The Impact of ABS Program Participation on Long-Term Economic Outcomes

Stephen Reder, PhD
Portland State University

December 2014

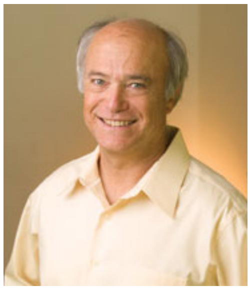
This report was produced under U.S. Department of Education Contract No. ED-VAE-11-O-0018 with NOVA Research Company, which included a subcontract with Portland State University. Joseph Perez served as the Contracting Officer's Representative. The views expressed herein do not necessarily represent the positions or policies of the Department of Education. No official endorsement by the U.S. Department of Education of any product, commodity, service, or enterprise mentioned in this publication is intended or should be inferred. For the reader's convenience, this publication contains information about and from outside organizations, including hyperlinks and URLs. Inclusion of such information does not constitute the Department's endorsement.

December 2014

This report is available on the Department's website at:
http://lincs.ed.gov/employer/1_ABS_Economic_Outcomes.pdf

Availability of Alternate Formats:
Requests for documents in alternate formats such as Braille or large print should be submitted to the Alternate Format Center by calling 202-260-0852 or by contacting the 504 coordinator via email at om_eeos@ed.gov.

Notice to Limited English Proficient Persons:
If you have difficulty understanding English, you may request language assistance services for Department information that is available to the public. These language assistance services are available free of charge. If you need more information about interpretation or translation services, please call 1-800-USA-LEARN (1-800-872-5327; TTY: 1-800-877-8339), or email us via the following link to our customer support team: Ed.Language.Assistance@ed.gov.



Stephen Reder, PhD
Professor, Department of Applied Linguistics
Portland State University

About the Author

Dr. Reder earned his PhD from Rockefeller University in 1977, and for the next nearly twenty years he conducted research in West Africa, Alaska, and the Northwest Regional Educational Laboratory. He joined the faculty of Portland State University (PSU) in 1995. His many interests include how adults learn language, literacy skills, language education, and the role of language, literacy, and technology in everyday life. He is an active member of the Literacy, Language, and Technology Research Group (LLTR) at PSU.

As part of his research activities, Professor Reder presents and publishes regularly. He co-edited a book, *Tracking Adult Literacy and Numeracy Skills: Findings from Longitudinal Research*, that was published by Routledge in 2009. His book *The State of Literacy in America* was published by the National Institute for Literacy in 1998. In that year he also co-edited *Learning Disabilities, Literacy, and Adult Education*, published by P. H. Brookes. Dr. Reder has also authored many journal articles and book chapters.

Acknowledgments

The author wishes to acknowledge the very able econometric assistance of Dr. Anita Alves Pena of Colorado State University and the helpful review of drafts by Dr. Michelle Yin of the American Institutes for Research.

Research Brief:
The Impact of ABS Program Participation on Long-Term Economic Outcomes

Introduction

National and international studies such as the recent Survey of Adult Skills[1] provide strong evidence of the need for and economic value of adult basic skills (ABS). A growing body of research indicates that there is strong economic return on basic skills at given levels of education.[2] Estimates have been made of the potential economic benefits that would accrue from increased educational attainment and levels of basic skills.[3] There is little rigorous research, however, showing that participation in basic skills programs directly impacts the skill levels, educational attainment, or social and economic well-being of adults with low levels of education. Most research on adult literacy development looks only at short-term changes as students pass through single ABS programs. Most studies use short follow-up intervals and include only program participants, making it difficult to see longer-term patterns of program participation and persistence and to assess long-term impact of ABS program participation.[4]

Although ABS program evaluation and accountability reports typically show small gains for program participants in test scores and other outcomes, these studies rarely include comparison groups of nonparticipants, and most studies that do include such controls have not found statistically significant ABS program impact.[5] Research is needed that compares adult literacy development among program participants and nonparticipants across multiple contexts and over significant periods of time to provide a life-wide and lifelong perspective on adult literacy development, and a better assessment of program impact on a range of outcome measures.

The Longitudinal Study of Adult Learning (LSAL) is one such lifelong and life-wide study. LSAL randomly sampled about 1,000 high school dropouts and followed them for nearly a decade from 1998–2007. LSAL followed both participants and nonparticipants in ABS programs, assessing their literacy skills and skill uses over long periods of time, along with changes in their social, educational, and economic status, offering a rich picture of adult literacy development.[6]

This is the first of a series of Research Briefs that utilize LSAL data to examine long-term impacts of ABS program participation on a range of outcome measures. Each Brief looks at a different outcome. This first report considers the long-term impact of participation on individuals' earnings. Subsequent reports will examine the impact of participation on literacy proficiency, General Educational Development (GED) attainment, engagement in postsecondary education, and voting in general elections (a measure of civic engagement).

This Research Brief addresses the following research questions:

- **What is the impact of participating in an ABS program on subsequent earnings?**
- **What is the time course of that impact?**
- **To what extent does GED attainment mediate the impact of participation on earnings?**

LSAL Design and Methodology

The overall design, methodology, population, and instrumentation of LSAL are described in detail elsewhere,[7] and only essential details are summarized here.

Population and Sample

The study population for LSAL was defined as adults who at the start of the study in 1998: lived in the Portland (Oregon) metropolitan area; were ages 18–44; had not completed high school nor were enrolled in high school or college; and were proficient but not necessarily native speakers of English. This defined population is a major segment of the target population of ABS programs operated by community colleges and other organizations in Oregon, and across the country. The sample was drawn through random digit dialing, with oversampling of current participants in ABS programs to ensure adequate numbers of both program participants and nonparticipants in the sampled "panel" of 934 adults who were followed from 1998–2007.[8] At study onset, the LSAL population had an average age of 28 and was evenly divided among males and females, with one-third from minority groups and one-tenth from immigrant populations. Nearly one in three reported having a learning disability.

