

**Assessing the Effectiveness of Financial Coaching:  
Evidence from the Boston Youth Credit Building Initiative**

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**Abstract:** Since the 2008 financial crisis, there has been renewed interest in providing financial education programs, including financial coaching, that are intended to improve consumer financial decision making, with a special emphasis on developing financial skills among youth. Using a randomized control trial, we estimate the causal effects of a financial coaching program aimed at young adults based on linked individual-level data from administrative credit reports and self-reported survey responses. Within the first six months of the program, individuals in the treatment group were 10 percentage points more likely to have established a credit score compared to the control group. By the end of the 18-month observation window, the average credit score among individuals with a credit file prior to the start of the program was 26 points higher for the treatment group relative to the control group, raising the likelihood of achieving a “good” credit rating by 8 percentage points. These effects persist six months beyond the end of the program and are even larger among younger individuals and African-Americans. Much of the impact of the program appears to be driven by improvements in financial self-efficacy—a finding confirmed by focus group discussions—which may have been the missing ingredient in prior financial education programs. Finally, we demonstrate that the program has meaningful impacts on individuals, such as reducing their reliance on alternative financial services and lowering the interest rate they pay on car loans. These findings are encouraging and offer important insights for states and localities seeking effective ways to incorporate financial education into youth workforce development programs.

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Since the 2008 financial crisis, there has been renewed interest in identifying which individuals are at greatest risk for experiencing poor financial outcomes and in providing financial education programs to improve their financial decision making. With the creation of the Consumer Financial Protection Bureau (CFPB) in 2011, the federal government now spends an additional \$44 million per year on financial education (CFPB 2017). Moreover, special emphasis has been placed on developing financial skills among youth. At the federal level, financial literacy became a required element for youth workforce development programs under the 2014 Workforce Innovation and Opportunity Act (WIOA). At the local level, the number of states requiring a personal financial education course in high school increased from 7 to 17 over the past decade (Council for Economic Education 2016).

However, prior studies evaluating the impact of financial education programs on outcomes have not produced consistent results, particularly for programs aimed at youth, making their cost-effectiveness uncertain at best (Lyons et al. 2006; McCormick 2009; Willis 2011; Hastings, Madrian, and Skimmyhorn 2013; Cole, Paulson, and Shastry 2016; Brown et al. 2016; Walstad et al. 2017). Approaches that focus on developing financial capability among youth by combining financial literacy with access to financial products appear to hold greater promise (Johnson and Sherraden 2007; Loke, Choi, and Libby 2015). Yet researchers have found that the effects of such programs are small (Fernandes, Lynch, and Netemeyer 2014) or affect only some outcomes (e.g., savings behavior) but not others (e.g., reducing defaults) (Miller et al. 2014).

In response, policymakers have increasingly turned to **financial coaching** as a way to **improve consumer financial decision making** by incorporating financial knowledge, access to financial products and services, and one-on-one coach-client interactions (Collins, Baker, and Gorey 2007). One of the largest such initiatives, the Working Families Success Network,

currently includes 115 sites in over 30 cities offering a range of services to low- and moderate-income adults. Financial coaching is an ongoing process that involves setting goals, establishing a concrete plan of action, and monitoring individual progress—all to change financial habits and improve long-term outcomes (Collins and O’Rourke 2012). Yet to date, only a handful of studies have assessed whether financial coaching improves economic knowledge, decision making, or well-being. Most studies fail to address issues of positive selection among individuals who choose to enroll in such programs, and few attempt to identify which program mechanisms may be important for improving outcomes.

In this paper, we estimate the causal effects of a one-year financial coaching program for young adults, the Boston Youth Credit Building Initiative (BYCBI), on a range of financial and behavioral outcomes. Using linked individual-level data from administrative credit reports and self-reported survey responses, we measure gains in credit scores and ratings as well as improvements in financial knowledge, behaviors, self-efficacy, and well-being. A randomized control trial provides a robust control group to assess program impacts, with stratification across demographic characteristics and recruitment sites to explore heterogeneity in outcomes.

