, the addition of financial management training was expected to increase the proportion of participants who set financial goals and budgeted since those in the *learn*$ave plus group received explicit instruction and assistance with goal setting and budgeting on top of the matched savings credits. It was expected that the enhanced case management would also reinforce participants' savings goals by identifying and providing referrals for participants experiencing difficulties.

The results in Table 6.1 show that the matched credits, both alone and in combination with the added services, had some modest positive impacts on setting financial goals. At month 54, the *learn*$ave-only group was more than 5 percentage points more likely to have set financial goals than the control group and the *learn*$ave-plus group was about 9 percentage points more likely to have done so. These positive impacts were broadly similar to those at 18 and 40 months.

Positive budgeting impacts were also observed. The *learn*$ave credits and services combined resulted in a 7 percentage point increase in the proportion of people who said they had a household budget at 54 months. This impact was about the same as it was at 40 months but less than it was at 18 months, again suggesting that the impact decreased over time. In contrast to what was observed at 18 and 40 months, however, the matched credits alone had no statistically significant effect on budgeting at 54 months. Given that new matched credits were no longer available to participants after month 36

Figure 6.1 Stages of *learn*$ave Program Participation

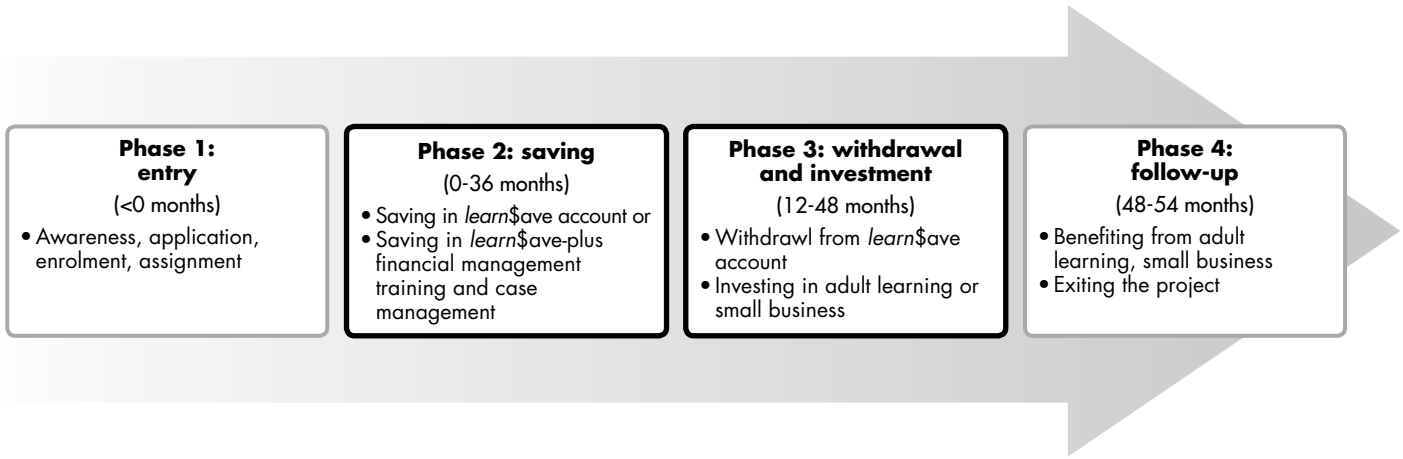


Table 6.1 Impacts on Incidence of Budgeting and Financial Goal Setting (Percentage Points) at 18, 40 and 54 Months, All Participants – Adjusted

	Control Group Incidence	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
At 54 Months				
% who set a budget	49.4	3.5	3.8	7.3***
% who set financial goals	59.3	5.4**	3.7	9.1***
At 40 Months				
% who set a budget	47.6	4.3*	2.2	6.5**
% who set financial goals	53.6	4.4*	7.6***	12.0***
At 18 Months				
% who set a budget	44.6	6.0**	5.1**	11.1***
% who set financial goals	56.2	5.1**	7.4***	12.4***

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively for the 54-month survey, 607, 833 and 814, for the 40-month survey, and 748, 920 and 915 for the 18-month survey.

Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

and could not be cashed out after month 48, however, this diminishing impact over time is perhaps not surprising.

These impacts, though modest, are potentially important. Financial goal-setting and budgeting have the potential to increase participants' savings and net worth both during the *learn*$ave project and after. The fact that these effects were still being observed at 54 months (some 18 months after the end of the 36-month saving period) is promising. However, they need not lead to increased savings and net wealth, as goals and budgets may not be kept or any freed-up resources may be spent rather than saved.

Impacts on savings and saving incidence

As discussed in Chapter 5, participants were expected to save in their *learn*$ave IDA during their first 36 months in the project. During this time, financial assets in the *learn*$ave account would be expected to increase; however, these same financial assets would have been expected to be withdrawn for eligible program purposes during Phase 3 of the project (Figure 6.1).

The available evidence from *learn*$ave confirms this expected pattern. Table 6.2 looks at savings measured as changes in the average value of total financial assets, including savings in *learn*$ave accounts, bank accounts, retirement savings, as well as other financial assets.[1] The table shows that, in the early stages of the project, between baseline and month 18, the *learn*$ave matched credits increased average financial savings by $583; the combined impact of the credits and services was slightly larger at $674. As observed in Chapter 5 (see Figure 5.3), by month 18, a large minority (41 per cent) of participants had saved the maximum savings eligible for matched savings credits pulling the average savings as a percentage of maximum program savings to 64 percent (or $957) at this point in the program model. It was further noted in Chapter 5 that early savers were likely to cash out their savings more fully and earlier than other participants. Therefore, it is perhaps not surprising that by month 40 (during which time no new credits would have been available to many participants) no significant effects on financial assets were apparent. By month 54, it is still the case that neither program group had significantly more average financial savings than the control group.

This pattern suggests that *learn*$ave participants saved more than the control group early in the program in order

1 Other financial assets include stocks, mutual funds, bonds, Guaranteed Income Certificates (GICs), term deposits, home ownership saving plans, savings at home or with family or friends, loans to family or friends, foreign financial assets and any other (unspecified) financial assets held by the participant.

to earn high-return matched credits. They then saved less than the control group during subsequent months when they were withdrawing their savings and cashing out their credits to purchase education or starting a small business. At the 54-month survey point, any positive returns to those investments in education or self-employment had yet not translated into positive growth in financial assets. Such a return in financial well-being could take place later on. However, as far as the available data suggest, the net result is that the only significant effects the project had on financial assets occurred early (at 18 months) and *learn*$ave had no significant effect on the amount of financial savings at the 54-month mark.

It is worth noting at this point that the observed pattern in the average value of overall financial assets and net worth (discussed below) is largely determined by the specific uses of savings considered eligible under the *learn*$ave program. Indeed, by their very nature, the two eligible uses of accumulated savings under *learn*$ave, adult education and business start-up, do not appear as "assets" in the financial balance sheet of participants at the time they are acquired. Instead, it is assumed that their acquisition of human capital would lead to future returns in terms of increased earnings or employment. If *learn*$ave had been opened to multiple uses, such as retirement income or the acquisition of a house, it would have been possible to record these types of assets at their market value as soon as they are acquired. In that case, we could have seen total financial assets increase at the beginning of the period and stay high during Phase 3, the time of assets acquisition.

Table 6.2 Impacts on Savings (Change in Financial Assets) (Average Dollars), All Participants – Adjusted

	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
Baseline to 18 months	583**	91	674***
Baseline to 40 months	-639	673	34
Baseline to 54 months	-536	303	-233

Source: Calculations from 54-month, 40-month and 18-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 748, 920 and 915, respectively, for the 18-month survey; 607, 833 and 814, respectively, for the 40-month survey; and 568, 842 and 859 for the 54-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

One could attempt to approximate the value of human capital acquisition by calculating a present value based on the expected future returns on investments made in education, learning, or micro-enterprise development. However, in the absence of a methodologically sound way to do that, and to avoid mixing up observations based on factual information with some broad estimates of future returns, we feel that is more appropriate to treat the investment in adult education and small business start-up as pure expenditures and to omit these items from the calculation of average assets and net worth. As a result, it is not surprising to see participants' savings and net worth decrease when participants cash out their *learn*$ave account (their own savings as well as the matched credits).

In addition to looking at changes in financial assets over time ("revealed saving behaviour"), another way to measure savings impacts is to ask participants whether they are saving money or not ("self-reported saving") and compare responses across research groups. Based on self-reported savings in the last 12 months, *learn*$ave did have some modest positive impacts on saving incidence. Table 6.3 shows that matched credits, alone and combined with the services, increased the proportion of self-reported savers by 6.5 and 5.0 percentage points, respectively. Further, *learn*$ave did have an effect on regular saving. An implicit goal of *learn*$ave was to encourage regular saving by requiring participants to make at least 12 monthly deposits into their *learn*$ave account to become eligible for matched credits. While Table 6.3 shows the credits alone had no effect on regular saving, the credits in combination with services did have a modest impact (5.8 percentage points). In other words, the program does seem to have a lasting impact on saving activity, even when no further matched credits were available to participants. The credits alone were sufficient to encourage a small but significant increase in self-reported savings of any kind in the past year, but only when combined with the services (in the *learn*$ave-plus group) did the project encourage participants to continue saving regularly by month 54.

Moreover, *learn*$ave had a positive impact on future saving intentions (data not shown). Compared to the control group, the program group was about 8.5

Table 6.3 Impacts on Self-Reported Saving over Past Year (Average and Percentage Points), at 40 Months and 54 Months, All Participants – Adjusted

	Control Group Average or Incidence	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Incentive + Services
Year prior to 40-month survey				
Amount saved in past year ($)	2,569	-561**	176	-384
Saved in past year (%)	52.2	2.1	3.7	5.8**
Saved regularly in past year (%)	29.6	1.2	2.4	3.6
Year prior to 54-month survey				
Amount saved in past year ($)	3,270	-49	112	63
Saved in past year (%)	57.3	6.5**	-1.5	5.0*
Saved regularly in past year (%)	35.2	1.6	4.2*	5.8**

Source: Calculations from 40- and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 607, 833 ad 814 for the 40-month survey and 568, 842 and 859, respectively, for the 54-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent. Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

percentage points more likely to describe as "very accurate" the following statement "It is my intention to regularly put aside some money to enable me to take the training or education I need." The credits plus the services also had a significant impact, of about 11 percentage points.

The available data do not allow us to draw any conclusions on whether making regular deposits and/or planning to save are related, over the longer term, to any measureable increase in financial assets. For example, someone might make regular deposits each month into a designated "savings" account but if these deposits are then withdrawn each quarter to make a lump-sum expenditure for a physical asset such as an appliance or a vehicle, then the greater saving frequency (incidence) will not result in an increase in financial assets. Which outcome is more important, saving incidence or assets accumulation, depends largely on the analytical perspective adopted. However, since *learn*$ave was heavily informed by asset-building theory, it is important to note that the results do not support the thesis that regular saving incidence will necessarily result in increases in financial assets or (based on the results discussed below) total net worth, at least in the medium term.[2]

Asset-building theory also proposes that assets increase future orientation and hopefulness (see Chapter 1). The results from *learn*$ave do not provide any evidence that *learn*$ave had a measurable impact on future orientation. To measure future orientation, participants were asked in the baseline and follow-up surveys a series of future time perspective questions.[3] Comparisons of the responses between program and control groups at month 54 (not shown) revealed no differences of note. This suggests that participation in *learn*$ave alone was insufficient to have any measurable impacts on future orientation and likely means that individual discount rates, considered important determinants of financial behaviour, were largely unchanged by the program during the 54-month period. It may well have been, since participants self-selected for the project, that they already had relatively low discount rates and were forward-looking enough to be interested in a project offering a generous savings incentive for adult learning, an investment whose returns are not immediate.

Savings by household income level

There is tentative evidence that the *learn*$ave matched savings credits alone and the credits plus services had effects that varied with income level at baseline.[4] Table 6.4 shows that those with the very lowest incomes saw their

2 This conclusion is however subject to the previously noted caveat regarding the treatment of spending on adult education as expenditure and not investment.

3 These were taken from the future orientation questions in the Zimbardo Time Perspective Inventory. See Zimbardo and Boyd (1999).

4 It should be borne in mind that all *learn*$ave participants had, at most, 120 per cent of the Low-income Cut-off when they joined the study. In this context, "higher-income" participants have only a modest income at enrolment and should not be interpreted as being well-off at that time, just better-off relative to other participants.

financial savings levels fall between baseline and month 54, while those with modest incomes saw positive but not significant increases in their financial savings.

Similarly, for self-reported saving incidence, the same table shows that, for the year prior to the 54-month survey, only those participants with modest incomes reported any increase in saving at all or saving regularly. For these participants at 54 months, it was the financial incentive alone that seems to have had an effect on the incidence of saving and a similar, though diminished, effect on the incidence of regular saving. By contrast and perhaps not surprisingly, those participants with the lowest incomes at enrolment were less likely (although not statistically significantly so) to report saving regularly or at all, even when the combined effects of the credits and the services are considered. More detail on these differential effects on savings levels and incidence by income is available in Appendix F.

Impacts on level and composition of net worth

As noted in Chapter 1, asset-building theory proposes that IDAs may be one way to encourage the acquisition of new and productive assets. The existing research literature on other IDA programs has focused on changes in savings in the IDA, overall savings levels or incidence, and at the imputed value of the asset purchased with the total IDA funds. However, an important policy question is whether the money in these accounts reflects net increases in saving and net wealth or simply a rearrangement of assets and debt within the portfolio. This is what we look at it in this section.

It was expected in the original program design that any asset accumulation would represent "new" saving rather than merely shifting of resources from other assets or increasing debt. Therefore, the matched credits should increase the net worth of participants for so long as participants maintained a positive *learn*$ave account balance and the addition of services should increase net worth above the effects of the credits alone. When *learn*$ave savings are drawn down during the cash-out phase (from at least month 12 to at most month 48), there may be a decrease in net worth; however, these resources should be replaced (or exceeded) by higher earnings generated after the investment in greater human capital. In other words, net worth should increase during Phase 2 (the savings period, see Figure 6.1 above), may decrease during Phase 3 (the investment or cash-out period when education investments are made), and may begin to recover or even grow in Phase 4 (follow-up). Further, observing changes in the composition of net worth, i.e., changes in its asset and debt components, would enable detection of changes in financial behaviour in response to the financial incentive and the services.

Control group performance

Before looking at the project impacts, it is instructive to examine what happened to the control group alone to uncover what the program groups would likely have done without *learn*$ave. Table 6.5 shows that, by month 18, control group members had accumulated on average a modest amount of assets ($19,560) and debts ($15,301) resulting in a low but positive net worth of 4,259. Over the next 36 months, the control group did increase their total debt to $43,309 but saw substantially more growth in their assets (to $72,153) so that they increased their net worth nearly 700 per cent to $28,844.

Much of this increase in net worth over time was due to an increasing proportion of the control group holding real property that was, in turn, increasing in value. Unsurprisingly, if this large asset (and its associated debt) were ignored, then the control group's average net wealth without net property would be substantially lower during all periods of the study. Still, average net worth without net property grew more than fourfold, from an average of $1,605 at 18 months to an average of $8,022 at 54 months.

These results show that, in the absence of *learn*$ave, the average program group participant would have experienced substantially increased net worth. This is not inconsistent with the patterns observed in population-wide surveys of changes in net worth distribution over time (see for example Statistics Canada, 2005).

Impacts on net worth

Returning to the discussion of *learn*$ave impacts, there is no evidence that the matched credits or services had a meaningful positive or negative effect on the average total net worth of participants at month 54. While the impacts on total net worth are all slightly negative, none is statistically significant (last row of Table 6.5). Nor are there any impacts on the average levels of total financial assets, total non-financial assets, total assets, and total liabilities shown elsewhere in the table. These all grew for program group members at a pace similar to that of the control group. What this suggests is that the incentive to save for education (or small business start-up) did not significantly induce participants to borrow or incur more debt to make deposits into their *learn*$ave account, since liabilities would have been expected to increase and net worth to decrease in that case. It also suggests, however, that the project did not lead participants to acquire new wealth, as least by month 54 after project enrolment. The incentive to save in their *learn*$ave account did not lead,

Table 6.4 Impacts on Saving Levels and Incidence, at 54 Months (Average $), by Household Income Level in Year Prior to Application, All Participants – Adjusted

Saving Measure and Income Subgroup (at baseline)	Sample Size	Control Group Mean	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
Savings Level (average $)					
Total Finance Assets			†	n.s.	†††
Less than $10,000	744	8,660	-2,787**	-641	-3,428***
Between $10,000 and < $20,000	921	6,761	254	400	655
$20,000 and over	604	7,033	787	1,405	2,192
Self-reported Savings (in year prior to 54-month survey)			n.s.	n.s.	††
Less than $10,000	722	3,819	-429	-590	-1,020*
Between $10,000 and < $20,000	895	3,119	-141	416	275
$20,000 and over	592	2,909	474	542	1,017*
Saving Incidence (percentage points)					
Proportion Reporting having Saved (in the year prior to 54-month survey)			†††	n.s.	††
Less than $10,000	739	65.3	-5.2	0.2	-5.0
Between $10,000 and < $20,000	918	54.8	13.8***	-4.7	9.1**
$20,000 and over	601	52.0	8.9*	1.5	10.5**
Proportion Reporting Saving "Regularly" (in the year prior to 54-month survey)			†	n.s.	†
Less than $10,000	744	42.1	-7.1	3.9	-3.2
Between $10,000 and < $20,000	921	32.2	7.3*	2.3	9.5**
$20,000 and over	604	32.0	3.0	7.8*	10.8**

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859 for the 54-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups. Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent for differences in impacts between research groups.

q-tests were applied to differences among income levels in estimated impacts. Statistical significance levels are indicated as follows †=10 per cent; ††=5 per cent; ††† =1 per cent. The abbreviation "n.s." indicates that the variation in impacts among the subgroups is not statistically significant.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

as of the 54-month survey, to an improvement in participants' financial well-being as measured by net worth.

It may be that the investments in *learn*$ave came at the expense of investments in other types of assets (or paying down existing liabilities). Indeed, the data do show changes in the composition of net worth. For example, at months 18 and 40, *learn*$ave had induced participants to have lower valued household assets such as household furniture and appliances (Leckie et al., 2009). This suggests that participants were "buying smarter" (making fewer or cheaper, or delaying, purchases), possibly to increase deposits in their *learn*$ave account during the period that the matched credits were available to them. This effect was no longer present at 54 months when the incentive of the matched credits was no longer present and participants would have had time to adjust their spending and saving habits. Another impact is that, by month 40, the matched credits had reduced *learn*$ave-only participants' average retirement savings by about $900 (Leckie et al., 2009) — again possibly to make greater contributions to *learn*$ave accounts or cover other expenses related to their education or small business. By month 54, the *learn*$ave-only group's average retirement savings had not yet caught up, as they remained $660 below the control group (Table 6.5). No similar impact

Table 6.5 Impacts on Asset and Debt Components of Net Worth (Average Dollars), at 18, 40 and 54 Months, All Participants – Adjusted

	Control Group Mean			Impacts at 54 Months		
	At 18 Months	At 40 Months	At 54 Months	Matched Saving Credits	Services when Offered with Credits[Σ]	Combined Credits & Services
Assets						
Financial Assets						
Bank accounts + *learn*$ave accounts	1,358	1,923	2,071	539	-145	393
Formal retirement savings plans	399	2,539	3,490	-660*	383	-278
Other financial assets	783	1,053	1,833	-414	65	-349
Total Financial Assets	**2,540**	**5,515**	**7,394**	**-536**	**303**	**-233**
Non-Financial Assets						
Vehicles	1,107	2,566	3,297	-119	-69	-188
Value of goods in house	4,225	7,241	5,929	-552	-12	-564
Home and other property	10,984	31,732	53,986	-2,621	3,134	513
Business assets	703	1,165	1,548	1,962*	-1,451	511
Total Non-Financial Assets	**17,019**	**42,704**	**64,759**	**-1,329**	**1,601**	**272**
Total Assets	**19,560**	**48,219**	**72,153**	**-1,866**	**1,904**	**38**
Liabilities						
Credit cards	1,202	1,718	1,705	-68	15	-53
Student loans	4,224	4,828	4,063	1,358**	-77	1,281**
Mortgages	8,329	20,880	33,164	-1,508	3,575	2,067
Business debts and liabilities	533	368	806	-197	32	-165
Other debts and liabilities	1,012	3,644	3,572	-102	395	292
Total Liabilities	**15,301**	**31,438**	**43,309**	**-517**	**3,940**	**3,423**
Net (assets-liabilities)						
Net business assets	170	797	742	2,159**	-1,483	676
Net property assets	2,654	10,853	20,822	-1,113	-442	-1,554
Net worth without net property	1,605	5,929	8,022	-236	-1,594	-1,830
Total Net Worth	**4,259**	**16,781**	**28,844**	**-1,349**	**-2,036**	**-3,385**

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey; 607, 833 ad 814, for the 40-month survey; and 748, 920 and 915, for the 18-month survey.

Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

[Σ] The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

was seen, however, for the ancillary services and matched credits combined in either period. For a discussion of potential reasons why the services did not play a larger role, see Box 6.1.

A significant impact that emerged at month 54 was on student loans. The *learn*$ave-only and *learn*$ave-plus groups had on average $1,358 and $1,281, respectively, more in student loans than the control group. Program group members in both groups probably supplemented their matched credits and savings either by taking on more student loans than the control group or by paying off their previous loans more slowly than the control group. As these groups were investing more heavily in

education than the control group (discussed in next chapter), this makes intuitive sense as participants either acquired new debt to finance the education that they began with *learn*$ave or did not have the same income to pay down previous loans which as a consequence grew. Not surprisingly, the education stream had this impact but not the micro-enterprise stream (not shown).

Table 6.5 also shows that the matched credits (alone) increased net business assets by $2,159 on average. It is not surprising that this effect was confined to micro-enterprise participants, who had about a $5,700 increase on average in net business assets due to the matched credits (not shown). However, no such effect is observed among participants who also received financial management training and enhanced case management services in addition to the credits (i.e., the *learn*$ave-plus group). In part, this is due to the lower rates of self-employment among *learn*$ave-plus participants (to be discussed in the next chapter).

Impacts on Mainstream Financial Integration

Access to the mainstream banking system is critical to saving, obtaining affordable credit and accumulating assets. While alternative financial services such as cheque cashers and payday lenders do offer a form of lending, only mainstream financial services provide an opportunity to hold a deposit account that is insured and may also offer interest. A deposit account is usually required, or at least makes it easier, to engage in electronic banking (including direct deposit, electronic funds transfer and debit card transactions) and to obtain most forms of credit, and has been observed to be a gateway to other forms of saving and investing such as registered savings products, mutual funds, etc. Credit cards also offer card holders with a convenient mechanism to conduct day-to-day financial transactions and may even be required by certain vendors such as hotels and car rental outlets. While deposit accounts are often taken as an indicator of financial inclusion, a more useful indicator may include access to both a deposit account and credit card.

No increases were observed in bank account and credit card possession as a result of *learn*$ave. Almost all (98 per cent) of *learn*$ave participants had at least one bank account at baseline and at each subsequent survey in this study (not shown). As for credit cards, while only 69 per cent of participants in the control group had one or more credit cards at baseline, by 54 months, 94 per cent had at least one (Table 6.6). More than 92 per cent had both a bank account and a credit card while less than a handful had neither (not shown). Since nearly all participants had access to these two basic features of the financial system by the end of the project, the possibility of observing greater financial inclusion based on these two indicators was essentially ruled out.

However, *learn*$ave did lead to a small increase in participants' self-reported comfort level with the mainstream financial system. At 54 months, the matched credits and services modestly increased (by 4.3 percentage points, Table 6.6) the proportion of participants who

Table 6.6 Impacts on Financial Integration (Percentage Points), at 54 Months, All Participants – Adjusted

	Control Group Incidence	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
Possession of a Credit Card				
% having credit card at baseline	68.9	-0.4	0.4	0.0
% having credit card at 54 months	93.7	-0.6	-0.4	-1.0
Comfort Level with Financial System				
"I am very comfortable dealing with banks, credit unions or other financial institutions on matters such as making deposits or withdrawals, borrowing money, etc."				
% saying statement was very accurate (5 on the scale)	66.0	2.8	3.5	6.3**
% saying statement was somewhat accurate or very accurate (4 or 5)	83.4	3.3*	1.0	4.3**

Source: Calculations from Baseline and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

said it was accurate or very accurate that they were very comfortable dealing with banks, credit unions or other financial institutions on matters such as making deposits, making withdrawals or borrowing money.

Hardship and life satisfaction effects

There is no evidence that *learn*$ave caused participants any significant hardship. There was some suggestion at the outset of the project that participants might be so motivated by the matched credits that they would reduce their consumption on necessary goods and endure hardship just to receive the return on their IDA deposits. However, Table 6.7 shows no appreciable effect on hardship levels for participants in the year prior to the 54-month survey. This was generally true at earlier stages of the project as well. The changes participants made, as observed above, in their assets and debt portfolio, including acquiring few or cheaper household goods, did not cause them, on average, undue hardship. In other words, participants in *learn*$ave, on average, made reasonable decisions in allocating their resources to meet their consumption needs and saving preferences.

Table 6.7 also shows that both program groups and the control group saw virtually all the hardship measures decline by nearly one-half between months 18 and 54. These measures include difficulty in meeting expenses, borrowing to meet needs, using the food bank, and having unpaid bills. However, about a quarter of participants in all research groups continued to experience at least one

Box 6.1 Why did the addition of services not lead to bigger impacts?

In almost all the experimental results reported in this chapter, and the previous and following ones, the addition of financial management education and more intensive case management services resulted in small incremental impacts, if any. One potential explanation for the services' lack of incremental effects is that the financial management training offered training or information that was already familiar to participants, most of whom had very high levels of prior education. Surveys in the United Kingdom (U.K.) have suggested that formal education predicts some knowledge of budgeting and other financial areas covered by the ***learn***$ave curriculum (see Atkinson et al., 2006) and Canada's own national survey on financial literacy is expected to show similar results. Indeed, as noted earlier, some more highly educated participants would like to have seen in the curriculum greater emphasis on instilling hard financial knowledge than was the case in the ***learn***$ave financial education curriculum.

Under this hypothesis, the financial management training may not be effective for highly knowledgeable participants but would be effective for less knowledgeable ones. Unfortunately, it is not known which individuals in the sample were the least financially knowledgeable as there was no baseline measurement of financial literacy. However, groups who would plausibly have less financial knowledge on average than other groups (again drawing on patterns from the U.K. survey results) can be analyzed. These groups are those who, at baseline, were young (under 30 years of age), had no prior post-secondary education qualification, had a household income of less than $10,000, were unemployed or out of the workforce, or were not regular savers (based on self-reported measures). In general, the services (including the financial management training) had no effect on any measure of savings for these potentially "less knowledgeable" groups at 18 months, 40 months or 54 months (data are not shown but are available upon request).

There were some exceptions. For example, there were approximately 10-percentage-point impacts on the incidence of "regular saving" at 54 months among ***learn***$ave-plus participants in household income brackets of $10,000 - $19,999 and $20,000 and greater (Table F6.4). However, these and other scattered exceptions should be treated cautiously because they could easily have occurred by chance. Taken as a whole, there is no substantial evidence that the financial management training as offered in ***learn***$ave meaningfully improved the saving behaviour of those subgroups that might be reasonably assumed to have less financial knowledge.

Alternatively, it might be argued that financial management training, if it is simple, would most likely have an impact on simple financial behaviours — such as reducing high-cost credit card balances. However, as the results of all surveys indicate, financial management training did not reduce credit card balances for any sub-group, nor for participants as a whole.

Other theories might explain the lack of results for ***learn***$ave services. First, this particular type of financial training course might have been effective for less knowledgeable non-participants who did not sign up for the project because they did not see its value or because they were specifically excluded (IA recipients, for example). Second, the courses may have taught useful lessons for participants but, as suggested earlier in this report, they were simply unwilling to implement those lessons in their daily lives possibly because financial difficulties were felt to be too great. Third, also as indicated earlier, research has shown that financial education may improve financial behaviour but it is not the only way, as people may learn from their own or others' experiences. (Hogarth, Beverly, & Hilgert, 2003). A hypothesis is that financial management training may have limited scope to increase savings because the basic principle of consuming less and/or increasing income is not something necessarily explored in a classroom or workshop setting. If this is the case, then experiential learning (learning by practicing with an IDA or any other savings instrument) would be more powerful — something that is borne out in the experimental impacts related to the matched saving credits. An extension of this hypothesis might be that difficulties in saving are more often related to larger barriers such as low incentives (something addressed by the introduction of matched credits) or institutional arrangements (such as automatic withdrawals) rather than a lack of knowledge.

Table 6.7 Impacts on Incidence of Hardship and Life Satisfaction Level (Percentage Points or Average), at 18, 40 and 54 Months, All Participants – Adjusted

	Control Group Incidence or Mean	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
At 18 Months				
Hardship (in the last 12 Months)				
% who had difficulty meeting expenses	33.5	-1.3	-1.6	-2.9
% who had to borrow to meet needs	25.3	-1.5	0.3	-1.2
% who used a foodbank	5.8	0.8	0.9	1.7
% who declared bankruptcy	0.9	-0.7*	0.3	-0.4
% who had overdue bills at month 40	3.8	-0.5	0.2	-0.3
% who had at least one of above items	41.3	-1.3	-0.2	-1.4
Average number of hardship items	0.7	0.0	0.0	0.0
At 40 Months				
Hardship (in the last 12 Months)				
% who had difficulty meeting expenses	23.9	2.1	-2.4	-0.3
% who had to borrow to meet needs	17.1	4.4**	-3.3*	1.1
% who used a foodbank	5.6	-2.1*	1.6	-0.6
% who declared bankruptcy	0.7	0.7	-1.1**	-0.4
% who had overdue bills at month 40	4.2	-1.3	1.7*	0.4
% who had at least one of above items	31.9	2.5	-2.8	-0.3
Average number of hardship items	0.5	0.0	0.0	0.0
At 54 Months				
Hardship (in the last 12 Months)				
% who had difficulty meeting expenses	18.4	-1.6	2.1	0.5
% who had to borrow to meet needs	14.2	0.5	-1.0	-0.5
% who used a foodbank	3.2	-0.2	0.4	0.2
% who declared bankruptcy	0.8	0.7	-0.3	0.4
% who had overdue bills at month 40	1.9	-0.1	-0.4	-0.5
% who had at least one of above items	23.8	-0.4	1.8	1.4
Average number of hardship items	0.4	0.0	0.0	0.0
Life Satisfaction (1-10 scale; 10 highest)				
% rating life satisfaction 10	3.4	3.6	1.8	5.4**
% rating life satisfaction 9 or 10	13.5	7.1*	1.1	8.2**
% rating life satisfaction 8, 9 or 10	44.5	4.2	-0.3	3.9

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively for the 54-month survey; 607, 833 and 814, for the 40-month survey; and 748, 920 and 915 for the 18-month survey.

Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Life Satisfaction question not asked in the 18- and 40-month surveys.

measure of hardship at month 54 (last row of first panel of Table 6.7).

As a final note, *learn*$ave had a positive impact on life satisfaction. Participants were asked at 54 months to summarize how satisfied they were with life using a 10-point scale.[5] Matched credits alone increased the percentage replying in top two categories by 7.1 percentage points (Table 6.7). Matched credits combined services had a slightly higher impact. This would reinforce the conclusion that participants did not undergo hardship.

In summary

The experimental results present in this chapter demonstrated that many participants in the control group were able to acquire substantial assets without special incentives, financial management training or assistance from caseworkers and that *learn*$ave did not produce any significant increase in net worth for program participants. While *learn*$ave program groups increased their financial assets at the beginning of the project, these increases disappeared at the end of the *learn*$ave saving period.

The lesson for policy-makers is that a matched savings incentive may increase financial assets but only while the credits are available and may not, depending on the targeted asset investment (education, housing, or retirement income), generate measurable increases in net worth over the medium-term.

However, *learn*$ave did have some effects on self-reported savings behaviours after participants exited the program. At 54 months, both matched credits, alone and combined with services, modestly increased the percentage of participants who reported saving at all in the past year. When the services were available alongside the matched credits, participants were more likely to report saving regularly in the past year. This effect was more short-lived for the lowest income participants but did endure for other participants. These results augur well for future saving. Indeed, *learn*$ave participants were more likely to say they intended to save regularly for education and training in the future. Whether or not those intentions will be realized is a matter of conjecture.

Some impacts on budgeting and financial goal setting were observed. In every period of the study, matched credits, alone and in combination with services, modestly increased the percentage of participants who set financial goals. Matched credits combined with services moderately increased budgeting at 54 months but the matched credits alone did not have an effect on budgeting. Budgeting and financial goal-setting have the potential, but not the guarantee, to free up financial resources that might be used for increased saving and asset accumulation during the program and, potentially, long afterwards. Similar effects were observed in the degree to which the program encouraged participants to increase their financial inclusion above and beyond holding a deposit account.

The results from Chapter 5 suggest that low-income participants will make use of an IDA when offered but that participants will show different patterns in the speed and degree to which they accumulate and use the matched credits. The results from the current chapter suggest that participants did not borrow to save and did not endure hardship to accumulate their *learn*$ave savings.

The *learn*$ave IDA program was designed mainly as a mechanism to encourage saving for the purpose of investing in adult learning — not as a mechanism to encourage saving for the sake of saving itself. The next chapter presents the results on adult learning outcomes (through formal education and small business) that are the real and best test of the program.

5 Note that this question was added late in the 54-month survey, with about one-third of participants left to respond. However, respondents to this question were no different from those who were not given this question, nor were the research groups different from each other among respondents to this question.