Some of these defining characteristics of LSAL's population changed over time. Everyone's age increased, of course, while some adults received GEDs and college degrees, experienced changes in their employment and family situations, or moved away from the Portland area. LSAL followed its panel members regardless of these and other changes, with about 90 percent of the original panel retained in the study until data collection ended in 2007.[9]

Interviews and Assessments

LSAL conducted a series of six periodic interviews and skills assessments in respondents' homes:[10]

Wave 1: 1998–1999

Wave 2: 1999–2000

Wave 3: 2000–2001

Wave 4: 2002–2003

Wave 5: 2004–2005

Wave 6: 2006–2007

Note that the spacing of successive interviews was one year between Waves 1, 2, and 3 and two years between Waves 3, 4, 5, and 6.[11]

The initial interview gathered background information (e.g., demographics, family-of-origin characteristics, K–12 school history). The initial and each successive interview collected information about recent social, economic, and educational activities (e.g., participation in basic skill programs; postsecondary education and training; employment, job characteristics, and earnings; household and family composition; and life goals and aspirations).[12]

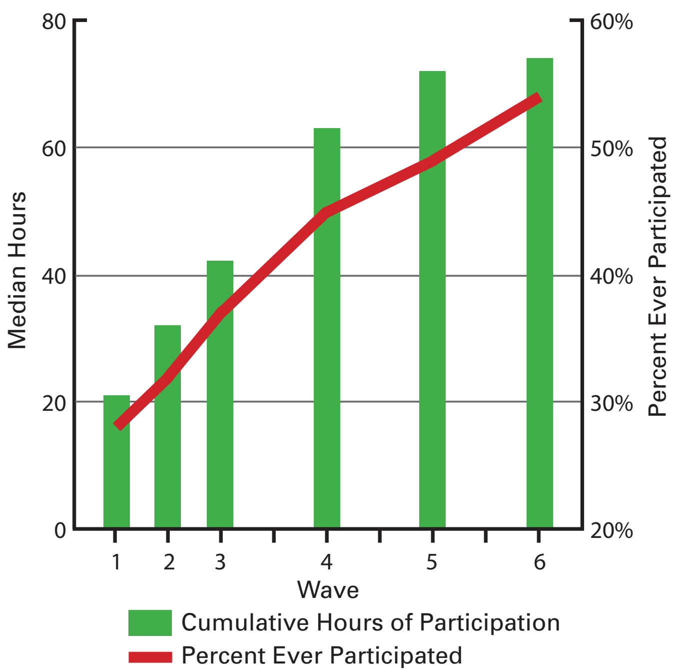


Figure 1. Percent of LSAL population who ever participated in ABS programs (line) and median hours of program attendance for those who participated (bars), by LSAL wave. LSAL waves 1–6 are placed on a time axis to represent their temporal spacing.

In each interview, individuals were asked if they currently were participating in adult basic skills programs to improve their reading, writing, or math skills or prepare for the GED Tests, or had done so within the preceding 12 months (asked in Wave 1) or since the time of their preceding interview (asked in Waves 2–6). Those who reported such participation were asked follow-up questions about timing, intensity, and duration of their participation. In the Wave 1 interview, they also were asked about their participation in such programs prior to 12 months before their first interview (back to the time they had dropped out of high school). These reports about ABS program participation were converted into variables for the number of hours of participation in each time period.

LSAL subjects were asked to consent to release of their Social Security Number (SSN)-matched administrative data from state agencies to assist the LSAL research. About 88 percent of the panel consented and provided their SSNs, which were matched to administrative records from Oregon and Washington state employment agencies to yield individuals' unemployment insurance hours and wages as reported quarterly by employers.[13,14,15] Eleven years (1997–2007) of hour and wage data from Oregon and Washington were collected.[16]

Key Findings

About two-thirds (68%) of the LSAL population had participated in an ABS program between the time of leaving high school and the end of LSAL in 2007. This is a much higher figure than what usually is reported for the percentage of the adult education target population that is served in a given program year. There are several reasons for LSAL's higher participation rate: (1) LSAL's 68 percent figure includes any participation over a long period of time rather than for a single program year; (2) LSAL's population excludes adults age 45 and above, an age group usually included in official counts of the target population but one that rarely participates in programs; and (3) LSAL's figure includes *any* participation rather than the 12-hours-per-year minimum typically required for inclusion in state and federal program reports.

Participation patterns in LSAL were often complex and fragmented, with many adults having multiple episodes of participation at different times and in different programs across the years of the study.[17] **Figure 1** shows the estimated percentage of the LSAL population that ever participated in an ABS program through each given wave of the study (line graph), as well as the median total hours of program attendance accumulated by participants (bar graph).[18] By the end of the study in 2007, over half (54%) of the LSAL population who had never participated in ABS programs when LSAL began had participated in ABS programs, accumulating a median of 74 hours of attendance between 1998–2006.

Figure 2 (top pane) shows average individual wages per year for the entire LSAL population from 1997–2007 in constant 1997 dollars. These are population estimates based on the subsample of 760 LSAL subjects with matching SSNs. Despite a dip in earnings midway in the time period (corresponding to the "dotcom bust" recession of the early 2000s), this population of high school dropouts showed a substantial 32 percent increase in real income over the ten-year period. This increase likely reflects gains in their work experience, skills, and educational credentials over this time period.