We find that the BYCBI helps build credit by increasing access as well as improving scores. Within the first six months of the program, individuals in the treatment group were 10 percentage points more likely to have established a credit score compared to the control group. By the end of the 18-month observation window, the average credit score among individuals with a credit file prior to the start of the program was 26 points higher for the treatment group relative to the control group, raising the likelihood of achieving a “good” credit rating by 8 percentage points. These effects are even larger among those who complied with the program. We further show that these improvements reflect significant changes in the underlying factors

that typically affect one's credit score, such as having a history of sustained on-time payments, and a lower likelihood of adverse events such as delinquencies and collections. Finally, we demonstrate that the program has meaningful impacts on individuals beyond improving their credit scores, such as reducing their reliance on alternative financial services and lowering the interest rate they pay on car loans—outcomes that are of interest to policymakers hoping to improve the financial well-being of low- and moderate-income groups.

Our findings offer several policy-relevant lessons for future program design. First, the subgroup analysis shows that the BYCBI has greater impacts on younger individuals and African-Americans, suggesting ways that cities with limited resources may want to target similar programs. Second, the mediation analysis demonstrates that much of the impact of the program appears to be driven by improvements in financial self-efficacy—a finding confirmed by focus group discussions—which may have been the missing ingredient in prior financial education programs. Finally, the path toward better credit evolves over time and across different dimensions—even after the program ended—suggesting that the program may affect the behavior of the treatment group beyond the short term. These findings are encouraging and offer important insights for cities and states seeking effective ways to incorporate financial education into youth workforce development programs to comply with the new WIOA requirements.

#### RELEVANT LITERATURE

While the general consensus is that financial education should have a positive effect, the findings from prior studies have been mixed—particularly for programs aimed at youth—making their cost-effectiveness uncertain at best (Lyons et al. 2006; McCormick 2009; Hastings, Madrian, and Skimmyhorn 2013; Brown et al. 2016; Walstad et al. 2017). For example, some studies have demonstrated that financial education in high school has a positive effect on

financial knowledge and behavior among youth as well as improved outcomes later in life (Bernheim, Garrett, and Maki 2001; Danes 2005; Varcoe et al. 2005). Yet others have shown that these programs do not have a significant effect on improving financial knowledge scores of high school students in the United States (Mandell 2005)—which may be the result of having been introduced during periods of high economic growth (Cole, Paulson, and Shastri 2016—and that the costs likely outweigh the benefits (Willis 2011).

In part, it's likely that the mixed results of prior studies reflect a lack of clarity regarding how to measure the effectiveness of financial education programs. Most studies use some measure of financial literacy, yet without a standard definition or construct, this may include measurements of financial knowledge, behavior, ability, or well-being—all of which can be thought of as distinct constructs. Huston (2010) provides a more structured approach to measure financial literacy that includes both the “knowledge and skills needed to make choices within a financial marketplace that all consumers face regardless of their particular characteristics.” Applying this framework enables researchers to better able to identify how well financial education improves the human capital needed to engage in appropriate financial behavior and make sound decisions. Yet it should be noted that improvements in financial literacy using this definition of both knowledge and skills may still fail to translate into enhanced financial well-being if other characteristics such as impulsiveness, behavioral biases, unusual preferences, or external circumstances also contribute to financial distress.

Building on this framework, efforts that incorporate access to financial products and services, in addition to the educational component, appear to be a more effective approach (Sherraden 2013). The general consensus is that the ability to put knowledge immediately into practice is most helpful in establishing the skills needed to develop healthy financial habits and

behaviors. For example, previous studies have found that combining education and credit card use increases mastery and self-esteem among young adults. These effects are greater for low-income young adults, who often lack the opportunity to gain the knowledge and skills necessary to establish healthy financial behaviors early on and instead end up repairing credit or managing excessive debt later on in life (Dwyer, McCloud, and Hodson 2011). Financial education programs that occur during the transition into early adulthood have been shown to be particularly effective by taking advantage of teachable moments when youth are receiving their first paychecks and making their first financial decisions, such as opening a bank account, acquiring a credit card, or preparing to pay for college (Loke, Choi, and Libby 2015). Financial coaching programs take this model one step further by providing individuals with information tailored to their situation at a time when they can apply it directly by changing their behavior, all while receiving feedback and guidance from a trained coach (Fernandes, Lynch, and Netemeyer 2014).

#### Theory: How Might Financial Coaching Improve Outcomes?

Financial coaching programs differ from other approaches primarily due to the continuous feedback loop that involves setting goals, establishing a concrete plan of action, and monitoring individual progress—with the objective of changing financial behaviors to improve long-term outcomes—rather than simply increasing financial knowledge or providing access to financial products (Collins, Baker, and Gorey 2007). Yet it's unclear whether financial coaching sessions are sufficient to help low-income young adults increase their credit scores. Some might think that the intervention is too low-touch given the complexity of financial products and the magnitude of the financial decisions that this population faces, such as taking out a student loan or living independently for the first time.