Chapter 7 Impacts on education, labour market outcomes, and small business start-up

The main test for the *learn*$ave project was whether participants would increase their participation in adult learning. At 18 months following enrolment, education and self-employment impacts could not yet be observed given that so many participants were still engaged in the savings portion of the project. By 40 months, significant education impacts were observed (Leckie et al., 2009) as more participants entered the third phase of the project (see Figure 7.1) and cashed out their savings to invest in eligible uses (adult education, training or micro-enterprise start-up). However, even after the 40-month mark, participants had another eight months to use their matched credits.

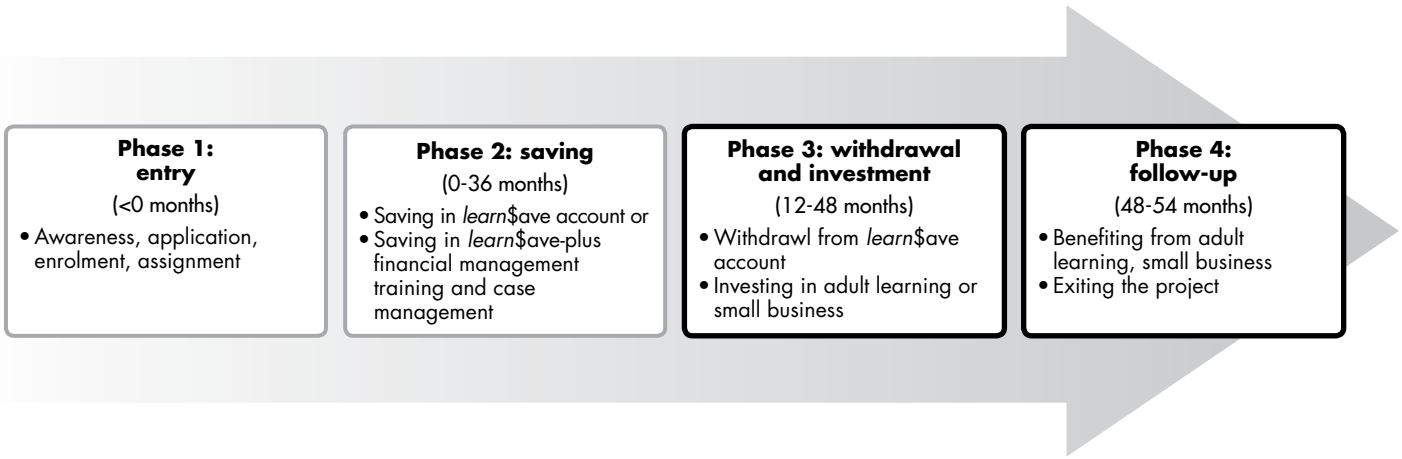
This report draws on data collected at the 54-month mark, the last data point available to evaluate the impact of *learn*$ave. This allows us to observe any further increases in education participation that occurred between the 40th and 48th months. This also allows us to evaluate the persistence of any impacts on education or on self-employment for the micro-enterprise stream for the first 6 months after participants left the project (the follow-up period illustrated in Figure 7.1). Finally, data collected at the 54-month mark may provide some indication of early returns on the education and small business start-ups through increased employment or higher earnings.

The first three sections of this chapter present evidence on *learn*$ave impacts on education and training for participants in the education stream. The first is concerned with attitudes toward education on the basis that participants may increase the value they give to education and become more oriented towards adult learning. The second section examines direct measures of education and training participation and the third looks at evidence on labour market outcomes to see whether participants saw any employment returns on the education taken during *learn*$ave. Because the project also included a limited number of accounts that could be used for micro-enterprise start-up, results for these participants are treated separately at the end of the chapter. Throughout this chapter, as in the two previous chapters, the experimental impacts are estimated as the regression-adjusted differences in outcomes of the three research groups in the experimental sites.

Impacts on attitudes toward education

Participants were expected to change their attitudes towards education through two avenues: First, the act of saving for an educational goal was expected to encourage participants to think more about their human capital needs and to increase the value they give to education and training. By saving and planning ahead for their own education, participants were expected to begin to see education as more important in their lives compared to when they first enrolled in the project. Second, the financial management training and enhanced case management services, provided to the *learn*$ave-plus group were expected to directly enhance attitudes toward education, as the focus of the services was on helping participants identify their goals and the education needed to meet them. At the same time, it is important to recall that participants were most likely to value education more than the general low-income population right from the start of the project as they had self-selected to take part in the project knowing their savings would be

Figure 7.1 Stages of *learn*$ave Program Participation



rewarded only when used for adult learning or small business start-ups.

To assess participants' attitudes toward education, survey respondents were asked on four-point scales whether they strongly disagree, disagree, agree, or strongly agree with each of four attitudinal statements on education, focused on the link between education and employment, but also including one on student debt tolerance. The first observation from the results of the survey questions on attitudes toward education, presented in Table 7.1, is that a large majority of *learn*$ave participants, whether they belonged to the control or program groups, had a positive attitude toward education. About 91 per cent of the control group agreed or strongly agreed that getting a good job depends on one's education. It is also noteworthy that a majority of the control group was tolerant of student debt: about three-quarters (13.5 + 64.4 per cent) of this group strongly disagreed or disagreed with the statement: "It is not worth going into debt to go to school."

A second observation is that *learn*$ave's impact on education attitudes at month 54 was positive but less so than at earlier survey points. Participants in the *learn*$ave-only group were statistically significantly more likely (by 5.6 percentage points) to strongly agree, and significantly less likely to disagree (by 3.8 percentage points), that getting a good job depends on one's education. Impacts were greater at 18 months (Table F.7.1 in Appendix F), and, as at 54 months, it was the matched credits that mainly drove the impacts as opposed to the services. As for the

Table 7.1 Impacts on Attitudes towards Education (Percentage Points) at Month 54, Education Stream Participants – Adjusted

	Control Group Percentage Distribution	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits and Services
Getting a good job depends on my education				
Strongly disagree	0.9	-0.1	-0.3	-0.4
Disagree	8.0	-3.8***	0.7	-3.1**
Agree	49.9	-1.8	2.6	0.8
Strongly agree	41.2	5.6*	-2.9	2.7
I need more schooling to find a good job				
Strongly disagree	1.9	0.1	0.2	0.3
Disagree	23.4	-1.5	-0.1	-1.6
Agree	46.9	0.7	5.9**	6.6**
Strongly agree	27.9	0.7	-6.0**	-5.3**
No matter how much education I get, I will most likely end up with a low-paying job				
Strongly disagree	27.7	-2.6	-2.3	-4.9*
Disagree	59.8	2.7	3.5	6.1**
Agree	9.9	0.4	-0.2	0.2
Strongly agree	2.6	-0.4	-1.1	-1.4*
It is not worth going into debt to go to school				
Strongly disagree	13.5	-0.7	-1.6	-2.2
Disagree	64.4	1.9	2.8	4.7
Agree	20.2	-2.3	-1.1	-3.4
Strongly agree	1.9	1.1	-0.1	1.0

Source: Calculations from 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and -plus groups are 568, 842 and 859, respectively for the 54-month survey.
Sample sizes vary for individual measures because of missing values.
Two-tailed t-tests were applied to differences in characteristics between the treatment and control groups.
Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.
Rounding may cause slight discrepancies in sums and differences.
Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

second and third measures in Table 7.1, the impacts are driven mainly by the services and they are ambiguous and difficult to interpret. For example, at 54 months the proportion of participants in the *learn*$ave-plus group agreeing that more education is needed to get a good job rose by 5.9 percentage points over the control group, but the proportion who strongly agreed with the same statement actually decreased. Again, unlike at 54 months, the impact of the credits on the second measure was significant at 18 and 40 months.

No impacts at 54 months were observed for the fourth attitudinal measure: tolerance or aversion towards student debt. At earlier points in the study, *learn*$ave program group participants had become more willing to take on student debt to achieve educational goals than their control group counterparts (Table F.7.1 in Appendix F) but this change did not persist to the 54-month mark. It may have been that the attitudinal changes were related to the act of saving and planning ahead for education, key activities present in the earlier phases of the project.

Impacts on education and training participation

The central goal of the *learn*$ave demonstration was to encourage participants to pursue further education. This section focuses on the extent to which this objective was attained by the end of the study period, i.e., 54 months after participants had enrolled in the project and six months after they had completed the saving and cash out phases of the project. Note that, unlike the point-in-time estimates of impacts on education attitudes presented in the preceding section, the results presented here are in the form of incidence and mean dollar estimates of education and training enrolment at any time during the full 54-month period. Participants who had saved fastest and cashed out fastest (see Chapter 5) would have had the longest stretch of time to participate in education before the 54th month. However, many participants took the full three years to save in the *learn*$ave IDA and the full 12 months afterward to cash out their savings credits so observations on education among these would be largely based on the six months following the last date to cash out their savings. The results in this section are for education stream participants only.

Few real restrictions or guidelines were placed on participants in making the choice of an educational or training program or course. Participants in the *learn*$ave-only and *learn*$ave-plus groups could pursue any form of education or training from an accredited institution recognized by the national Canada Student Loans Program. Because tuition costs are not the only direct financial costs to participants, *learn*$ave funds could also be used for so-called "supports to learning," such as books, computers, child care services and disability supports so long as the funds spent on these did not exceed 50 per cent of the total savings and credits accumulated in the IDA, to a maximum of $1,500.

The first remarkable finding in the results presented in Table 7.2 (data column 1) is that a very large majority of the control group had participated in some education or training over the 54 months. About 82 per cent of the control group engaged in education or training of some kind, with about 56 per cent enrolled in courses as part of a certificate or degree program and about 48 per cent in individual courses (outside of a program). These proportions are about two to three percentage points higher than what they were at the 40-month mark (see Table F.7.2 in Appendix F for impacts over time).

These enrolment rates are very high compared to the incidence of education and training among the working age Canadian population as a whole (see Chapter 1). However, the *learn*$ave surveys, collectively, asked participants whether or not they had taken education or training since the time of enrolment in the project, i.e., over a 54-month period, whereas the national training surveys (from which statistics on the incidence of education and training are derived) ask respondents to report on education and training activities in the last 12 months only.[1] Furthermore, as mentioned earlier, *learn*$ave participants in both the control and program groups would have already been inclined toward education and training by virtue of having volunteered for a project promoting adult learning.

The second important finding from Table 7.2 is that the *learn*$ave matched saving credits had a positive impact on education and training enrolment and that this impact grew over time.[2] At 54 months, the credits increased participation rates in adult learning by 6.6 percentage points, a stronger showing than the 4.7 percentage points increase in educational participation at the 40-month mark (Table F.7.2 in Appendix F). In contrast to experimental impacts on account activity and other financial behaviour, the enhanced case management and

1 Results from the Statistics Canada 2003 Adult Literacy and Life Skills Survey (ALL), part of the International Adult Literacy and Skills Survey, indicate that the rate of participation in adult education and training for medium/highly literate Canadians 16-65 years of age was 50 per cent in 2002. However, the *learn*$ave results are for those at all literacy levels (though the fact that *learn*$ave participants tended to have a university education suggests they would be highly literate as well) and the ALL results cover those 16–17 years of age who might be expected to have lower adult learning participation rates (whereas the *learn*$ave age criterion was 18 years and over). See Rubenson, Desjardins, and Yoon (2007) and Chapter 1 of this report.

2 Note that all education and training the program group has taken is included in the impact estimate, regardless of whether or not the education or training was sponsored under *learn*$ave.

Table 7.2 Impacts on Participation in Education and Training (Percentage Points or Average), during the 54 Months, Education Stream – Adjusted

	Control Group Incidence or Average	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
Overall (Program or Individual Course)				
Enrolled in any education/training since baseline (%)	81.5	6.6***	1.7	8.2***
Educational Programs				
Enrolled in courses toward a degree, diploma, or certificate (%)	56.0	9.1***	3.5	12.6***
Program Type (first program) (%):				
▪ English as a second language (ESL)	4.1	0.2	1.0	1.2
▪ High school	2.8	1.1	-0.9	0.3
▪ Registered apprenticeship	6.1	0.6	-1.4	-0.9
▪ Community college	30.0	3.3	1.9	5.2*
▪ University	18.4	6.7***	2.5	9.2***
Completed program (%)	39.5	5.0*	1.0	6.0**
Individual Courses, not Part of a Program				
Enrolled in other (non-program) education courses, seminars, etc. (%)	47.5	4.3	0.3	4.6
Number of courses (average)	0.9	0.2**	-0.1	0.1
Completed one or more courses (%)	43.7	1.5	-1.1	0.4
Spouses (among those with a non-student spouse at baseline)				
Enrolled in any education since baseline (%)	62.3	4.3	1.5	5.8
Enrolled in courses toward a degree, diploma, or certificate (%)	41.9	11.0**	-0.6	10.4*
Enrolled in other (non-program) education courses, seminars, etc. (%)	35.8	-2.8	5.1	2.4

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and -plus groups are 568, 842 and 859, respectively for the 54-month survey, while the sample sizes of participants with a spouse not in school at baseline are 135, 203 and 205, respectively.

Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to differences in characteristics between the program and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

financial management training services combined with the matched credits offered to the *learn*$ave-plus group led to an non-negligible increase in educational participation, of 8.2 percentage points. In U.S. IDA programs, as noted in Chapter 1, no education impacts were reported in the evaluation of the American Dream Demonstration IDA experiment, though impacts on homeownership were found (Mills et al, 2008a), whereas large education impacts (versus the comparison group) were reported in the evaluation of the Assets for Independence IDA program (Mills et al., 2008b).

More interesting than just looking at the overall rate of participation in adult learning of any kind, the *learn*$ave IDA program had a positive impact on participation in the type of education that is most likely to lead to returns in the form of greater employment earnings: certificate or degree programs at the post-secondary level. The matched credits alone and the credits and services combined had a significant impact on enrolment in programs of any type (by 9.1 and 12.6 percentage points, respectively), which again represents improvements over impacts at 40 months. Moreover, the main programs impact was on PSE participation: university (by 6.7 and 9.2 percentage points for credits alone and combined with the services, respectively) and college (by 5.2 percentage points, credits and service combined). No enrolment impacts were observed for other types of educational programs, i.e., high school equivalency, English as a Second Language (ESL), or apprenticeship.

In proportional terms, the impacts on post-secondary educational enrolment were large. Relative to the control group's enrolment rate, the impacts were 50 per cent (9.2/18.4 percentage points) and about 17 per cent (5.2/30.0 percentage points) for university and college programs, respectively. The proportional impact on participation in education programs overall was 23 per cent (12.6/56.0 percentage points) and, on education or training of any kind, it was about 10 per cent (8.2/81.5 percentage points).

The large impact at the university level is interesting for two reasons. First, the greatest labour market returns to education are derived from university degrees (Statistics Canada, 2009) and it is therefore promising that such impacts on university enrolment are taking place. Second, as observed in Chapter 3, about half the participants already had a university education when they entered the project with a substantial overlap between the participants with higher education and those who where recent immigrants to Canada. On the basis of this overlap, the impacts on participation in university level education suggests several participants might have been using *learn*$ave to upgrade their university education or to obtain a Canadian recertification for a foreign credential.

The evidence also indicates that *learn*$ave had an impact on the completion of programs. The *learn*$ave matched credits, alone and in combination with *learn*$ave services, increased the completion rate of educational programs, by 5 and 6 percentage points, respectively. This is somewhat surprising given that the data were collected just six months after participants could no longer cash out their earned credits for education or training. Since so many programs are of a longer duration than six months, this finding may be linked to early take-up of education and training by the sub-group of participants identified earlier as "early savers, high investors."

Contrary to expectations, *learn*$ave did not affect participation in individual shorter term courses. The underlying hypothesis of *learn*$ave did not differentiate between programs and courses, instead suggesting that *learn*$ave would increase participation in both forms of adult learning. However, one might have expected greater effects for individual courses which cost less. In fact as shown, statistically significant impacts were observed for just programs. However, while *learn*$ave did not have an impact on the proportion taking courses, it did have an influence on the number of courses taken: the average number of courses was 0.2 higher for the *learn*$ave-only group than the control group (a difference that is significant at the 5 per cent level), indicating that the matched credits had some impact on that indicator, albeit quite small.

If participants were unable or unwilling to use some or all the funds accumulated in their *learn*$ave IDA, they had the option of transferring the right to use them to another adult member of their immediate family who was not a full-time student and who would otherwise have been eligible for the project themselves at baseline. This is a feature of the program design not dissimilar to transfer rules under existing policy instruments such as Registered Retirement Savings Plans. To measure what this feature had on the results, additional analysis focused on spouses, as they were the ones who typically met the age and education requirements (18 years or older and not going to school) at baseline.

The results of this analysis indicate that participants' spouses reported higher enrolment rates in education programs than control group participants' spouses — by 11 percentage points for the *learn*$ave-only group and 10.4 percentage points for the *learn*$ave-plus group (see the bottom panel of Table 7.2). When this extra education taken by spouses is included, the total impact of matched credits on participation in education programs (not shown in table) rises by 1.4 percentage points over the increase in participation among *learn*$ave accountholders themselves for the *learn*$ave-only group; however, the impact does not change for the *learn*$ave-plus group.

Education participation program impacts by subgroup

Analysis was conducted to determine if impacts pertaining to participation in education programs varied appreciably for certain subgroups. Table 7.3 shows impact results by selected subgroups as defined by the participants' baseline characteristics associated with participation in education and training: enrolment age, labour force status, educational attainment, household income in the year prior to entry in the project and immigration status. Also shown are results for a variable relevant in the present context: self-reported saving regularity. The degree to which education program enrolment impacts were statistically significant for particular subgroups is indicated by asterisks. The degree to which the impacts varied between subgroups (say between age groups) is indicated by daggers. More detailed results are presented in Appendix F, Table F.7.3.

The analysis reveals that the impacts were widespread and particularly large for certain subgroups.

- *Labour force status at baseline: learn*$ave-plus participants who worked for pay or were self-employed significantly increased their participation in education by

18.2 and 23.4 percentage points, respectively. Participants who were unemployed at baseline did not record any change in program enrolment during the *learn*$ave project. These participants may not have been able to find the resources to enter an education or training program despite their desire to do so.

- *Household income in the year prior to baseline:* Income levels did not make a difference in whether participants enrolled in education as a result of *learn*$ave (as shown by the lack of daggers in Table 7.3 for this variable). Though participants in the two lowest income categories who had received a *learn*$ave account were modestly more likely to enter into an educational program, the differences in the impacts at the three income levels were not found to be statistically significant.

Table 7.3 Impacts on Education Program Enrolment (Percentage Points), during the 54 Months, by Selected Characteristics at Baseline (%), Education Stream, Participants – Adjusted

Characteristic at Baseline	Sample Size	Control Group Per cent	Impact of Matched Saving Credits	Impact of Services when Offered with Credits Σ	Combined Impact of Credits + Services
All	1,844	56.0	9.1***	3.5	12.6***
Age					
Less than 30 years	665	64.8	10.0**	3.0	13.0***
Between 30 and 40 years	885	52.9	8.5**	3.2	11.7***
Over 40 years	294	45.6	8.9	5.6	14.5**
Labour Force Status			†		†††
Work for pay	1,039	52.1	13.8***	4.4	18.2***
Self-employed	176	44.4	13.3	10.2	23.4**
Jobless: Unemployed/ Out of labour force[a]	628	65.8	0.2	-0.2	0.0
Household Income[b] (in year prior to application)					
Less than $10,000	587	58.4	5.8	6.8	12.6**
$10,000 to $19,999	750	53.4	13.2***	2.1	15.2***
$20,000 and over	507	57.2	6.9	1.7	8.6
Highest Level of Education					
Some PSE or HS certificate or less[c]	450	54.6	10.1*	2.2	12.2**
College (or equivalent) diploma, certificate or degree	360	56.3	1.9	4.8	6.6
University degree	1,034	56.1	11.9***	3.3	15.2***
Years Since Immigrating			††		
Born in Canada	586	45.3	18.3***	1.1	19.4***
Immigrated < 4 years ago	950	62.4	7.4*	3.3	10.7***
Immigrated 4 + years ago	308	58.4	-4.3	8.6	4.3
Saving Regularity					
Saved regularly	279	51.5	13.7*	3.2	16.9**
Did not save regularly	1,555	58.0	7.0**	3.7	10.8***

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: Significance levels: impacts of a specific subgroup category (based on two-tailed t-tests): * = 10%, ** = 5%, *** = 1%; differences between impacts of categories of a subgroup (based on q-tests): † = 10%, †† = 5%, ††† = 1%.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

[a] Includes student, at home, retired, looking for work, and unemployed.

[b] Baseline annual income is household income in the calendar year prior to application. For those who immigrated to Canada in the year prior to application, annual income is based on a formula

[c] May have some post secondary, but did not receive a degree, diploma or certificate.

- *Education attainment level at baseline:* Prior education did not make much of a difference in *learn*$ave impacts on education program enrolment. The education program enrolment rate of control group members was similar across all levels of baseline educational attainment (55–56 per cent), and there was no statistical difference in the impacts of *learn*$ave at different education levels. Still, it is notable that program group (both *learn*$ave only and *learn*$ave-plus) participants at the lowest level of education (those with no PSE certification) demonstrated an important increase (10.1 and 12.2 percent percentage points) in education program participation compared to the control group. Not only would those participants have the most to gain from further education, but, as discussed in Chapter 1, prior education has been shown to be a strong predictor of future participation in education and *learn*$ave may have had some impact in changing this trend.

- *Immigration status at baseline:* Immigration status was a factor in the education program impacts (as indicated by daggers in Table 7.3). Those who had immigrated to Canada four or more years prior to entering the project were no more likely to enrol in education as a result of *learn*$ave than those in the control group. Among new immigrants, who represent about half the participants in the experimental sample, participation in education did rise (10.7 percent) as a result of *learn*$ave. However, compared to similar participants in the control group, Canadian-born participants in the project dramatically increased their participation in education when they received the *learn*$ave-plus or *learn*$ave-only program, by 18.3 percentage points and 19.4 percentage points, respectively.

  It is worth noting that recent immigrants had the highest rate of education participation in the control group (62.4 percent compared to 56 percent for the control group as a whole). Given the overlap between new immigrants and university degree holders in the sample, a new bivariate subgroup variable was created: education interacted with immigrant status. It was hypothesized earlier that that perhaps a large number of highly educated immigrants were using *learn*$ave to accredit prior education obtained abroad. However, while the impacts for immigrant degree-holders were significant, they were just barely so, and the impacts were not as large as they were for Canadian-born participants, with a BA or not (not shown in table). This suggests that immigrants were not necessarily using *learn*$ave to "Canadianize" their foreign PSE credentials.

- *Saving regularly at baseline:* As regular savers are expected to have a strong orientation toward the future, they should be more inclined to treat education as an investment than non regular savers. However, how regularly one saved at baseline did not affect education enrolment in *learn*$ave. While, regular savers were more likely to benefit from *learn*$ave (16.9 versus 10.8 percentage points for non-regular savers), this difference was not significant in statistical terms (as indicated by the lack of daggers in the table).[3] Nonetheless, these results do show that non-savers can be induced to save and enrol in education or training programs.

Impacts on education and training spending and intensity

The research was concerned not only with measuring impacts on education enrolment but also on how much participants spent on education, how they funded it, and how much time they invested in it. Estimates of each of these impacts are presented in Table 7.4 for all education stream participants. Note that these are education expenditures funded from all sources, including participants' own funds.

Both *learn*$ave matched credits, alone and combined with the services, resulted in participants spending more on education and training, particularly in programs rather than courses. Over the 54-month period, the *learn*$ave credits and services together led *learn*$ave-plus participants to spend, on average, $2,142 more on education programs leading to PSE certification compared to the control group. This total included $1,968 in tuition and $173 in books. The impact was derived not only from the matched credit incentives alone, as the addition of the services contributed an additional $918 to the overall $2,142 increase in individual spending on education. Though the *learn*$ave financial management training and enhanced case management services did not have much incremental impact with regard to education enrolment on top of the effect of the credits, the positive impacts on educational expenditures imply that the services may have encouraged members of the *learn*$ave-plus group to seek out education with greater earnings potential, which tends to be more costly.

As noted, these are educational expenditures funded by all sources, including the matched saving credits. An argument could be made for not including this grant to which the control group did not have access. Indeed, subtracting the average matched saving credits used by the *learn*$ave-only and *learn*$ave-plus groups ($2,342 and $2,529, respectively; Chapter 5) would eliminate the impact on educational expenditures.

3 Despite their statistical significance, the estimated impacts should be interpreted with some caution based as they are on a small non-random sample of those who were regular savers at baseline (n=279).

As for funding sources for the education and training costs, the only impact to emerge was a positive impact of the services on education loans for courses ($94). The additional services might have encouraged participants to borrow for their courses, but, other than that, *learn*$ave did not induce participants much to pursue alternative sources of education funding in the form of student debt or grants.

The results also indicate that *learn*$ave (credits and services combined) increased the average total time spent by participants in education programs by 159 hours and in courses by 52 hours. The 159 hours translates into about 10 weeks of a university semester or about 6 weeks of a college semester including in-class labs. Once again, the impact was driven mainly by the matched credits (130 and 61 hours for programs and courses, respectively). Note that there is real potential to record greater intensity impacts in the future, as only about 45 per cent of program group participants had completed their programs or courses at the time of the survey (recall Table 7.2).

Table 7.4 Impacts on Education, Funding and Training Costs and Intensity (Average), during the 54 Months, Education Stream Participants – Adjusted

	Control Group Mean	Impact of Matched Saving Credits	Impact of Services when Offered with Credits[Σ]	Combined Impact of Credits + Services
Educational Expenditures (average $)				
Tuition Fees				
Programs	3,038	1,191***	778**	1,968***
Courses	781	270**	-10	260**
Programs and courses	3,821	1,426***	795**	2,222***
Book Purchases				
Programs	604	33	141**	173***
Courses	57	81***	-10	71***
Programs and courses	661	113	131**	244***
Total Educational Expenditures (Tuition and Books)				
Programs	3,642	1,223***	918**	2,142***
Courses	838	351***	-21	330**
Programs and courses	4,482	1,539***	926**	2,465***
Education Funding (average $)				
Grants for Programs and Courses	1,248	-46	-4	-50
Education Loans				
Programs	3,905	-142	417	275
Courses	36	-16	94*	78
Programs and courses	3,941	-158	511	353
Educational Intensity (average hours)				
Programs	606	130**	29	159***
Courses	148	61**	-9	52*
Programs and courses	755	192***	20	211***

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

[Σ] The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Labour market outcomes

Over the longer-term, *learn*$ave was expected to lead to increases in education and formal skills that could generate a return through higher earnings in the labour market. It was posited that higher savings would enable greater participation in education which would eventually lead to a better job and better earnings. However, few if any labour market impacts could be expected to be observed within the 54-month period. As already noted, only 45 per cent had completed a course or program between the time they enrolled in the project and the 54-month survey and, even for those, it is unlikely there would be sufficient time for them to change jobs and/or realize any wage gains. In fact there was a real possibility of seeing employment decline overall as participants may have had to reduce their work time to go to school.

The results confirm that, up to month 54, *learn*$ave had not had an impact on the current labour force status of participants in the education stream (Table 7.5). None of the differences between the research groups are statistically significant. About 83 per cent of participants in all three groups were employed or self-employed at the time of the 54-month survey interview, earned about $2,800 per month and worked about 38 hours a week. The employment and earnings gains that might eventually be realized following increases in education participation cannot be observed in the project period.

Finally, results from a new question added to the 54-month survey suggest that *learn*$ave positively affected the proportion of people who felt positive about the future labour market impacts of their investment in education. The matched credits, alone and in combination with the additional services, had a positive impact on the proportion of people who strongly agree with the statement that "the education or training they took since their first *learn*$ave interview helped to improve their career opportunities," by 16.1 and 14.1 percentage points, respectively (not shown). To the degree that respondents are able to accurately assess their own future employment prospects, *learn*$ave may have increased the likelihood that participants will enjoy a better job and higher earnings in the future.

Micro-enterprise stream results

Outside of the education stream, roughly 1 in 5 participants belonged to the micro-enterprise stream and planned to use their savings and credits toward the start-up of a new micro-enterprise.[4] In addition to showing

Table 7.5 Impacts on Labour Force Outcomes (Percentage Points or Average), at 54 Months, Education Stream Participants – Adjusted

	Control Group Mean or Incidence	Impact of Matched Saving Credits	Impact of Services when Offered with Services Σ	Combined Impact of Credits + Services
Current Labour Force Status (%)				
Working for pay	77.1	-0.9	0.9	0.0
Self-employed	6.5	-1.2	1.0	-0.2
Unemployed	9.8	0.6	-2.0	-1.4
Not in the labour force	6.6	1.6	0.1	1.6
Working for pay or Self-employed	83.6	-2.2	1.9	-0.3
Not working	16.4	2.2	-1.9	0.3
Earnings and Hours of work (in the last four weeks)				
Total earnings ($)	2,798	-36	-52	-89
Average weekly hours worked	38	-1	1	0

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

4 Note that these estimates are less accurate since the sample size for this stream is fairly small (about 20 per cent of survey respondents). The results should be treated with some caution.

impacts relating to small business start-ups, findings presented below summarize the observed impacts on a number of outcomes shown earlier for the education stream as micro-enterprise stream participants had the choice of using their matched credits for education and training as well as business start-up.

Over the 54-month period, *learn*$ave had the expected impact of increasing the incidence of self-employment for participants in the micro-enterprise stream (Table 7.6). The matched saving credits significantly increased (by 24.5 percentage points) the chances that a program group member be self-employed during the 54 months, a considerable gain over the control group's 42.1 per cent rate of self-employment. Interestingly, *learn*$ave services contributed negatively to this impact. When added to the credits, the services decreased the chances of having any self-employment jobs by 9.9 percentage points, reducing the combined impact to 14.6 percentage points. The services also contributed negatively to the chances of incorporation. Micro enterprise savers were required to complete a business plan that had been approved by an external, qualified local business development organization. The negative result for services is not surprising , therefore, as this additional review would have encouraged participants to think twice about starting small business. Control group members did not have this opportunity.

Other findings in Table 7.6 show that the *learn*$ave matched credits had a significant impact on total self-employment income increasing it by almost $4,000 or 68 per cent above the control group mean of $4,506. The credits also led participants to work an additional 7.2 hours per week in self-employment, a 100 per cent increase over the control group mean. Once again, the services appeared to be contributing negatively to the impacts. On average, the services reduced self-employment income by about $2,700, the length of time on a self-employment job by 13 months, and the self-employment work week by 4.6 hours.

Many self-employed persons typically combine or alternate their self-employment with paid standard employment — working for an employer while pursuing their own business. Indeed, the credits reduced the rate at which *learn*$ave participants in the micro-enterprise stream worked for an employer by 18.5 percentage points (not shown), which could be consistent with an increase in the intensity of the self-employment activity related to *learn*$ave. However, as noted, when the services were added to the credits, they had a negative impact on self-employment incidence.

In the previous chapter, results for assets and debts revealed some positive impacts on the value of certain assets and debts from *learn*$ave for the entire participant pool. Table 7.7 presents results on the types of assets and debts pertinent specifically to the micro-enterprise stream: business assets and debts.[5] The expectation was that *learn*$ave would lead to greater business assets for this stream.

Table 7.7 indicates that the matched credits had indeed a significantly positive impact on average business assets of $3,300 and on net business assets (i.e., net of business debts) of $5,694. Relative to the control group's average assets and net assets of $1,819 and $715, respectively, these are large impacts. The *learn*$ave services did not play role as they did not have a statistically significant impact on these outcomes (Impacts of Services column), but the negative signs suggest they may have acted to reduce the business asset and debt effects of the matched credit incentive, in similar fashion to their effect on self-employment income and intensity observed above.

Turning to education impacts for micro-enterprise stream participants, *learn*$ave had a positive impact on one measure of educational attitudes (out of the four questions asked on the subject). The matched credits decreased the proportion of participants at 54 months who disagreed with the statement about getting a good job depends on one's education by 9.1 percentage impacts, over the control group proportion of 14.3 per cent. The impacts on educational attitudes had been stronger at 40 months, as Leckie et al., 2009 indicated. Note as well that, not surprisingly, a somewhat smaller proportion of participants in this stream had a positive attitude towards education compared to education stream participants.

Finally, no significant impacts of *learn*$ave on participation in education or training were observed for participants in the micro-enterprise stream. None of the differences in enrolment between research groups was statistically significant. However, in all research groups, the education/training participation rate was, as expected, considerably lower for the micro-enterprise stream (who, as noted, could use their credits for education but whose primary motive was to start a business) than for the education stream (who could use their credits for education only). For example, about 39 per cent of the control group members in the micro-enterprise stream had participated in an education or training program since baseline, compared to about 56 per cent of control group members in the education stream.

5 One of the measures shown is "goodwill," which was computed as the difference between (1) the current equity of the business (excluding debts) and (2) the book value of the business assets at purchase.

Table 7.6 Impacts on Self-employment (Percentage Points or Average) over the 54 Months, Micro-enterprise Stream Participants – Adjusted

	Control Group Mean or Incidence	Impact of Matched Saving Credits	Impact of Services when Offered with Services Σ	Combined Impact of Credits + Services
Overall Self-employment Incidence (%)				
0 self-employment jobs	58.2	-22.8***	10.3*	-12.6**
1 self-employment job	29.9	18.8***	-7.8	11.1*
2 or more self-employment jobs	12.0	4.0	-2.5	1.5
Having any self-employment jobs since baseline	42.1	24.5***	-9.9*	14.6**
Formally incorporated (%)	19.2	19.2***	-6.5	12.7**
Income and Intensity of Self-employment Jobs				
Total income from self-employment jobs ($)	4,506	3,912**	-2,716*	1,195
Length of time on self-employment jobs (months)	25.0	6.0	-13.0***	-7.0
Hours per week on self-employment jobs (hours)	7.2	7.2***	-4.6**	2.7
Paid Staff in Self-employment Jobs				
Having paid staff (%)	8.4	1.3	-0.9	0.4
Total payroll ($)	1,906	1,549	-2,391	-842

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 117, 150 and 157, respectively, for the 54-month survey. However, 18 cases were mistakenly skipped over the questions on self-employment, comprising 8 in the *learn*$ave group, 8 in the *learn*$ave-plus group and 2 in the control group. These 18 cases were excluded from this table. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

In summary

The results indicate that a major objective of the *learn*$ave IDA — to encourage low-income people to participate in education and training — has been met. It would appear that the increased bank/*learn*$ave account balances and enhanced education attitudes observed in earlier reports have led to increased enrolment in education and training. Most of the impact was derived from the matched credits, as the services did not play much of an additional role in education impacts.