Given the complex patterns of program participation described above, there are a number of ways to define and measure participation for analyses that compare wages of participants and nonparticipants and estimate the impact of ABS program participation on wages. In the simplest formulation, the bottom pane of **Figure 2** shows annual wages by year for two LSAL subpopulations—those who had participated at some point in an ABS program between 1998 and 2006 and those who had never participated.

The wage trajectories of these two subpopulations are remarkably different. Participants started off in 1997 with earnings much lower than those of nonparticipants and experienced a gradually rising income across time, while the nonparticipants started at a much higher average income level in 1997, which remained fairly constant across the decade despite some ups and downs.[19] As participants' incomes increased and those of nonparticipants remained roughly stable, the income gap between the two subpopulations diminished until the mean income of participants finally exceeded that of nonparticipants in 2007. Between 1997 and 2007, the mean income of participants rose 53 percent (in constant 1997 dollars), from \$7,699 to \$11,792, while that of nonparticipants dropped 2 percent, from \$11,779 to \$11,580, over the same time period.

Estimating the Impact of Program Participation on Earnings

Propensity Score Matching Methods

The large overall difference in wage trajectories of participants and nonparticipants, at least at face value, suggests that ABS program participation may be central to sustained income growth for this low-education population. Care must be taken, however, in interpreting these differences. First, individuals in the target population self-selected in terms of participating in ABS programs, and there may be other important differences between the two groups as well. The effects of those other differences may be confounded with the effects of participation; this often is termed *selection bias* in program evaluation literature.[20] Some selection bias in LSAL could be due to differences in observable characteristics of participants and nonparticipants such as age, amount of education, race/ethnicity, immigration status, and so on. *Propensity score matching* methods are used to control for selection bias attributable to these observable individual

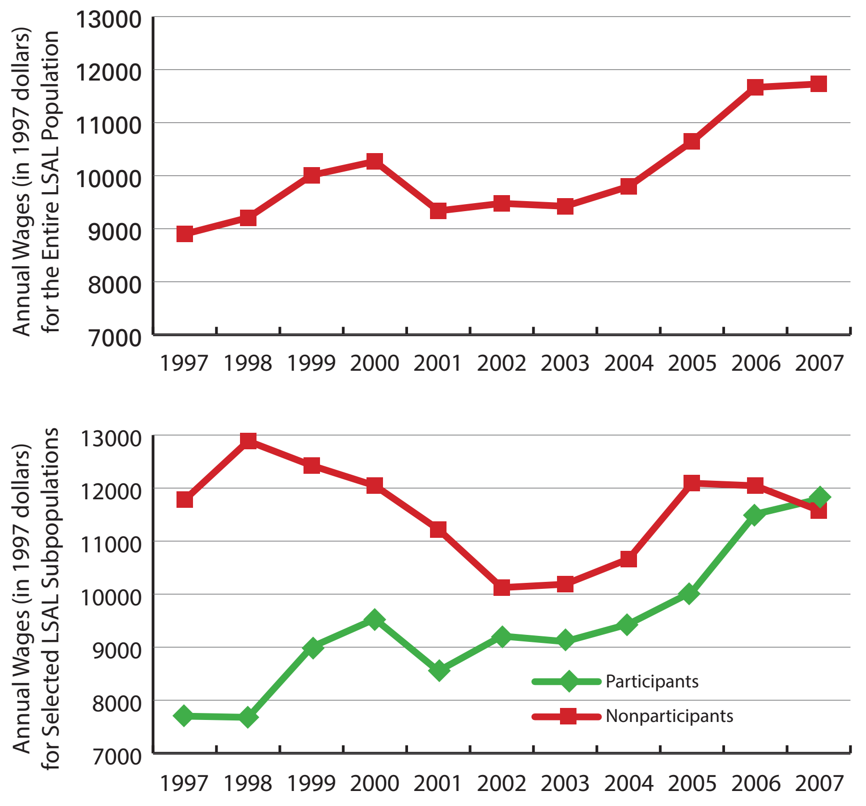


Figure 2. Mean annual income by year in Consumer Price Index (CPI)-adjusted 1997 dollars, for entire LSAL population (top pane) and subpopulations who did and did not participate in ABS programs (bottom pane).

characteristics. A propensity score in this context can be thought of as an estimated probability that an individual is a participant (received "treatment" of ABS programs) versus a nonparticipant (did not receive "treatment" and therefore can be thought of as a member of a "control" group).

Treatment Effects

Propensity scores were calculated for participation in ABS programs using individuals' age, gender, race/ethnicity, age at school dropout, years of schooling completed (before dropping out), presence of learning disabilities, enrollment in special education classes in school, immigration status, level of parental education, and 1997 earnings prior to possible ABS participation. These propensity scores were matched[21] to identify groups of participants and nonparticipants (who aside from their participation status were statistically alike) to compare their 2007 post-ABS program participation earnings using *treatment effects* models.

Five treatment effects models were examined to estimate the impact of ABS program participation on 2007 earnings. The models differed in how many total hours of attendance were required between 1998 and 2006 to constitute "participation." For each model, propensity score-matched participants so defined and nonparticipants (who had never participated in a program) were compared in terms of their 2007 earnings. As detailed in **Table A1** in the Appendix (page 9), there was no significant treatment effect when participation was defined as *any* attendance (i.e., one or more hours). The average treatment effect on participants was not significant when participation was defined as 25 or more hours of attendance. When participation was defined as 75 or more hours of attendance, the average treatment effect on participants almost reached statistical significance (t = 1.942, p = 0.053). At 100 hours or more, the average treatment effect on the treated was statistically significant and had an effect size of 0.45, nearly one-half of a standard deviation of participants' 2007 incomes, corresponding to $6,635 in 1997 dollars or $9,621 in 2013 dollars. This is an estimate of the average increase in ABS program participants' annual incomes by 2007 as a result of participating in the programs.