On the other hand, the BYCBI can be thought of as an “early intervention” to boost

financial capability and develop good financial behaviors at a formative time when individuals may be earning their first paychecks and starting to build a credit history. Indeed, previous studies (e.g., Atkinson et al. 2006, Taylor 2011) have found that young adults are most at risk of financial difficulties that arise from poor financial planning. Young adulthood is also a crucial period for developing an internal locus of control and a sense of self-efficacy—characteristics that have been correlated with successful users of credit, even when controlling for income differences (Tokunaga 1993, Norvilitis et al. 2006, Caputo 2012). By targeting young adults, age 18-29, who are less likely to have developed bad habits and more likely to apply new knowledge and good behaviors, the program may have a higher benefit-to-cost ratio compared to similar interventions typically aimed at older adults.

What are the mechanisms by which the BYCBI might achieve better financial outcomes for individuals? Following Huston (2010), we hypothesize that the financial coaching provided by the BYCBI could help individuals change their financial behaviors and achieve higher credit scores through two primary channels: (1) improved financial knowledge and (2) enhanced skills to apply that financial knowledge to make sound financial decisions. Improved financial knowledge is assessed based on achieving a score of 75 percent or better on a series of 18 true/false questions related to budgeting, saving, borrowing, and the use of credit. Enhanced skills to apply financial knowledge is evaluated based on a scaled measure of financial self-efficacy that included questions about confidence in one's own knowledge and skills as well as satisfaction with one's ability to manage debt and save (Collins and O'Rourke 2012).

We examine the relationship between these two aspects of financial literacy by conducting a mediation analysis where the outcomes are either the individual's credit score or their use of alternative financial services. These two measures capture changes in both increasing

behaviors that lead to better outcomes as well as reducing behaviors that lead to poor outcomes. Based on the literature, we expect that both financial knowledge and financial self-efficacy play a role in improving financial outcomes, yet we have no prior expectations about whether one channel might be more important than the other in producing improved outcomes.

### Financial Coaching Programs: What Do We Know?

Research that specifically evaluates financial coaching is relatively new—with most of this early literature relying on descriptive work, and few studies demonstrating a causal relationship between coaching and changes in behavior or outcomes. A number of descriptive studies have shown several consistently positive associations between coaching and client outcomes, including greater confidence, budgeting, and saving, as well as improvements in debt reduction and credit building (Collins and O'Rourke 2012, Moulton et al. 2013, NeighborWorks America 2013, Center for Financial Security 2015). Other quasi-experimental studies using matched comparison groups find a positive association between credit scores and coaching provided in the context of employment (Roder 2016) and housing (Geyer et al. 2017) programs. However, the lack of a robust control group has made it difficult to extrapolate these results to the general population, highlighting the need for additional research. Of critical importance is the need to disentangle the development of financial management skills from selection into the program. In addition, a robust control group is needed to account for improvements over time that occur as part of the normal course of development as youth learn new skills over time.

Only a handful of studies have used an experimental design to study the effectiveness of financial coaching, and low take-up rates among the treatment group have often made it difficult to generate conclusive evidence due to the likelihood of selection among those who choose to participate. Indeed, individuals who are overconfident in their own ability to pay down their



debt, relative to their actual debt repayment behavior, are less likely to take up offers for coaching (Moulton et al. 2013). One randomized experiment of more than 100,000 credit card clients in Mexico failed to find any impact of a financial education workshop and personalized coaching on the treatment group due to a take-up rate of less than 1 percent, yet demonstrated a higher likelihood of paying credit cards on time among those who chose to participate (Lara Ibarra, McKenzie, and Ruiz Ortega 2017). Another randomized experiment assigned 295 first-time homebuyers to receive an online financial planning module and quarterly financial coaching and found a 20 percent reduction in mortgage delinquency, although the authors note that the effect was only significant at the 10 percent level due to the “relatively small sample size” and a low take-up rate of only one-third (Moulton et al. 2015). A third randomized study of two community-based programs, each with roughly 200-250 individuals assigned to treatment, detected positive improvements across both programs—but for only two outcomes: the number of deposits into savings accounts and turning a credit line from 30 days delinquent to satisfactory (Theodos et al. 2015). This is despite finding positive impacts on a wider range of outcomes among those who chose to participate. The authors note that the impacts on the treatment group were likely diminished due to low take-up rates of only one-third to one-half, “rendering the true effects of coaching less detectable.”