This increased enrolment has taken place mostly in university and college programs, not courses. This is promising, as completion of PSE programs is more likely to pay off in terms of improved labour market outcomes down the road, than the completion of individual courses at lower education levels.

The impacts on education program enrolment were widespread across socio-demographic subgroups but reveal some interesting differences. First, particularly Canadian-born participants realized large gains as a result of *learn*$ave, enabling the former to close the educational attainment gap with the immigrant population. Second, large gains in participation were recorded among those with no post-secondary education certification, which is good news as this group is the most in need of skills upgrading and may end up benefitting the most. Large gains were also experienced by those with a university degree at baseline, suggesting *learn*$ave was being used to upgrade prior education. Third, whether or not one was a regular saver or future oriented at baseline did not make much of difference to education enrolment impacts, but the fact that non-regular savers realized gains suggests they can be induced to save for their education. Fourth, those who were working at baseline benefited from the matched credits much more than those who were not working, suggesting this may be a program better suited to those who have a market source of income. Finally, the matched credits positively affected participants' education enrolment at the two lowest income levels, suggesting that even those at the bottom of the income distribution can be induced to participate in adult education.

Table 7.7 Impacts on Average Business Assets and Liabilities (Average Dollars), at 54 Months, MicroEnterprise Stream Participants – Adjusted

	Control Group Mean			Impacts at 54 Months		
	At 18 Months	At 40 Months	At 54 Months	Matched Saving Credits	Services when Offered with Services[Σ]	Combined Credits + Services
Business assets (book value)	1,226	2,585	1,819	3,300*	-2,127	1,172
Goodwill[1]	147	-762	1,458	2,115	-1,899	217
Total Business Assets	**1,373**	**1,823**	**3,277**	**5,415**	**-4,026**	**1,389**
Total Business Debts and Liabilities	**1,087**	**734**	**2,562**	**-278**	**-1,056**	**-1,334**
Total Net Business Assets	**287**	**1,089**	**715**	**5,694***	**-2,971**	**2,723**

Source: Calculations from 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 117, 150, 157, respectively, for the 54-month survey.

Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

[1] Computed as the difference between: (1) the current equity of the business (excluding liabilities) and (2) the book value of the business assets when purchased.

[Σ] The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

No employment impacts were observed for the education stream at this time. There likely had been insufficient time for large numbers of participants to have completed the education and training and translated their increased human capital into improved labour market outcomes by the time of the last survey. Indeed, most employment impacts, should they materialize, will be experienced outside the timeframe of this project.

Finally, the micro-enterprise stream experienced large impacts in terms of self-employment incidence, all driven by the matched credits. There was also evidence of gains in business assets as a result of *learn*$ave, as well as income from and duration of the self-employment jobs. However, the added services have contributed negatively to the incidence and intensity impacts of the credits, as the external review of participants' business plans may have encouraged them to think twice about their business start-up.

Chapter 8 Cost-effectiveness analysis

Thus far the demonstration has been evaluated to learn about its implementation (Chapter 4) and to learn about its impacts (Chapters 5 through 7). A comprehensive evaluation of the project needs to include an analysis of whether or not the delivery of IDAs as they were delivered under *learn*$ave represents a good and efficient use of public funds. This chapter presents an analysis of the cost-effectiveness of the project. Cost-effectiveness analysis provides information about the cost of an intervention relative to other means of achieving the same desirable policy or program outcomes. Ideally, thorough cost–benefit analysis would have been conducted to assess the net benefits provided by *learn*$ave, but this was not possible (see Box 8.1). Specifically, this chapter explores the following questions:

- What were the costs of the various components of *learn*$ave?
- How does the cost-efficiency of *learn*$ave compare to other social programs? Given that *learn*$ave's main objective is to induce low-income people to participate in education or small business as a way of increasing their economic welfare, was *learn*$ave more or less efficient than other initiatives in encouraging skill development?
- Is *learn*$ave cost-effective in encouraging education? Is *learn*$ave cost-effective in encouraging self employment?
- Would it be possible to improve the cost-effectiveness of *learn*$ave by targeting a certain group of participants?

The methodology and framework applied to this cost-effectiveness analysis are described below.

Methodology

This chapter presents *learn*$ave cost figures over the entire 54-month period based on data from the three experimental sites (Vancouver, Toronto and Halifax). At the other seven sites, there were insufficient data to break costs down into their detailed components. All program and control group members — not just those who actively participated in the program — were included in the calculations. Because *learn*$ave was a new project, certain costs were incurred during the development and start-up of the project. These developmental and start-up costs are excluded from the analysis to allow a better comparison with ongoing programs. Generally unobserved costs were also excluded. These include, for example, the opportunity costs to participants of taking time out of work to attend *learn*$ave financial training classes or the costs of forgone consumption to save more in a *learn*$ave account.

Box 8.1 Cost-effectiveness vs. cost-benefit analysis

While a cost-effectiveness study can tell us whether the program was relatively expensive or not compared to other available policy alternatives or programs, only a cost-benefit analysis can really tell us whether the total costs of ***learn***$ave were "worth it" given all the benefits it generated. A cost-benefit analysis has to include a full accounting for all costs to all players — participants, the host organizations, government, participating financial institutions and others — as well as all the benefits generated by the project to those same players. If the total costs to all are less than the total benefits to all, then the project is seen to be a good use of funds. Combined with a cost-effectiveness study, a cost-benefit study can tell whether a particular policy is a good use of resources in light of the benefits generated and relative to all other alternative uses for the same resources.

While many of the costs and benefits in ***learn***$ave can be observed or imputed, the available data do not allow us to calculate the total project benefits. Specifically, any increases in earnings due to ***learn***$ave's impact on education cannot be calculated using the available information. A robust projection of any returns in earnings would require not only information on the participants themselves, but also on local labour markets, the exact course taken or qualifications gained as a result of ***learn***$ave and average earnings for other similar members of the labour force with comparable qualifications. Even this approach is potentially complicated by the fact that many participants already had post-secondary education when they enrolled in the project and so their low earnings would have to be explained by other factors. An alternative approach would be to take advantage of rates of return to education derived in past research. However, in ***learn***$ave's case, where most participants entered post-secondary education of some kind, cost-benefit analysis would have been particularly difficult to conduct as estimates of returns to education in the literature are typically expressed relative to a high school certificate.

For these reasons, a cost-benefit analysis was not conducted for the ***learn***$ave project and a cost-effectiveness analysis was carried out instead. Table G in Appendix G presents the differences between a cost-benefit analysis and cost-effectiveness analysis in tabular form, along with an indication of the largest expected costs and benefits as well as those costs that can and were estimated in this study. It is important to note that a cost-effectiveness study does provide at least the benchmark on costs which benefits must either meet or exceed to have a positive cost-benefit outcome.

The time period for the cost estimates begins at the month of project enrolment and ends on the date of the final participant follow-up survey interview at the 54th month. This period includes all 4 phases of the project: 1) participant recruitment, enrolment and orientation; 2) the three-year saving period during which matched credits were earned, 3) the cash-out period beginning as early as the 12th month in the project and extending up to month 48; and 4) up to 6 months of post-*learn*$ave activity including participation in education, employment and self-employment. All cost figures used in this chapter are expressed in constant 2002 dollars, using a five per cent annual social discount rate. Even though the federal Treasury Board Secretariat (1998) suggests that a higher social discount rate be used in cost-effectiveness analyses, current capital cost conditions would suggest that a five per cent return on investment would be an appropriate assumption. Alternative estimates using various annual discount rates up to 12.5 per cent are shown in Table H in Appendix H.

Administration and operating costs of *learn*$ave at the experimental sites were measured using accounting records and program administrative data obtained from the SEDI and local site offices. To further break the cost down into detailed activities, data at three points in the project were obtained from staff time-use studies conducted at the three experimental sites. SEDI also provided information on approximate cost breakdowns according to activity. The estimates of the effects of the *learn*$ave project on savings and education enrolments are identical to those presented in Chapters 5 through 7 of this report, apart from the fact that education impacts for this chapter were computed for the education and micro-enterprise streams combined to simplify calculations, whereas in Chapter 7, impacts were presented for just the education stream. These estimates were based on data collected from the participant baseline and three follow-up surveys. The costs of time provided by the financial institution partners to set up and administer the *learn*$ave accounts and share information were imputed.

In arriving at estimates of actual cost-effectiveness of producing *learn*$ave impacts, the approach employed was to first generate estimates for intermediate cost measures relating to inputs and outputs: the cost-economy of program activities and the cost-efficiency of program outputs. Then, to scale or benchmark the results, *learn*$ave's costs were compared with those of programs directed at similarly disadvantaged target populations, to the extent the date exist.

It should be noted that the cost-effectiveness analysis of *learn*$ave differs from prior such analyses of IDAs (see for example Schreiner, 2005) for three reasons. First, *learn*$ave participants were rewarded only for saving for adult learning or small business development, whereas other IDA programs used matched savings for a wider range of goals such as home purchase or repair and retirement savings. In *learn*$ave, the main outcomes used to evaluate cost-effectiveness are enrolment in education acquired and self employment whereas other IDA programs have examined the value of the real property or financial assets acquired through the program. That said, the evaluation of *learn*$ave is consistent with standard cost-effectiveness research on other publicly-funded programs. Second, a subgroup analysis was conducted to help to better target future IDA programs. Other studies typically do not this.

Thirdly and most importantly, this cost-effectiveness analysis takes windfall gains into consideration, unlike many such analyses. Although the cost of education and

Box 8.2 A note on terminology

Throughout this chapter we refer to certain concepts that, while widely used in cost-effectiveness analysis, are prone to being misunderstood. For that reason, we provide readers with the following definitions of terms used in this study:

Cost-economy: the average cost needed to complete a unit of program activity or input. For example, the average cost to run a program per hour. Usually for an existing program, it is used to identify the cost areas for improvement.

Cost-efficiency: the average cost to produce a unit of direct program output. For example, the average cost to run a program per active participant or for each dollar saved by a participant. A common element of IDA projects is the accumulation of savings. Therefore, the cost per dollar saved in the ***learn***$ave account is a unit by which its cost-efficiency can be compared to other IDA projects

Cost-effectiveness: the average cost to produce a unit of program outcome or impact. For example, the average cost to run a program per participant who enrolled in education or who started a small business. Cost-effectiveness measures discount outcomes that would have happened in the absence of the program (i.e., those of the control group). The main outcomes of ***learn***$ave are the incremental effects (impacts) on education enrolment and self-employment. The first unit of cost-effectiveness measured is the cost of increasing education enrolment by one participant. The cost-effectiveness of ***learn***$ave in increasing education enrolment can be calculated by type or level of education (e.g., college and university, courses and programs). A second unit of measure is the cost-effectiveness per self-employment job created.

training is not trivial, it is typically lower than the cost of acquiring a physical asset like a home. Many *learn*$ave participants would likely have acquired the education regardless of *learn*$ave and these participants are seen to have enjoyed a windfall gain by using the *learn*$ave incentive to subsidize an expenditure they would otherwise have been willing to bear alone. To discount these windfall effects, therefore, cost-effectiveness calculations were based on incremental impacts as measured by differences in outcomes between the program and control group participants. Compared to other studies of IDAs, this study is the first true measure of IDA cost-effectiveness because it considers the incremental outcomes. Other studies of IDAs, such as Schreiner (2005), are more properly studies of the cost-efficiency of IDAs because they have not taken into consideration the performance of the control group and therefore have not discounted windfall gains.

Three main types of delivery costs were considered in this analysis: the administrative costs of program activities, the cost of the earned matched credits cashed out by participants, and general operation costs. Since the intensity of different program activities varied over the life the project and staff time was allocated according to need (e.g., more staff would be needed at the start for recruitment and intake and later when credits were being cashed out), the cost-economy of an activity at a particular point in the *learn*$ave project may not reflect the true cost-economy of running an ongoing program for which recruitment and cash-out, for example, would be needed over the lifespan of the program. Separate cost analyses[1] were conducted for both the experimental site offices and also for SEDI to estimate the contribution of the different types of project partners to the overall results of the project. SEDI was responsible for the overall coordination of delivery across the community organizations and financial institutions at the sites, while the community organizations were responsible for delivery at each of the sites. A working hypothesis was that these two separate types of activity may have very different cost profiles that would be concealed in an overall estimate for the project alone.

At the three experimental sites, SRDC conducted a time/cost study at three points in the life of the project (August 2003, June 2004, and November 2007). Data was collected on staff time spent in the previous month on various types of project activities. The information, alongside information on site labour costs, was used to measure the proportion of human resources used in each program delivery activity and to disaggregate gross labour costs according to activity. The unit cost of each activity was then calculated using the time/cost study data as well as records from the Participant Management Information System. Based on the timing and scale of operation, the figures from the site that would be most representative of the unit costs of program activities in an ongoing program were chosen.[2]

SEDI provided similar information on its human resources allocation and the gross operation costs of program activities in each fiscal year. Activity intensities of all sites calculated from records from the PMIS were used to estimate the unit costs of activities in each fiscal year.[3]

Program delivery: Cost-economy and cost-efficiency

As preliminary steps in estimating cost-effectiveness of *learn*$ave outcomes, estimates of cost-economy and cost-efficiency in regard to *learn*$ave delivery were generated and these are presented first.

Note that cost results are presented as present value costs. A present value cost takes into account the fact that a dollar in the future is worth proportionally less than a dollar today because of the time value of money. The average cost per program group member of each item in the tables is the product of (1) the unit cost and (2) the average number of units of activity provided per program group member over the project period. These average costs are further multiplied by a discount factor to obtain the present value per group member cost as of the start of the program.[4]

Note as well that individual cost items are presented by a common unit, program group member, for comparison purposes. However, this is not how the costs were originally incurred nor how they would be in an IDA program in the future. Therefore, for future program planning purposes, costs are presented in Table I in Appendix I according to their "natural" unit: if the cost is more or less fixed regardless of activity intensity, then it is presented per program group member; if the cost

1 The details are in two internal research notes: Hui (2008a) and Hui (2008b).

2 There were also variations in how the program was delivered at different sites. For example, financial management training was delivered mainly in large classes at Vancouver and this contributed to a more cost-economical/efficient operation, compared to other mixed delivery approaches. Since it is assumed that future implementation will adopt the best practice, the unit cost of financial management training was estimated based on Vancouver's figures.

3 The financial cost incurred and the human resources devoted to each activity by SEDI were only aggregate figures across all program sites, including the non-experimental sites.

4 Each discount factor is based o a social discount rate (5 per cent per annum) and the timing of the activities. For example, as enrolment happened only at the beginning of the project, its discount factor is 1. Matched withdrawal orientation happened later in the project when the participants prepared to use their earned matched credits, and the discount factor is thus lower at 0.9. The even lower discount factor of 0.85 for account closure reflects that closures took place at the end of the program.

is sensitive to activity level, then it is presented by their appropriate unit of activity.

Cost-economy of program activities

The estimates suggest that recruitment, withdrawal processing, and case management were the highest cost activities (Table 8.1). The initial recruitment difficulties as documented in Kingwell et al. (2005) and in Chapter 4 of this report are reflected in the high cost of promoting the project and responding to inquiries ($135 per participant) at the site offices. Promotion costs to pay for staff time and promotional materials may have been somewhat lower if *learn*$ave had been a well-established government program with a strong track record but we have no way of determining these kinds of cost-savings.[5] Even in an ongoing program, however, some costs would still be incurred for outreach to low-income people who could make use of such an IDA program. Case management services were also a high-cost item, not surprisingly being higher for *learn*$ave-plus participants (about $160) who received an enhanced version of these services, than *learn*$ave-only participants (about $115). Note as well that, because the enhanced case management services were delivered on a one-on-one basis, they were much higher than the other service delivered to *learn*$ave-plus participants, financial management training (about $65), which was delivered in group settings.

However, the most costly project activity was the processing of the matched savings credits withdrawals (almost $300 per program group member). Chapter 4 has already discussed the sometimes cumbersome and labour-intensive process used in *learn*$ave in the absence of a more automated or electronic alternative. Both of the highest-cost activities, outside general operating costs, involved labour-intensive verification of documents to comply with project rules and to prevent fraud. Such verification was needed in recruitment, project enrolment and in processing matched withdrawals. This would suggest that streamlining the application and matched withdrawals processing or linking these processes to existing administrative systems (e.g., tax records, EI records) would improve the cost-economy of these *learn*$ave activities.

Note that an effort was made to cost the services provided by staff at the financial institutions holding the *learn*$ave accounts (see line "Bank administration" in Table 8.1). Participants in *learn*$ave were required to open a special account into which they deposited their savings for purposes of earning matched credits.[6] Other than slightly different account-opening procedures and direct monthly sharing of financial records with SEDI, *learn*$ave accounts functioned in much the same way as any deposit account from the perspective of the financial service providers. For the purposes of this study it was assumed that there was no extra cost incurred by the financial institutions for daily operations besides the cost for *learn*$ave account-opening and setting up of record-sharing with SEDI.[7] Because no data were collected on the direct costs to the institution of providing these services under *learn*$ave, the costs had to be imputed by assuming that the institutions devoted, on average, one person-hour of customer service labour to each participant. The market replacement value of such work was about $12.98,[8] the figure used in the total program delivery cost calculation.

Overall, the total program activity costs were very similar between the *learn*$ave-only and *learn*$ave-plus program groups with a difference of just about $125 per participant between them. The *learn*$ave-plus group was designed to be a much more labour-and service-intensive program model so the small difference in both program impacts (described in previous chapters) and program costs is somewhat surprising. This theme is discussed in greater detail later in this chapter.

The general operating costs for the service providers at the experimental sites were substantially higher than for SEDI on a per participant group basis. This is also true of individual program delivery items, of which the sites bore the lion's share of the cost (see Table 8.1 above). One reason is that SEDI did not deal with participants directly so its cost per program group member was rather fixed. The sites, however, dealt with participants on demand and since the program went on for some time, their cost was higher. Also, it is important to recall differences in the roles played by SEDI as the national coordinator and by

5 However, the extra labour costs associated with recruiting the control group (which would not be needed in an ongoing program) were taken into consideration and discounted.

6 In *learn*$ave, the financial institutions imposed a $1 administration fee on all withdrawals from the *learn*$ave account. This cost was not included in these cost calculations as it was thought that such costs would have to be incurred by participants if they had another account beside the *learn*$ave account. At any rate, the cost to each participant was low and would not have figured prominently in the cost calculations.

7 For more on this, see Chapter 4 for results of the service delivery case study.

8 The Labour Market Information website of HRSDC shows that average hourly wages in 2007 were $14.50, $14.85, and $16.03 for a bank customer service representative in Halifax, Toronto, and Vancouver, respectively:
- www.labourmarketinformation.ca/standard.aspx?ppid=81&lcode=E&prov=12&gaid=25164&occ=1433
- www.labourmarketinformation.ca/standard.aspx?ppid=81&lcode=E&prov=35&gaid=9219&occ=1433; and
- www.labourmarketinformation.ca/standard.aspx?ppid=81&lcode=E&prov=59&gaid=25565&occ=1433. After adjusting for inflation, the average of these three figures is $12.98 (in 2002 dollars).

Table 8.1 *learn*$ave Present Value Cost-Economy Per Program Group Member and Cost-Efficiency ($), by Program Group, All Program Group Participants

	learn$ave-only			*learn*$ave-plus		
	Units per PGM	Discount Factor	Unit PV Cost	Units per PGM	Discount Factor	Unit PV Cost
Cost-Economy (per Program Group Member)						
Program Activities						
Recruitment	1.0	1.000	135.21	1.0	1.000	135.21
Enrolment	1.0	1.000	95.14	1.0	1.000	95.14
Financial Management Training	0.0	1.000	0.00	13.1	0.966	64.86
Case Mangement Services	3.3	0.880	113.71	4.6	0.893	160.22
Matched withdrawal orientation	0.6	0.905	16.23	0.7	0.904	17.39
Matched withdrawal processing	3.2	0.880	279.67	3.3	0.879	291.70
Account closure	1.0	0.847	33.04	1.0	0.846	32.99
Bank administration (imputed)	1.0	1.000	12.98	1.0	1.000	12.98
Total for all activities	…	…	**685.97**	…	…	**810.49**
General Operating Cost						
SEDI	1.0	0.907	426.21	1.0	0.907	426.21
Sites	41.2	0.916	822.08	41.5	0.915	828.18
Total for SEDI and sites	…	…	**1,248**	…	…	**1,254**
Total Program Delivery Cost	…	…	**1,934**	…	…	**2,065**
Matched credits awarded (2002 $)	2,128	0.888	**1,890**	2,291	0.886	**2,030**
Total Cost-Economy	…	…	**3,824**	…	…	**4,095**
Cost-Efficiency						
Active participants as a proportion of all participants (%)	…	…	80.4	…	…	84.2
Cost per Active Participant	…	…	**4,755**	…	…	**4,861**
Average peak *learn*$ave savings (2002 $)	955	0.969	925	1,016	0.967	982
Cost per Dollar Saved	…	…	**4.13**	…	…	**4.17**
Participants receiving education as a proportion of all participants (%)	…	…	84.7	…	…	86.7
Cost per Participant Receiving Education	…	…	**4,516**	…	…	**4,724**

Source: Calculations based on *learn*$ave site staff time studies, Program Management Information System, accounting records, baseline survey, and three follow-up surveys.

Note: PGM = Program group member
PV = Present value
… Figure not applicable as a total.

the local delivery agencies. For the national coordinator, the role was somewhat more constrained and there was also the potential to benefit from economies of scale, compared to the local agencies. To the degree that national coordinators might be able to absorb more of the activity and costs related to the delivery of a *learn*$ave-like program, it may be possible to improve upon the cost-economy observed here.

Finally, the results indicate that general operations were the highest cost delivery item while total delivery costs were similar to the cost of the saving matches. General operations costs ($1,200–$1,300 per program participant) were about two-thirds of the total program delivery cost (about $2,000 per program participant) (Table 8.1). Individual program activities accounted for the remainder of the delivery cost. Overall, total delivery costs were about the same as the value of the saving

credits paid out to participants, in the $1,900-$2,000 range per program participant.

Relative cost-economy

To compare *learn*$ave's cost-economy, we need one or more other programs targeted to low-income people. No other IDA programs in Canada offered a similar scope and the necessary data to enable such a comparison. Therefore, comparisons were made with a similar U.S. IDA program (the American Dream Demonstration (ADD) IDA project in Tulsa, U.S), with Ontario's provincial income assistance program (Ontario Works), and with the Canada Education Savings Program, a national matched education savings program for children.

Cost-economy for *learn*$ave was generally lower than they were for the ADD IDA project in Tulsa, U.S. First, *learn*$ave average delivery costs of about $2,000 (expended over the 2000–2007 period) were comparable to the average administration costs for the ADD of U.S.$1,950 per participant[9] (expended over the 1998–2003 period) (Schreiner 2005). Second, while about 50 per cent of the financial cost of *learn*$ave was spent on administration and delivery, this was lower than in the comparable American IDA program in which about 70 per cent of the cost was spent on operations.[10]

Another point of comparison comes from the provincial Ontario Works program that provides means-tested income assistance to low-income persons in Ontario. The median cost to administer Ontario Works in Ontario is $8,852 per case for a 48-month period (Ontario Municipal Benchmarking Initiative, 2008),[11] suggesting the cost of delivering *learn*$ave (about $2,000) was comparably very low. However, it should be borne in mind that traditional income assistance programs have, of course, a different objective from *learn*$ave's (i.e., to reduce the effects of poverty as opposed to encouraging enrolment in adult education) and serve a much more disadvantaged clientele.

A final point of comparison is the national Canada Education Savings Program (CESP). Like *learn*$ave, the CESP aims to boost participation in education by offering an incentive for savings in a Registered Education Savings Plan that can only be used for post-secondary education, but, in the case of the CESP, this is primarily for a child's education. Under the CESP, a savings account can be opened for any child in Canada and savings will be matched annually with a Canada Education Saving Grant (CESG) at a rate tied to income. Lower-income families may also receive the Canada Learning Bond, a lump-sum payment intended to kick-start family deposits into the account. Based on data obtained from HRSDC,[12] the annual administrative costs to government for each beneficiary (where one CESG beneficiary may have more than one education savings account) has averaged $12.85 over the past six fiscal years. The annual administrative costs to government relative to each dollar of financial incentive paid out by government have averaged only $0.06 over the past six fiscal years. Unlike *learn*$ave, the CESG relies on financial institutions which provide the accounts to cover the costs of outreach, enrolment, case management and processing withdrawals. The implementation research (see Chapter 4) suggests that the role of the community-based agencies was key to much of these activities in *learn*$ave. The CESG results do not include these costs borne by financial institutions. While the CESG offers a remarkable low-cost benchmark for the cost-economy of an ongoing matched education savings program and tacking a matching grant program on top of an existing administrative structure, there would be trade-offs in transforming *learn*$ave into this more streamlined approach. These are considered again at the end of the chapter.

Cost-efficiency of *learn*$ave outputs

This section turns its attention to the cost-efficiency estimates for *learn*$ave, where the focus shifts to the cost per unit of output: active participant, dollar saved, person enrolled in education (lower panel of Table 8.1). The results above suggested a higher cost per participant (lower cost-economy) for the *learn*$ave-plus program group relative the *learn*$ave-only group. However, lower cost-economy does not necessarily imply lower cost-efficiency because the present value cost per program group member does not take activity intensity into account. Some participants did not actively participate in the program after enrolment (i.e., make deposits of $10 in each of 12 months and become eligible to use matched credits). In fact, participants in the *learn*$ave-plus group were more likely to be active in using their account (about 84 per cent actively saved in at least 12 months)

9 Note that the latter is comprised of the costs of program activities, but excludes extraordinary recruitment costs as well as evaluation costs; however, they do not discount costs related to the "pioneering work" of the ADD as well as to its "extraordinarily 'high-touch' service."

10 Note that the match rate of the American Dream Demonstration IDA Project was only 2:1 or 1:1, compared to *learn*$ave's 3:1 match rate. As a result the proportion of cost spent on match funds in ADD would be lower. The 71 per cent figure is computed as the total cost of ADD match funds of $377,947 as a percentage of total cost of program ($922,473 in operations cost + $377,947) (Schreiner, 2005, page 17).

11 The median monthly administration cost of social assistance was $237 per case at 2007 dollars (or $203 per case in 2002 dollars). The present value of administering a social assistance case for 48 months was $8,852 at an annual discount rate of 5 per cent. See Ontario Municipal Benchmarking Initiative (2008).

12 Data were obtained directly from the CESP branch, based on figures reported in annual departmental reports for 2003-04 to 2008-09.

compared to the *learn*$ave-only group (just 80 per cent met the same test of activity). If the program output is measured by the number of active participants, the average present value cost was about $100 lower for the *learn*$ave-only group ($4,755 per active participant) than for *learn*$ave-plus group ($4,861). The small increase in active participation for the *learn*$ave-plus group exceeded the small increase in cost due to the additional services provided.

Results for another program output measure, the amount of *learn*$ave savings accumulated, indicate minimal increases in unit costs due to the services. The average present value of peak *learn*$ave savings was $925 for *learn*$ave-only group members and $982 for *learn*$ave-plus (as observed in Table 8.1). As a result, the average costs per dollar of participant savings were $4.13 and $4.17, respectively. This means that, while the addition of the services to the matched savings credit generated a somewhat higher delivery cost for the *learn*$ave-plus group, the additional services slightly reduced cost-efficiency, as the increased savings induced by the services outweighed the additional cost incurred for them.

Compared to studies of the cost-efficiency of the U.S. ADD IDA program, *learn*$ave was more cost-efficient in encouraging savings. As noted, the total cost per participant-dollar-saved was between $4.13 and $4.17, both lower than the ADD cost of $4.56.[13] If only delivery costs are considered (i.e., excluding the cost of the matching savings credits), then *learn*$ave-plus costs were $2.10 per dollar saved (about $2.065 / $982; Table 8.1), compared to $3.06 for the ADD program (Schreiner, 2005). Although *learn*$ave compares very favourably, there were important differences, however, between the projects that may help to explain some of these differences in cost-efficiency. For example, the ADD results are based on an IDA with a much broader range of savings goals, which may have affected the costs of the matched withdrawals process. The ADD results are also based on data from an earlier period (1998–2003) than *learn*$ave with different interest rates and it used a lower match rate (just 1:1 or 2:1 compared to *learn*$ave's 3:1).

A third output measure is the number of participants attending education or training. Because education enrolment rates were similar to the proportions of active participants in each program group, the costs per participant-attending education were at a similar level ($4,516 for *learn*$ave-only group members and $4,724 for *learn*$ave-plus). These figures are 1.5 times the average cost of an EI Employment Benefits and Support Measure (EBSM) intervention of $2,977 per person per year,[14] though it should be noted that EI-supported training tends to be short term.

Cost-effectiveness

Cost-efficiency does not necessarily imply cost-effectiveness. If most participants would have taken education or training in the absence of *learn*$ave, then the project's cost-effectiveness would be very low. Cost-effectiveness estimates are meaningful only when the impacts of the program are statistically significantly positive. The current discussion is limited to those *learn*$ave impacts that were statistically significant and positive (as discussed in Chapter 7). Note, again, that education impact estimates used here are for all participants, whereas those presented in Chapter 7 are for just education stream participants. Table 8.2 shows the cost-effectiveness of *learn*$ave in encouraging participation in adult learning. On top of the 79.2 per cent of control group members who took education or training during the 54-month duration of the project, the impacts were a 5.5-percentage-point increase for matched credits alone on the *learn*$ave-only group and a 7.5-percentage-point increase for matched the credits and services combined for the *learn*$ave-plus group. This means that, of the 84.7 per cent of the *learn*$ave-only group who were induced into education through the matched credits, 93.5 per cent (79 / 84.5) would have done so without the credits. In other words, for each person who was induced into education by the matched credits and would have not done so without the credits, the government paid for about 14.5 others who would have gone to school without the credits. This has implications for cost-effectiveness, as will be discussed below.

The results indicate that, based on the per capita cost of $3,824 for *learn*$ave-only, the financial incentive (matched credits) cost the government $70,168 ($3,824 / 5.45 per cent) to induce each additional person to acquire education and training, which includes the cost of the windfall gains of those who would have entered education or training without the *learn*$ave credits. When the financial incentive is combined with the enhanced services in the *learn*$ave-plus group, the cost-effectiveness actually improves to about $55,000 per participant ($54,966 = $4,095 / 7.45 per cent). The difference is due to the larger impacts in the *learn*$ave-plus group even though this same group showed higher costs per participant. For a

13 SRDC calculations are based on information from Schreiner (2005). Inflation is not controlled for, though it would not greatly affect the results because of the short timeframe of the projects and low inflation present over that period.

14 According to the 2007 EI Monitoring and Assessment Report (HRSDC, 2007), total expenditures on EBSM interventions was about $2,0867 million in fiscal year 2006/2007 and the number of claimants was 618,202. After adjusting for inflation, the average cost was $2,977 per person per year in constant 2002 dollars.

Table 8.2 *learn*$ave Present Value Cost-Effectiveness, by Program Group, All Program Group Participants

	***learn*$ave-only**	***learn*$ave-plus**
Impact on enrolment in education and training overall (%)[1]	5.5	7.5
Cost per Additional Person Receiving Education and Training ($)	**70,168**	**54,966**
Impact on enrolment in an education program (%)[1]	7.8	10.8
Cost per Additional Person Enrolled in an Education Program ($)	**48,902**	**37,952**

Source: Calculations based on *learn*$ave site staff time studies, Program Management Information System, accounting records, baseline survey, and three follow-up surveys.

Note: [1] Enrolment estimates differ somewhat from those presented in Chapter 7, as the latter were based on only education stream participants.

discussion of how the costs vary by saving stream, see Box 8.3.

To properly evaluate the cost-effectiveness of *learn*$ave, a reference or benchmark is needed. As with the above comparisons of cost-economy and cost-efficiency, one way to do this is to compare *learn*$ave's cost-effectiveness to other programs with similar purposes. Regrettably, insufficient information was available to enable this comparison. While a cost-efficiency comparison was made with the Employment Insurance Part II employment and skills training measures, the available data did not permit us to calculate a true estimate of cost-effectiveness for this comparison program. Indeed, estimates of true (incremental) impact of any existing programs are rarely generated.

Another comparison method is to examine the increase in average participant annual earnings needed to cover the financial cost of encouraging them (including the participants identified as having a windfall benefit) to enter into education.[15] This is not a traditional way of assessing cost-effectiveness but it at least provides a basic benchmark (breakeven point) to place *learn*$ave IDA program costs into context. Since a typical *learn*$ave participant was about 33 years old at the time of enrolment in the project (see Chapter 3), the average participant would have 32 more earning years if retirement age is assumed to be 65. Using an annual discount rate of 5 per cent for a 32-year period, *learn*$ave participants would have to earn an additional $4,440 annually for *learn*$ave-only participants and $3,478 annually for *learn*$ave-plus participants to cover the financial costs of *learn*$ave. These are substantial amounts and would mean large annual increases over participants' baseline average annual income of $11,000. For those participants who invested their *learn*$ave dollars in education or training courses, previous research suggests the returns will be small and unlikely to meet this threshold test. As a result, we conclude that *learn*$ave is not likely to be a

15 Even when the annual increase in earnings can cover the financial cost, however, cost-effectiveness does not imply there is a net social benefit. There are other costs beside financial ones, including the opportunity cost of lost earnings while in school. However, if the potential annual additional increase in earnings resulting from the intervention is lower than the financial cost, it would be more cost-effective to simply transfer an equivalent amount of money from the government to the participants since the program is not likely to be viable. See Table G in Appendix G for more on this.