Figure 3 illustrates the income trajectories between 1997 and 2007 of propensity score-matched participants who had attended for a total of at least 100 hours and controls who had not participated at all, corresponding to the treatment and control groups shown in Model D of **Table A1**. Although the two groups had similar income trajectories early in the time period, they diverged after 2002 as the treatment effects became apparent as the earnings of the program participants continued to rise while the earnings of the nonparticipants fell.

Difference-in-Differences (DID)

The *difference-in-differences* model compares changes over time in incomes of propensity score-matched groups of participants and nonparticipants, looking at their pre-LSAL (1997) and post-LSAL (2007) incomes.[22] The difference-in-differences and treatment effects models show similar results:

- There is no statistically significant impact of program participation when participation is defined simply as *any* attendance.
- There is a statistically significant positive impact when participation is defined as 100 or more hours of attendance. The estimated post-participation impact is substantial, raising participants' annual post-program incomes in 2007 by an average of about $10,000 (in 2013 dollars): $10,179 is the DID estimate; $9,621 is the treatment effects estimate. The similarity of results obtained with the two models adds to the robustness of these central findings.

Fixed Effects (FE) Panel Regression Methods

Although both the treatment effects and difference-in-differences models show strong impact of ABS program participation on earnings, they also share important methodological limitations. One limitation is their use of propensity score matching, based on *observed* individual characteristics, to control the comparisons of participants and nonparticipants. Useful as it is, propensity score matching is not able to control for differences in *unobserved* individual characteristics. A second limitation of these models is that

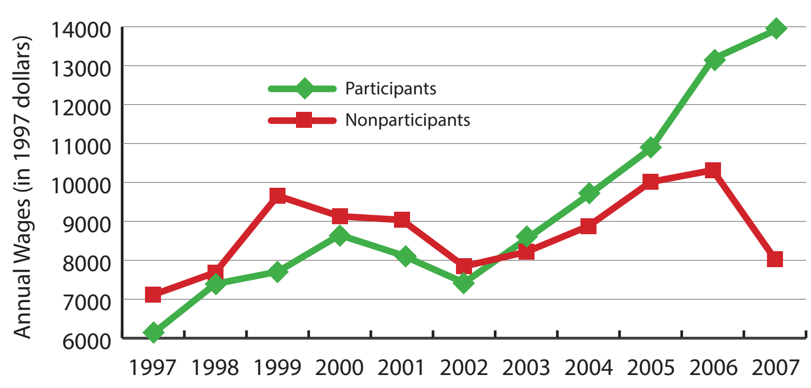


Figure 3. Income trajectories of propensity score-matched participants and nonparticipants. Participants had attended programs for a total of at least 100 hours between 1998 and 2006. Nonparticipants had never attended a program through 2006.

they do not allow for systematic examination of the time course of participation and its impact on earnings. The models were used to look in different ways at changes in income between 1997 and 2007, the years bracketing program participation, so the earnings impact of participation was estimated regardless of when it occurred during this decade-long period. This prevents us from seeing the time course of the impact. For example: Does participation give rise to an immediate or a slowly growing impact, or to a long-lasting or a gradually dissipating impact, and so forth? The complementary approach of *fixed effects panel regression* was used to address these issues.

Fixed effects panel regression models analyze variations in year-to-year income *within* individuals. Because they analyze variation over time within individuals, the FE models eliminate bias in comparing participants and nonparticipants due to differences in *all* time-invariant individual characteristics, both observed and unobserved. These panel regressions analyze variations in year-to-year income in relation to individuals' observed year-by-year program participation. The models are used to estimate the impact of participation on earnings as well as the time course of that impact. Effects of other observed time-varying variables such as GED attainment also can be examined within this framework.

The panel regressions of yearly income were conducted with fixed effects of individuals and fixed effects of the six LSAL time periods. The fixed period effects eliminate common differences across individuals across time periods, such as changes over time in labor market conditions. By fixing the effects of both individuals and time periods, these panel regressions highlight the effects of observed time-varying ABS participation on income trajectories. To measure different aspects of the complex participation patterns observed, several time-varying participation variables were contrasted in various FE models. These participation variables are alike in equaling zero at time points before an individual participated and equaling zero at all time points for individuals who never participated. Added to the fixed effects of individuals and time periods in the panel regression models, these time-varying predictors provide sharp measures of the effects of program participation on income trajectories.

Four different FE panel regression models are described, with estimates of key parameters, in **Table A4** (page 11). The main results are consistent with the preceding findings and add some details about the time course of the impact of program participation on earnings. The number of hours of program attendance an individual accumulated through a given point in time is a statistically significant, positive predictor of that individual's earnings at that point in time. The elapsed time since an individual started participating in an ABS program is also an important predictor of the individual's earnings at later points in time. The intensity of participation is also important, with about 100 hours again being a critical level. The strongest predictor of future earnings combines both intensity and elapsed time since the onset of participation: the more time elapsed since an individual accumulated 100 hours of attendance, the greater the individual's earnings tended to be.

Since elapsed time after participation onset is a positive predictor of earnings, it is important to examine what may be happening during that elapsed time period that helps drive incomes upward. This can be examined by adding other observed time-varying variables to the fixed effects panel regression models. One important variable is likely to be GED attainment. All individuals in the LSAL population were high school dropouts, many of whom participated in ABS programs to prepare for GED Tests.[23] We also know from previous research that many individuals prepare for or take GED Tests without going to ABS programs. Time-varying measures of GED attainment status were added to FE models that included measures of ABS participation. The Appendix provides details about specifications and results for these models. The principal result is that receipt of the GED credential has a significant positive impact on earnings over and above the impact of program participation. Like the impact of ABS program participation, the full impact of GED attainment on earnings takes several years to develop.