This evaluation contributes to the emerging literature in several ways. First, we use an experimental design with sufficient power in terms of sample size (N=300) and take-up rate (67 percent) to better estimate the causal impact of financial coaching. Second, we use rich administrative data from individual credit reports at six-month intervals to assess whether program impacts persist over time for a variety of outcomes and across different subgroups. Third, we follow Huston (2010) and explore which aspects of financial literacy are associated

with better consumer financial decision making. To do this we link the credit report outcomes to self-reported survey data on changes in financial knowledge and skills, and confirm our findings using insights provided by focus groups. Finally, we provide direct evidence on a population of substantial policy interest: low-income young adults who are either working or enrolled in a workforce development program.

### THE BOSTON YOUTH CREDIT BUILDING INITIATIVE (BYCBI)

The BYCBI was developed by the Boston Mayor's Office of Financial Empowerment (OFE) and implemented by Working Credit NFP over the course of one year from March/April of 2016 through March/April of 2017. OFE recruited individuals for the study during the two months prior to the start of the program, targeting low-income young adults, age 18-29, who were currently working or in a workforce development program. The goal of the BYCBI was to help individuals gain access to credit and/or build strong credit scores by increasing their knowledge of credit building, supplying them with credit building and saving products, and providing individualized advice through coaching over the course of one year. The treatment included the following program components:

**(1) One-hour workshop.** The content of the workshop focused on understanding a credit report, how the credit reporting system works, the consequences of having no or poor credit, and how to use different financial products to improve one's credit score. In addition to making payments on time, specific rules of thumb were given such as keeping one to four open lines of credit, having a mix of installment and revolving credit, having a sufficient amount of available credit for emergencies, and keeping the utilization ratio for each line of credit below 30 percent. At the end of the workshop, individuals were urged to sign up for a one-on-one coaching session with a credit building counselor, either immediately after the workshop or at a later date.

**(2) One-on-one coaching.** The initial coaching session was a one-hour in-person meeting that included a review of the individual’s credit report and the development of an individualized budget and credit action plan focused on increasing the individual’s credit score. Following the first appointment, the counselor pulled the individual’s credit report at six-month intervals and shared the results, along with additional credit building guidance, in person or by email.

**(3) Enrollment in CW-3™ matched savings account.** During the first coaching session, the counselor also assessed the individual’s eligibility for the CW-3™ product, a “locked” savings account where the individual opens a 12-month \$300 installment loan but does not take the loan proceeds; instead they are kept by the lender in an account until the loan is paid off. The individual then makes 12 monthly payments of \$26 that are reported by the lender to the credit bureaus, building a positive track record for the individual. At the end of the loan term, the individual has accumulated \$300 in savings as well as an improved credit score. There is no risk of delinquency or default. If an individual fails to make a loan payment, Working Credit pays off the loan with the money from the “locked” savings account. To be eligible to enroll in the CW-3™ product, the individual had to meet the following criteria: (1) have fewer than three open revolving accounts or no installment account, (2) be able to afford to save \$26 per month, and (3) not be currently past due on any account. The data show that these criteria were met for 53 individuals in the treatment group who participated in the program, and of those who were offered the product, 60 percent decided to take it up. See the appendix for more details.

## METHODS

To evaluate the impact of the BYCBI, we compare the outcomes of randomly selected individuals in the treatment group to those in the control group over time. Since the number of individuals applying for the program exceeded the number selected for participation, we

randomly assigned participation in the program so that those individuals who applied but were not randomly selected to participate served as a control group for the evaluation. Individuals in both the treatment and control groups received a \$150 financial incentive to participate in the study for one year. The control group received a \$25 gift card when notified that they were not selected for the program and a \$125 incentive for completing the post-survey. The treatment group received \$150 at the end of the program after completing the post-survey.

#### Experimental Design: Recruitment and Random Assignment

Working Credit's program is typically delivered to a group of employees within a large firm where individuals have both a steady income for the duration of the program as well as regular and strong attachment to their employer. However, large firms serve only a small share of the low-income young adult population targeted for this intervention. In addition, there was interest in delivering the BYCBI to individuals in the context of a workforce development program to pilot the use of such interventions under the new WIOA requirements. As a result, it was necessary to cast a wider net for recruitment, with a total of 18 different organizations participating in the study (see Table A1). While these educational and community-based organizations serve low-income young adults, they do not conform to the typical Working Credit delivery model. To account for this, we categorized organizations as "typical," "near-typical," and "atypical" based on having: (1) regular/strong contact with individuals, and (2) an employment tenure that covered the duration of the BYCBI. See the appendix for more details.