Box 8.3 Differences in costs by saving stream

In addition to considering differences in the costs for the two different program groups and for two different types of education activity (programs and courses), the study of the ***learn***$ave costs also examined whether there were differences in costs between the education and micro-enterprise streams. Micro-enterprise stream participants were allowed to use their earned matched credits on small business start-up and/or education and training, while education stream participants had to use them for education or training only. Not unexpectedly, the education participation rate as well as ***learn***$ave's impacts on education were lower among the micro-enterprise stream.

Based on the results in Table 8.3, we conclude that the cost-efficiency of ***learn***$ave was marginally better for the education stream than for the micro-enterprise stream. Specifically, the costs per active-saving-participant, per participant-dollar-saved, and per participant-receiving-education (rows 2 through 4) were somewhat lower for the education stream than for the micro-enterprise stream.

Moreover, the results would suggest that ***learn***$ave could be more cost-effective in encouraging self-employment among micro-enterprise stream members than encouraging education among education stream members. As indicated in Chapter 7, ***learn***$ave increased the incidence of self-employment by about 25 percentage points and 15 percentage points for the ***learn***$ave-only and ***learn***$ave-plus groups, respectively. This means that the costs for each new participant who entered into self-employment were $14,901 for ***learn***$ave-only (per participant costs of $3,658.14 / 0.25) and $27,166 for ***learn***$ave-plus ($3,968.91 / 0.146). This means that, to cover the financial cost of encouraging participants to enter self-employment, ***learn***$ave-only group members induced into self-employment would have to earn less than education stream members induced into education or training, i.e., they would have to earn only $943 more a year for 32 years from their self-employment job in increased earnings, while ***learn***$ave-plus participants would have to earn almost twice that: $1,719 per year.

Table 8.3 *learn*$ave Present Value Cost-Economy, Cost-Efficiency, and Cost-Effectiveness ($), by Saving Stream and Program Group, All Program Group Participants

	Education Stream		Micro-Enterprise Stream	
	***learn*$ave-only**	***learn*$ave-plus**	***learn*$ave-only**	***learn*$ave-plus**
Cost-Economy				
Cost per Program Group Member	**3,660**	**3,971**	**3,658**	**3,969**
Cost-Efficiency				
Active participants as a proportion of all participants (%)	81.7	85.5	74.9	79.0
Present Value Cost per Active Participant	**4,480**	**4,643**	**4,885**	**5,026**
Average peak *learn*$ave savings	934	991	886	946
Present Value Cost per Dollar Saved	**3.92**	**4.01**	**4.13**	**4.20**
Proportion of participants who enrolled in education/training (%)[1]	88.0	89.7	68.1	73.5
Present Value Cost per Participant Enrolled in Education or Training	**4,159**	**4,428**	**5,375**	**5,401**
Proportion of participants with self-employment (%)	…	…	66.7	56.7
Present Value Cost per Participant with Self-Employment	…	…	**5,485**	**6,995**
Cost-Effectiveness				
Impact on enrolment in education or training (percentage points)	6.6	8.2	-4.0	1.5
Cost per Additional Person Enrolling in Education or Training	**55,872**	**48,303**	**N/A**	**N/A**
Impact on enrolment in an education program (percentage points)[1]	9.1	12.6	-2.3	0.3
Cost per Additional Person Enrolled in an Education Program	**40,260**	**31,537**	**N/A**	**N/A**
Impact on self-employment (percentage points)	…	…	24.6	14.6
Cost per Additional Person in Self-Employment	…	…	**14,901**	**27,166**

Source: Calculations based on *learn*$ave site staff time studies, Program Management Information System, accounting records, baseline survey, and three follow-up surveys.

Note: … Self-employment outcomes not cost-effective or appropriate for the education stream.

[1] Enrolment estimates differ somewhat from those presented in Chapter 7, as the latter were based on only education stream participants.

cost-effective way to increase participation in courses among low-income adults in Canada.

However, *learn*$ave does show better promise, from a cost-effectiveness perspective, as a way to encourage low-income adults to enrol in education or training programs. Education program participation among all participants was 7.8 per cent higher among the *learn*$ave-only group (compared to the control group) and 10.8 per cent higher for the *learn*$ave-plus group. The higher impacts on education program enrolment translate into greater cost-effectiveness for education program outcomes than for education and training outcomes overall: $48,902 for the *learn*$ave-only group and $37,952 for the *learn*$ave-plus group. To cover these costs, *learn*$ave-only participants would need to receive $3,095 a year in increased earnings, and *learn*$ave-plus participants $2,402 a year. Again, research indicates that returns to education are greatest when the education is a program that leads to a post-secondary certification or qualification.[16] To the extent that *learn*$ave participants were encouraged or limited to using their accounts to fund education programs, the cost-effectiveness would likely be much higher.

Costs by immigrant status

Given that such a sizeable subset of the *learn*$ave research sample were recent immigrants to Canada, the study of *learn*$ave cost-efficiency and cost-effectiveness

16 Based on 2006 Census data on 2005 median earnings for full-time earners 25-64 years of age by highest level of educational attainment (Statistics Canada, 2009), the returns (increase in earnings) to a high school certificate (relative to not having a high school certificate) are $5,374; and the returns to a post-secondary education (relative to a high school certificate) are: $2,593 for a trade certificate; $5,534 for a college certificate; and $18,645 for a Bachelor's degree. These figures are not adjusted for any systematic differences between graduates at different levels of education.

also examined differences between recent immigrants and Canadian-born participants. The previous chapter indicated that *learn*$ave had the largest impact on the education participation of Canadian-born participants and the least on the participation of immigrants who had been in Canada for some time. Our main finding is that (holding the delivery costs between subgroups equal) *learn*$ave was most cost-effective for the Canadian-born participants. However, while Canadian-born participants represented less costly clients in terms of cost-economy, they were more expensive savers (in terms of cost-efficiency) as their cost per participant-dollar-saved was higher.

With respect to cost-economy, for *learn*$ave-only participants, the average cost per Canadian-born participant was about $3,200, compared to about $4,400 and $3,500 for recent and earlier immigrants, respectively (Table 8.4). The lower cost of Canadian-born reflected their lower usages of service time, matched withdrawals and matched credits.

Turning to cost-efficiency, the costs per active *learn*$ave-only participant were about the same for the Canadian-born and immigrant participants, at about $4,700 to $4,800, while the cost per dollar saved was higher for the Canadian-born at $4.85, compared to $3.80 and $4.41 for recent and earlier immigrants, respectively). However, the cost per participant enrolling in education was lowest for the Canadian-born participants at just $27,000 to $29,000. Similarly, the cost per participant enrolling in an education program was also lowest for Canadian-born participants at just $17,000. For both of these cost measures, the ratios were lower in part because of the lower total cost per program group member but also the high incidence of education enrolment for this group compared to that of the immigrants.

The earnings increase needed from further education to cover the financial cost of inducing one Canadian-born learn$ave-only participant to enrol in an education program would be only $1,120 a year for 32 years, which is considerably lower than the $2,827 and $5,304 increase earnings to recover the total costs to induce recent and earlier immigrants into adult education, respectively. The relatively lower cost-effectiveness among immigrants was due to the higher windfall gains, lower cost-efficiency as well as the small or even negative impacts on education or training for this group.

Whether or not the project was truly cost-effective for recent immigrants is unclear as their circumstances were much different from others.[17] Given the low earnings of recent immigrants, the labour market returns to Canadian education credentials for recent immigrants could be much higher than the marginal returns for Canadian-born adults. However, the results presented in Chapters 5 through 7 suggest that recent immigrants who volunteered for *learn*$ave were highly motivated and likely to invest in their education, with or without the incentive of *learn*$ave. While this suggests significant demand for higher education among this subgroup of low-income Canadians, it does not appear that a *learn*$ave-like vehicle would be the most cost-effective model to assist them in entering such education.

In summary

The study of cost-effectiveness for *learn*$ave represents an important improvement over past efforts to measure IDA cost-effectiveness. First, efforts were made to discount the extraordinary set-up costs associated with the *learn*$ave demonstration as well as the research costs. Second, the current analysis is a true cost-effectiveness analysis in that it discounts the outcomes of participants who would have participated in education or small business without the assistance of *learn*$ave (i.e., the control group, representing the counterfactual), which was not done in prior studies.

Surprisingly, the cost-effectiveness of the financial management training and enhanced case management services combined with the matched credits turned out to be better than the matched credits alone. Despite the fact that the additional impact on savings from services when provided with the matched credits were rather small, the somewhat larger impacts on education participation more than made up for the extra costs of administering the services on top of the matched credits. The cost-effectiveness of *learn*$ave was actually improved by the combination of services and matching savings credits even if the services did not contribute greatly to the impacts.

Even though a substantial proportion of micro-enterprise stream participants would have been self-employed regardless of *learn*$ave, the matched credits were fairly cost-effective in encouraging self-employment. Additional income from the self-employment would have to amount to just $1,000 per person per year over the balance of the average participant's working career for the present value

17 The labour markets for recent immigrants, earlier immigrants and Canadian born participants were and are probably very different. Foreign credential recognition is a common problem among recent immigrants, and so pursuing Canadian education might be an alternative to "Canadianizing" previous education attained in a foreign country. For some highly educated recent immigrants without Canadian credentials, they would have to work in low-wage jobs requiring minimum skills.

Table 8.4 *learn*$ave Present Value Cost-Economy, Cost-Efficiency and Cost-Effectiveness ($), by Immigration Status at Baseline and Program Group, Education Stream Participants

	Canadian Born		Recent Immigrants		Earlier Immigrants	
	***learn*$ave-only**	***learn*$ave-plus**	***learn*$ave-only**	***learn*$ave-plus**	***learn*$ave-only**	***learn*$ave-plus**
Cost-Economy (per Program Group Member)						
Average Program Delivery Cost	**1,766**	**1,870**	**2,128**	**2,280**	**1,953**	**2,048**
Average Matched Credits Awarded	**1,436**	**1,554**	**2,280**	**2,482**	**1,593**	**1,548**
Total	**3,201**	**3,424**	**4,408**	**4,762**	**3,546**	**3,596**
Cost-Efficiency						
Active participants as a proportion of all participants (%)	67.1	71.2	93.5	96.3	75.1	80.8
Cost per Active Participant	**4,771**	**4,806**	**4,712**	**4,947**	**4,719**	**4,450**
Average peak *learn*$ave savings	660	708	1,159	1,228	805	832
Cost per Dollar Saved	**4.85**	**4.84**	**3.80**	**3.88**	**4.41**	**4.32**
Proportion of participants who enrolled in education/training (%)	88.2	88.1	89.5	93.1	83.2	81.8
Cost per Participant Enrolling in Education	**3,631**	**3,884**	**4,924**	**5,118**	**4,264**	**4,394**
Cost-Effectiveness						
Impact on enrolment in education or training (%)	11.5	11.5	7.6	11.1	-6.9	-8.3
Cost per Additional Person Enrolling in Education or Training	**27,909**	**29,902**	**58,384**	**43,016**	…	…
Impact on enrolment in an education program (%)	18.3	19.4	7.4	10.7	-4.3	4.3
Cost per Additional Person Enrolled in an Education Program	**17,483**	**17,694**	**59,568**	**44,670**	…	**83,817**

Source: Calculations based on *learn*$ave site staff time studies, Program Management Information System, accounting records, baseline survey, and the three follow-up surveys.

Note: … Cost-effectiveness not computed because of negative impacts.

of the income to exceed the present value of the financial cost of the program. This is much lower than the $4,400 that would have to be earned by education stream participants induced into education or training by *learn*$ave, or than even the $3,100 for education programs.

Though the cost of *learn*$ave to encourage education enrolment was rather high due to windfall gains, its cost-effectiveness was greater for education programs, than for courses. Similarly, the cost-effectiveness was greater for the Canadian-born than for immigrants. This is particularly true with regards to the relatively low earnings needed to recover the financial cost of the Canadian-born, compared to immigrants.

Asset-building programs are often criticized for their high program administration costs relative to the dollar value of the benefits transferred to participants. With half of *learn*$ave operation costs attributed to administration, *learn*$ave's costs must bear some of the same criticism.

However, *learn*$ave also shows promise in its cost-effectiveness compared to other IDA projects and to other interventions aimed at low-income populations. The current study finds that *learn*$ave's cost-efficiency, as measured by delivery costs per participant-dollar-saved, was better than the US American Dream Demonstration project. Furthermore, if *learn*$ave is compared to more traditional forms of social programs targeted at low-income people such as social assistance or Employment Insurance, then its program administration costs are comparable if not lower. However, the comparison to the Canada Education Savings Program (CESP) suggests both that large-scale asset-based programs with a track record can reach very, very low cost ratios and that, as designed, *learn*$ave has a long way to go to reach that level of cost-efficiency.

The largest driver of cost in *learn*$ave was the cost of the project's administration and, within that cost area, some of the largest component were those labour-intensive services to check documents for project rules at entry, enrolment and cash out. One important way of reducing costs would be to tie a savings-based program to existing means-tested or targeted government programs and information systems, as in the case of the CESP, and take advantage of the latter's administrative and information structures. For example, tying eligibility to the Working Income Tax Benefit or the National Child Benefit might reduce the costs of recruitment and IDA program enrolment. Similarly, targeting a savings-based program to clients receiving transfer income (such as EI or provincial social assistance, the latter of whom our research results presented in Chapter 5 showed could save) might also reduce the costs of recruiting low-income participants if administrative databases can be used. Further, the cost of administering credits withdrawals could be reduced if the approved education or training institutions could claim tuition directly from the program (as is done under the Canada Student Loan Program or in certain international examples of voucher-based programs) through an electronic funds transfer.

Chapter 9 Lessons learned

The *learn*$ave research and demonstration project was designed to test how effective a matched savings incentive would be in inducing low-income adults to save for and participate in adult education and training or for starting a small business. The idea was conceived by Social and Enterprise Development Initiatives based on the asset-building concept of Individual Development Accounts, pioneered in the early 1990s in the United States where they have been used to combat poverty by encouraging low-income people to acquire productive assets ranging from a home or a vehicle, to micro-enterprise, education, and retirement savings. However, the promise of IDAs in increasing adult education enrolment and business start-ups, which were the purposes *learn*$ave focused on, was largely unproven in Canada in 2000 when this project was implemented.

At the core of IDAs is a matched saving incentive. In the *learn*$ave IDA, most participants received, for every dollar they put aside up to $1,500 in a special account over a three-year period, $3 in virtual credits that could be used for only education and training or small business start-up. To join the project, participants typically must have had household income over the previous year that amounted to more than 120 per cent of the Low-income Cut-off, not have had more than $3,000 in household assets, and not have been a full-time student.

The project was delivered in 10 communities representing a mix of large- and medium-sized urban areas and rural communities. At three of the sites (Halifax, Toronto, and Vancouver) the project was implemented using an experimental design with three different groups: a program group receiving the matched credits only; a program group receiving financial literacy training and enhanced case management supports in addition to the matched credits; and a control group made up of participants not receiving any credit or ancillary benefits.

To estimate impacts, the outcomes of two groups of participants receiving program benefits were compared to those of the control group. The control group represented the counterfactual, i.e., what program group members would have done if they had not participated in this intervention. As individuals were randomly assigned to the groups, they were socio-demographically similar and the differences in outcomes between the groups could then be ascribed to *learn*$ave. This is considered to be the best way of measuring *incremental* impacts of an initiative, as opposed to the traditional program evaluation approach of using simple pre- and post-comparisons of outcomes.

The *learn*$ave demonstration project is unique and noteworthy in several ways. First, it is one of a handful of innovative social program ideas that have been submitted to rigorous tests in Canada over the last two decades. *learn*$ave is also the second of two experiments worldwide aimed at evaluating the effectiveness of IDAs. In addition, the project provided the opportunity to test the contribution of financial literacy training over and above the use of financial incentive to save. Lessons learned throughout the life of the project constitute a rich body of information and insights for any public authority or non-profit organization that is considering the introduction of similar approaches to assist low-income or low-skilled individuals.

Implementation and delivery

The initial recruitment difficulties experienced with *learn*$ave indicated that if an IDA program like *learn*$ave were offered nationwide, take-up would be slow at first. It would take considerable time and effort to inform and attract the target population and build a track record, which is what took place in *learn*$ave and is typically the case for any new program. A national campaign extolling the benefits of participation and education would have helped *learn*$ave reach its target earlier by increasing the low-income public's comfort with the concept of asset-building.

The program was attractive only to a small portion of the target population. Evidence gathered in preparation for *learn*$ave, and during the project itself, revealed that many low-income individuals may value education, but do not see it as a viable option for them personally, likely because of negative past experiences or life constraints. Thus, a program like *learn*$ave that promotes education enrolment would not be appropriate for all low-income earners — it should appeal to those already inclined toward education. Alternatively, allowing for additional types of asset acquisition besides adult education would increase the appeal of IDAs for the broader target population.

Another lesson learned from the implementation is that it is important to come to some kind of agreement with government authorities over the treatment of the IDA proceeds. In the interests of recruitment and fairness, it was necessary to negotiate with provincial officials to raise asset limits or otherwise disregard the *learn*$ave matches when computing income assistance levels for participants. Similarly, federal tax officials had to be consulted to ensure that the matched credits were not considered as taxable income.

The role of community organizations seemed essential in reaching the target clientele and supporting it throughout the process. This was particularly true in terms of providing assistance to participants at the sign up and cash-out stages, which were seen as onerous from a paper burden perspective.

This is not to suggest that efficiency gains cannot be realized by streamlining the IDA delivery model. For instance, bundling the IDA offer with the information systems associated with existing social programs could facilitate sign-up by generating a pool to recruit individuals from and by reducing the need to check certain eligibility criteria and to collect information already gathered by the host program. Permitting electronic deposits into the IDA and automatic transfers from other income sources could also be attractive to participants. There would also be gains from increasing flexibility on the timing and other aspects of the cash-out process, from using administrative structures of existing student financial assistance programs, and from optimizing collaboration between the financial and educational institutions, including sending money directly to the latter.

Saving outcomes

This project demonstrated that low-income Canadians, even those on income assistance, can save. This finding contrasts with concerns expressed by critics of asset-building approaches that low-income populations do not have resources to allow any savings at all. Low-income Canadians recruited into *learn*$ave did make much use of the accounts and financial incentives offered by the project. Nearly all opened an account, most saved something and indeed average deposits amounted to about $1,100 over three years, and most used the matched credits. The analysis further showed that savings and matched credit use did not vary by income level and financial constraints. However, the results also showed that many participants took full advantage of the program to quickly earn and use all their credits; many of these would likely have been able to so without *learn*$ave assistance. Conversely, there was a sizable number of participants who did not use all the credits they did earn, suggesting life circumstances can interfere with attaining one's goals.

The *learn*$ave project also showed that a matched saving program can be effective in promoting regular saving behaviour and financial integration among low-income people. This was done by the saving match and the requirement to save a minimum amount each month for 12 months to qualify for credits and to open and make deposits in an account in a financial institution. As well, the project showed that such a program can be successful in encouraging low-income people to budget and set financial goals and to alter their household spending and other expenditure patterns and to do so without incurring undue financial hardship, which is a distinct risk when people with scarce resources are offered a generous saving subsidy.

The research showed as well that IDA program parameters can play a role in saving performance. Raising the saving match rate increased saving regularity and the amount saved, though at a declining rate past a 3 to 1 matching rate. Increasing the match cap — the maximum amount qualifying for the saving match — also positively affect the amount saved. Reducing the length of the saving period — the period during which IDA savings qualified for matches — impacted positively on saving regularity but had no influence on savings level.

One of the original ideas behind the concept of asset-building initiatives like the *learn*$ave IDA is that the accumulation of assets confers on holders a number of financial and psychological benefits that eventually enable them to enjoy greater economic well being. While the project revealed the *learn*$ave IDA experience did have a positive effect on life satisfaction, no impacts were recorded on average net worth. While *learn*$ave program groups increased their financial assets at the beginning of the project, these increases disappeared at the end of the *learn*$ave period.

Impacts on education enrolment and small-business start-ups

A program like *learn*$ave would enable a significant number of low-income Canadians to achieve adult education objectives who would not have done so otherwise. While many people interested in education would enrol in education courses and programs even without an IDA program, such a program could still increase the level of participation in PSE programs by over 20 per cent. The project showed that impacts on education enrolment were widespread, occurring for those who were at the lowest and highest educational levels, who were in the lowest income bracket, or who were not regular savers. The high rate of education enrolment of control group members also points to the value of a control group in measuring effectiveness of programs, without which the impacts would have been vastly over-estimated.

The project also proved that a program like *learn*$ave could be used to increase small business start-ups. The matched credits had a beneficial impact on the incidence of self-employment among those in the micro-enterprise saving stream. As well, positive effects were observed on

self-employment duration and income, thus pointing to the durability of the small business.

Whether or not these positive impacts on education and training enrolment and micro-enterprise start-up lead to improved employment outcomes and greater prosperity over the long term remains to be seen. Nevertheless, the fact that the significant education enrolment impacts were recorded on university and college programs augurs well for the future, given the positive returns that have been demonstrated in the literature for education at this level. However, only with additional follow-up of participants could such outcomes be corroborated.

Role of additional services

The financial management training and enhanced case management services when delivered with the matched saving credits did not have a strong incremental impact on saving or education outcomes. Despite prior expectations about these additional services, the credits alone were typically as effective as the services combined with the credits. This suggests that the very act of saving for an education goal, as induced by the matched credits, may be more effective in promoting saving than being instructed on how to do it. Or, it may suggest that the main obstacle for low-income populations interested in education is more a lack of financial resources than a lack of financial knowledge. Another potential explanation for the lack of services' impacts is that the content and amount (15 hours) of financial training delivered may not have been completely suitable for the clientele.

Costs

The analysis of the cost of administrating and operating *learn*$ave indicates that its cost-economy (the average cost of providing services to one program group participant) and cost-efficiency (the average cost of per unit of output, e.g., cost per dollar saved or cost per participant receiving education) are comparable to that of the U.S. Assets for Independence IDA program and those of provincial income assistance and EI training assistance programs.

As for cost effectiveness (i.e., the average cost to produce a unit of program impact), results indicated that the estimated cost per additional person prompted to enrol in an education program by *learn*$ave matched saving credits and services would be fairly high — around $38,000, at best. While this may seem high, it may not be unreasonably so in light of expected returns from post-secondary education programs. To cover this cost, a 33-year-old new enrolee would have to earn an extra $2,400 per year over the rest of his or her career. This is not an unlikely outcome if an individual enrols in a college or university program, but it is not likely to happen as a result of attending one or two courses.

By far, the main reason why the cost of the program per incremental student is so high is that the introduction of such a program would provide windfall gains to a large number of participants who would have enrolled in adult education in the absence of the program. In fact this is a problem that undoubtedly exists in other social programs, but is not typically measured.

Conclusion

All in all, *learn*$ave has demonstrated that an IDA program with a generous incentive to induce savings could contribute to increase the number of low-income Canadians enrolling in education in a non-negligible way. As well, it was shown that this type of assistance for disadvantaged populations cannot be dismissed on the basis that it would be more expensive to provide than other types of government assistance. This is not to say that alternative delivery mechanisms cannot be introduced to lower operational and administrative costs.

Still, the analysis suggests that such a program would be quite expensive. Perhaps, other means — such as direct grants (or loans) or subsidized time off — could achieve the same objective at a lower unit cost. However, these alternative approaches may not generate other IDA-related benefits, such as regular saving behaviour, commitment to goal attainment, and increased comfort with the financial system. In the absence of complete data on the benefits of the various student financial assistance models, and without similar experiments on competing forms of student support, the merits of a *learn*$ave program compared with these alternative measures remain uncertain.

Appendix A Approaches to encouraging higher education and learning

This appendix is in two parts. The first is concerned with Canadian federal and provincial initiatives to support education and training by employers (for their employees) and by individual Canadians. The second part of the appendix is devoted to measures and approaches used in other countries in support of adult education and training.

Canadian measures to support learning

As part of the review of the context for the *learn*$ave project, SRDC conducted an environmental scan of the approaches that Canadian governments have used to encourage adults to invest in human capital. The discussion below is not intended as an exhaustive or detailed summary of all federal, provincial or territorial (F/P/T) measures to promote adult learning and participation in higher education. Rather it is a general review of the policy tools currently available. Most governments have conducted major reviews of their own approaches to adult learning, literacy and education, often leading to new statements of policy and strategy for adult learning in their jurisdiction. That said, many jurisdictions use similar types of measures. Furthermore, all federal, provincial and territorial governments pursue a three-track approach of: 1) supporting education and learning infrastructure through funding to universities, colleges and other education providers; 2) encouraging employers to invest in skills training and development for their employees; and 3) offering incentives to individual learners to increase their own education and skills. In the section below we focus on the latter two for comparison to the objectives of the *learn*$ave demonstration project, as discussed in Chapter 2 of this report.

Employer-sponsored training: Encouraging employers to invest in their employees

According to a review of 2003 data by Rubenson, Desjardins and Yoon (2007), employers already carry out the largest share of the expense for adult education and training. Among Canadian adult learners, about 55 per cent were employer-sponsored while only 8 per cent had access to government funding and the balance relied on their own or family resources. However, surveys of employers and employees alike suggest that much of the employer-sponsored training goes to highly skilled managers and knowledge workers, leaving out lower-skilled service and goods producing workers (de Broucker and Myers, 2006). Governments at the federal and provincial levels support employer-sponsored training and education through three different types of mechanisms. These are briefly reviewed below with illustrative examples.

Tax credits and deductions for employer costs of training and education

Federal and provincial governments offer employers reductions in their corporate income taxes for eligible costs of training employees. At the federal level, training costs can only be deducted if they are to maintain a set of skills or knowledge, not to acquire new ones. Provinces offer various tax benefits for training costs. For example, Ontario offers the Apprenticeship Training Tax Credit, allowing eligible employers to deduct from their provincial income taxes a portion of the wages paid to an apprentice in training. Similarly, Manitoba offers employers tax credits for job creation and cooperative programs to promote employer-paid on-the-job learning opportunities for working age adults in the province.

Levy-and-grant (social-insurance funded) and "train-or-pay" systems

In a traditional levy-and-grant system, all employers pay a certain percentage of their payroll into a common fund which is used to sponsor new training they do. Thus, from an employer-perspective, the Employment Insurance system is in effect a levy-and-grant system, in which EI premia collected from all employers are partially used to fund grants for skills development projects with private sector partners through Employment Benefits and Support Measures (EBSMs) under Part II of the Employment Insurance Act. According to the most recent EI Monitoring and Assessment Report (HRSDC, 2008), about $90 million was paid in Targeted Wage Subsidies to reduce the costs to employers of providing employment and on-the-job training to EI-eligible clients. Some employers may also take part in the Job Creation Partnerships that paid out about $50 million in 2007-2008 to support local job creation opportunities to help EI-eligible clients to gain work experience and new skills.

Quebec is alone in Canada in pursuing a variant of this approach in their provincial "train-or-pay" system. Employers with payrolls greater than $1 million annually are required to demonstrate they have invested at least 1 per cent in training and education for their staff. Those employers who do not meet this benchmark are required to pay into a provincial fund that is then used to bolster training opportunities for working age adults in the province. In 2002-03 employers paid about $47 million into that fund (van Walraven, 2005) but the system does seem to have had a positive impact on the rate of participation in employer-sponsored training in the province. The incidence of workplace training in Quebec rose from 21 per cent in 1997 to 33 per cent in 2002, the fastest increase of any province in Canada and largely closing

Quebec's previous gap with the rest of Canada where the incidence went from 29 per cent to 35 per cent during the same period (Bélanger and Robitaille, 2008).

Grants and transfers

Outside of the EI-funded programs, F/P/T governments also offer straight grants to fund training by employers. Governments have provided several incentives for employers to invest in training through partnerships with sector councils (e.g., initiatives to maintain the skills of temporarily laid off forestry workers in partnership with the Forest Products Sector Council). The federal Workplace Skills Initiative funds demonstration projects to promote innovation in employee skills development. Several provinces offer grants, wage subsidies and other direct funding to promote employer investments in basic and higher skills through programs outside the EI system. For example, Ontario offers apprenticeship funding grants to eligible employers offering apprenticeship training. New Brunswick offers the Workforce Expansion wage subsidy to encourage eligible employers to hire and train unemployed workers in the province. Saskatchewan's basic education strategy includes programming to transfer training funds to employers (as well as labour and community organizations) to reduce the costs of providing basic skills training to workers in regions coping with major economic transitions.

Estimates by van Walraven (2005) indicate that in 2002 total employer spending on training was about $4 billion while public expenditures were about $70 million plus an additional $280 million for apprenticeship programs. He estimates that public subsides of workplace training in Canada represent just under 9 per cent of employer expenditures on such training (including apprenticeships). Given that employers already bear most of the current costs for employee training and given that the distribution of that training does not always flow to those most in need of skills upgrading, using employer-targeted policy levers to effect substantial change for low-skilled and low-wage adults may be somewhat limited. In fact, aside from funding for education providers, F/P/T governments aim most of their initiatives to promote training and higher education directly at individual learners

Individual investments in learning: Encouraging adults to invest in their own education

Surveys of adult learners find that about 40 per cent are self-financed or rely on family support to cover their education and training needs (Rubenson, Desjardins, and Yoon, 2007). As compared with measures to promote training by employers, the SRDC scan uncovered a much greater range of policy measures to create incentives for individual Canadians to invest in their own learning. Some of these measures reimburse a portion or all of the expenses of eligible learners; others offer cash assistance or vouchers to limited subgroups, repayable loans, or incentives for savings to be used to cover future education costs. However, as the discussion below will make clearer, none of the strategies is widely available to or accessed by low-income and low-skilled adults. In some cases, the benefits are tied to eligibility to, or receipt of benefits from, other programs such as EI or IA. In other cases, the initiatives are targeted to specific subgroups such as older workers or skilled new immigrants to Canada. In still other cases, the programs are largely designed with young students in mind rather than working adults returning to school for a second chance.

In-kind services

A large amount of provincial and territorial activity is aimed at increasing access to adult learning by funding the direct delivery of adult learning and education services. The largest such example is support for adult secondary education programs. In all provinces and territories, governments fund the delivery of free or very low-cost programs to help low-skilled adults complete or upgrade their secondary education. Often these are delivered through local school boards, publicly-funded post-secondary education (PSE) institutions or distance-education mechanisms. Manitoba now funds a network of freestanding Adult Learning Centres across the province that offers basic skills education and secondary certification programs at no cost to adult learners. Federal and provincial governments also fund community organizations and training providers to deliver programs and services for adult learners. Some of these are available to unemployed or low-skilled workers, such as literacy skills training and self-employment training. Other funded training services are aimed at special groups. For example the federal Targeted Initiatives for Older Workers funds organizations to run skills development programs that can enhance the employability of older working age adults. Similarly, federal and provincial governments fund language training, employment skills training for recent immigrants. Bridge training programs also help foreign trained skilled workers meet Canadian qualifications or prepare for certification or licensing exams.

Policy initiatives to increase the supply of affordable, accessible and responsive training may encourage some low-income working age adults to engage in new training, but these initiatives cannot overcome some of the participation barriers that low-income adults face, such as in sufficient time for school, foregone income, or low expectations about the benefits of returning to school possibly based on bad prior experiences. The funded services also tend to be targeted to particular subgroups

of learners — those in need of settlement services as newcomers to Canada, older workers with "obsolete" skills, and those with very low skills in need of secondary-school education. As such, they may exclude adult learners who do not fit into program-specific criteria.

Social insurance-funded measures

Other supports and programs for training of individuals are funded through Canada's EI program that collects employer and employee premiums as a payroll tax. While most of the funds go to income replacement through regular and special benefits under Part I of the Employment Insurance Act, EI funds can also be used under Part II to support certain employment benefits and support measures (EBSMs). In 2007-08, the total amount paid out for EBSMs was $2.1 billion (HRSDC, 2008). Nearly all of EBSM funds are transferred to provincial and territorial governments through bilateral Labour Market Development Agreements for local delivery of active measures (referred to as Employment Benefits) with a small amount reserved for pan-Canadian initiatives, research and the National Employment Service. While unemployed workers who do not meet the eligibility criteria for income support benefits can still receive more limited help with job searches and employment counselling (referred to as Employment Services), only current and recent EI income benefits clients are eligible for any skills training programs funded by EI.

Skills Development programs operate largely like a voucher system. Eligible participants receive direct cash assistance to find and pay for eligible training that can improve their employability. The assistance may include paying for the cost of tuition and learning materials, apprenticeship training costs and income support. Clients might also receive individual or group counselling to help them develop an employment plan, including training. Self-Employment programs offer eligible clients extended income support while they take part in small business development training and start-up programs offered by local business development agencies. Outside of these two measures, EI funds are also used to support wage subsidies, job creation partnerships and targeted wage supplements to increase employment creation and participation for EI clients. These may lead to opportunities for the kind of workplace-based learning discussed earlier in the section on policy to promote employer-sponsored training.