Discussion

The results of this research are clear. Three different methods—treatment effects, difference-in-differences, and fixed effects panel regressions—all show statistically significant and financially substantial impacts of ABS program participation on earnings growth. Individuals who participate in programs have higher future earnings as a result of participating, income premiums are larger with more intensive participation, and minimal levels of participation do not produce statistically significant premiums.

It is important to note that this income premium takes time (on the order of years) to develop after participation. Because of the complexity of the program participation patterns observed, LSAL's relatively small sample size limits the precision with which estimates can be made of how many hours of attendance and how long a follow-up period are required to see a significant earnings premium of a given size. Details vary with the measure of participation and analytical method used. The specifics likely vary with characteristics of programs and participants. However, it is clear from the LSAL analyses conducted that higher intensities of participation, with a threshold of around 100 hours, have substantial impact on future earnings, an impact that typically takes several years to develop after participation. Comprehensive reviews of program evaluation and persistence research have concluded that 100 hours of attendance is the approximate point at which program impact on basic skills development becomes discernible as well.[24] Additional research with larger longitudinal data sets and those drawn from other contexts can help clarify some of these important details. The impact models developed here could address these questions more precisely if applied to larger longitudinal data sets that follow comparable ABS program participants and nonparticipants over longer time periods.

GED attainment mediates a small portion of the impact of ABS program participation on income. GED attainment has its own direct impact on income in the LSAL data. Just as the impact of participation takes time to develop, so apparently does the impact of GED attainment, with the income premium increasing with the number of years since receipt of the credential. This is consistent with previous research on the economic effects of GED attainment.[25] It is of considerable interest that around 100 hours or more of ABS participation retain a positive, increasing impact on earnings even with effects of GED attainment taken into account. Other Research Briefs in this series look more closely at the impact of participation on literacy proficiency, GED attainment, and postsecondary engagement.

Notes and References

1 Hanushek, E. A., Schwerdt, G., Wiederhold, S., & Woessmann, L. (2013). *Returns to skills around the world: Evidence from PIAAC* (OECD Education Working Papers No. 101). Paris: OECD Publishing. Retrieved from http://dx.doi.org/10.1787/5k3tsjqmvtq2-en. Organisation for Economic Cooperation and Development. (2013). *OECD Skills Outlook 2013: First results from the Survey of Adult Skills*. Paris: OECD Publishing. Retrieved from http://dx.doi.org/10.1787/9789264204256-en. Organisation for Economic Cooperation and Development. (2013). *Time for the U.S. to reskill? What the Survey of Adult Skills says*. Paris: OECD Publishing. Retrieved from http://dx.doi.org/10.1787/9789264204904-en.

2 Acemoglu, D., & Autor, D. H. (2011). Skills, tasks, and technologies: Implications for employment and earnings. In D. Card & O. Ashenfelter (Eds.), *Handbook of labor economics* (Vol. 4; pp. 1043-1171). Amsterdam: Elsevier-North Holland. Hanushek, E. A., Jamison, D. T., Jamison, E. A., & Woessmann, L. (2008). Education and economic growth: It's not just going to school, but learning something while there that matters. *Education Next, 8* (2), 62-70. Heckman, J. J., Stixrud, J., & Urzua, S. (2006). The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. *Journal of Labor Economics, 24* (3), 411-482. Pryor, F. L., & Schaffer, D. L. (1999). *Who's not working and why: Employment, cognitive skills, wages, and the changing U.S. labor market.* Cambridge, MA: Cambridge University Press. Reder, S. (2010). *Adult literacy and economic growth*. Washington, DC: National Institute for Literacy.

3 Parker, J., & Spangenberg, G. (2013). *Stepping up to ROI in adult education: A survey of state activity.* New York: Council for the Advancement of Adult Literacy. Retrieved from http://www.caalusa.org/SteppingUptoROI.pdf.

4 Beder, H. (1999). *The outcomes and impacts of adult literacy education in the United States* (Report No. 6). Cambridge, MA: National Center for the Study of Adult Learning and Literacy. Retrieved from http://ncsall.net/index.php@id=669.html. Brooks, G., Davies, R., Ducke, L., Hutchison, D., Kendall, S., & Wilkin, A. (2001). *Progress in adult literacy: Do* learners *learn?* London: The Basic Skills Agency. Lesgold, A., & Welch-Ross, M. (Eds.). (2012). *Improving adult literacy instruction: Options for practice and research*. Washington, DC: National Academies Press. Miller, B., Esposito, L., & McCardle, P. (2011). A public health approach to improving the lives of adult learners: An introduction to the *Special Issue* on adult literacy interventions. *Journal of Research on Educational Effectiveness, 4*, 87-100. Smith, M. C. (2009). The educational psychology of reading in adulthood. In M. C. Smith (Ed.), *Handbook of research on adult learning and development* (pp. 201-218). New York and London: Routledge.

5 Reder, S. (2012). *The Longitudinal Study of Adult Learning: Challenging assumptions.* Montreal: The Centre for Literacy. Smith, M. C. (2009). The educational psychology of reading in adulthood. In M. C. Smith (Ed.), *Handbook of research on adult learning and development* (pp. 201-218). New York and London: Routledge. Vorhaus, J., Litster, J., Frearson, M., & Johnson, S. (2011). *Review of research and evaluation on improving adult literacy and numeracy skills* (Research Paper No. 61). London: Department for Business Innovation and Skills. Wolf, A., & Evans, K. (2011). *Improving literacy at work*. London: Routledge.