As part of the application process, individuals supplied information to assess their basic eligibility, which required that they be at least 18 years of age and currently working or enrolled

in a workforce development program.<sup>1</sup> Applicants were also required to provide a written request to perform the baseline credit check as well as subsequent credit pulls at 6, 12, and 18 months. Of the 300 individuals eligible to participate in the study, we randomly assigned applicants to one of the following two groups:

- **Treatment Group:** This group of 150 individuals was assigned to receive the financial workshop and the one-on-one coaching. They were also offered the CW-3<sup>TM</sup> product if they were eligible based on the criteria discussed above.
- **Control Group:** This group of 150 individuals received no intervention at all.

We also stratified our random assignment by age (18-24 versus 25-29), race (African-American versus non-African-American), and gender (male versus female) to test for heterogeneity in treatment effects, which have been shown to be important (Kaiser and Menkhoff 2017). For example, consistent with human capital theory, previous studies in the literature (Taylor 2011) have reported a negative relationship between financial capability and age. However, this relationship between score and age does not hold uniformly across racial and ethnic groups. Among African-American and Hispanic adults, growing older does not make them more likely to obtain a credit score, because these groups are less likely to participate in the mainstream economy as they age (Brevoort, Grimm, and Kambara 2015). Finally, a gender gap in financial literacy has become a stylized fact in the literature (Lusardi and Mitchell 2014), which may also translate into gender differences in program impacts across individuals in the treatment group.

While we chose to stratify our sample by the characteristics discussed above, the

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<sup>1</sup> Individuals were excluded from the research study if they were not 18 years old or if they were not working or enrolled in a workforce development program at the time of the application. This resulted in 18 individuals who were deemed ineligible for the study (3 were under 18 years of age and 15 were not currently employed or enrolled).

distribution of the remaining demographic factors across the treatment and control groups was left to chance, as is the case with random assignment. Table 1 shows that the treatment and control groups were roughly equivalent across almost all other observable characteristics, including ethnicity, employment tenure, marital status, household size, number of children, health insurance, homeowner status, household income, and confidence in their ability to save \$26 per month for the CW-3™ product. The only significant differences at baseline were that the treatment group had a higher share of individuals who were Asian and a lower share of individuals with just “some college.” We note that having two statistically significant differences at the  $p < 0.10$  level would be expected by random chance when testing 15 different categories of characteristics. As such, given the randomization design, we do not expect these small differences to affect the program outcomes we observe across the treatment and control groups.

Despite having applied for the program, only two-thirds (67 percent) of the individuals assigned to the treatment group attended at least the workshop or one coaching session. We call these individuals “study compliers” because they complied with the requirements of the program after being assigned to treatment.<sup>2</sup> In contrast, the remaining one-third of the individuals assigned to the treatment group did not participate at all, choosing not to attend the workshop nor sign up for any coaching sessions. We call these individuals “study non-compliers,” because despite being assigned to receive the program, they did not comply with the requirements and chose not to participate. This is not uncommon among randomized control treatment studies of financial coaching programs where half to two-thirds of individuals drop out even when services are offered for free (Theodos et al. 2015).

As one can imagine, it is typically lower-income and underserved populations that have

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<sup>2</sup> Of those in the treatment group who attended a workshop, 96 percent signed up for the one-on-one coaching. As such, we cannot disentangle the impacts of these two program components.

“second thoughts” after applying and choose not to participate (Rothwell and Han 2010). In our study, the non-compliers were about one year younger on average and one-third less likely to have a college degree (see Table A2). In addition, non-compliers were twice as likely to have children, suggesting that perhaps scheduling constraints made it difficult to attend the workshop, even though several make-up sessions were held. Despite having longer tenure with their employers, non-compliers were less likely to have employer-provided health insurance. Finally, non-compliers were more likely to have been recruited from an atypical organization and less likely to indicate that they would be able to save \$26 per month.

### Data

We employed a mixed-methods approach using both quantitative information from credit reports and surveys, as well as more narrative qualitative information gathered from focus groups. See Figure 1 for a timeline showing the program’s implementation and data collection.