While Skills Development and Self-Employment training make up nearly 90 per cent of all expenditures on employment benefits, very few Canadians actually receive these benefits, as Table A indicates. Because low-income and low-skilled workers are more likely to have poorer labour market attachment, they are less likely to qualify for EI income benefits which require a minimum number of hours of insurable employment. In turn, this means they are less likely to be able to access the training initiatives supported through EI premia even if they have paid into the insurance program at some time. Also, because the primary aim of EI programming is to facilitate a return to employment, much of the eligible training is geared towards relatively short-term skills training that might lead quickly to a job. This is in contrast with, for example, measures to promote participation in PSE programs that take longer but lead to a degree or diploma.

Education tax credits

Education tax credits provide some tax relief to students (or their families) for the cost of tuition and books, or for interest paid on eligible student loans, and also exempt some scholarship income from taxable income. They have been introduced at both the federal and provincial levels.

At all levels of government, tax expenditures on education credits were about $2.1 billion in 2007-2008 (Berger, Motte and Parkin, 2009), with about $1.5 billion at the federal level (Department of Finance, 2008). While the credits are available to any full- or part-time student, most of this tax expenditure reflects transferred credits from students to another taxpayer — most likely a parent. This suggests that most of the benefit of the credits is going to younger students, rather than adults returning to school to upgrade or enhance their education and learning. Moreover, the majority of the benefits from education tax credits is flowing to students from higher-income families (Berger, Motte and Parkin, 2009). These higher-income households will, first, be able to afford

Table A Training under Employment Insurance (EI) Part II Benefits, 2007-08

	Number or %	Expenditure ($ in millions)
Self-Employment (SE) 2007-08	10,155	139.8
Skills Development (SD) 2007-08	142,782	962.6
Total training (SE + SD) 2007-08	152,937	1,102.4
Proportion of workforce receiving EI benefits in average month in 2007 (%)	2.8	
Proportion of workforce receiving EI-funded training in 2007 (%)	0.09	

Source: Statistics Canada, Human Resources and Skills Development Canada (2008), and calculations by SRDC.

more expensive forms of education, generating larger amounts to claim as tuition credits, and second, will have larger tax liabilities against which to claim a reduction. For low- and modest-income households with little or no net tax liability, the tax credits are unlikely to offer any meaningful incentive to invest in higher education.

At the provincial level, Saskatchewan, Manitoba, Nova Scotia and New Brunswick have introduced tax credits for recent PSE graduates (regardless of age) who live in or move to the province. In Nova Scotia, Manitoba and New Brunswick, the credit is a non-refundable amount that can reduce income taxes payable but can't lead to a refund of the difference. Furthermore, in Nova Scotia, the value of the non-refundable credit is very low at just $2,000 compared to the credit in Manitoba where it is worth up to $25,000 against taxes owed (spread over several years following graduation). Only in Saskatchewan does the measure operate as a refundable credit, making it worth up to $20,000 (paid out over several years) to eligible PSE graduates living in the province. However, as Berger, Motte and Parkin (2007) note, these measures are largely aimed at attracting or retaining young graduates in provinces with rapidly aging workforces that are in high need of young, skilled workers. Their effects in promoting participation in higher education among traditional students, let alone working-age adults, are unclear at best.

Direct cash transfers to individuals and families (non-repayable)
These include grants, scholarships and bursaries to students distributed by federal, provincial and territorial governments on the basis of merit or need.

In 2000, the federal government spent a total of about $500 million through research council scholarships, Canada Study Grants (Treasury Board Secretariat, 2001 and 2008), and the Millennium scholarships and bursaries (Canada Millennium Scholarship Foundation, 2000 and 2008). By 2008, that figure had grown to over $750 million through expansion of the Canada Study Grants, larger annual expenditures from the Millennium Scholarship Foundation, and the introduction in 2004 of the Canada Access Grants for first-time PSE students from low-income families. The grants offer non-repayable assistance to certain students with particularly high needs on top of repayable student loans and students must first apply and be eligible for a Canada Student Loan. The scholarships through research councils are generally merit-based and awarded based on performance and/or contributions to graduate level research.

In partnership with the Canada Millennium Scholarship Foundation, provinces and territories also offer needs-based grants to students but almost exclusively restrict these to full-time students and, in many cases, limit eligibility to single students who are still dependent on parental support, thereby excluding working-age adults even if they meet the income test. Only Nunavut and Manitoba have joint access grants with the Foundation that are more geared to adult learners. In Nunavut the grants are available to Nunavut residents and land claims agreement beneficiaries for part-time or full-time studies. In Manitoba, adults who have recently obtained an adult secondary diploma and are continuing in PSE studies (on full- or part-time basis) may be eligible for the Millennium Adult Learner Bursary worth up to $5,000 for students in their first year of a program. In 2009, the federal government introduced a new suite of grants under the Canada Student Grants Program, to replace the programs offered by the Foundation which closed in 2010. The new program includes specific grants for each of low- and middle-income families, low-income part-time students, low-income students with dependents and students with disabilities. According to the Minister's announcement, the policy intent is to offer assistance to larger numbers of students (Human Resources and Skills Development Canada, 2009), even if this meant reducing the per capita amount received by students. In the program's Grant for Part-time Students with Dependents, perhaps the grant most likely to reach working-age adults, the grant is capped at just $1,920 per year. It will not be clear for some time whether or not the new grants will be an improvement over the Millennium grants.

Outside partnerships with the federal government, provinces and territories offer assistance through scholarships, training grants and allowances, income support, and transfers for new apprentices. Ontario, for example, offers several needs- and merit-based bursaries to undergraduate and graduate students but generally only if they are pursuing full-time studies (limited special bursaries are available or part-time students with high needs). Alberta's Heritage Scholarship Fund offers scholarships for First Nations students and cost-shared (with industry) scholarships for apprentices. The province also offers 200 scholarships worth $500 each to recent adult graduates of secondary upgrading or equivalency programs through the Adult High School Equivalency Scholarship available when they enrol in full-time PSE studies. In 2005, Alberta launched the Alberta Centennial Premier's Scholarships as a national initiative to make 25 scholarships of $2,005 each available to all other provinces and territories for local delivery to students. In nearly all cases, the scholarships are available only to full-time students and in several cases (e.g., New Brunswick, PEI, and Yukon) they are available only to young students leaving secondary school. Newfoundland and Labrador offer a merit-based scholarship (the Successful PSE Transitions Scholarship)

for full-time PSE students who were recent graduates of an adult secondary upgrading or equivalency program. Yukon offers grants for full-time PSE students who are not dependent on family support but ties the level of support to their academic achievement in their PSE program. Apprenticeship grants or scholarships are available in most provinces and territories but are usually offered only after students have completed the first year of an eligible program.

Provincial IA programs provide some support for adult education through cash benefits both in the form of income support as well as cash or voucher-like benefits. Training benefits might be used to cover the costs of supports to learning such as childcare expenses, transportation costs or start-up expenses for a new course or program. Voucher-like programs cover the eligible tuition costs for certain skills training or education programs. In most cases to benefit, clients must first apply for IA benefits and meet the associated income and asset-tests. Exceptions include the following:

- Saskatchewan's Provincial Training Allowance pays a modest weekly sum to low-income adult learners enrolled in one of three provincial basic education or skills training programs (eligibility is still subject to income- and asset-testing);
- Quebec's provincial manpower training program offers unemployed adults an employment assistance allowance and reimburses clients for eligible training-related expenses when they take part in full-time training programs;
- Yukon's Training allowance, a means-tested benefit for full-time students in college or approved skills training programs, pays between $100 and $200 per week depending on household size; and
- Alberta's Part-time Training Grant offers low-income workers up to $5,000 per year when they maintain employment income and take part in eligible training programs.

While direct cash transfers may be promising instruments for reaching low-income target populations, many have design features that are likely to reduce their effectiveness as incentives for low-income working age adults to return to education or training. In many cases, the assistance specifically excludes adult students by limiting eligibility to youth or reduces the attractiveness to adult students by requiring full-time participation in education and training. For working age adults with even modest employment, the forgone wages to pursue full-time studies are almost certain to be a large disincentive and none of the cash transfers above are likely to fully replace that income. Similarly, the voucher-like benefits available through provincial income assistance programs generally first require participants to be eligible for welfare benefits, meeting stringent income and asset tests designed to ensure use of the program is limited to cases of last resort. Even among the examples of exceptional provincial programs listed above, there is limited evidence for impacts on participation in adult education and training. For example, a 2000 evaluation of Saskatchewan's Provincial Training Allowance noted that it functions primarily as an income assistance program and found little impact on participation in or completion of training programs as compared with similar adults outside the program (PRA, 2000).

Repayable assistance

For individual learners, the largest group of programs to support higher education in Canada consists of provincial, territorial and federal student loans programs. In most provinces and territories, the federal and regional loans are administered cooperatively. In Quebec, Nunavut and the Northwest Territories, the provincial or territorial program has replaced the federal one altogether.

Student loans programs provide repayable loans to eligible post-secondary students as determined by federal and provincial or territorial criteria. In the majority of provinces, provincial loans are available for only full-time students while federal loans are available for both full-time and part-time studies. Applications to these federal and provincial loans programs also generally serve as the screening mechanism for many direct grants and bursaries mentioned above, such as the Canada Study Grants and Millennium Scholarships. Repayment of the borrowed amount begins only after the student leaves school and the repayment burden has been somewhat subsidized through tax relief (discussed above), interest relief and forgiveness of portions of the total debt. As of August 2009, the interest relief and loan forgiveness will be replaced by ceilings on the amount of repayment, determined as a proportion of earned income as well as by time (generally up to 15 years). The loans themselves have undergone several reforms since *learn*$ave was launched, shifting from a risk-shared financing program whereby loans were issued by private sector financial institutions but the risk was financed by the federal government), to a direct loan program. By way of illustration, expenditures on the federal portion of student loans alone were about $1.0 billion in 2008-2009 (HRSDC 2010) and about 3 in 10 Canadian post-secondary students relying on government student loan programs (Berger, Motte, &

Parkin, (2009)., most of whom would be young student entering directly from secondary school.

Previous research on education financing suggests that low-income consumers are particularly debt averse when it comes to education (Carmichael and Finnie, 2009) and tend to over-estimate the tuition costs of PSE while underestimating returns to PSE in higher earnings (Usher, 2005). A review by de Broucker and Mortimer (2005) finds that low-income students are much more receptive to grants than to repayable loans. Taken together, this strongly suggests that repayable assistance may be a very limited instrument for encouraging low-income adults to return to school. This target group is more likely to over-estimate the costs and how much they would need to borrow and would be less confident in their ability to repay the loan through higher earnings after graduation (Usher 2005). Average student loan debt when leaving school has been estimated to be $24,047 (Berger, Motte, and Parkin, 2007), likely to be a dissuasive amount of debt to anyone with low income and few if any assets. Furthermore, so long as there are loan providers or guarantors, governments run the significant risk of loan recipients defaulting on their loan. National averages on default rates continue to hover at 17 per cent (HRSDC, 2007, Table 10.)

There are also interaction effects between repayable assistance, non-repayable assistance and savings. On the one hand, student loans programs operate as the administrative gateway to most publicly-funded non-repayable assistance. Furthermore, eligibility for repayable assistance is directly reduced by the presence of other assets and student debt burdens may present a barrier to acquiring other formal productive capital at least in the short term. Research by Andres and Adamuti-Trache (2008) suggests that student loan burdens are associated with lower rates of homeownership and one recent quantitative study found that many homeless shelter residents cited student loan burdens as a major source of financial hardship (Robson, 2009). Since working-age adults are likely to have several competing preferences for asset-holding (such as homeownership and saving for their retirement) and are more likely to have dependents, they may be less willing to risk the financial costs of incurring a large debt to pay for an education with uncertain returns.

Assisted education savings

These measures provide saving grants, matching incentives and tax benefits for financial capital saved in Registered Education Savings Accounts (RESPs) or withdrawn under the Life-long Learning Plan (LLP) from Registered Retirement Savings Plans (RRSPs). Generally RESPs are aimed at families saving for a dependent child whereas LLP withdrawals are expected to finance ongoing adult learning.

RESPs can be opened by any adult at any time for themselves or another person but for adult learners there are no cash savings incentives (such as the grants or bonds discussed in Box A) meaning that all contributions must come from private sources, benefitting only from a tax shelter on the investment income earned in the Plan. In 2000, the net federal cost (adjusted for any taxes paid on RESPs) of foregone taxes on RESP savings in Canada was $135 million. In 2008, that figure rose to $170 million. It is reasonable to suggest that most of this expenditure went to families with dependent children in PSE where the family income was in the middle or upper range. In fact, HRSDC's 2003 evaluation of the Canada Education Savings Grant (CESG) (see Box A below and Human Resources and Skills Development Canada, 2003), a matching grant designed to increase RESP contributions, found that families with annual incomes over $80,000 made up more than 36 per cent of all RESP subscribers (but just about 17 per cent of the general population) while families with incomes under $20,000 made up just 8.6 per cent of RESP subscribers (but just over one-third of the general population).

The LLP allows an RRSP owner and her or his spouse or partner to withdraw up to $10,000 each in a year, up to a maximum of $20,000. The program shelters these withdrawals from the regular income tax due on RRSP withdrawals when the tax filer is enrolled in a recognized full-time education or training program. The funds withdrawn have to be repaid into the same RRSP within 10 years to continue benefitting from the tax shelter. Neither the Department of Finance nor the Canada Revenue Agency publishes statistics on the number or value of sheltered withdrawals from RRSPs. However, a study by Statistics Canada found that in the first 6 years after the LLP was introduced in 1998, there were 49,000 withdrawals from RRSPs for education and learning worth $363 million (Statistics Canada, 2006). Using this number, the total withdrawals from RRSPs under the LLP are estimated to be $60 million per year. Using a very conservative estimate of a 15 per cent tax rate, this represents just $9 million annually in foregone taxes, making it a very modest public program. Given that RRSP assets rise steeply with household income (Statistics Canada, 2005), it is probable that nearly all of the benefits from this policy measure flow to higher income, higher wealth Canadians.

Neither RESPs nor RRSPs offer much real help to low-income working age adults who may be interested in

Box A Asset-based education savings incentives in Canada

The Canada Education Savings Grant matches family RESP contributions at a basic rate of $0.20 for each $1 saved (up to the annual maximum of $500 in matching grants on $2,500 in family contributions). As of 2004, for low and modest income families, the first $500 saved can be matched at higher rates of $0.30 or $0.40 for each $1 saved. The government grant is only available after a family deposits money into an eligible account for an eligible child. When the account matures and the beneficiary student begins to draw from the fund (in education assistance payments), these are taxed as part of the student's income. In 2004, the federal government also introduced the Canada Learning Bond to encourage more low-income families to save for their children's education. No family contributions are required to receive the initial grant of $500 and then annual top-ups of $100 in each year the child is eligible. However families must open an RESP and the policy aims to kick-start education savings by more low-income families.

Alberta, BC and Quebec now all have similar provincial measures to promote education savings. Alberta's Centennial Education Savings Plan pays grants of $500 at birth and top-ups of $100 each ages 8, 11, 14 into RESPs for all children born or living in the province in 2005 and later. As with the Canada Learning Bond, families must first open an RESP to receive the provincial grant. BC's plan works very differently as funds are saved by the provincial treasury in a pooled account and are then paid out as eligible recipients turn 17 and enrol in PSE. Quebec's Education Savings Incentive program is most similar to the CESG in offering a match of up to 10 per cent on RESP savings up to a maximum of $250 per year in QESI grants or $300 for low-income families.

While these bond and grant measures have much in common with a savings project like ***learn***$ave and, in many cases, were informed by the experience of the design and implementation of ***learn***$ave, none is available to adult learners. In all cases, these education savings incentives are aimed strictly at savings for children in primary or secondary education with the hope that small amounts, saved over time and benefitting from compound interest, can result in significant dollar amounts to promote secondary school completion and PSE planning and participation.

returning to education or training. In the case of RESPs it is primarily because the savings incentives are targeted entirely toward younger students. In the case of RRSPs, the LLP is available only for full-time studies, likely discouraging many adult learners. Furthermore, the tax benefits from RRSP savings are largest for those with larger incomes who can afford larger contributions and who have larger tax liabilities.

Comparing assisted education savings and other policy instruments

Employers might be engaged in contributing to education savings plans but so far this has not been the case in Canada outside of a handful of unionized environments and even now has been the subject of cost-reduction measures (Canadian Autoworkers Union, 2009). Furthermore, given the current balance in how training and education costs are covered in Canada, it is unclear whether public incentives could meaningfully increase employer contributions, particularly in a copayment system with employees.

While in-kind services may increase the supply of adult learning opportunities, it appears that they cannot on their own to generate or support the demand-side of the equation. Here, assisted education savings may be able to both generate demand for more and better educational opportunities for adults and other students, and to provide support to cover the financial costs of learning. Direct transfers or grants appear to be a critical instrument for reaching low-income learners and are not incompatible with education savings, as the Canada Learning Bond demonstrates. However, they may not have the same attitudinal or behavioural impacts of a savings-based instrument if the latter is able to strengthen individual aspirations and commitment to attain a future goal through the mechanism of regular deposits towards that goal. As compared with social insurance-funded approaches, assisted savings need not be tied to eligibility for particular income benefits or to past labour market participation and may be more flexible to life-course and labour market changes over time. As compared with repayable assistance, matching savings as deposits are made may be a more efficient approach per dollar of government funding than providing up-front loans and trying to collect later on. Furthermore, the evidence (Bynner and Paxton, 2001; Andres and Adamuti-Trache, 2008) suggests that debt can have adverse effects on life outcomes following the transition out of school into the workforce.

However, assisted education savings instruments do enforce a delay between the decision to plan for a return to school and access to the public benefits that support that decision. For low-income families, it will take time, even with generous savings incentives, to save up an amount sufficient to cover most or even some of the expenses related to education or training. The intervening period may increase the likelihood that life course events, such as job changes, illness, family changes or others, will overtake the original plan, leaving the saver with

less steam to reach his or her educational goal. That said, this is a risk for any policy instrument relying on delayed gratification for beneficiaries.

International experience with learning accounts for adult learners

The purpose of this section is to look at and learn from measures and approaches used in other countries to support of adult education and training. In his discussion of international approaches to support adult learning, Schuetze (2005) suggests there are three basic types of models:

- integrated model in which a single system is used for all learning activities;
- coordinated model in which multiple systems are used for different learners or activities but together form a coordinated approach;
- complementary model in which separate approaches are used for different learners or activities but where these measures do not intersect or link up.

Integrated systems, often associated with Nordic countries, envisage all learners entitled to free or very low-cost education and tend to rely heavily on covering the costs of education delivery and subsidizing the incomes of learners. Using Schuetze's typology, Canada appears to have a primarily complementary approach to financing adult learning. Introducing learning accounts into the mix is unlikely to change this. The academic and applied literature has tended to discuss learning accounts and vouchers as nearly interchangeable. However, Schuetze characterizes vouchers as "coordinated" approaches, whereas measures such as income-contingent loans or learning accounts are characterized as "complementary." This distinction is important because much of the international experience and evidence to date regarding learning accounts is, in fact, evidence regarding voucher-like systems that are very different from co-financed learning accounts (CEDEFOP, 2008). Voucher systems require little or no contribution from the recipient, instead allocating a prescribed benefit that can be drawn-down within certain limits (usually related to the duration, dollar value or supplier of the education or learning experience). By contrast, *learn*$ave and other account-based mechanisms leverage individual contributions alongside public or employer contributions in a savings account that can then be drawn down in a similar fashion to a voucher. Both are demand-oriented approaches that empower learners to make their own choices about their own training (Schuetze, 2005). The key difference lies in the degree to which beneficiaries are required to make their own investment (CEDEFOP, 2008).

While learning accounts have been explored or piloted in 10 countries, evidence and best practice regarding the use of savings accounts to finance individual learning was, until the *learn*$ave project, very slim. In the sections below we briefly review international examples of so-called individual learning accounts from the countries where more information has been made available: the United Kingdom, Italy, Sweden, the Netherlands and the U.S.

United Kingdom

Individual Learning Accounts (ILAs) were proposed in the UK as early as 1998 as part of a national green paper on skills and education released by the Labour government (Gravatt, 1998). The intent of the policy was to widen participation in adult learning, particularly among working age adults with lower skills and less formal education and to drive improvements in the supply of adult learning by generating new demand. Following a brief pilot phase, ILAs were made available across England as of September 2000 with comparable initiatives in Scotland and Wales, managed by the respective regional education authorities (Gray, Peters, Fletcher, & Kirk, 2002). In England, the first one million accounts opened were eligible for a £150 contribution towards eligible learning activities so long as the adult learner contributed £25 of their own funds. The program also offered a further 20 per cent reduction against tuition fees for eligible courses, up to an annual maximum of £100 and an 80 per cent reduction against tuition fees for specific literacy, numeracy and information technology courses prioritized by the government, up to a maximum of £200 per year. The accounts were distributed through a network of local ILA Centres in England. Participants in the program received an account card which they could use at any participating training provider, either drawing down their credit or claiming their discount. Providers in turn received refunds for the discount or credits through a government web-based system.

An interim evaluation report (Gray et al., 2002) for the then Learning and Skills Development Agency administering the ILA program looked at the program results up to March 2001, by which time more than 680,000 accounts had been opened. Similar to results from IDA programs in the U.S., ILA participants were more likely to be women (58 per cent of accountholders), in their 30s or early 40s and employed full-time (63 per cent of accountholders). The interim evaluation concluded that the program had been well-received by the general public because it was a universal rather than targeted measure but did note

that generally only adults who had already decided to return to formal education opened accounts, a conclusion backed up by the finding that the 37 per cent of accountholders had learned about the program through an education or training provider. The same ILA program encountered two significant challenges: First, while the rate with which accounts were opened was impressive, accountholders appeared very reluctant to make use of the benefits to which they were entitled. In fact Gray et al. (2002) reported that some 60 per cent of accounts opened remained unused but it is not clear why this was the case. The second and even more serious concern, which eventually led to the suspension of the program, was that large numbers of accounts were being misused to defraud the government. Some training providers were found to have offered questionable training and some accountholders were found to have claimed funds for questionable purposes, perhaps sharing in some portion of the refund offered to the training provider (Schuetze, 2005). The parallel programs in other regions of the UK were suspended at roughly the same time.

More recently, and on the heels of a major national review of skills and learning policy, ILAs are again being introduced in the UK. Scotland's education authority first re-introduced a learning account scheme in late 2004 (Scottish Government, 2008). Like the original ILA, the goal was to broaden participation in adult learning, to use demand to improve the supply of adult learning, and to promote individual ownership and investment in their own learning and skills development. The program, called ILA Scotland, offers eligible learners with low incomes a voucher-like account, similar to the original British ILA, worth between £200 and £500 per year, depending on the nature of the course. Since it was launched, some changes have been made such as cancelling the £10 personal contribution, lowering the age limit from 18 years to 16 years of age, and cancelling a universal £100 per year account previously available to all adults regardless of income.

The evaluation of the program included a longitudinal survey of ILA accountholders as well as analysis of administrative data. It found that between 2005 and 2007, a total of 91,000 learning accounts were opened in Scotland and a total of 56,500 courses were taken using the accounts, with the pace of both account openings and courses taken growing over time (Scottish Government, 2008). This undoubtedly reflects spreading familiarization with the program through word of mouth, which was the experience of *learn*$ave and new programs generally. Again, most accountholders are women (72 per cent), adults in their 30s or 40s, and are working full-time (41–81 per cent depending on the type of account). Finally, as with the original program, most participants in ILA Scotland cannot be characterized as "new" or reluctant learners: only 14–16 per cent of accountholders had engaged in no formal learning since leaving school prior to entering the program.

In England, an Adult Learner Accounts program was piloted in 2007 and eventually transitioned into the new Skills Accounts program which is being phased in until it is available to all adults aged 19 or over in England by 2010 (United Kingdom, 2009). The accounts are administered by local skills councils and are targeted primarily at unemployed or underemployed adults. The accounts do not actually confer any new financial benefits on participants but instead are a way of simplifying and coordinating various other funding sources for which they might qualify. Participants who open an account work with a counsellor to develop a personalized learning plan as well as a voucher pre-approving them for funding to take certain courses. The account is also expected to serve as a way for participants to keep track of their own learning with a strong emphasis on certification of certain skills or credentials.

In large measure, the Skills Accounts program is essentially an exercise in sign-posting: offering information and referrals tailored to the needs of the individual client. It does not leverage any individual investment like a matched savings account, nor does it offer new money like a traditional voucher. But the idea of a seamless way to organize personalized information about learning opportunities, funding and certification is intriguing. It remains to be seen whether this way of constructing and delivering an account-based program can have a real impact on participants. The concerns regarding abuse in the original ILA scheme notwithstanding, the UK experience with ILAs suggests that adults will use new incentives for adult learning when they are offered. It is not clear, however, whether or not the incentives are meeting the policy objectives of broadening demand for or improving the supply of adult education in the UK. Furthermore, because individual contributions have been so small or even eliminated, the UK experience with ILAs cannot inform policy-makers interested in savings-based approaches.

Italy, Sweden, and Netherlands

In Italy, learning accounts have been piloted in Tuscany through local training authorities (Cattini, 2007). The pilot was originally launched in 2006 and planned as a one year project but was eventually extended. In total 2,800 accounts were made available across four participating provinces. The project aimed to ensure at least 10 per cent of accountholders were non-standard workers

(for example part-time workers, dependent contractors and self-employed persons) and each province also established its own priority target groups. Each account came with a maximum allocation of €2,500 to be spent on vocational training within two years. Accounts were linked to a debit-like card available through local financial institutions. As with the UK experience, the majority of those who availed themselves of the program were women. Moreover, in line with the UK experience, a very early assessment of the program concluded that significant staff time within sponsoring agencies was needed to monitor the use of the accounts and avoid fraud or misuse.

Learning accounts have been discussed and debated in Sweden but never introduced outside of a small program offered by one employer. After four years of consultations with stakeholders, the Swedish government had introduced legislation to create a national system of individual learning accounts in 2002. The bill proposed a new tax deduction (labelled a competence premium) to be made available to adults who saved and withdrew funds from a designated account to fund a learning activity. The deduction or premium would only be made available once accountholders had used their own funds for the adult education and would only be paid out to a set maximum once in every three years. According to Schuetze (2005), the Swedish policy-makers expected as much as 10 per cent of the national labour force to open learning accounts within the first 10 years, but it topped out at account-holding among 30 per cent of the labour force after 10 years following their introduction.

Haukka et al. (2004), however, points out that the policy was not progressive in its design: there was little incentive for those with low incomes to participate in a saving scheme given a more restricted capacity to build certain target-sized assets to cover the costs of higher education. Concerns were raised among stakeholders about the potential for the policy instrument to create incentives for youth to postpone their plans for post-secondary education to take advantage of a windfall gain. As a result, in the design phase, the accounts were restricted to adults aged 25 and older. Given evidence in Canada and abroad of increasing tendencies among youth to enter, leave and re-enter school over extended periods of time (see, for example, Beaujot and Kerr, 2007), it is not clear that such a restriction is either desirable or practicable. Concerns were also raised among employers about their ability to influence the use of the education or training funds when they were contributing to the accounts. This latter point was never adequately addressed in the policy design stage and the scheme was ultimately abandoned in the Swedish parliament by 2004.

In the Netherlands, eight pilot projects of learning accounts have been tried in three different types of organizations: sector councils, regional education authorities and regional education bureaus (Haukka et al., 2004). Each pilot site offered up to 150 accounts to participants under one of two models: the first saw case managers deal with individual clients to determine learning needs and set educational investment plans; the second saw case managers deal with individual clients as well as clients' employers to negotiate a personalized training plan. In all cases, accountholders were expected to demonstrate relevance of the training courses (full educational programs were not eligible) to their employment-related skills and were not permitted to use the funds to pursue personal interests or recreational courses. Similar to the Italian model, the accounts worked more as a limited personal expense account and no individual contributions were required of the beneficiaries of the accounts.

Haukka et al. (2004) also concluded that the pilots were successful in reaching low-skilled workers who would otherwise have been unable to take part in training. They noted that the success of the pilots was evident in both small and large firms and that pilots were most successful when they adopted a sector-specific approach in recruiting participating employers. Finally, the Dutch experience suggests that there was significant value to having a neutral third party case manager provide advice on the training needs of individual account beneficiaries but that accounts were best managed (in terms of outcomes on uptake of benefits, frequency of cash-outs, etc.) when left in the hands of individuals rather than jointly negotiated with employers.

United States

Two pilots of account-based programs to support adult learning have been tried in the U.S. In contrast with the international experience reviewed above, at least one pilot (the Life-long Learning account or LiLa implemented by the Council for Adult and Experiential Learning or CAEL) has used a matched savings model that could be comparable to the *learn*$ave program design. As noted earlier, most international programs have in fact been voucher systems rather than matched savings-based instruments. Before turning to the CAEL pilot project below, it is instructive to look at a series of voucher-like pilots launched within the U.S. federal government between March and September 2000 (U.S. Office of Personnel Management, 2001).

At the recommendation of the President's Task Force on Federal Training and Technology, 12 federal agencies ran 15 different pilot programs of individual learning

accounts for their civil service employees. Some pilots were available to all agency staff while others were targeted to workers with particular skill profiles, particular occupational categories or certain levels of prior education. While potentially very large given the size of the U.S. civil service workforce, the pilots in fact only saw 6,000 accounts opened. As with the European examples discussed above, the U.S. government pilot did not generally ask employees to make personal financial contributions (one exception was the pilot at the U.S. Department of Defence that paid 75 per cent of eligible tuition fees but required participants to cover the remaining 25 per cent). Moreover, in many cases, financial contributions were not even paid by the sponsoring employer. Across the pilots, some employers used a dedicated source of funds to offer training vouchers to employees while others had no budget and offered only paid time off to employees to take part in free or low-cost training opportunities. According to the U.S. Office of Personnel Management (2001) and Public Policy Associates (2006), the pilots found that significant marketing and outreach efforts were needed to recruit employees into the programs, that very few of the participating employees were in low-income brackets (either individually or as members of a low-income household), and that the link to the employers' own goals was often unclear.

The CAEL LiLA project ran from 2001 to 2007 as a demonstration of learning accounts in partnership with 80 employers of various sizes (range of 3 employees to 5,500 employees), in 3 U.S. cities and across several different industrial sectors. The 80 participating employers were each assigned to a comparison or a program group (43 in the comparison group and 37 in the program group). Participating employers allowed CAEL to give on-site to employees information on the process to take advantage of a LiLa account and allowed the pilot accounts to be available to all of their employees, regardless of income, education or skill level. This latter decision was taken to avoid stigmatizing employees while at the same time using employer recruitment to try to reach low-wage and low-skilled workers. Both employees and employers were expected to contribute to the account and CAEL in turn matched these contributions at a 1:1 rate to a maximum of (U.S.) $1,000 so that accountholders could accumulate up $2,000 per year for training purposes. Employees were expected to contribute at least (U.S.) $120 in a year to be eligible to receive both the employer and CAEL match but could not contribute more than (U.S.) $500 per year to their own LiLa account. These amounts were annual and employees were able to save again and accumulate more LiLa account benefits following a cash-out in a previous year. The funds (matched and unmatched) were to be used for only career-related training and education including certain supports to learning such as the purchase of a personal computer or educational supplies, but excluding other related costs such a transportation and childcare expenses. Each participating employee was expected to develop a personal learning plan in consultation with his or her employer, giving participating employers some assurance that they would see a return on their investment in employee education.

According to a 2006 evaluation conducted by Public Policy Associates (2006), average quarterly deposits into the LiLa accounts were, depending on the sector, between (U.S.) $52 and (U.S.) $85, well above the required minimum of $120 annually. The evaluation also found that participating employees responded very strongly to the offer of career counselling services and that accountholders were very likely to attribute their decision to take training to their LiLa account experience and, relative to the comparison group without LiLa accounts, were more likely to report an intention to take further training following the pilot program. However, as pointed out, it was the higher-earnings employees who intended to participate in them. The evaluation found that there was a general trend towards increases in both personal and household income among accountholders over the duration of the pilot but the source of the increase was not entirely clear since there was only uneven evidence for any increase in account holders' employment wages. Finally the evaluation concluded that there was evidence that employees with accounts did in fact participate in more training than those without and that, despite employer concerns, there was little evidence of any increases in employee turnover. Previously, some employers had worried that by investing in the skills of their employees, they would see an increase in attrition and turnover as better skilled employees left to find better pay for their new level of skill.

Conclusion

Based on the review of domestic practice in Canada and experience with so-called learning accounts for adults abroad, it is clear that no prior pilot or program has yet tried to use a matched savings instrument to try to boost participation in training and education among adults. In fact, only one prior pilot can be truly said to have used a matched savings instrument (the CAEL pilot) and in that case the three-party structure (employee/employer/CAEL) meant that the orientation and priorities of the program model were very different (employment-centric) from *learn*$ave's. If savings-based instruments have any promise as adult education and training policy tools, then they need to be tried and rigorously tested in a wide range of settings and structures. Caution should be exercised in drawing conclusions from voucher-like

systems on the efficacy of savings-based instruments and similarly in introducing savings-based instruments on a permanent basis within existing Canadian adult learning policy until and unless they can show their merit. Ideally policy-makers will want to compare results across types of programs and pilots to best understand which instruments work best for which kinds of adult learners and under what circumstances. The results from the *learn*$ave demonstration offer an important first step towards this goal.

Appendix B *learn*$ave financial management training curriculum

This appendix provides a more detailed description of the *learn*$ave financial management training curriculum than provided in Chapter 3 of the report. The curriculum combined the concept of Prior Learning Assessment and Recognition (PLAR)with the more standard elements of financial management training. The PLAR element was intended to help participants recognize existing skills and personal attributes that they possess and that would help them achieve their goals. As part of PLAR, participants were asked to identify barriers that could prevent them from achieving their goals as well as strategies to overcome those barriers. As for the financial management component, several standard financial topics formed part of the curriculum, including spending patterns and consumerism, household budgets, credit ratings and investing.