6 Reder, S. (2013). Lifelong and life-wide adult literacy development. *Perspectives on Language and Literacy*, *39* (2), 18-21.

7 Reder, S. (2009). The development of literacy and numeracy in adult life. In S. Reder & J. Bynner (Eds.), *Tracking adult literacy and numeracy: Findings from longitudinal research* (pp. 59-84). New York and London: Routledge.

8 Sampling weights calculated for each panel member were used to make estimates for the defined target population from the sampled panel data.

9 Analysis of missing interviews indicates that they were *missing at random* (MAR) with respect to the variables examined.

10 Respondents were paid for each of these sessions.

11 Individuals were interviewed at about the same time in each wave so that there was approximately constant spacing among individuals' successive interviews and assessments (e.g., a respondent interviewed in February 1999 in Wave 1 was interviewed during February 2000 for Wave 2, February 2001 for Wave 3, etc.).

12 The interview instruments are available at www.lsal.pdx.edu/instruments.html.

13 A few individuals with no unemployment insurance (UI)-covered income during a given quarter may have been employed in Oregon or Washington, but not in UI-covered occupations or industries. Approximately 92 percent of all paid employment in Oregon was UI-covered during the years 1997–2007 (S. C. Williams, personal communication, June 30, 2010). Major exceptions include certain self-employed, agricultural, government, and religious workers. A few other individuals moved to and may have worked in third states. Because LSAL also has interview-based information about individuals' employment and income histories, such potential biases within UI coverage can be addressed. Initial indications are that these biases are relatively small and do not alter the principal findings reported here.

14 Of the 822 individuals who provided this consent, 62 had no UI-covered employment reported at any time during the 11-year administrative window (1997–2007) and were not included in analyses of UI data. Although some of these individuals may not have worked at all during the 11-year period, other individuals evidently provided erroneous Social Security Numbers that failed to match state administrative records. These two patterns are indistinguishable in the administrative records. Bias would be introduced by treating these individuals as if they had had no employment for the 11 years; a different bias would be introduced by omitting them. The interview-reported weeks worked in the previous year for the group providing unmatched SSNs and for those refusing to provide SSNs are not significantly different, so the unmatched SSN group was omitted from the data rather than assuming that all of these individuals had had no employment over the 11-year period.

15 The composition of the consenting SSN-matched population differed slightly from that of the nonconsenting population of LSAL subjects. There were slightly lower rates of consent among LSAL subjects who were immigrants, who had lower literacy proficiency scores, and whose parents had lower levels of education. There were no significant differences between consenters and nonconsenters in terms of gender, race, or ethnicity; prevalence of learning disabilities; engagement in literacy practices; years of schooling completed (before dropping out); or participation in programs. Zero values were imputed for individuals for quarters for which they had no income or hours reported by either Oregon or Washington. The quarterly UI earnings data for 1997–2007 were annualized and then adjusted by the Consumer Price Index into constant 1997 dollars.

16 My appreciation goes to the staff of the Oregon Office of Community Colleges and Workforce Development, the Oregon Employment Department, the Washington State Board of Community and Technical Colleges, and the Washington State Employment Security Department for their cooperation and assistance with this data matching.

17 More detailed discussion is given in: Reder, S. & Strawn, C. (2001). Program participation and self-directed learning to improve basic skills. *Focus on Basics*, *4* (D), 14-17.

18 Individuals who had participated in ABS programs prior to 1998 were not included because information about timing and hours of participation was incomplete prior to 1998.

19 The term "income" in this Brief is used interchangeably with "(wage) earnings."

20 Angrist, J. D. (1998). Treatment effect heterogeneity in theory and practice. *The Economic Journal*, *114* (1), C52-C83. Imbens, G. W., & Wooldridge, J. M. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature, 47* (1), 5-86.

21 Probit models were used to generate propensity scores, which were balanced within a region of common support for participants and nonparticipants.

22 Individuals who first participated in ABS programs prior to 1998 or after 2006 were excluded from these analyses so that all ABS participation occurred between the "pre" and "post" comparison years of 1997 and 2007.

23 Of the LSAL population, 27 percent received a GED credential at some point during the study. Of those who received a GED, 76 percent had participated in an ABS program.

24 Comings, J. P. (2007). Persistence: Helping adult education students reach their goals. In J. Comings, B. Garner, & C. Smith (Eds.), *Annual review of adult learning* (Vol. 7; pp. 23-46). Mahwah, NJ and London: Lawrence Erlbaum. Vorhaus, J., Litster, J., Frearson, M., & Johnson, S. (2011). *Review of research and evaluation on improving adult literacy and numeracy skills* (Research Paper No. 61). London: Department for Business Innovation and Skills.

25 Tyler, J. H. (2004). Does the G.E.D. improve earnings? Estimates from a sample of both successful and unsuccessful G.E.D. candidates. *Industrial and Labor Relations Review, 57* (4), 579-598.

Appendix: Supplementary Information and Tables

Treatment Effects

Table A1 summarizes treatment effects analyses of ABS participation on future earnings.[1] Each row of the table summarizes a separate analysis in which "treatment" is defined as a specified minimum number of hours of ABS program participation between 1998 and 2006. In each analysis, controls are individuals who *never* participated. In the first row of results, the analysis compares "treated" and "control" individuals in which *any* amount of attendance (i.e., one hour or more) is considered "treatment." In the second row of results, attendance of 25 or more hours is considered "treatment," and so forth.