#### *Administrative Credit Report Data*

With each individual’s consent, Working Credit collected administrative data on credit histories for all individuals in both the treatment and control groups. These credit pulls occurred at the initial time of application (baseline), and again at 6 and 12 months after the start of the program, to be able to detect the impact of positive changes in behavior on credit scores (which can take up to six months). One additional credit pull occurred at 18 months to determine whether the impacts persisted beyond the end of the program.

The credit report data offer several key advantages over most previous studies. First, the data provide a relatively complete financial profile for most of the outcomes related to the BYCBI intervention, although it does not capture alternative financial services such as payday loans, auto title loans, or informal borrowing from family and friends. Second, administrative

data do not suffer from selection bias which can arise when using self-reported survey data, such as the tendency for respondents to overestimate or underestimate their financial situation. Third, the data enable us to view the path of change over time at precise six-month intervals with each credit pull—an assessment that would be less feasible if relying solely on survey data.

Using these data, we evaluate the program’s impact on a range of outcomes related to building an optimal credit profile, including access to credit as well as specific practices conveyed during the workshop and financial coaching. These include the individual’s credit score and credit rating (e.g., poor/fair/good/excellent) as well as the factors that affect one’s credit score, such as the number of open lines of credit, having a mix of types of credit (e.g., revolving and installment), the amount of available credit, the utilization ratio, the number of delinquent lines of credit (e.g., 30 days past due), and the number of outstanding negatives (e.g., collections, charge-offs, or judgments). We also assess loan history, including whether the individual has a student loan or a car loan, the interest rate on the car loan, and whether the individual has a history of sustained on-time payments or a history of any loan delinquencies.<sup>3</sup>

In terms of baseline pre-program measures of outcomes, the administrative data show no significant differences before the start of the program based on the credit report data, as would be expected based on random assignment (Panel A of Table 2). About one-third of both the treatment and control groups had no credit score, the majority of whom (70 percent) fall into the category of “credit invisibles”—a term used by the CFPB to describe individuals without any credit record (Brevoort, Grimm, and Kambara, 2015). The remaining individuals with no credit score are what the CFPB considers to be “unscorable,” meaning that they have a credit record,

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<sup>3</sup> A history of sustained on-time payments is defined as having at least one 12-month period during the entire credit history in which the individual paid on time to an installment account (e.g., no delinquencies). A history of any loan delinquencies is defined as having any accounts (open or closed) that were ever delinquent.



but it is a “thin” file, mostly due to accounts that have become inactive.

Among those individuals with a credit score before the start of the program, the average score was roughly 660, with no significant difference between the treatment and control groups. We also followed industry guidelines to predict a score for those with “thin” credit files (VantageScore, 2016) using a Heckman selection correction model, which yielded a slightly lower average score of about 650—again with no significant difference between the treatment and control groups.<sup>4</sup> This method allows us to categorize most of the individuals in our sample (N=226) into the typical credit ratings prior to the start of the program, with the majority of individuals falling into the “Fair” to “Good” range. There were also no significant differences among any of the other factors listed on the credit report that would be expected to affect one’s credit score.<sup>5</sup> About 40 percent of the individuals in our sample had a student loan with an average of \$28,000 in debt, revealing that our sample of young adults do indeed make important financial decisions at this point in their lives.

#### *Self-Reported Survey Data*

All individuals in the treatment and control groups were asked to complete both a pre-and post-program survey that captured their current financial situation as well as their knowledge and behaviors related to credit building. Individuals were asked to complete the pre-survey when they applied for the program and were given a small monetary incentive (e.g., a \$5 gift card plus a raffle to win one of 10 iPads) to incentivize completion. The post-survey was deployed via email to both the treatment and control groups, and completion was required to receive the final

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<sup>4</sup> Recently, credit agencies have developed models to begin scoring this group, finding that they “are in many ways very similar to conventional credit users” (VantageScore, 2016). See the appendix for more detail.

<sup>5</sup> Compared to the general population, individuals in the study are as likely to have poor credit, more likely to have fair credit, and less likely to have good or excellent credit. This is likely due to the program serving individuals who are younger and have had less opportunity to build good credit. See the data appendix for more detail.

installment of the \$150 financial incentive for participating in the program. Almost identical response rates of 64 to 65 percent were achieved across the two groups (see Table A3).