In most cases the curriculum was delivered in the form of five three-hour modules; however; sometimes alternate formats were used — such as two modules presented together on a Saturday. In such cases the order of the exercises was often reorganized to make them more appropriate for an all-day session.

In each of the modules the facilitators were expected to cover all of the topics but there was some flexibility to adapt the module to the needs of their groups. For example, facilitators could vary the amount of time on any given topic, engage guest speakers for given topics, and use additional resources such as web sites.

In order to address concerns that site staff had about the curriculum, SEDI convened a workshop in the fall of 2002. Based on the results of that workshop, the curriculum was revised. This summary of the curriculum is based on the revised facilitators' guide and exercises distributed in February 2003.

Module 1: Introduction to learning and *learn*$ave accounts

The first module introduced participants to some of the key concepts of PLAR, which emphasizes that people learn much of what they know outside formal classrooms. Some of the exercises in the first module were intended to help participants recognize the difference between formal and informal learning. As well, the exercises helped participants to "evidence" informal learning and realize how this learning can be transferred to different contexts. Evidencing means articulating and providing proof of skills that have been learned — for example, a participant could show a pair of woollen mittens to illustrate that they can knit.

During the first module facilitators introduced the *learn*$ave portfolio which participants were expected to assemble after the course. The portfolio was meant to provide evidence of the participant's past learning efforts and achievements and to state the participant's future goals. Participants were asked to compile all of the relevant exercises that they had completed as part of the curriculum and include them in the portfolio. They were also encouraged to add additional documents such as certificates that would provide proof of their prior learning success.

The first module included a review of the key aspects of the *learn*$ave account protocols. Participants were also asked about their personal financial situation as part of a "financial fitness quiz." For a home assignment they were given a template and asked to record all of their spending transactions for a period of one week. They were also encouraged to obtain their personal credit report and were given information on how this report can be obtained.

Module 2: Traits, passions, dreams, and goals

During the second module, facilitators asserted that money is a value-laden term. Participants engaged in a discussion about the perspectives that their family, friends and society have on money and how those views have influenced them. They also debated the relationship between spending and making choices. A central issue that often arose in this discussion concerned the extent to which many expenses are fixed or can be varied, i.e., postponed. These discussions led into a discussion about budgeting: facilitators presented templates with which participants could record their income and expenses and a template to summarize the important features of their budgets.

The other component of this module concerned personality type, values and goals. Participants engaged in a series of exercises to help determine their personality type and values. During one exercise, participants were told that they had been invited to six parties and they had to select three of them. Each party was intended for one of the six Holland Code personality types: Realistic/Practical, Investigative/Inquiring, Artistic/Creative, Social/Helping, Enterprising/Persuading, and Conventional/Organizing. Participants were then asked to relate their personality type and values to the skills necessary for their *learn*$ave goal. It was hoped that by learning more about themselves they would be in a better position to make better choices about possible *learn*$ave goals. As an assignment, participants were asked to

interview someone who could give them guidance that would help them meet their savings goal. For example, this person could be an instructor at an educational institution or someone who is working in the field that the participant preferred.

Module 3: Managing money

The third module focused heavily on consumerism. The first exercise asked participants to give examples of some of the "consumerism trends" of the past five decades — for example, a TV set in every home was a trend that began in the late 1950s. Then facilitators discussed contemporary consumerism and the media — they talked generally about the "buy, buy, buy" culture as well as some specific techniques that advertisers use to sell particular products. Participants were in turn asked to think about how the media influences their own spending decisions.

Another component of the module presented the topic of saving and investing. Facilitators handed out a chart showing the amount participants had to save each month in order to meet certain *learn*$ave savings goals. Facilitators usually discussed some of the basics of longer term investing such as rate of return and the risk and return trade-off. As a home assignment, participants were asked to consider several questions related to their *learn*$ave deposit plan such as how much they planned to deposit, challenges that might prevent them from making those deposits, and changes they had to make in order to meet their *learn*$ave savings goal.

Module 4: Managing your credit

One of the key components of the fourth module addressed credit. For this module, participants were encouraged to bring their credit reports to the session — examples were also on hand for anyone who had not obtained their own report. Facilitators spoke about the substance of credit reports and credit bureaus. Facilitators also presented information about the length of time information was kept on file, the credit rating system, how creditors used the information, finding errors on one's file and correcting errors. Participants discussed the smart use of credit — such as the types of credit available and the amount of debt that people can reasonably carry.

Another component of this module helped participants to think about their educational and career goals. Participants were asked to discuss what they learned from the interview about their savings goal that they were asked to set after the second module. They were then asked to answer a series of questions that helped to "evidence" that they have some of the general skills necessary to meet their educational or small business goal.

Finally in preparation for the next module, participants were asked to think about what they had learned so far from the training.

Module 5: Skills and savings — Looking forward and creating your *learn*$ave portfolio

The facilitators usually began this module by discussing the things participants said they had learned from the training. Participants were then asked to write down their *learn*$ave savings goal and their interim goals. Facilitators listed four questions that participants should keep in mind when determining their goals: (1) How realistic is the goal (within reach of the participant)? (2) Is attaining the goal something they really want? (3) How can the goal and its attainment be measured? (4) What will be the reward from attaining the goal?

Participants were then asked to look at some of the exercises from previous modules such as the Holland Codes in order to determine what would help them meet their goals. They were also asked to use previous exercises to identify gaps between their skills, values and abilities and their chosen goal. They were asked to think about a plan to address these gaps.

During this module participants once again completed the financial fitness quiz that they had completed during the first module to measure the progress in their financial management skills since they started the course. Near the end of the module, facilitators presented a possible detailed outline of the portfolio that participants were expected to complete after the course. They were also given a chance to discuss the curriculum as well as what they had learned from other participants.

Appendix C Methodology and data sources for implementation and service delivery research

The implementation and service delivery research findings presented in Chapters 4 and 5 of this report were drawn from several lines of evidence, comprising, each of which is described in greater detail below:

- focus groups with participants and non-participants at a number of sites;
- interviews with representatives of all 10 sites, on-site observations, and exit interviews with 39 participants at the early stages of the project;
- a mini 10-month follow-up survey of participants;
- a market research survey of participants; and
- interviews with about 45 representatives of all sites, SEDI, and the participating financial institutions at the later stages of the project.

Focus groups with participants and non-participants

SRDC conducted two rounds of focus groups during the fall of 2002 and the fall of 2003. The first round primarily addressed questions regarding the recruitment of participants from the eligible population of low-income adults. Table C below summarizes the 12 focus groups that were held with a total of 102 respondents during the fall of 2002 and the 24 held in 2003.

SRDC held focus groups at the three experimental sites — Halifax, Vancouver, and Toronto — and two of the non-experimental sites — and at two non-experimental sites, Calgary and Digby. The sites were selected based on several criteria including their current and past levels of enrolment and geographic location. In each location, two separate types of focus groups were held: one with participants in *learn*$ave, and another with local residents who had inquired about the project, been found to be eligible but declined an opportunity to apply to the project. Because of the large numbers of recent immigrants[1] who had enrolled in Toronto, two extra focus group sessions were held with this subgroup of each participants and non-participants.

Each site office was asked to recruit focus group members from a broad cross-section of *learn*$ave participants and non-participants who had met project eligibility criteria.[2] In recruiting, site staff used their existing telephone and mailing lists to identify and contact participants and non-participants.[3] SRDC provided a telephone screening tool and a recruitment letter to the site offices to promote recruitment consistency across the sites. Focus group respondents were paid an honorarium of $50 to attend the session.

The second round of focus groups addressed questions related to saving, as well as the role played by the case management and financial management training services offered to participants in the project. Table C summarizes the 24 focus group sessions that took place in the fall of 2003 in Halifax, Vancouver, and Toronto with 147 respondents in total.

To ensure focus group respondents would have had sufficient time to attempt to save, participants invited to the sessions had to have been enrolled in learn$ave for nine months or more by the end of August 2003. Separate focus groups were formed of those who saved regularly and those who did not save regularly. "Regular savers" were defined as those who had made a net deposit of at least $10 in at least 75 per cent of the available months since they enrolled in the project. "Irregular savers" were defined as those who had made deposits in less than 50 per cent of the available months. Recent immigrants in the project were also invited to focus groups.

To accommodate these variations, six types of focus groups took place in the second round (see Table C above). Using information from the PMIS, SRDC provided the three site offices with lists of potential participants for each of these six types of focus groups. The site offices recruited focus group members from these lists based on a telephone screening tool and a recruitment letter developed and provided by SRDC. Again, focus group participants were paid an honorarium of $50 to attend the session.

Because the focus group research is of a qualitative rather than quantitative nature, responses from these groups cannot be interpreted to reflect the views of the overall project sample or beyond. However they are useful in helping to explain the reasons for the findings presented later on.

1 "Recent immigrants" were defined for the purposes of the project as individuals who had landed in Canada within the previous 24 months before they inquired about the project.

2 Income assistance recipients were not included in this round of focus group discussions. In spite of the intention to invite non-participants who were eligible, there were nine non-participants who indicated during the discussions that they had not met the eligibility criteria. Efforts were then made to adapt the focus group questions to accommodate these individuals through exploration of the experience and opinions of those who had shown interest *learn*$ave but had been unable to enrol due to project criteria.

3 The site offices had kept lists of people who had enquired about *learn*$ave and/or had attended an application session.

Table C Summary of *learn*$ave Focus Groups Held in 2002 and 2003

Location	Groups	Notes
2002 Focus Group Sample (12 groups)		
Experimental sites		
▪ Halifax	▪ *learn*$ave participants (1), eligible non-participants (1)	Groups of participants and non-participants conducted separately
▪ Vancouver	▪ *learn*$ave participants (1), eligible non-participants (1)	
▪ Toronto	▪ *learn*$ave participants (1), eligible non-participants (1) ▪ *learn*$ave participants who are recent immigrants (1) ▪ eligible non-participants who are recent immigrants (1)	Conducted in Toronto only, based on recruitment patterns
Non-experimental sites		
▪ Calgary	▪ *learn*$ave participants (1), eligible non-participants (1)	
▪ Digby	▪ *learn*$ave participants (1), eligible non-participants (1)	
2003 Focus Group Sample (24 groups)		
Experimental sites		
▪ Halifax	▪ *learn*$ave-only regular savers (1) and irregular savers (2) ▪ *learn*$ave-plus regular savers (1) and irregular savers (1)	
▪ Vancouver	▪ *learn*$ave-only regular savers (3) and irregular savers (2) ▪ *learn*$ave-plus regular savers (2) and irregular savers (1)	
▪ Toronto	▪ *learn*$ave-only regular savers (2) and irregular savers (2) ▪ *learn*$ave-plus regular savers (2) and irregular savers (1) ▪ recent immigrant *learn*$ave-only regular savers (2) ▪ recent immigrant *learn*$ave-plus regular savers (2)	

Interviews with site representatives, on-site observations, and orientation exit interviews

In the early stages of the project, SRDC conducted in-person interviews with nearly all project site staff and mangers between June 2000 and April 2003. Interviewees were asked to comment on recruitment, the financial management training curriculum, cash out procedures, and other aspects of project delivery. During these visits as well, SRDC observed information/application, orientation, and financial management training sessions.

In addition to interviewing site staff and managers and observing project sessions, SRDC conducted exit interviews with *learn*$ave-only and *learn*$ave-plus participants following their orientation session. A total of 39 applicants were interviewed in these exit interviews.

Ten-month follow-up survey

In order to gain information about participants' subjective experiences in the project, SRDC conducted an additional short telephone interview of *learn*$ave-only and *learn*$ave-plus participants.[4] This interview, conducted by POLLARA Incorporated, took place approximately 10 months after the baseline interview. The purpose of the survey was to determine participants' knowledge of key *learn*$ave rules, their savings techniques and challenges, and their satisfaction with the project.

The sub-sample for this mini-survey was drawn from both program groups (but not the control group) in all three experimental sites. In order to obtain the necessary number of interviews, all Toronto participants who completed their baseline interviews between September 2002 and May 2003 were contacted for the 10-month interview between July 2003 and March 2004. All Halifax and Vancouver participants who completed their baseline interviews between September 2002 and July 2003 were contacted between July 2003 and May 2004. Overall 1,139 participants were contacted and 868 interviews were completed, resulting in a response rate of 76 per cent. The numbers for each site were roughly in proportion to their numbers in the overall sample for this project.

Market research survey (MRS)

SRDC undertook a two-phase market research survey between April and June 2003 in order to obtain more information about reactions to *learn*$ave in the eligible population who were not in the project. The first purpose of this study was to determine the proportion of the

4 The original research plan included a number of questions on project satisfaction as part of the 18-month interview. However, these questions would have made the 18-month interview too long.

eligible population who were already aware of *learn*$ave and to ask for their impressions of *learn*$ave. The other purpose was to examine people's reactions when they heard about *learn*$ave for the first time and determine how many would apply for *learn*$ave once they knew about the project. This information is helpful in understanding the general appeal of the project, examining the effectiveness of recruitment strategies and understanding issues that arose in recruiting participants.

The goal was to reach a representative sample of the population in Toronto and Vancouver who would have been eligible for *learn*$ave. While it may have been methodologically preferable to call any number in these cities at random and determine whether the respondent was eligible for *learn*$ave, this approach was deemed to be too labour intensive and costly. In order to improve the efficiency of the surveying process, only people in low-income neighbourhoods were called. Where a respondent consented to the interview, they were asked a series of questions to determine whether or not they were eligible for *learn*$ave — those who qualified completed the full survey.

For each postal walk in the City of Toronto and the Greater Vancouver Regional District, Statistics Canada's Small Area Data Division calculated the proportion of the population below the Low Income Measure (LIM) based on people's income tax records. POLLARA chose one-quarter of the postal walks[5] that had the highest proportion below LIM and then compiled a list of all available phone numbers in the areas. From that list, POLLARA randomly selected the phone numbers to be called. Almost 70,000 numbers were dialled in order to reach 7,855 people willing to complete the interview.

Upon answering the phone, respondents were not immediately told about *learn*$ave. Rather they were told that the interviewer was calling from POLLARA on behalf of a project sponsored by the Government of Canada. Respondents who were willing to complete the survey were asked about their age, family size, income, liquid assets and student status in an attempt to determine their eligibility for *learn*$ave. Among the 7,855 respondents who agreed to do the survey, 1,259 were likely eligible for *learn*$ave — an eligibility rate of 16 per cent.

Interviews with site staff, SEDI, and financial institutions

About 45 interviews were conducted as part of the service delivery case study in the latter stages of the project. Over 40 in-person interviews were conducted with *learn*$ave staff and organization directors of the 10 community-based organizations that delivered *learn*$ave as well as the national non-profit organization that oversaw its implementation and delivery, SEDI. Also telephone interviews were conducted with representatives of the financial institutions that partnered on this project.

This study also used secondary analysis of a body of qualitative data collected by SRDC researchers as part of the implementation research process, from re-analyzed and interpreted from the perspective of site deliverer. This evidence included transcripts from focus groups as well as researchers' notes from interviews and observations conducted during site visits throughout the project.[6] These data contributed to a comprehensive picture of service delivery from multiple perspectives.

The qualitative data analysis software program, NVIVO 7, was used in the data coding and analysis, as it was used in the analysis of qualitative data collected as part of the earlier implementation research. Initially, an analysis coding framework was developed to ensure coverage of all themes identified at the outset of the study in the interview questions. The framework was expanded as iterative coding identified more subtle themes and interpretations.

5 A postal walk is the area covered by an individual letter carrier or his or her daily route.

6 Two rounds of focus groups were conducted by SRDC researchers; 12 sessions in the fall of 2002, involving 102 individuals across five sites: Vancouver, Toronto and Halifax, as well as Calgary and Digby. The second round took place during the fall of 2003 and consisted of 24 sessions involving 147 individuals in Vancouver, Toronto, and Halifax.

Appendix D Detailed description of methodology: Missing data imputation, impact estimates adjustment, and response bias testing

This appendix provides details surrounding methodologies used in this report, particularly for results presented in Chapters 6 and 7 of this report. Specifically, it discusses the methodology employed: (1) when dealing with missing and outlier data in the calculation of net worth and its various components, (2) adjusting impact estimates, and (3) determining if survey attrition is affecting impact estimates.

Dealing with missing and outlier data

At the outset, it should be noted that in the survey in which the data were collected attempts were made to reduce missing and outlier data. If respondents could not provide a response to a question asking about the value of an asset or debt or about income, they were asked if they could at least indicate a range within which the value or income laid. For those who did give responses, if the answer exceeded a certain high value specific to the asset or debt, the respondent was automatically re-asked to confirm the value.

While missing data were not a problem for any individual asset or debt question, the fact that a large number of survey questions were used in the calculation of net worth was potentially problematic. Overall, 56 survey questions were used in the calculation of net worth. Respondents were asked not only about the value of specific assets or liabilities but also to state what corresponding share of that value was theirs. For the overwhelming majority of questions, only a small portion of responses was missing. However, technically speaking, an observation could be eliminated if any of the 56 variables is missing. Thus, the odds were high that there would only be a small number of observations available for computing mean net worth.

To further illustrate the potential extent of the problem, even if there were only 10 missing responses for each of the 56 questions (from about 2,600 cases), there would be a loss of 560 observation items when calculating an overall measure of net worth. This was judged unacceptable and it was decided that imputation would be used to minimize the impact of the missing data.

Before describing the approach taken to deal with missing data, it is instructive to consider the types of such data. There are three types: (1) purely missing where no response was given at all; (2) where the respondent indicated "Don't Know;" and (3) where the respondent indicated "Refused." The latter two are not technically missing, as the respondents provided a response, but as they are not data per se, they are considered missing.

There are two main types of imputation: hot deck single and hot deck multiple. Single imputation involves replacement of the missing value of a respondent with a value based on the responses provided by other respondents with characteristics similar to the respondent. A simple version of the single imputation approach is to assign the mean value of all non-missing responses to the missing value.

Multiple imputation (MI) is similar to hot deck single imputation, but involves several rounds of repeated imputation (Rubin, 1987). One of the strengths of this approach is that it takes into account both model variation and additional variation resulting from the imputation process itself. That is, it incorporates the variation that results from the imputation process into the analytical results. The major weakness of MI is that it is the most complicated and resource-intensive form of imputation methodology. In the case of *learn*$ave, this multi-stage approach required the creation of three different datasets (called "implicates") while all analyses were performed three times.

For multiple hot deck, the MI procedure used here, a model was developed to assign imputed values to missing responses. For a particular question, a respondent with missing data was matched to a group of similar respondents without missing data for that question (the "donor pool"). Characteristics used to define "similar" in the matching model used in this study were age, gender, and research group (*learn*$ave-plus, *learn*$ave-only, and control), as it was determined that these were important factors in the possession of assets and debts.[1] For property questions, site was added as a donor characteristic, given the importance of location in determining property values. Specifically, matched participants had to be within five years in age (plus or minus) of each other, of the same gender, part of the same research group, and, in the case of property, associated with the same site location. If there was no donor found within the default age group of plus/minus five years, the interval was widened to as much as 10 years over and under the age of the respondent. For each variable used in the calculation of net worth, responses to the respective question from the

1 Another donor variable considered was current household income but this was poorly answered in the survey. Baseline household income was also considered but it was considered to be too long in the past to be representative of current conditions.

donor pool were randomly chosen and imputed to the variable in question for the particular respondent with the missing data. This was done three times, once for each of the three implicates.

Estimates of summary statistics were computed by simply averaging the corresponding statistic computed from each of the three individual implicates. Variances were calculated using the following equations, as adopted from Rubin (1987):

MI estimate

$$\overline{Q} = \frac{1}{m}\sum_{i=1}^{m} Qi$$

where:
Q_i is the parameter estimate from the ith implicate
m is the number of implicates (rounds of imputation)

Within round variance

$$\overline{U} = \frac{1}{m}\sum_{i=1}^{m} U_i$$

where:
U_i is the variance of the parameter estimate for the ith implicate
m is the number of implicates

Between round variance

$$B = \frac{1}{m-1}\sum_{i=1}^{m} (Q_i - \overline{Q})^2$$

where:
Q_i is the parameter estimate from the ith implicate
$\overline{Q}$ is the MI parameter estimate
m is the number of implicates

Total variation

$$T = \overline{U} + (1 + \frac{1}{m})B$$

where:
T is total variation
$\overline{U}$ is within-round variance
B is the between-round bariance
m is the number of implicates

Degrees of freedom

$$df = (m-1)(1 + \frac{m\overline{U}}{(m+1)B})^2$$

where:
$\overline{U}$ is the within-round variance
B is the between-round variance
m is the number of implicates

One reason for choosing MI methodology is the presence of a small non-response bias in the data. Both the hot deck single imputation and the multiple imputation methodologies assume no systemic response bias, i.e. that the data are missing at random (MAR). In other words, missing observations can be explained by other observed variables and not the variable itself. The data seem to support this assumption: e.g., income level is statistically significant when looking at the incidence of missing data for credit card debt. Although, we cannot fully test the MAR assumption, this finding seems to support it.

A recursive methodology was employed to impute missing data for those who answered "don't know" to the question on the actual value of specific assets and liabilities. People who answered "don't know" concerning a value of a particular asset or debt were asked a series of follow-up "bounds" questions to attempt to at least determine the range in which the value fell. Imputation started with the last (narrowest) bounds question where data were missing and worked "backwards" using the bounds questions to select an appropriate donor pool from respondents in increasingly broader bounds. Where the bounds information was specified by the respondent, the donor pool was adjusted to ensure the imputed value fell within the bounds given by the respondent.

Sensitivity testing involving an examination of the MI results in comparison to the pre-MI results revealed that there were no real major changes to the net worth and component values computed without the missing data. The mean values of all components of net worth containing multiple imputed missing data, apart from business assets, were within 10 percentage points of the values computed from the raw data, discounting observations with missing data. Note as well that, in computing composite variables, any missing values remaining after imputation were assumed zero. Further checking found no substantial difference in estimates with or without this assumption.

Sensitivity testing was carried out without the top three values and without the top one per cent of values. This was done to determine whether or not the findings were strongly influenced by particular outlier observations. The results of the analysis indicated that, for statistically significant impacts, there were no changes to the sign (i.e., the direction — positive or negative — of the impact) and strength of significance, though some non-trivial differences in the impact estimate did arise.

Another potential concern regarding net worth data is the presence of outliers. This is a common problem when collecting dollar-value data. Values that were viewed as

particularly unlikely were altered "by hand" based on values of other assets and income of the respondent in the same survey as well as earlier surveys.

Further to this, it was discovered that for property assets/debts in particular, MI of missing values led to some impossible results, such as high mortgages with zero property value. Because property represents such a large proportion of net worth and relatively few people actually own property, it was determined that the data should be investigated further, to ensure there were no "impossible" values. Additional analysis determined that a source of the discrepancy lay in the share question. Respondents were asked if they shared the asset or debt and how much their share was, and this was not always answered well. Some indicated they shared the asset but not the corresponding debt, or did not answer the question on asset after indicating they possessed the corresponding asset, which led to large discrepancies. Each case was dealt with on an individual basis.

Values of property assets and debts were further checked to ensure logical consistency, i.e. no large negative net values of property. MI was restricted to produce non-negative net value of property. A few cases of problematic asset share and debt were analytically corrected using information from previous surveys. There were also a few cases of outliners of property values, which were checked against the property values around the respondent's neighbourhood to eliminate input errors by the data collection agency. A record of edits made was maintained.

Regression adjustment of impact estimates

The main body of this report presents regression-adjusted results. Unadjusted impacts were estimated by calculating the difference between the mean outcome levels of the program and control groups. Adjusted results were generated by estimating a regression in which the outcome variable was modeled as a linear function of the respondents' research group and a range of socio-economic and demographic characteristics measured before random assignment. Although random assignment ensures that there are no systematic differences between the program and control groups, small differences can (and did) arise by chance — particularly in smaller samples. Also, as a result of survey attrition in the 54-month follow-up survey, small differences between the program and control groups were introduced.

Regression was used to adjust the (unadjusted) impact estimates for differences between the program and control group at baseline and at 18 months (that arose from unbalanced attrition). The reasons why linear regression could be used are twofold: the size of the sample was large and the co-variates in the adjustment model had very little explanatory power over and above the research group variable. The adjusted estimate of the impact was derived from the coefficient on the research group variable in the estimated model. Two-tailed t-tests were used to determine the statistical significance of the estimates.

There are two main advantages to regression-adjusted impact estimates. First, given that observed baseline differences between the program and control groups can be accounted for, the regression-adjusted impact estimates are potentially more accurate than the unadjusted mean differences in outcomes. Second, even in the absence of differences at baseline, regression adjustment can improve the statistical precision of impact estimates. Standard errors of regression-adjusted estimates of the program's impact may be lower (when correlation between the characteristics and the outcome is accounted for in the regression), which results in improved statistical power.

Regression adjustment has some drawbacks, however. One of the main disadvantages is that adjusted estimates are not as well understood and not as easily interpreted as unadjusted results (the latter being simple differences between the program and control groups). Also, for many outcomes, the improvement in statistical precision achieved through regression adjustment is typically quite small (and was proved in this case). Nevertheless, because regression adjustment capitalizes on the wealth of information generated and brings greater precision to the estimates, the decision was made to present regression-adjusted impact estimates in this report.

In total, each outcome variable observed at 54 months (the "dependent" variable, corresponding to each of the outcomes discussed in Chapters 6 and 7 of the report) was regressed on a set of variables that comprised the research group plus 14 co-variates (the "independent" or explanatory variables). Both continuous and binary explanatory variables were included in the model, all of which were measured using the application form or the baseline survey administered prior to random assignment.

The explanatory variables, measured at baseline, comprise the following:

- Research group (*learn*$ave-plus, *learn*$ave-only, and control)
- *learn*$ave site
- Gender

- Age group
- Highest level of education (attained prior to project enrolment)
- Marital status
- Whether or not there were children under 18 years of age in the household
- Immigration status
- Whether or not activity limitations were reported (disability)
- Labour force participation (employed by others; self-employed; unemployed or out of the labour force)
- Household income (during year before project enrolment)
- Monthly payments for household expenses
- Difficulty making payments
- Whether or not there was a household budget

- Future time perspective

The regression adjustment procedure used the PROC GLM command in the Statistical Analysis System (SAS). The GLM procedure uses the method of ordinary least squares to fit General Linear Models. This was applicable even in the case of binary outcome variables where bias could arise from using linear regression.

Response bias testing

This section is focused on the question of whether or not survey attrition — a phenomenon common to longitudinal surveys such as in the *learn*$ave project — has created any bias in the data used to observe trends and estimate impacts of the intervention being tested in this project. In this section, the analysis is limited to the 2,269 *learn*$ave enrollees at baseline who completed the 54-month survey, which includes 842 *learn*$ave-only group members, 859 *learn*$ave-plus group members and 568 control group members. This represents a 63.3 per cent survey response rate from the original baseline sample of 3,584 enrollees. However, the response rates were appreciably higher in the program groups (70.5 per cent and 71.9 per cent in the *learn*$ave-only and *learn*$ave-plus groups, respectively) than in the control group (47.5 per cent).

The substantial difference in response rates between the program and control groups in the 54-month survey raises a concern as to whether or not the groups remained comparable and therefore able to generate reliable estimates of impacts. As shown in the *learn*$ave implementation report (Kingwell et al., 2005) random assignment was implemented successfully as there were no systematic differences between program and control groups at baseline. While there were some differences due to sampling variation, it was determined that they would not result in biased impacts if every person in the baseline sample responds to follow-up surveys. However, not all participants did respond to follow-up surveys (e.g., 36.7 per cent of the baseline sample did not respond to the 54-month follow-up survey). This could affect impact estimates if the non-response occurred systematically, i.e., if it was concentrated in a certain subgroup of the sample. If the composition of the samples was different from survey to survey, estimates derived from the different waves would not be directly comparable. More importantly, if non-response affected program and control groups differently in a survey, the estimated program impacts derived from the survey sample might be biased.

While there is no direct way of assessing of the severity of non-response bias, observable characteristics of respondents and non-respondents can be used to evaluate whether there is systematic differences in survey attrition. Here, the extent to which estimates may have been affected by potential non-response bias by comparing the baseline characteristics of: (1) respondents in the 54-month and baseline survey samples; (2) respondents and non-respondents to the 54-month survey, across program and control groups. Ultimately, if there is no substantial difference in baseline characteristics of respondents between program and control groups (collectively known as research groups), non-response is likely to be independent of membership in these groups, and estimated impacts are not likely to suffer from non-response bias.

The first question to address is whether or not non-responses to the 54-month survey were distributed randomly and independent of observed characteristics for participants. Specifically, how different were follow-up survey respondents from the original baseline sample? For each subgroup characteristic, examining the first two sets of columns in Table D.1 by research group (e.g., males in the control group at 54 months compared to males in the control group at baseline) indicates that 54-month survey respondents were more likely, than the baseline enrolees, to be women, married, have one or more children, or hold a university degree. However, overall, the differences between these two samples are not great nor were they large between other survey samples (not shown). Thus, differences in impact estimates across survey samples should be interpreted with some caution since some of the difference may have been the result of unbalanced survey attrition.

The second question to address is whether or not certain subgroups of participants responded differently between the program and control groups. In Table D.2, differences in characteristics among respondents are compared to those of non-respondents for each research group and subgroup characteristic. For each subgroup

characteristic, a statistically significant difference between the inter-group differences of respondents (in the column headed by Diff-R) and the inter-group differences of non-respondents (in the column headed by Diff-NR is indicated by daggers (†) in the columns headed by R-NR.

The results indicate that sample attrition appears to vary by research group. Specifically, for example, control group members who were lone parents, were Canadian born (not shown), or had a high school certificate or some postsecondary education were more likely to respond to the 54-month survey than their counterparts in the *learn*$ave-only group. Conversely, control group members with a university degree or those who were out of the labour force were less likely to respond than those in the *learn*$ave-only group. Also, participants who were under the age of 21, single, Canadian-born (not shown); or who had some postsecondary education but no university degree; or who were self-employed or whose household income was between $20,000 and $25,000 were more likely to respond when they were in the control group than when in the *learn*$ave-plus group.

However, these statistically significant differences in characteristics also reflect the small differences in the baseline sample at the outset and subsequent survey non-response might serve to exacerbate or correct some of these differences. In fact, it appears that 54-month survey non-response helped to better align the characteristics of participants in the program groups with those of the control groups. An example is married participants who represented a greater share of participants in the control group than in the two program groups at baseline, but who represented similar proportions of 54-month survey respondents across the three research groups. In other words, the higher sample attrition of married participants in the control group helped to make all three groups more similar. There were also fewer control group members with some postsecondary education than members of the program groups at baseline, but this was "reversed" in the 54-month survey sample such that differences in the proportion with some PSE were no longer statistically significant.

Examining the differences between program and control groups in the 54-month survey sample (in the columns of Table D.2 headed by Diff-R) reveals several small differences in characteristics. These differences, however, do not necessarily indicate the existence of substantial systematic differences between the groups. One way of determining if there are systematic differences between program and control groups is to use ordinary least square to regress binary (0-1) group membership indicators on observed characteristics. If there is any systematic difference in the characteristics, the estimated model would not be able to predict whether a participant belongs to one group or the other. The goodness-of-fit test of the model is used to test whether there is any systematic difference.

Table D.3 presents the regression results of comparing *learn*$ave-only to control group memberships, *learn*$ave-plus to *learn*$ave-only group membership, and *learn*$ave-plus to control group membership. Similar to the statistics in Table D.2, there were a few small characteristics differences between research groups. Respondents in the *learn*$ave-only and *learn*$ave-plus groups were more likely to be Canadian born than control group respondents. More *learn*$ave-plus respondents had activity limitations than *learn*$ave-only respondents. However, none of the models had substantial explanatory power.[2] Non-response seems to be independent of program or control group membership. The differential non-response between the program and control groups increases our confidence that there is no substantial bias in the impact estimates.