Table A1. Treatment Effects of Participation in ABS Programs on 2007 Earnings

Model	Minimum Hours of Attendance to Be Considered "Treated"	Number of Propensity Score-Matched "Treated"	Number of Propensity Score-Matched "Controls"	Average Treatment Effect on the "Treated" (ATET)	*t*	p	Effect Size
A	1	396	105	1.111	1.411	0.159	n.s.
B	25	216	82	0.789	0.953	0.342	n.s.
C	75	216	80	2.014	1.942	0.053	n.s.
D	100	197	68	2.142	2.469	0.014	0.45
E	150	154	62	2.272	2.085	0.039	0.48

For each analysis, the table displays the number of propensity score-matched "treated" and "control" individuals involved in nearest neighbor paired comparisons of the 2007 (log) earnings outcome.[2] In the first specification shown as Model A in the table, for example, 396 participants and 105 controls (i.e., individuals who did not participate in an ABS program) were matched in terms of propensity scores; the average treatment effect on the treated (ATET) of ABS participation so defined is estimated to be 1.111, which has a *t*-value of 1.411 for the paired comparison of their 2007 incomes, a difference which is not statistically significant (p=0.159). As the table shows, comparisons of controls with treated are not statistically significant when treatment is defined as 25 or more hours of program attendance (Model B) or even 75 or more hours of program attendance (Model C), although the ATET approaches statistical significance (p=0.053) for 75 or more hours of attendance.

As shown for Model D, the estimated ATET is statistically significant if treatment is defined as 100 or more hours of program attendance (ATET=2.142, *t*=2.469, p=0.014) and corresponds to a substantial effect size of 0.45 standard deviations in 2007 income. With treatment defined as 150 or more hours of attendance in Model E, the ATET is slightly larger and is again statistically significant (ATET=2.272, *t*=2.085, p=0.039), corresponding to an effect size of 0.48.

Difference-in-Differences (DID)

A second model based on propensity score matching was used to explore the robustness of the findings of the treatment effects model. The *difference-in-differences* model compares changes over time in incomes of propensity score-matched groups of participants and nonparticipants, looking at their pre-LSAL (1997) and post-LSAL (2007) incomes.[3] The DID calculates the income differences between participants and nonparticipants in 1997 and in 2007, and then examines the difference in these two differences (i.e., the DID). Subject to limitations of propensity score matching, a significant DID provides evidence of the effect of participation on income changes.

With ABS participation defined as *any* amount of program attendance between 1998 and 2006, the estimated DID shown in **Table A2** (page 10) is not statistically significant (DID for log earnings=0.866, standard error=1.122, *t*=0.77, p=0.441). On the other hand, **Table A3** (page 10) shows that with ABS participation defined as 100 or more hours of attendance, the DID for participants and nonparticipants (who had never attended a program) is highly statistically significant in the direction of positive earnings growth associated with participation (DID for log earnings=3.074, standard error=1.061, *t*=2.90, p=0.004). The DID for 100 or more hours of participation corresponds to $7,020 in 1997 dollars (illustrated on page 11 in **Figure A1**) and $10,179 in 2013 dollars.

Table A2. Difference-in-Differences for Any Participation Versus No Participation

Number of Propensity Score-Matched Observations: 1,020			
Population	Baseline (1997)	Follow-Up (2007)	Total
Control	135	135	270
Treated	375	375	750
Total	510	510	1,020

Control = no participation.
Treated = any participation.
R-square: 0.05111.

Difference-in-Differences Estimation							
Outcome Variable	Baseline Control	Baseline Treated	Baseline Diff	Follow-Up Control	Follow-Up Treated	Follow-Up Diff	DID
Log Income	7.622	6.722	-0.900	5.337	5.302	-0.034	0.866
Std. Error	0.534	0.414	0.676	0.637	0.630	0.896	1.122
t	14.27	5.45	-1.33	4.03	5.81	0.07	0.77
$P > t$	0.000	0.000	0.183	0.000	0.000	0.969	0.441

Means and Standard Errors are estimated by linear regression.

Table A3. Difference-in-Differences for Participation of 100 or More Hours Versus No Participation

Number of Propensity Score-Matched Observations: 680			
Population	Baseline (1997)	Follow-Up (2007)	Total
Control	143	143	286
Treated	197	197	394
Total	340	340	680

Control = no participation.
Treated = 100 or more hours.
R-square: 0.05394.

Difference-in-Differences Estimation							
Outcome Variable	Baseline Control	Baseline Treated	Baseline Diff	Follow-Up Control	Follow-Up Treated	Follow-Up Diff	DID
Log Income	7.651	6.126	-1.525	5.386	6.935	1.549	3.074
Std. Error	0.513	0.468	0.695	0.617	0.513	0.802	1.061
t	14.91	4.39	-2.19	3.98	9.85	2.31	2.90
$P > t$	0.000	0.000	0.029	0.000	0.000	0.054	0.004

Means and Standard Errors are estimated by linear regression.

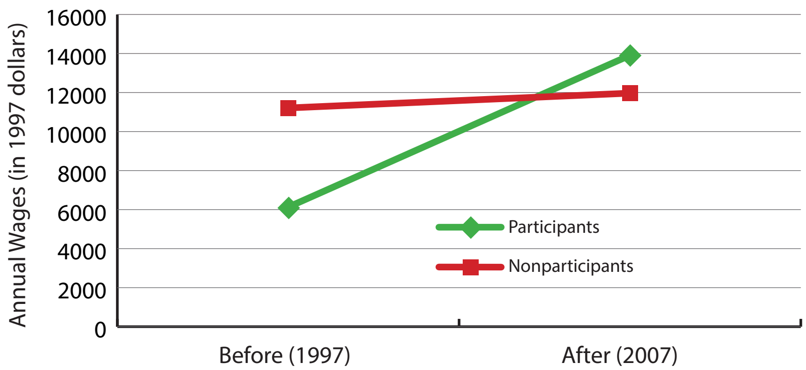


Figure A1. Plot of results for difference-in-differences analysis of changes in income (in 1997 dollars) for propensity score-matched ABS program participants (100 or more hours of attendance) and nonparticipants (no participation at all).