Surprisingly, individuals in the treatment group who responded to the survey exhibited characteristics that indicate they were *less* positively selected compared to survey responders in the control group, setting a high bar for detecting improvements over time.<sup>6</sup>

The survey data assessed changes in financial behaviors and well-being as well as gains in financial knowledge and self-efficacy. Financial behaviors were quantified regarding the use of mainstream (e.g., banking, credit cards, loans) as well as alternative (e.g., check-casher, payday lender, pawn shop, borrowing from friends and family) financial services. Individuals were asked to assess their own financial well-being with regard to future planning (e.g., applying for a mortgage or car loan) as well as adverse events (e.g., collection, repossession, eviction, foreclosure, and bankruptcy). Financial knowledge was evaluated based on the percent of correct answers to a series of true/false questions related to budgeting, saving, borrowing, and use of credit, including what is reported on a credit report and how that information is used. Self-efficacy was assessed using a measure based on questions about confidence in one's knowledge and skills, as well as satisfaction with one's ability to manage debt and save.<sup>7</sup> To compare impacts across our constructed measures of financial behaviors, literacy, and self-efficacy, we constructed z-scores based on the responses to the underlying questions that are standardized to have a mean of zero and a standard deviation of one.<sup>8</sup>

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<sup>6</sup> Treatment responders were more likely to have only a high school diploma, receive health insurance through Medicaid, and rent rather than own their home (see Table A3). Note that the direction of the bias goes against the detection of program impacts for the survey responders in the treatment versus the control groups. Nonetheless, we control for both demographic characteristics and baseline outcome measures to minimize the bias.

<sup>7</sup> Although there are several widely accepted psychological measures of general self-efficacy, no reliable and valid measure specific to financial behavior exists (Tokunaga 1993). We follow Lown 2011 and use factor analysis to construct a measure of self-efficacy based on a combination of the statements that measure an individual's confidence as well as their satisfaction with their knowledge and ability. See the data appendix for more details.

<sup>8</sup> See the data appendix for a full listing of questions and responses for each underlying component.

There were virtually no significant differences before the start of the program in terms of the self-reported survey data (see Panel B of Table 2). Individuals in both the treatment and control groups reported similar levels of financial well-being in terms of having to pay a deposit to a cell phone or utility company and having their wages garnished, their utilities disconnected, or their car repossessed. Similar percentages of both groups had experienced eviction and foreclosure, had been contacted by a collection agency, or had been in bankruptcy. There were also no significant differences in terms of financial behaviors, with about half of both the treatment and control groups having used mainstream financial services and about 15 percent of each group having engaged in alternative financial services. Both groups answered roughly 75 percent of the questions correctly on our test of financial knowledge. There was also no difference in terms of our overall measure of financial self-efficacy nor most of the underlying components, with the exception that those in the treatment group were slightly less concerned about their personal financial situation than those in the control group. Again, having at least one statistically significant difference at the  $p < 0.10$  level across 40-plus different outcome measures would be expected even with random assignment. As such, we maintain that observed differences between the treatment and control groups in the post-program outcome measures can be attributed to the impact of the program.

#### *Focus Group Data*

We held two sets of focus groups, at the beginning and at the end of the program, separately for individuals in the treatment and the control group. The first set of focus groups were held in May 2016, shortly after the treatment group had participated in the workshop and the initial one-on-one coaching provided by Working Credit. The goal was to get an early assessment of how the program was going, as well as to uncover additional insights about take-

up among the treatment group. In addition, we wanted to learn more about the barriers faced by individuals in both the treatment and control groups when it came to accessing and building credit. The second set of focus groups was held in May 2017, just after the program had ended, with the aim of developing a better understanding of the program's overall impacts and mechanisms.

Each group was composed of five to seven young adults selected at random from the treatment and the control groups. Individuals were offered a modest financial incentive (a \$50 gift card) to encourage participation and compensate individuals for their time.<sup>9</sup> Focus group members were fairly representative of the full cohort in terms of observable characteristics such as age, gender, race, and type of organization from which they were recruited. Comparing their credit histories and baseline survey responses, there was no evidence that focus group members had more difficult or extreme financial circumstances compared to the full study sample. If anything, focus group members were slightly more highly educated and slightly less likely to be experiencing problems with credit.

### Model Specification

Because participation is randomly assigned, we obtain causal estimates using a simple comparison of means on the outcome of interest. This *Intent to Treat (ITT)* estimate measures the impact of *offering* the program on the outcome. In many cases, this is the policy-relevant estimate because program administrators often want to account for program take-up in assessing the degree to which financial coaching could improve outcomes among the pool applicants, not just those who choose to participate (e.g., study compliers). Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom,

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<sup>9</sup> We attempted to recruit the same individuals for both sets of focus groups, but only half were able to attend both sessions, so we recruited additional members with similar demographic characteristics as replacements.