2 All F statistics are small and all p-values are substantially higher than the usual level of significance.

Table D.1 Baseline Characteristics (Means, %) of Enrollees at Baseline and for 54-month Survey Respondents and Non-Respondents, by Research Group

	Baseline Sample			Respondents at 54 Months			Non-Respondents at 54 Months		
Characteristic at Baseline	***learn*$ave-only**	***learn*$ave-plus**	**Control**	***learn*$ave-only**	***learn*$ave-plus**	**Control**	***learn*$ave-only**	***learn*$ave-plus**	**Control**
Gender									
Male	46.4	48.2	48.5	44.5	45.8	43.8	50.7	54.3	52.6
Female	53.6	51.8	51.6	55.5	54.3	56.2	49.3	45.7	47.4
Age									
Under 21 years	0.7	1.4	1.3	0.4	0.7	1.2	1.4	3.3	1.4
21-30 years	40.9	41.1	39.9	40.5	40.4	39.3	41.9	43.0	40.5
31-40 years	43.1	42.4	42.0	43.7	44.0	43.3	41.6	38.2	40.8
41-50 years	12.7	12.1	14.1	12.8	11.9	13.9	12.5	12.5	14.4
52-65 years	2.5	3.0	2.6	2.5	3.0	2.3	2.6	3.0	2.9
Average age (years)	33.4	33.4	33.6	33.5	33.5	33.7	33.2	32.9	33.5
Marital Status									
Single	45.9	46.7	43.7	44.7	43.4	42.4	49.0	54.9	44.8
Married	41.8	41.0	44.1	44.2	44.2	44.7	36.0	32.5	43.5
Separated, Divorced or Widowed	12.3	12.4	12.2	11.2	12.3	12.9	15.0	12.5	11.6
Equity Groups									
Visible minority	64.2	65.3	65.6	63.9	64.8	62.0	64.9	66.5	68.9
Aboriginal	1.1	1.3	1.3	1.0	0.9	1.4	1.4	2.4	1.1
Have activity limitation (disability)	5.9	8.0	7.5	5.8	8.0	8.3	6.2	7.8	6.9
Basic Economic Family Type									
Unattached individuals	46.3	47.8	43.9	44.8	45.3	39.8	49.9	54.3	47.7
Couples without children < 18 years old	13.3	12.1	13.9	13.3	12.0	12.3	13.3	12.5	15.3
Couples with one or more children < 18 years old	26.2	26.5	28.0	29.0	29.5	31.3	19.6	18.8	25.0
Single parents with one or more children < 18 years old	8.3	7.8	8.5	7.1	8.0	9.9	11.1	7.2	7.2
Other economic family types	5.9	5.8	5.7	5.8	5.2	6.7	6.2	7.2	4.8
Year of Entry into Canada (Immigrants)									
Before 1993	17.3	16.2	13.8	16.7	14.0	13.2	18.7	22.4	14.2
1993 - 1997	7.1	8.9	7.6	6.4	7.6	7.6	8.9	12.4	7.7
After 1997	75.6	74.9	78.6	77.0	78.5	79.2	72.3	65.2	78.2

Table D.1 Baseline Characteristics (Means, %) of Enrollees at Baseline and for 54-month Survey Respondents and Non-Respondents, by Research Group (Continued)

	Baseline Sample			Respondents at 54 Months			Non-Respondents at 54 Months		
Characteristic at Baseline	***learn*$ave-only**	***learn*$ave-plus**	**Control**	***learn*$ave-only**	***learn*$ave-plus**	**Control**	***learn*$ave-only**	***learn*$ave-plus**	**Control**
Highest Level of Formal Education									
Less than high school graduation certificate	2.5	2.7	3.3	1.7	2.3	1.9	4.5	3.6	4.5
High school graduation certificate	7.3	7.4	8.2	5.9	6.9	9.0	10.5	8.7	7.5
Some post-secondary education	17.4	16.8	15.6	15.3	15.3	17.1	22.4	20.6	14.2
Non-university certificate or diploma	21.3	19.9	21.3	21.6	19.6	21.5	20.4	20.9	21.2
University degree	51.6	53.3	51.6	55.5	56.0	50.5	42.2	46.3	52.6
Labour Force Status									
Work for pay	54.9	55.9	54.7	54.9	56.5	55.2	55.0	54.5	54.2
Self-employed	11.1	12.4	10.6	10.9	12.0	12.7	11.6	13.5	8.6
Unemployed	25.4	22.9	26.3	24.1	22.7	24.7	28.3	23.4	27.8
Out of labour force (student, at home, retired, and not working for pay)	8.5	8.8	8.5	10.0	8.9	7.4	5.1	8.7	9.4
Household Income									
Under $5,000	14.6	14.7	13.7	14.4	14.4	10.2	15.0	15.3	16.8
$5,000 – $9,999	19.2	19.8	21.5	18.8	19.4	19.7	20.3	20.6	23.2
$10,000 – $14,999	24.3	22.2	21.7	23.9	22.3	22.0	25.2	21.9	21.5
$15,000 – $19,999	19.8	21.1	18.8	19.1	22.0	20.5	21.5	18.8	17.3
$20,000 – $24,999	11.3	11.7	12.0	12.4	11.3	14.2	8.6	12.8	10.1
$25,000 – $29,999	4.6	4.9	6.2	5.4	4.8	7.0	2.8	5.3	5.5
$30,000 and more	6.3	5.7	6.1	6.1	5.8	6.4	6.8	5.3	5.7

Source: Baseline and 54-month surveys

Note: There were 2,269 respondents (842 in *learn*$ave-only, 859 in *learn*$ave-plus, and 568 in the control group) and 1,315 non-respondents (353 in *learn*$ave-only, 335 in *learn*$ave-plus, and 627 in the control group) to the 54-month survey.

Rounding may cause some discrepancies in sums.

Table D.2 Differences (Pecentage Points) between Research Groups in Baseline Characteristics of Enrollees at Baseline and for 54-month Survey Respondents and Non-Respondents, by Research Group

	***learn*$ave-only vs Control**			***learn*$ave-plus vs *learn*$ave-only**			***learn*$ave-plus vs Control**		
Characteristic at Baseline	**Diff-R**	**Diff-NR**	**R - NR**	**Diff-R**	**Diff-NR**	**R - NR**	**Diff-R**	**Diff-NR**	**R - NR**
Gender									
Male	0.7	-1.9		1.2	3.6		1.9	1.7	
Female	-0.7	1.9		-1.2	-3.6		-1.9	-1.7	
Age									
Under 21 years	-0.9	0.0		0.3	1.9**		-0.5	1.9**	†††
21-30 years	1.2	1.4		-0.1	1.1		1.1	2.5	
31-40 years	0.4	0.8		0.3	-3.4		0.7	-2.6	
41-50 years	-1.1	-1.9		-1.0	0.1		-2.0	-1.8	
52-65 years	0.2	-0.3		0.5	0.4		0.7	0.1	
Marital Status									
Single	2.2	4.2		-1.2	5.9		1.0	10.1***	††
Married	-0.5	-7.6**	†	0.1	-3.4		-0.5	-11.0***	††
Separated, Divorced or Widowed	-1.7	3.4	†	1.2	-2.5		-0.5	0.9	
Equity Groups									
Visible minority	1.9	-3.9		0.9	1.5		2.8	-2.4	
Aboriginal	-0.5	0.3		0.0	1.0		-0.5	1.3*	†
Have activity limitation (disability)	-2.5*	-0.6		2.2*	1.5		-0.2	0.9	
Basic Economic Family Type									
Unattached individuals	5.0*	2.2		0.5	4.5		5.5**	6.6**	
Couples without children < 18 years old	1.0	-2.0		-1.3	-0.8		-0.3	-2.8	
Couples with one or more children < 18 years old	-2.4	-5.5*		0.5	-0.7		-1.9	-6.2**	
Single parents with one or more children < 18 years old	-2.7*	3.9**	†††	0.9	-3.9*	†	-1.8	0.0	
Other economic family types	-0.9	1.5		-0.6	0.9		-1.5	2.4	†
Year of Entry into Canada (Immigrants)									
Before 1993	3.5	4.5		-2.7	3.7		0.8	8.2***	†
1993 - 1997	-1.2	1.3		1.2	3.4		0.0	4.7**	
After 1997	-2.3	-5.8*		1.5	-7.1*	†	-0.8	-12.9***	†††
Highest Level of Formal Education									
Less than high school graduation certificate	-0.3	0.1		0.7	-1.0		0.4	-0.9	
High school graduation certificate	-3.0**	3.0*	†††	0.9	-1.8		-2.1	1.2	
Some post-secondary education	-1.8	8.2***	†††	-0.1	-1.8		-1.8	6.4**	††
Non-university certificate or diploma	0.1	-0.8		-2.1	0.5		-1.9	-0.3	
University degree	4.9*	-10.4***	†††	0.5	4.1		5.5**	-6.4*	†††
Labour Force Status									
Work for pay	-0.3	0.7		1.5	-0.5		1.3	0.3	
Self-employed	-1.8	3.0	†	1.1	1.9		-0.7	4.9**	††
Unemployed	-0.6	0.6		-1.4	-5.0		-2.0	-4.4	
Out of labour force (student, at home, retired, and not working for pay)	2.6*	-4.3**	†††	-1.1	3.6*	†	1.4	-0.7	

Table D.2 Differences (Pecentage Points) between Research Groups in Baseline Characteristics of Enrollees at Baseline and for 54-month Survey Respondents and Non-Respondents, by Research Group (Continued)

	***learn*$ave-only vs Control**			***learn*$ave-plus vs *learn*$ave-only**			***learn*$ave-plus vs Control**		
Characteristic at Baseline	**Diff-R**	**Diff-NR**	**R - NR**	**Diff-R**	**Diff-NR**	**R - NR**	**Diff-R**	**Diff-NR**	**R - NR**
Household Income									
Under $5,000	4.2**	-1.8	†	0.0	0.3		4.2**	-1.5	†
Between $5,000 and $9,999	-1.0	-2.9		0.7	0.4		-0.3	-2.5	
Between $10,000 and $14,999	1.9	3.7		-1.6	-3.3		0.3	0.4	
Between $15,000 and $19,999	-1.3	4.2		2.9	-2.7		1.6	1.5	
Between $20,000 and $24,999	-1.8	-1.5		-1.1	4.2*	†	-2.9	2.8	††
Between $25,000 and $29,999	-1.6	-2.8*		-0.6	2.6		-2.2*	-0.2	
$30,000 and more	-0.3	1.0		-0.3	-1.4		-0.7	-0.4	

Source: Calculations based on 54-month survey

Note: There were 2,269 respondents (842 in *learn*$ave-only, 859 in *learn*$ave-plus, and 568 in the control group) to the 54-month survey and 1,315 non-respondents (353 in *learn*$ave-only, 335 in *learn*$ave-plus, and 627 in the control group).

Diff-R denotes difference between program groups of 54-month survey respondents, while **Diff-NR** denotes those of the non-respondents. These differences are tested using t-tests. Statistical significance levels of differences between research groups are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

R-NR denotes the results of t-tests of the difference between respondent and non-respondent differences for earch research group and subgroup characteristic. Statistical significance levels of differences between respondent and non-respondent differences are indicated as † = 10 per cent; ††= 5 per cent; †††= 1 per cent, based on a q-test.

Rounding may cause slight discrepancies in differences.

Table D.3 Results of Linear Regression of Binary Research Group Membership Indicator on Baseline Characteristics, for 54-month Respondents

	***learn*$ave-only vs Control**		***learn*$ave-plus vs *learn*$ave-only**		***learn*$ave-plus vs Control**	
Characteristic at Baseline	**Coeff.**	**S.E.**	**Coeff.**	**S.E.**	**Coeff.**	**S.E.**
Gender (ref: Female)						
Male	0.000	0.000	0.000	0.000	0.000	0.000
Age (ref: Under 21 years old)						
21-30 years	0.002	0.002	-0.001	0.002	0.001	0.001
31-40 years	0.002	0.002	-0.001	0.002	0.001	0.001
41-50 years	0.002	0.002	-0.001	0.002	0.001	0.002
52-65 years	0.002	0.002	0.000	0.002	0.002	0.002
Marital Status (ref: Single)						
Married	0.000	0.001	0.000	0.001	0.000	0.001
Separated, Divorced or Widowed	0.000	0.001	0.000	0.001	0.000	0.001
Equity Groups (ref: No)						
Visible minority	0.000	0.000	0.000	0.000	0.000	0.000
Aboriginal	-0.001	0.001	0.000	0.001	0.000	0.001
Have activity limitation (disability)	-0.001	0.001	0.001	0.001**	0.000	0.001
Basic Economic Family Type (ref: Unattached individuals)						
Couples without children < 18 years old	0.000	0.001	-0.001	0.001	-0.001	0.001
Couples with one or more children < 18 years old	-0.001	0.001	0.000	0.001	-0.001	0.001
Single parents with one or more children < 18 years old	-0.001	0.001	0.000	0.001	0.000	0.001
Other economic family types	0.000	0.001	-0.001	0.001	-0.001	0.001

Table D.3 Results of Linear Regression of Binary Research Group Membership Indicator on Baseline Characteristics, for 54-month Respondents (Continued)

	***learn*$ave-only vs Control**		***learn*$ave-plus vs *learn*$ave-only**		***learn*$ave-plus vs Control**	
Characteristic at Baseline	**Coeff.**	**S.E.**	**Coeff.**	**S.E.**	**Coeff.**	**S.E.**
Year of Entry into Canada (ref: Immigrated before 1993 or Canadian-born)						
1993 - 1997	-0.001	0.001	0.001	0.001*	0.000	0.001
After 1997	-0.001	0.001	0.000	0.001	0.000	0.001
Highest Level of Formal Education (ref: Less than high school certificate)						
High school graduation certificate	0.000	0.001	-0.001	0.001	-0.001	0.001
Some post-secondary education	0.000	0.001	-0.001	0.001	-0.001	0.001
Non-university certificate or diploma	0.000	0.001	-0.001	0.001	-0.001	0.001
University degree	0.001	0.001	-0.001	0.001	0.000	0.001
Labour Force Status (ref: Work for pay)						
Self-employed	0.000	0.000	0.000	0.000	0.000	0.000
Unemployed	0.000	0.000	0.000	0.000	0.000	0.000
Out of labour force (student, at home, retired, and not working for pay)	0.001	0.001*	0.000	0.000	0.000	0.001
Household Income (ref: Under $5,000)						
Between $5,000 and $9,999	-0.001	0.001	0.000	0.000	-0.001	0.000
Between $10,000 and $14,999	-0.001	0.000	0.000	0.000	-0.001	0.000
Between $15,000 and $19,999	-0.001	0.001	0.001	0.000*	0.000	0.001
Between $20,000 and $24,999	-0.001	0.001	0.000	0.001	-0.001	0.001
Between $25,000 and $29,999	-0.001	0.001	0.000	0.001	-0.001	0.001
$30,000 and more	-0.001	0.001	0.000	0.001	-0.001	0.001
R-squared	0.03		0.02		0.02	
F Value of Model	1.02		0.84		0.96	
Pr > F	0.44		0.73		0.53	
Sample size	1,314		1,602		1,330	

Source: Calculations based on the 54-month survey.

Note: Marginal effect of each factor is tested using t-tests. Statistical significance levels of marginal effects are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Some variables included in the regression equation are not shown, including immigration status and whether or not highest education was obtained in Canada.

Appendix E Characteristics of sample for analysis of effects of IDA program parameters on *learn*$ave saving activity

This appendix contains a table presenting socio-demographic characteristics and *learn*$ave deposit activity status by *learn*$ave site. The data profile the sample used in the analysis of the role played by program parameters and other control variables in *learn*$ave deposit activity, the results of which are presented in Chapter 5 of this report. The participants comprising this sample are *learn*$ave-plus group members and IA recipients from the experimental sites, plus all participants at the non-experimental sites.

Table E Baseline Characteristics of *learn*$ave-plus and Non-Experimental Participants (Weighted Percentage Distribution), for Analysis of Effects of Program Parameters, by *learn*$ave Site

Characteristics at Baseline	All Participants	Vancouver	Toronto	Halifax	Calgary	Winnipeg	Waterloo
Gender							
Male	33.5	42.1	51.3	28.3	45.2	37.3	24.2
Female	66.5	57.5	48.7	71.7	54.8	62.7	75.8
Age (years)							
25 or less	21.4	18.9	10.7	23.9	29.5	18.7	21.5
26 to 40	55.3	61.5	71.8	51.6	47.3	61.3	59.1
41 or more	23.4	20.0	17.5	24.5	23.3	20.0	19.5
Marital Status							
Single	48.6	52.4	40.8	61.0	56.8	53.3	37.6
Married	26.4	30.4	47.1	10.7	20.5	26.0	38.3
Separated/Divorced/Widowed	25.1	17.1	12.1	28.3	22.6	20.7	24.2
Children in Household							
No children	53.1	65.5	58.3	54.1	65.1	50.7	42.3
One child	21.3	16.5	27.9	19.5	13.0	20.0	32.9
Two or more children	25.6	17.9	13.9	26.4	21.9	29.3	24.8
Citizen/Immigrant							
Citizen	76.4	64.9	33.9	96.9	73.3	63.3	56.4
Immigrant	22.4	34.9	62.8	3.1	22.6	32.7	43.6
Education							
High school or less	23.6	15.1	10.4	34.6	26.7	36.0	18.1
Some post-secondary	26.3	14.9	17.5	26.4	29.5	30.7	33.6
College/Vocational dipolma/ certificate	18.7	19.6	10.4	15.1	21.9	14.0	13.4
University degree	25.5	41.0	45.0	21.4	14.4	15.3	28.2
Graduate degree	5.9	9.5	16.7	2.5	7.5	4.0	6.7
Household Income							
Less than $12,500	56.0	52.9	44.3	66.7	55.5	62.7	51.7
$12,500 or more	44.0	47.1	55.7	33.3	44.5	37.3	48.3
IA Receipt							
Received IA	24.5	12.2	11.8	47.2	11.6	32.0	26.2
Did not receive IA	75.5	87.8	88.2	52.8	88.4	68.0	73.8
Saving Stream							
Education stream	75.8	83.7	86.0	79.2	81.5	81.3	81.2
Micro-enterprise stream	24.2	16.3	14.0	20.8	18.5	18.7	18.8
Dependent Variables (averages)							
Matchable savings in *learn*$ave account (average $)	792	1,041	1,160	520	840	823	813
Proportion of eligible months with eligible savings	0.374	0.392	0.390	0.288	0.454	0.430	0.309
Sample size	2,412	624	635	159	146	150	149
IA sample size	466	75	75	75	17	48	39

Source: Participant Management Information System (PMIS)

Note: Sample sizes vary for individual measures because of missing values. Rounding may cause slight discrepancies in sums and differences. Distributions may not add to 100 per cent due to rounding.

Table E Baseline Characteristics of *learn*$ave-plus and Non-Experimental Participants (Weighted Percentage Distribution), for Analysis of Effects of Program Parameters, by *learn*$ave Site (Continued)

Characteristics at Baseline	Grey-Bruce	Montreal	Fredericton	Annapolis-Digby
Gender				
Male	12.9	32.9	32.9	28.0
Female	87.1	67.1	67.1	72.0
Age (years)				
25 or less	16.8	20.8	32.9	20.0
26 to 40	49.5	57.0	45.6	48.0
41 or more	33.7	22.1	21.5	32.0
Marital Status				
Single	39.6	68.5	42.3	33.3
Married	19.8	10.7	29.5	30.7
Separated/Divorced/Widowed	40.6	20.8	28.2	36.0
Children in Household				
No Children	31.7	75.2	49.0	39.3
One Child	26.7	12.1	20.8	23.3
Two or more children	41.6	12.8	30.2	37.3
Citizen/Immigrant				
Citizen	97.0	90.6	91.3	96.7
Immigrant	3.0	9.4	8.7	3.3
Education				
High school or less	27.7	7.4	18.8	41.3
Some post-secondary	29.7	16.1	42.3	22.7
College/Vocational dipolma/ certificate	25.7	23.5	21.5	22.0
University degree	12.9	47.7	16.8	12.0
Graduate degree	4.0	5.4	0.7	2.0
Household Income				
Less than $12,500	63.4	53.7	51.0	58.7
$12,500 or more	36.6	46.3	49.0	41.3
IA Receipt				
Received IA	36.6	16.8	24.8	25.3
Did not receive IA	63.4	83.2	75.2	74.7
Saving Stream				
Education stream	43.6	80.5	81.2	59.3
Micro-enterprise stream	56.4	19.5	18.8	40.7
Dependent Variables (averages)				
Matchable savings in *learn*$ave account (average $)	699	609	887	526
Proportion of eligible months with eligible savings	0.334	0.432	0.371	0.341
Sample size	101	149	149	150
IA sample size	37	25	37	38

Source: Participant Management Information System (PMIS)

Note: Sample sizes vary for individual measures because of missing values. Rounding may cause slight discrepancies in sums and differences. Distributions may not add to 100 per cent due to rounding.

Appendix F *learn*$ave impacts on savings, net worth, and education: Detailed results

This appendix presents the detailed results for the adjusted estimates of *learn*$ave impacts. As noted in the body of the report and described in Appendix D, impact estimates — computed as the difference in outcomes between the outcomes of research groups — were adjusted using regression to control for a few socio-demographic differences among research groups and to bring greater precision to the estimates. The impact of the matched saving credits is equal to the adjusted difference in outcomes between the *learn*$ave-plus and control groups; the impact of the financial management training and enhanced case management services when delivered with the saving impacts is equal to the adjusted difference in outcomes between the *learn*$ave-plus and *learn*$ave-only control groups; and the combined impact of the credits and services is the difference in outcomes between the *learn*$ave-plus and control groups.

The table numbers in this appendix correspond to the original table in the body of text. For example, Table F6.1 is the detailed version of Table 6.1 in Chapter 6. Tables beginning with '6' are those concerned with budgeting, saving, assets, debts, net worth and hardship impacts (Chapter 6). Those that start with '7' are concerned with education and training and small business impacts (Chapter 7).

Table F.6.1 Impacts on Incidence of Budgeting and Financial Goal Setting, at 54 Months, All Participants – Adjusted

	Outcome Incidence, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits & Services	
	learn$ave-only	*learn*$ave-plus	Control	Diff	S.E.	Diff	S.E.	Diff	S.E.
% who had a budget	52.8	56.6	49.4	3.5	(2.7)	3.8	(2.4)	7.3***	(2.6)
% who set financial goals	64.8	68.4	59.3	5.4**	(2.6)	3.7	(2.3)	9.1***	(2.6)

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively for the 54-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.6.3 Impacts on Self-Reported Saving in Year Prior to the 54 month Survey (Average and Percentage Points), All Participants – Adjusted

	Outcome Average or Incidence, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Incentive + Services	
	learn$ave-only	*learn*$ave-plus	Control	Diff	S.E.	Diff	S.E.	Diff	S.E.
Amount saved in past year ($)	3,222	3,333	3,270	-49	(300)	112	(267)	63	(299)
Saved in past year (%)	63.8	62.3	57.3	6.5**	(2.6)	-1.5	(2.3)	5.0*	(2.6)
Saved regularly in past year (%)	36.8	41.0	35.2	1.6	(2.6)	4.2*	(2.3)	5.8**	(2.6)

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey, Sample sizes vary for individual measures because of missing values. Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups. Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent. Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.6.4 Impacts on Saving Level and Incidence at 54 Months by Household Income in Year Prior to Application, All Participants – Adjusted

Saving Measure and Subgroup (at enrollment)	Sample Size	Mean Outcome Level, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
		learn$ave-only	*learn*$ave-plus	Control	Diff	S.E.	Diff	S.E.	Diff	S.E.
Savings Level (average $)										
Total Financial Assets					†		n.s.		†††	
Less than $10,000	744	5,873	5,232	8,660	-2,787**	(1,263)	-641	(1,067)	-3,428***	(1,240)
Between $10,000 and < $20,000	921	7,015	7,415	6,761	254	(1,101)	400	(1,062)	655	(1,219)
$20,000 and over	604	7,819	9,224	7,033	787	(1,381)	1,405	(1,217)	2,192	(1,391)
Self-Reported Savings in Past Year					n.s.		n.s.		††	
Less than $10,000	722	3,389	2,799	3,819	-429	(532)	-590	(462)	-1,020*	(527)
Between $10,000 and < $20,000	895	2,978	3,394	3,119	-141	(468)	416	(421)	275	(472)
$20,000 and over	592	3,384	3,926	2,909	474	(574)	542	(524)	1,017*	(565)
Saving Incidence			Per cent				Percentage Points			
Reported Saving in Past Year					†††		n.s.		††	
Less than $10,000	739	60.1	60.3	65.3	-5.2	(4.6)	0.2	(4.0)	-5.0	(4.6)
Between $10,000 and < $20,000	918	68.6	64.0	54.8	13.8***	(4.1)	-4.7	(3.7)	9.1**	(4.1)
$20,000 and over	601	61.0	62.5	52.0	8.9*	(5.0)	1.5	(4.6)	10.5**	(5.0)
Reported "Saving Regularly" in Past Year					†		n.s.		†	
Less than $10,000	744	35.0	38.9	42.1	-7.1	(4.6)	3.9	(4.0)	-3.2	(4.6)
Between $10,000 and < $20,000	921	39.5	41.7	32.2	7.3*	(4.1)	2.3	(3.7)	9.5**	(4.1)
$20,000 and over	604	35.0	42.8	32.0	3.0	(5.0)	7.8*	(4.6)	10.8**	(5.0)

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859 for the 54-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

q-tests were applied to differences among subgroups in estimated impacts. Statistical significance levels are indicated as follows †=10 per cent; ††=5 per cent; ††† =1 percent. The abbreviation "n.s." indicates that the variation in impacts among the subgroups is not statistically significant.

The abbreviation "n.s." indicates that the variation in impacts among the subgroups is not statistically significant.

The abbreviation "n.a." means "not available."

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.6.5 Impacts on Savings and Other Components of Net Worth (Average Dollars), at 54 Months, All Participants – Adjusted

	Mean Outcome Level, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Assets									
Financial Assets									
Bank accounts + *learn*$ave accounts	2,610	2,465	2,071	539	(348)	-145	(323)	393	(403)
Formal retirement savings plans	2,830	3,213	3,490	-660*	(378)	383	(362)	-278	(406)
Other financial assets	1,418	1,483	1,833	-414	(403)	65	(358)	-349	(398)
Total Financial Assets	**6,858**	**7,161**	**7,394**	**-536**	**(737)**	**303**	**(648)**	**-233**	**(806)**
Non-Financial Assets									
Vehicles	3,178	3,109	3,297	-119	(296)	-69	(265)	-188	(298)
Value of goods in house	5,377	5,364	5,929	-552	(515)	-12	(464)	-564	(526)
Home and other property	51,365	54,499	53,986	-2,621	(5,443)	3,134	(4,767)	513	(5,454)
Business assets	3,510	2,058	1,548	1,962*	(1,154)	-1,451	(1,096)	511	(1,104)
Total Non-Financial Assets	**63,429**	**65,030**	**64,759**	**-1,329**	**(5,700)**	**1,601**	**(5,070)**	**272**	**(5,814)**
Total Assets	**70,288**	**72,191**	**72,153**	**-1,866**	**(5,767)**	**1,904**	**(5,165)**	**38**	**(5,914)**
Liabilities									
Credit cards	1,637	1,652	1,705	-68	(313)	15	(271)	-53	(306)
Student loans	5,421	5,344	4,063	1,358**	(576)	-77	(513)	1,281**	(570)
Mortgages	31,656	35,231	33,164	-1,508	(3,548)	3,575	(3,095)	2,067	(3,482)
Business debts and liabilities	609	641	806	-197	(342)	32	(300)	-165	(340)
Other debts and liabilities	3,470	3,864	3,572	-102	(518)	395	(448)	292	(579)
Total Liabilities	42,793	46,732	43,309	-517	(3,708)	3,940	(3,229)	3,423	(3,674)
Net (Assets - Liabilities)									
Net business assets	2,901	1,418	742	2,159**	(997)	-1,483	(980)	676	(946)
Net property assets	19,709	19,267	20,822	-1,113	(3,181)	-442	(2,552)	-1,554	(3,131)
Net worth without net property	7,786	6,192	8,022	-236	(1,703)	-1,594	(1,637)	-1,830	(1,663)
Total Net Worth	**27,495**	**25,459**	**28,844**	**-1,349**	**(3,581)**	**-2,036**	**(3,132)**	**-3,385**	**(3,666)**

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn* $ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively. Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.6.6 Impact on Measures of Financial Integration (Percentage Points), at 18, 40 and 54 Months, All Participants – Adjusted

	Outcome Incidence, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Possession of a Credit Card									
% having credit card at baseline	68.5	68.9	68.9	-0.4	(1.8)	0.4	(1.8)	0.0	(1.8)
% having credit card at 18 months	88.2	85.8	86.6	1.6	(1.6)	-2.4	(1.5)	-0.8	(1.6)
% having credit card at 40 months	90.6	90.1	92.1	-1.5	(1.5)	-0.5	(1.4)	-2.0	(1.5)
% having credit card at 54 months	93.0	92.6	93.7	-0.6	(1.4)	-0.4	(1.2)	-1.0	(1.3)
Comfort Level with Financial System (at 54 Months)									
"I am very comfortable dealing with banks, credit unions or other financial institutions on matters such as making deposits or withdrawals, borrowing money, etc."									
% saying statement was very accurate (5 on the scale)	68.8	72.3	66.0	2.8	(2.7)	3.5	(2.4)	6.3**	(2.6)
% saying statement was somewhat accurate or very accurate (4 or 5)	86.7	87.7	83.4	3.3*	(2.0)	1.0	(1.8)	4.3**	(2.0)

Source: Calculations from Baseline, 18-month, 40 month and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey; 607, 833 ad 814, for the 40-month survey; and 748, 920 and 915 for the 18-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.6.7 Impacts on Incidence of Hardship and Life Satisfaction level (Percentage Points or Average), at 54 Months, All Participants – Adjusted

	Outcome Incidence or Mean, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Hardship (in the last 12 Months)									
% who had difficulty meeting expenses	16.8	18.9	18.4	-1.6	(2.0)	2.1	(1.8)	0.5	(2.0)
% who had to borrow to meet needs	14.7	13.7	14.2	0.5	(1.8)	-1.0	(1.6)	-0.5	(1.8)
% who used a foodbank	3.0	3.4	3.2	-0.2	(1.0)	0.4	(0.8)	0.2	(0.9)
% who declared bankruptcy	1.5	1.2	0.8	0.7	(0.6)	-0.3	(0.5)	0.4	(0.6)
% who had overdue bills	1.7	1.4	1.9	-0.1	(0.7)	-0.4	(0.6)	-0.5	(0.7)
% who had at least one of above items	23.4	25.3	23.8	-0.4	(2.2)	1.8	(2.0)	1.4	(2.2)
Number of hardship items (average)	0.4	0.4	0.4	0.0	(0.0)	0.0	(0.0)	0.0	(0.0)
Life Satisfaction									
% indicating life satisfaction as 10	7.0	8.9	3.4	3.6	(2.4)	1.8	(2.1)	5.4**	(2.4)
% indicating life satisfaction as 9 or 10	20.6	21.7	13.5	7.1*	(3.7)	1.1	(3.2)	8.2**	(3.7)
% indicating life satisfaction as 8, 9 or 10	48.7	48.4	44.5	4.2	(4.7)	-0.3	(4.1)	3.9	(4.7)

Source: Calculations from 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively .

Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.1 Impacts on Attitudes towards Education (Percentage Points), at Months 18, 40 and 54, Education Stream Participants – Adjusted

	Outcome Percentage Distribution, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Services Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Month 54									
Getting a good job depends on my education									
Strongly disagree	0.9	0.5	0.9	-0.1	(0.5)	-0.3	(0.5)	-0.4	(0.5)
Disagree	4.2	4.9	8.0	-3.8***	(1.4)	0.7	(1.2)	-3.1**	(1.4)
Agree	48.2	50.7	49.9	-1.8	(3.0)	2.6	(2.7)	0.8	(3.0)
Strongly agree	46.7	43.8	41.2	5.6*	(3.0)	-2.9	(2.7)	2.7	(3.0)
I need more schooling to find a good job									
Strongly disagree	2.0	2.2	1.9	0.1	(0.9)	0.2	(0.8)	0.3	(0.9)
Disagree	21.9	21.8	23.4	-1.5	(2.5)	-0.1	(2.2)	-1.6	(2.5)
Agree	47.6	53.5	46.9	0.7	(3.1)	5.9**	(2.7)	6.6**	(3.1)
Strongly agree	28.6	22.6	27.9	0.7	(2.7)	-6.0**	(2.4)	-5.3**	(2.7)
No matter how much education I get, I will most likely end up with a low-paying job									
Strongly disagree	25.1	22.8	27.7	-2.6	(2.6)	-2.3	(2.3)	-4.9*	(2.6)
Disagree	62.4	65.9	59.8	2.7	(3.0)	3.5	(2.6)	6.1**	(3.0)
Agree	10.3	10.1	9.9	0.4	(1.8)	-0.2	(1.6)	0.2	(1.8)
Strongly agree	2.3	1.2	2.6	-0.4	(0.8)	-1.1	(0.8)	-1.4*	(0.8)
It is not worth going into debt to go to school									
Strongly disagree	12.9	11.3	13.5	-0.7	(2.1)	-1.6	(1.8)	-2.2	(2.1)
Disagree	66.2	69.1	64.4	1.9	(3.0)	2.8	(2.6)	4.7	(3.0)
Agree	17.9	16.8	20.2	-2.3	(2.4)	-1.1	(2.1)	-3.4	(2.4)
Strongly agree	3.0	2.9	1.9	1.1	(1.0)	-0.1	(0.9)	1.0	(1.0)
Month 40									
Getting a good job depends on my education									
Strongly disagree	0.3	0.7	1.9	-1.6***	(0.6)	0.4	(0.5)	-1.2**	(0.6)
Disagree	5.1	5.6	6.4	-1.3	(1.4)	0.4	(1.3)	-0.8	(1.4)
Agree	48.9	50.4	51.7	-2.9	(3.0)	1.5	(2.7)	-1.3	(3.0)
Strongly agree	45.7	43.4	40.0	5.7*	(3.0)	-2.3	(2.7)	3.4	(3.0)
I need more schooling to find a good job									
Strongly disagree	1.1	1.2	2.9	-1.7**	(0.8)	0.0	(0.7)	-1.7**	(0.8)
Disagree	16.1	19.0	20.5	-4.4*	(2.3)	2.9	(2.1)	-1.5	(2.3)
Agree	49.4	53.5	50.7	-1.3	(3.0)	4.0	(2.8)	2.8	(3.0)
Strongly agree	33.3	26.4	25.9	7.4***	(2.7)	-6.9***	(2.5)	0.5	(2.7)
No matter how much education I get, I will most likely end up with a low-paying job									
Strongly disagree	26.8	25.0	27.3	-0.5	(2.6)	-1.8	(2.4)	-2.3	(2.6)
Disagree	59.5	58.6	57.2	2.3	(3.0)	-0.9	(2.7)	1.4	(3.0)
Agree	12.5	14.4	12.0	0.5	(2.0)	1.9	(1.8)	2.4	(2.0)

Table F.7.1 Impacts on Attitudes towards Education (Percentage Points), at Months 18, 40 and 54, Education Stream Participants – Adjusted (Continued)

	Outcome Percentage Distribution, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Services Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Strongly agree	1.2	2.0	3.5	-2.3***	(0.9)	0.8	(0.8)	-1.5*	(0.9)
It is not worth going into debt to go to school									
Strongly disagree	12.0	11.7	7.9	4.0**	(1.9)	-0.3	(1.7)	3.7*	(1.9)
Disagree	63.1	63.7	65.0	-1.9	(3.0)	0.6	(2.7)	-1.3	(3.0)
Agree	21.6	22.2	23.6	-2.0	(2.6)	0.6	(2.3)	-1.4	(2.6)
Strongly agree	3.3	2.4	3.4	-0.2	(1.1)	-0.9	(1.0)	-1.1	(1.1)
Month 18									
Getting a good job depends on my education									
Strongly disagree	0.9	0.8	1.8	-1.0*	(0.6)	-0.1	(0.5)	-1.1*	(0.6)
Disagree	5.7	6.5	10.1	-4.4***	(1.4)	0.7	(1.4)	-3.6**	(1.4)
Agree	51.4	50.1	54.6	-3.3	(2.7)	-1.3	(2.6)	-4.6*	(2.7)
Strongly agree	42.1	42.7	33.4	8.6***	(2.7)	0.7	(2.5)	9.3***	(2.7)
I need more schooling to find a good job									
Strongly disagree	0.7	0.6	1.3	-0.6	(0.5)	0.0	(0.5)	-0.6	(0.5)
Disagree	9.9	10.0	14.6	-4.7***	(1.7)	0.1	(1.6)	-4.6***	(1.7)
Agree	52.0	55.1	51.3	0.7	(2.8)	3.1	(2.6)	3.9	(2.8)
Strongly agree	37.4	34.2	32.9	4.5*	(2.6)	-3.1	(2.5)	1.4	(2.6)
No matter how much education I get, I will most likely end up with a low-paying job									
Strongly disagree	25.1	26.3	23.4	1.7	(2.4)	1.2	(2.2)	2.9	(2.4)
Disagree	61.2	59.3	55.0	6.2**	(2.8)	-1.9	(2.6)	4.3	(2.8)
Agree	12.5	13.0	18.4	-5.9***	(1.9)	0.5	(1.8)	-5.5***	(1.9)
Strongly agree	1.2	1.4	3.1	-1.9**	(0.7)	0.2	(0.7)	-1.7**	(0.8)
It is not worth going into debt to go to school									
Strongly disagree	13.8	9.6	11.8	1.9	(1.8)	-4.1**	(1.7)	-2.2	(1.8)
Disagree	61.6	66.0	56.1	5.6**	(2.8)	4.4*	(2.6)	10.0***	(2.8)
Agree	21.5	21.4	28.2	-6.7***	(2.4)	-0.1	(2.3)	-6.8***	(2.4)
Strongly agree	3.1	3.0	3.9	-0.8	(1.0)	-0.1	(1.0)	-0.9	(1.0)

Source: Calculations from 18-month, 54-month and 40-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey; 607, 833 ad 814, for the 40-month survey; and 748, 920 and 915 for the 18-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to differences between the treatment and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.2 Impacts on Participation in Education and Training (Percentage Points or Average), during 18, 40 and 54 Months, Education Stream – Adjusted

	Outcome Mean or Incidence, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
During first 54 months									
Overall (Program or Individual Course)									
Enrolled in any education (%)	88.0	89.7	81.5	6.6***	(2.0)	1.7	(1.8)	8.2***	(2.0)
Educational Programs									
Enrolled in courses toward a degree, diploma, or certificate (%)	65.1	68.6	56.0	9.1***	(2.8)	3.5	(2.5)	12.6***	(2.8)
Program Type (first program) (%):									
▪ English as a second language (ESL)	4.4	5.3	4.1	0.2	(1.3)	1.0	(1.1)	1.2	(1.3)
▪ High school	3.9	3.1	2.8	1.1	(1.1)	-0.9	(0.9)	0.3	(1.1)
▪ Registered apprenticeship	6.6	5.2	6.1	0.6	(1.4)	-1.4	(1.3)	-0.9	(1.4)
▪ Community college	33.3	35.3	30.0	3.3	(2.8)	1.9	(2.5)	5.2*	(2.8)
▪ University	25.0	27.6	18.4	6.7***	(2.5)	2.5	(2.2)	9.2***	(2.5)
Completed program (%)	44.5	45.5	39.5	5.0*	(3.0)	1.0	(2.6)	6.0**	(3.0)
Individual Courses, not Part of a Program									
Enrolled in other (non-program) education courses, seminars, etc. (%)	51.8	52.1	47.5	4.3	(3.0)	0.3	(2.7)	4.6	(3.0)
Number of courses (average)	1.1	1.1	0.9	0.2**	(0.1)	-0.1	(0.1)	0.1	(0.1)
Spouses (among those with a non-student spouse at baseline)									
Enrolled in any education since baseline (%)	66.6	68.1	62.3	4.3	(5.3)	1.5	(4.7)	5.8	(5.4)
Enrolled in a program: courses toward a degree, diploma, or certificate (%)	52.9	52.3	41.9	11.0**	(5.5)	-0.6	(4.9)	10.4*	(5.6)
Enrolled in other (non-program) education courses, seminars, etc. (%)	33.0	38.2	35.8	-2.8	(5.5)	5.1	(4.9)	2.4	(5.6)
During first 40 months									
Overall (Program or Individual Course)									
Enrolled in any education (%)	83.3	86.0	78.6	4.7**	(2.2)	2.7	(2.0)	7.4***	(2.2)
Educational Programs									
Enrolled in courses toward a degree, diploma, or certificate (%)	62.6	66.4	54.4	8.2***	(2.8)	3.8	(2.6)	12.0***	(2.8)
Program Type (first program) (%):									
▪ English as a second language (ESL)	4.4	5.9	4.8	-0.3	(1.3)	1.4	(1.2)	1.1	(1.3)
▪ High school	4.3	3.3	2.4	1.9*	(1.1)	-1.0	(1.0)	0.9	(1.1)
▪ Registered apprenticeship	6.1	5.1	6.1	0.0	(1.4)	-1.0	(1.3)	-1.0	(1.4)
▪ Community college	31.1	34.1	28.5	2.5	(2.7)	3.0	(2.5)	5.6**	(2.8)
▪ University	22.3	24.8	16.8	5.5**	(2.4)	2.5	(2.2)	8.0***	(2.4)
Completed program (%)	33.1	36.2	31.8	1.3	(2.8)	3.1	(2.6)	4.3	(2.8)
Individual Courses, not Part of a Program									
Enrolled in other (non-program) education courses, seminars, etc. (%)	46.1	47.8	44.3	1.7	(3.0)	1.8	(2.7)	3.5	(3.0)
Number of courses (average)	0.8	0.8	0.8	0.0	(0.1)	0.0	(0.1)	0.1	(0.1)

Table F.7.2 Impacts on Participation in Education and Training (Percentage Points or Average), during 18, 40 and 54 Months, Education Stream – Adjusted (Continued)

	Outcome Mean or Incidence, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
During first 18 months									
Overall (Program or Individual Course)									
Enrolled in any education in first 18 months (%)	64.5	66.4	65.9	-1.3	(2.6)	1.9	(2.4)	0.5	(2.6)
Educational Programs									
Enrolled in courses toward a degree, diploma, or certificate (%)	46.2	47.4	44.3	1.9	(2.7)	1.1	(2.5)	3.1	(2.7)
Program Type (first program) (%):									
▪ English as a second language (ESL)	3.8	5.1	4.8	-1.0	(1.1)	1.3	(1.1)	0.3	(1.1)
▪ High school	3.6	2.7	2.5	1.1	(0.9)	-1.0	(0.9)	0.2	(0.9)
▪ Registered apprenticeship	5.5	3.9	5.5	0.1	(1.2)	-1.6	(1.1)	-1.5	(1.2)
▪ Community college	18.4	20.3	20.0	-1.6	(2.2)	1.9	(2.1)	0.3	(2.2)
▪ University	15.8	16.3	12.6	3.2*	(1.9)	0.5	(1.8)	3.7*	(1.9)
Completed program (%)	13.1	12.7	12.3	0.8	(1.8)	-0.4	(1.7)	0.4	(1.8)
Individual Courses, not Part of a Program									
Enrolled in other (non-program) education courses, seminars, etc. (%)	26.3	26.1	29.5	-3.2	(2.4)	-0.2	(2.3)	-3.4	(2.4)
Number of courses (average)	0.3	0.4	0.4	-0.1	(0.0)	0.0	(0.0)	0.0	(0.0)

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively, for the 54-month survey; 607, 833 ad 814, for the 40-month survey; and 748, 920 and 915 for the 18-month survey. Sample sizes vary for individual measures because of missing values.

Two-tailed t-tests were applied to differences between the program and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.3 Impacts in Education Programs (Percentage Points), during the 54 months, by Subgroup, Education Stream Participants – Adjusted

Characteristic at Baseline	Sample Size	Outcome Per cent, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Services Σ		Combined Impact of Credits + Services	
		learn$ave-only	*learn$ave*-plus	Control	Diff	S.E.	Diff	S.E.	Diff	S.E.
All	**1,844**	**65.1**	**68.6**	**56.0**	**9.1*** **	**(2.8)**	**3.5**	**(2.5)**	**12.6*** **	**(2.8)**
Age					n.s.		n.s.		n.s.	
Less than 30 years	665	74.8	77.8	64.8	10.0**	(4.8)	3.0	(4.1)	13.0***	(4.8)
Between 30 and 40 years	885	61.4	64.6	52.9	8.5**	(4.0)	3.2	(3.6)	11.7***	(4.0)
Over 40 years	294	54.5	60.1	45.6	8.9	(7.0)	5.6	(6.4)	14.5**	(6.8)
Gender					n.s.		n.s.		n.s.	
Male	809	59.2	61.8	52.0	7.2*	(4.3)	2.6	(3.8)	9.8**	(4.2)
Female	1,035	69.8	74.0	59.3	10.5***	(3.8)	4.2	(3.3)	14.8***	(3.8)
Employment and Income										
Baseline Labour Force Status:					†		n.s.		†††	
▪ Work for pay	1,039	65.9	70.3	52.1	13.8***	(3.8)	4.4	(3.3)	18.2***	(3.7)
▪ Self-employed	176	57.6	67.8	44.4	13.3	(8.9)	10.2	(8.3)	23.4***	(8.7)
▪ Unemployed or Out of labour force[a]	628	66.0	65.8	65.8	0.2	(4.8)	-0.2	(4.3)	0.0	(4.9)
Household Income in year prior to application[b]:					n.s.		n.s.		n.s.	
▪ Less than $10,000	587	64.2	71.0	58.4	5.8	(5.0)	6.8	(4.4)	12.6**	(5.0)
▪ $10,000 to $19,999	750	66.6	68.7	53.4	13.2***	(4.4)	2.1	(3.9)	15.2***	(4.4)
▪ $20,000 and over	507	64.1	65.8	57.2	6.9	(5.3)	1.7	(4.8)	8.6	(5.4)
Highest Level of Education					n.s.		n.s.		n.s.	
Some PSE or less[c]	450	64.7	66.8	54.6	10.1*	(5.7)	2.2	(5.1)	12.2**	(5.6)
Non-university degree, diploma or cert.	360	58.2	62.9	56.3	1.9	(6.2)	4.8	(5.8)	6.6	(6.3)
University degree	1,034	68.0	71.3	56.1	11.9***	(3.8)	3.3	(3.3)	15.2***	(3.8)
Years Since Immigrating					††		n.s.		n.s.	
Born in Canada	586	63.6	64.7	45.3	18.3***	(4.9)	1.1	(4.5)	19.4***	(4.9)
Immigrated less than 4 years ago	950	69.8	73.1	62.4	7.4*	(4.0)	3.3	(3.4)	10.7***	(3.9)
Immigrated 4 or more years ago	308	54.2	62.7	58.4	-4.3	(7.0)	8.6	(6.1)	4.3	(7.1)
Saving Regularity					n.s.		n.s.		n.s.	
Saved regularly at baseline	279	65.3	68.4	51.5	13.7*	(7.1)	3.2	(6.5)	16.9**	(7.0)
Did not save regularly	1,555	65.1	68.8	58.0	7.0**	(3.1)	3.7	(2.7)	10.8***	(3.1)

Table F.7.3 Impacts in Education Programs (Percentage Points), during the 54 months by Subgroup, Education Stream Participants – Adjusted (Continued)

Source: Calculations from 54-month survey data.

Note: Sample sizes vary for individual measures because of missing values. The subgroups are defined according to characteristics at the time of enrollment in the study. Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups. Statistical significance levels are indicated as * = 10%; ** = 5%; *** = 1%; differences between impacts of categories of a subgroup: † = 10%, †† = 5%, ††† = 1%. Rounding may cause slight discrepancies in sums and differences.

[a] Includes student, at home, retired, looking for work, and unemployed.

[b] Baseline annual income is household income in the calendar year prior to application. For those who immigrated to Canada in the year prior to application, annual income is based on a formula

[c] May have some post secondary, but did not receive a degree, diploma or certificate.

[Σ] The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.4 Impacts on Expenditures, Funding, and Intensity in Education and Training (Average), during the 54 Months, Education Stream Participants – Adjusted

	Outcome Mean, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Services Σ		Combined Impact of Credits & Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Educational Expenditures (average $)									
Tuition Fees									
Programs	4,228	5,006	3,038	1,191***	(408)	778**	(361)	1,968***	(407)
Courses	1,052	1,041	781	270**	(121)	-10	(107)	260**	(120)
Programs and courses	5,248	6,043	3,821	1,426***	(410)	795**	(362)	2,222***	(408)
Book Purchases									
Programs	637	778	604	33	(67)	141**	(59)	173***	(67)
Courses	138	127	57	81***	(22)	-10	(20)	71***	(22)
Programs and courses	774	905	661	113	(69)	131**	(61)	244***	(69)
Total Educational Expenditures									
Programs	4,865	5,784	3,642	1,223***	(451)	918**	(399)	2,142***	(450)
Courses	1,189	1,169	838	351***	(133)	-21	(118)	330**	(132)
Programs and courses	6,021	6,947	4,482	1,539***	(451)	926**	(399)	2,465***	(450)
Educational Funding (average $)									
Grants for programs and courses	1,202	1,198	1,248	-46	(235)	-4	(207)	-50	(234)
Education Loans									
Programs	3,763	4,180	3,905	-142	(577)	417	(510)	275	(575)
Courses	20	114	36	-16	(56)	94*	(50)	78	(56)
Programs and courses	3,783	4,294	3,941	-158	(581)	511	(513)	353	(579)
Educational Intensity (average hours)									
Programs	736	766	606	130**	(61)	29	(54)	159***	(61)
Courses	210	200	148	61**	(29)	-9	(26)	52*	(29)
Programs and courses	946	966	755	192***	(65)	20	(58)	211***	(65)

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively. Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.5 Impacts Labour Force Outcomes (Percentage Points or Average), at 54 Months, Education Stream Participants – Adjusted

	Outcome Mean or Incidence by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Current Labour Force Status (%)									
Working for pay	76.1	77.1	77.1	-0.9	(2.5)	0.9	(2.2)	0.0	(2.5)
Self-employed	5.3	6.3	6.5	-1.2	(1.4)	1.0	(1.3)	-0.2	(1.4)
Unemployed	10.5	8.5	9.8	0.6	(1.8)	-2.0	(1.6)	-1.4	(1.8)
Not in the labour force	8.1	8.2	6.6	1.6	(1.6)	0.1	(1.4)	1.6	(1.6)
Working for pay or self-employed	81.4	83.4	83.6	-2.2	(2.3)	1.9	(2.0)	-0.3	(2.3)
Not Working	18.6	16.7	16.4	2.2	(2.3)	-1.9	(2.0)	0.3	(2.3)
Earnings and Hours (in the previous 4 weeks)									
Total earnings ($)	2,762	2,710	2,798	-36	(113)	-52	(101)	-89	(112)
Average weekly hours worked	37.4	38.0	38.4	-1.0	(0.9)	0.7	(0.8)	-0.4	(0.9)

Source: Calculations from 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 568, 842 and 859, respectively.

Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.6 Impacts on Self-employment (Percentage Points or Average), over the 54 Months, Micro-enterprise Stream Participants – Adjusted

	Outcome Mean or Incidence, by Reseach Group			Impact of Matched Saving Credits		Impact of Services when Offered with Services Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Overall Self-employment Incidence (%)									
0 self-employment jobs since baseline	35.3	45.6	58.2	-22.8***	(6.0)	10.3*	(5.4)	-12.6**	(5.9)
1 self-employment job since baseline	48.7	40.9	29.9	18.8***	(6.3)	-7.8	(5.7)	11.1*	(6.2)
2 or more self-employment jobs since baseline	16.0	13.5	12.0	4.0	(4.3)	-2.5	(3.9)	1.5	(4.2)
Having self-employment jobs since baseline (%)	66.7	56.7	42.1	24.5***	(5.9)	-9.9*	(5.4)	14.6**	(5.8)
Formally incorporated (%)	38.4	31.9	19.2	19.2***	(5.9)	-6.5	(5.4)	12.7**	(5.8)
Income and Intensity of Self-employment Jobs									
Total income from self-employment jobs ($)	8,418	5,701	4,506	3,912**	(1,789)	-2,716*	(1,627)	1,195	(1,750)
Length of time on self-employment jobs (months)	31.0	18.0	25.0	6.0	(4.6)	-13.0***	(4.2)	-7.0	(4.5)
Hours per week on self-employment jobs (hours)	14.4	9.8	7.2	7.2***	(2.4)	-4.6**	(2.2)	2.7	(2.3)
Paid Staff in Self-employment Jobs									
Having paid staff (%)	9.7	8.8	8.4	1.3	(3.6)	-0.9	(3.3)	0.4	(3.5)
Total payroll ($)	3,455	1,064	1,906	1,549	(2,548)	-2,391	(2,317)	-842	(2,492)

Source: Calculations from 18-month survey data, 40-month survey data and 54-month survey data.

Note: Overall sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 124, 150 and 151, respectively. However,18 cases were mistakenly skipped over the questions on self-employment, comrpising 8 in the *learn*$ave group, 8 in the *learn*$ave-plus group and 2 in the control group. These 18 cases were excluded from this table. Sample sizes vary for individual measures because of missing values.

Standard errors of estimated impacts are in parenthesis.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Table F.7.7 Impacts on Average Business Assets and Liabilities (Average Dollars), at 54 Months, MicroEnterprise Stream Participants – Adjusted

	Mean Outcome Level, by Research Group			Impact of Matched Saving Credits		Impact of Services when Offered with Credits Σ		Combined Impact of Credits + Services	
	***learn*$ave-only**	***learn*$ave-plus**	**Control**	**Diff**	**S.E.**	**Diff**	**S.E.**	**Diff**	**S.E.**
Business assets (book value)	5,119	2,992	1,819	3,300*	(1,997)	-2,127	(1,819)	1,172	(1,942)
Goodwill[1]	3,573	1,675	1,458	2,115	(2,685)	-1,899	(3,050)	217	(1,907)
Total Business Assets	**8,692**	**4,666**	**3,277**	**5,415**	**(3,559)**	**-4,026**	**(3,656)**	**1,389**	**(3,103)**
Total Business Debts and Liabilities	**2,283**	**1,228**	**2,562**	**-278**	**(1,399)**	**-1,056**	**(1,266)**	**-1,334**	**(1,357)**
Total Net Business Assets	**6,409**	**3,439**	**715**	**5,694***	**(2,969)**	**-2,971**	**(3,200)**	**2,723**	**(2,323)**

Source: Calculations from 18-month, 40-month and 54-month survey data.

Note: The sample sizes for the control, *learn*$ave-only and *learn*$ave-plus groups are 117, 150, 157, respectively, for the 54-month survey; 124, 150 ad 151, for the 40-month survey; and 143, 172 and 176, for the 18-month survey.

Sample sizes vary for individual measures because of missing values.

Two-tailed tests were applied to impacts estimated by regression-adjusted differences in outcomes between research (program and control) groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

[1] Computed as the difference between: (1) the current equity of the business (excludiing liabilities) and (2) the book value of the business assets when purchased.

Σ The figures in this column show the extra impact of the financial management training and enhanced case management services when given to those eligible to receive matched credits. It does not represent the impact of those services alone for those not eligible to receive the matched saving credit; it represents the impact of the services when provided with the credits.

Appendix G Cost-effectiveness analysis versus cost-benefit analysis

Cost-effectiveness analysis (CEA) does not take into account the values of the impacts nor any cost other than the direct financial cost to deliver the program. From the perspective of *learn*$ave, a CEA cannot take into consideration the increases in indirect costs incurred by governments (e.g., increased expenditure in post-secondary education due to increased enrolment induced by the program), by participants and their families (e.g., costs of forgone leisure and earnings while in school), and by society in general. CEA assumes that the outcomes of the program are beneficial to the society and its main objective is to find the most efficient way to achieve these outcomes. Only a cost-benefit analysis (CBA) can weigh the dollar value of benefits and costs of a program and determine a program's viability. Although CEA and CBA have different objectives and a cost-effective program does not necessarily imply a viable program or vice versa, results from a CEA can yield fruitful results from perspective of costs saving.

If a CBA were to be done on *learn*$ave, the basic framework is presented in Appendix Table G at the end of this appendix. This framework contains only components that *learn*$ave had impacted on.[1] The signs represent the expected impacts of *learn*$ave: net costs are denoted by "–", net benefits are denoted by "+", and no impact by "0". These impacts differ from the perspective of the participants and of the government. In the case of transfer payments, like the *learn*$ave matched saving credits, the gains/losses by the government are exactly compensated for by the losses/gains of the participants and net gains to the society are zero, as the value to society is the sum of the impacts for the participants and the government.

The framework corresponds to the program effects model. The government covered the cost of program delivery of the matched saving credits and financial management training and case management services; the program (the credits alone and in combination with the services) induced participants to save and earn credits; and the beneficiaries (participants or eligible family members) spent the credits on education, training or small business start-up. Indirectly, the program might induce beneficiaries to work less while in the education or while establishing the business, and other family members might need to work more to make up for the participant's lost earnings. The changes in work hours indirectly changed the amount that employers paid to the government in the form of Employment Insurance/ Canada Pension Plan premia. Government expenditures on education institutions and loans and grants would also increase due to the increased enrolment. After the program, participants (and family) could benefit from the increased earnings because of the growth in human capital or the higher income from self employment. In turn, government revenues from social program premiums and taxes would increase while payments to cover the matched saving credits provided under *learn*$ave and their administration cost would fall.

Components with the largest expected values are bolded in the table. The gains from the higher productivity of family members during the program were expected to be smaller than the lost productivity of the beneficiaries. EI and IA administration costs saved are expected to be small. Therefore, it was expected that most of the net benefits to society would come from the increased post-program earnings due to the returns to additional education acquired and the increased post-program income from additional self-employment. The major components of costs to society are the administration cost of *learn*$ave, the costs of education (tuition, education support, grants, and expenses), and the costs of business start-up. The relative magnitudes of these components would probably determine a program's viability.

Two CEA outcomes that correspond to the two largest components of the program benefits to participants and society are the post-program increase in earnings from employment and self-employment (cells with asterisks in the Participants and Family column in Table G). The two highest cost items from the perspective of government are the cashed-out credits and the cost of delivery (cells with asterisks in the Government Budgets column in Table G). Data were available or projections made for all these four items. There are of course other costs that cannot be estimated and so the CEA cost figures substantially underestimate the total cost to society. Therefore, if the dollar value of a program's outcomes is less than the direct financial cost of its delivery, it is not viable. In other words, the *learn*$ave IDA program is not likely to be viable if, at a minimum, the present value of participants' earnings increases resulting from enrolment in education programs funded and induced under *learn*$ave is not greater than the direct financial cost of delivery and the credits used, which represent, in effect, the breakeven point.

1 The original framework in Kingwell et al. (2005) included numerous components that the program could have impacted on. For comparison purpose, only major components were included in the basic framework used here.

Table G Basic Framework of *learn*$ave Cost-Benefit Analysis

Major Components	Accounting Perspective: Participants and Family	Government Budgets	Society
learn$ave Accounts			
Opportunity cost of *learn*$ave deposits	–	0	–
Matched credits awarded	+	–*	0
Employment Earnings			
Earnings:			
▪ In-program foregone earnings (beneficiary)	–	0	–
▪ In-program compensating earnings of family	+	0	+
▪ Post-program earnings increase	**+***	**0**	**+**
▪ Post-program earnings of family	–	0	–
Self-employment / business:			
▪ Post-program self-employment income increase	**+***	**0**	**+**
Education/Business Expenses			
Tuition and fees for education	**–**	**0**	**–**
Additional expenses incurred	**–**	**0**	**–**
Opportunity cost of business start-up	**–**	**0**	**–**
Intangible Costs			
Value of time forgone (beneficiary)	–	0	–
Value of time forgone (family members)	–	0	–
Costs of *learn*$ave			
Cost of *learn*$ave program delivery	**0**	**–***	**–**
Costs and Benefits of Other Programs Affected			
Government grants to education institiutions	**0**	**–**	**–**
Government expenditure on student supports	**0**	**–**	**–**
Taxes and Premia:			
▪ In-program premia paid by employers (beneficiary)	0	–	–
▪ In-program premia paid by employers (family)	0	+	+
▪ Post-program premia paid by employers	0	+	+
▪ Post-program income tax revenue	–	+	0
Social Safety Net:			
▪ EI / IA benefits	–	+	0
▪ EI / IA administration cost	0	+	+

Source: Calculations from *learn*$ave site staff time studies, Program Management Information System and accounting records

Note: Components with the largest expected values are bolded.
The positive and negative signs represent the expected cost and benefit impacts of *learn*$ave:
– denotes net cost
+ denotes net benefit
0 denotes no impact
* indicates expected benefits and costs that are large and have been estimated in Chapter 8.
Bolded entries represent components with large expected values.

Appendix H Discount values and costs

In the report, results were discounted assuming a five per cent discount factor on investment, based on current capital cost conditions. Alternative estimates using various annual discount rates up to 12.5 per cent are shown in Table H below.

Table H ***learn*$ave Present Value Cost-Economy, Cost-Efficiency and Cost-Effectiveness ($), by Program Group and Different Discount Rates, All Program Group Participants**

	***learn*$ave-only**					***learn*$ave-plus**				
	2.5%	**5.0%**	**7.5%**	**10.0%**	**12.5%**	**2.5%**	**5.0%**	**7.5%**	**10.0%**	**12.5%**
Cost-Economy (average per Program Group Member)										
Program Activities										
Recruitment	135	135	135	135	135	135	135	135	135	135
Enrolment	95	95	95	95	95	95	95	95	95	95
Financial Management Training	0	0	0	0	0	66	65	64	63	62
Case Mangement Services	121	114	107	100	95	169	160	152	144	136
Matched withdrawal orientation	17	16	15	15	14	18	17	17	16	15
Matched withdrawal processing	298	280	263	248	233	311	292	274	258	243
Account closure processing	36	33	30	28	26	36	33	30	28	26
Bank administration (imputed)	13	13	13	13	13	13	13	13	13	13
General Operating Costs										
at SEDI	447	426	406	388	371	447	426	406	388	371
at sites	858	822	788	755	725	865	828	793	761	730
Total Program Delivery Cost	2,021	1,934	1,853	1,777	1,706	2,156	2,065	1,979	1,899	1,825
Matched credits awarded	2,005	1,890	1,783	1,683	1,590	2,156	2,030	1,913	1,804	1,703
Total Cost Economy	4,026	3,824	3,636	3,460	3,296	4,312	4,095	3,892	3,704	3,528
Cost-Efficiency										
Proportion who are active participants (%)	80.4	80.4	80.4	80.4	80.4	84.2	84.2	84.2	84.2	84.2
Cost per Active Participant	5,006	4,755	4,521	4,303	4,098	5,119	4,861	4,620	4,397	4,188
Average peak *learn*$ave savings	940	925	910	896	882	999	982	966	950	935
Cost per Dollar Saved	4.28	4.09	3.91	3.75	3.59	4.32	4.12	3.94	3.77	3.61
Participants receiving education as a proportion of all participants (%)	84.7	84.7	84.7	84.7	84.7	86.7	86.7	86.7	86.7	86.7
Cost per Participant Receiving Education	4,754	4,516	4,293	4,086	3,892	4,974	4,724	4,490	4,273	4,070
Cost-Effectiveness										
Impact on enrolment in education/training overall (percentage pts)	5.5	5.5	5.5	5.5	5.5	7.5	7.5	7.5	7.5	7.5
Cost Per Additional Person Receiving Education and Training	73,869	70,168	66,707	63,494	60,475	57,880	54,966	52,245	49,717	47,358
Impact on enrolment in an education program (percentage pts)	7.8	7.8	7.8	7.8	7.8	10.8	10.8	10.8	10.8	10.8
Cost Per Additional Person Enrolled in an Education Program	51,482	48,902	46,490	44,251	42,147	39,963	37,952	36,073	34,327	32,698

Source: Calculations from *learn*$ave site staff time studies, Program Management Information System and accounting records

Appendix I Cost-economy of *learn*$ave activities, administration, overhead, and matched credits in their "natural" units

Costs are presented in Table I below according to their "natural" unit of activity: if the cost is more or less fixed regardless of activity intensity, then it is presented per program group member; if the cost is sensitive to activity level, then it is presented by their appropriate unit of activity. In the text, to facilitate comparisons across items, costs are presented by a common unit, program group member.

Table I Cost-Economy of *learn*$ave Activities, Administration, Overhead and Matched Credits ($), Program Group Participants

Activity	Unit Cost (2002 dollars)
Recruitment	
▪ SEDI	8.50 / program group member
▪ Site recruitment/promotion labour	34.98 / program group member
▪ Site recruitment/promotion overhead	44.27 / program group member
▪ Site recruitment inquiries	47.46 / program group member
▪ Total	**135.21 / program group member**
Enrolment	
▪ SEDI	19.80 / program group member
▪ Site application sessions	55.72 / program group member
▪ Sites orientation sessions	19.62 / program group member
▪ Total	**95.14 / program group member**
Financial Management Training	
▪ SEDI	1.14 / participant hour
▪ Sites	3.97 / participant hour
▪ Total	**5.11 / participant hour**
Case Mangement Services	
▪ SEDI	6.96 / participant hour
▪ Sites	31.89 / participant hour
▪ Total	**38.85 / participant hour**
Matched Withdrawal Orientation	
▪ Sites	**27.77 / withdrawing participant**
Matched Withdrawal Processing	
▪ SEDI	41.20 / withdrawal requested
▪ Sites	59.04 / withdrawal requested
▪ Total	**100.24 / withdrawal requested**
Account Closure Processing	
▪ Sites	**39.00 / program group member**
General Operations	
▪ SEDI:	
Administration	78.44 / program group member
PMIS	159.08 / program group member
Overhead	232.60 / program group member
Total at SEDI	**470.12 / program group member**
▪ Sites:	
Administration	12.03 / participant-month
PMIS	1.07 / participant-month
Overhead	8.70 / participant-month
Total at sites	**21.80 / participant-month**
Matched Credits Awarded	
▪ *learn*$ave-only	**2,128 / program group member**
▪ *learn*$ave-plus	**2,291 / program group member**

Source: Calculations based on *learn*$ave site staff time studies, Program Management Information System, accounting records, baseline survey, and three follow-up surveys.

learn$ave glossary

Term	Description
Active participant	A participant who has saved at least $10 in each of 12 months.
Active saving month	A month in which a participant has deposited at least $10. Participants must have 12 active saving months to qualify for matched credits.
Case management	See Enhanced case management.
Cash-out period	Participants must use all their earned matched credits by month 48 after starting in the project.
Dissaving(s)	See unmatched withdrawals.
Eligibility period	The length of time since the program start that it took a participant to qualify for matched credits. See also Active saving month.
Enhanced (or intensive) case management	Beyond what program group participants receive in terms of basic administrative help from *learn*$ave sites, *learn*$ave-plus participants also receive encouragement to meet savings targets, assistance to identify and address problems in meeting those targets, and referrals to appropriate agencies to deal with other problems.
Financial management training, or financial training	Training on the principles of money management, including strategies for budgeting, spending, and the use of credit, plus assistance in developing realistic goals based on existing skills and education.
learn$ave account closing balance	The total amount deposited in a *learn*$ave account, net of matched and unmatched withdrawals.
Legitimate uses/ purposes of earned credits	The purposes for which the matched credits can be used, namely education or training at an accredited institution (as indicated on the Canada Student Loan Program list) or to start a small business.
Matched credits (earned), or matched saving credits	Amount of matched credits earned from matchable deposits in a *learn*$ave account (maximum = $4,500).
Matched credits used or cashed out	Credits withdrawn, used, or cashed out for legitimate purposes.
Matchable savings/ deposits	The amount deposited in a *learn*$ave account qualifying for matched credits, monthly and total (matched withdrawals do not reduce this amount, but unmatched withdrawals do).
Matched withdrawal amount	Sum of earned matched credits and matchable savings withdrawn in a matched withdrawal (maximum = $6,000; $1,500 in savings + $4,500 in credits).
Matched withdrawals	Withdrawal, usage, or cash out of some or all of deposits and matched credits for legitimate purposes.
Maximum matchable savings/deposits/ amount	Deposits in *learn*$ave account of up to $250 in a month or $1,500 in total during the saving period that earn matched credits.
Non-matchable/ excess savings/ deposits	The amount deposited in a *learn*$ave account that does not qualify for matched credits (in excess of the maximum).
Participant Management Information System (PMIS)	The administrative data system that keeps track of participants' deposit and withdrawal activity in *learn*$ave accounts.
Peak savings	The highest closing amount saved during the saving period, net of unmatched withdrawals.
Saving period	The 36-month period participants have to qualify for matched credits.
Services	The financial management training and enhanced case management provided to *learn*$ave-plus participants.
Unmatched withdrawals	Withdrawals for reasons other than accredited education or small business start-up.

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