Fixed Effects Panel Regressions

Table A4 shows four fixed effects panel regression models of log annual income. Each of the models, A through D, contains one or two ABS program participation variables as time-varying predictors, as well as the fixed effects of individuals and time periods that are not shown in the table. The table shows estimated regression coefficients for participation variables involved in each model, along with their standard errors and *t*-values. Asterisks on the *t*-values indicate levels of statistical significance.

Model A examines the effect of CUMHOURS on annual income. CUMHOURS is the total hours of program participation an individual accumulated through each LSAL time point. CUMHOURS is zero at the beginning of LSAL and increases at each subsequent time point by the number of hours the individual attended an ABS program. For individuals who never participated, CUMHOURS remains at zero. The positive, statistically significant regression coefficient of CUMHOURS indicates that within individual income trajectories, the more hours of participation accumulated by individuals through a given point in time, the higher their incomes tend to be at that point in time.

Table A4. Summaries of Fixed Effects Panel Regressions of Log Annual Income

Model	Participation Variables	Coeff. in Panel Regr.	Robust Std. Err.	*t*
A	CUMHOURS	.0032	.0014	2.39*
B	YEARS	.1221	.1756	0.70
C	YRSCUM100	.4005	.1510	2.65**
D	YRSCUM100 CUMHOURS	.3106 .0020	.1411 .0013	2.20* 1.55

Regressions contain one or two time-varying measures of participation, as shown, as well as fixed effects of individuals and time periods, which are omitted from the table.
* p<.05; ** p<.01.

In contrast, Model B examines variation of income in relation to elapsed time since the onset of an individual's participation. Elapsed time could be relevant if, for example, program impact on income develops over time as new skills gradually find traction in the workplace. We see in Model B that the YEARS variable, measuring elapsed time since onset of participation, does *not* have a statistically significant regression coefficient. There is, thus, no significant linear increase in income impact following onset of participation as measured by this model.

Model C is similar to Model B, but sets the onset of participation to the time when the individual had accumulated 100 or more hours of attendance in ABS programs. YRSCUM100 measures elapsed time in years since the onset of participation so defined. YRSCUM100 has a statistically significant regression coefficient, reflecting a significant linear growth of impact over time following onset of a sufficient amount of attendance.

Thus, both hours of accumulated participation and elapsed time since onset of participation are significant time-varying predictors of income growth. Model D includes both CUMHOURS and YRSCUM100 as predictors in a panel regression to determine whether they are distinct predictors of income and which is the stronger predictor. The results shown for Model D indicate that only YRSCUM100 has a statistically significant regression coefficient. In this model, CUMHOURS—a significant predictor in Model A—is no longer a significant predictor of income once effects of elapsed time following a sufficient amount of participation are taken into account.

Models E-H in **Table A5** (page 12) explore the impact of GED attainment on earnings when added to a baseline model containing a significant time-varying measure of ABS participation. The two baseline ABS participation measures used are CUMHOURS (Models E and F) and YRSCUM100 (Models G and H). One of two time-varying

GED attainment status variables was added to each of the baseline models. The variable HAVEGED, added in Models E and G, is a "step function" that has value zero until the individual received a GED and value one thereafter; for individuals who never attained a GED, HAVEGED is always zero. Although the time-varying measures of ABS participation remain statistically significant positive predictors of earnings when HAVEGED is added to Models E and G, HAVEGED does not have a statistically significant regression coefficient. These results suggest that receipt of the GED credential does not have a significant step-like impact on income when the effects of program participation are taken into account.

Table A5. Summaries of Fixed Effects Panel Regressions of Log Annual Income

Model	Participation Variables	Coeff. in Panel Regr.	Robust Std. Err.	*t*
E	CUMHOURS HAVEGED	.0030 .4793	.0013 .4648	2.25** 1.03
F	CUMHOURS YEARSGED	.0028 .4160	.0013 .1394	2.17* 2.98**
G	YRSCUM100 HAVEGED	.3848 .5360	.1478 .4542	2.60** 1.18
H	YRSCUM100 YEARSGED	.3025 .3821	.1437 .1364	2.10* 2.80**

Regressions contain a time-varying measure of participation and a time-varying measure of GED attainment, as shown, as well as fixed effects of individuals and time periods, which are omitted from the table.
* p<.05; ** p<.01.

YEARSGED, another time-varying measure of GED attainment status, was added to the baseline Models F and H. YEARSGED measures the number of years since the individual received the GED credential. It equals zero before receipt of the GED (and thus equals zero at all times for individuals who never received the GED). Both YEARSGED and the baseline program participation variable have statistically significant, positive coefficients in Models F and H. Receipt of the GED has a positive impact on earnings that increases over time with the effects of program participation taken into account.[4]

Notes

1 Logarithm ("log") of income is used here, as often is done in similar analyses, to make distribution of income more bell-shaped.

2 See, for example, Dehejia, R. H., & Wahba, S. (2002). Propensity score matching methods for non-experimental causal studies. *Review of Economics and Statistics*, *84* (1), 151-161.

3 Individuals who first participated in ABS programs prior to 1998 or after 2006 were excluded from these analyses so that all ABS participation occurred between the "pre" and "post" comparison years of 1997 and 2007.

4 The coefficient of YRSCUM100 in Model H is 0.3025 whereas it is 0.4005 in Model C, indicating that only a small portion of program impact on income is attributable to the impact of GED attainment.