2006), we also use a regression framework to include baseline characteristics, including pre-program measures of outcomes, to improve the precision of our estimates using equation (1):

$$Y_{it} = \alpha_1 + \pi_1 TREAT_i + \beta_1 Y_{i0} + \gamma_1 X_{i0} + \mu_{it1} \quad (1)$$

where  $Y_{it}$  is the post-program outcome for individual  $i$  during post-randomization period  $t$ ,  $TREAT_i$  is a dummy variable indicating the individual received an offer to participate,  $Y_{i0}$  is the pre-program measure of the same outcome,  $X_{i0}$  is a set of pre-existing baseline characteristics collected when the individual applied to the program, and  $\mu_{it1}$  is a stochastic error term.

Nonetheless, because not all individuals accept the offer, the ITT estimate will understate the effects of the program for those youth who choose to participate. As such, we also provide *Treatment-on-the-Treated* (TOT) estimates, which assess the program's impact, independent of the take-up rate. Under the usual relevance and exogeneity assumptions for instrumental variables, this latter set of effects can be recovered from the experimental data.<sup>10</sup> We perform this estimation through a two-stage least squares strategy, in which random assignment ( $TREAT_i$ ) is an instrument for actual participation ( $P_{it}$ ), and  $P'_{it}$  is the predicted probability of participation from equation (2):

$$P_{it} = \alpha_2 + \pi_2 TREAT_i + \beta_2 Y_{i0} + \gamma_2 X_{i0} + \mu_{it2} \quad (2)$$

$$Y_{it} = \alpha_3 + \pi_3 P'_{it} + \beta_3 Y_{i0} + \gamma_3 X_{i0} + \mu_{it3} \quad (3)$$

If all individuals respond the same way to the program (i.e., if treatment effects are constant across youth), then equations (2) and (3) also yield an estimate of the *average treatment effect* (ATE) across this population of low-income young adults. Given that treatment effects are likely to be heterogeneous across young adults, then the coefficient  $\pi_3$  estimates the *local average treatment effect* (LATE)—the effect of participation on those who comply with random

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<sup>10</sup> In order for the random assignment variable,  $TREAT_i$ , to be a valid instrument, it must be correlated with program participation,  $P_{it}$ , and uncorrelated with  $\mu_{it3}$ .

assignment. As long as there is no control crossover (no always-takers) in this setting,  $\pi_3$  provides an estimate of the treatment-on-the-treated.

Finally, we explore the program's mechanisms by conducting a mediation analysis that relates individual outcomes such as the credit score and the use of alternative financial services to our two components of financial literacy: financial knowledge and self-efficacy. In the case of assessing the effectiveness of the BYCBI program, we theorize that there could be both a direct and indirect effect. The direct effect of the program could arise from the compliers simply following the advice of the coach, which would directly improve their credit score. However, the literature also suggests two potential indirect effects that could also be at work. The first is that attending the financial workshop increases financial knowledge, which would help individuals in the treatment make better financial decisions and improve their credit scores. The second is that the financial coaching increases self-efficacy to address one's own financial situation, which would also help individuals make better financial decisions and improve their credit scores.

To conduct the mediation analysis, we draw on the model developed in Preacher and Hayes (2008) and further described in Zhao, Lynch, and Chen (2010). Using this model, the only requirement to establish mediation is that the indirect effect is significant. Although it is not necessary for there to be a statistically significant direct effect, to be mediated, the presence of the direct effect can inform theorizing about other mediators. As discussed earlier, we would expect to observe complementary mediation for both the indirect effects of financial knowledge and self-efficacy on credit score if both the indirect path and the direct path are significant (Zhao, Lynch, and Chen 2010). Similarly, because both the direct and indirect effects on the use of alternative financial services is likely to be negative, we would again expect to see complementary mediation for that outcome as well.

To assess the indirect effects using this framework, we use the following system of Structural Equation Models (SEM) to estimate both the direct and indirect effect parameters simultaneously:

$$M_{it} = \alpha_4 + \pi_4 TREAT_i + \eta_4 M_{i0} + \gamma_4 X_{i0} + \mu_{it4} \quad (5)$$

$$Y_{it} = \alpha_5 + \pi_5 TREAT_i + \eta_5 M_{it} + \beta_5 Y_{i0} + \gamma_5 X_{i0} + \mu_{it5} \quad (6)$$

where  $M_{it}$  = mediating variable (e.g., financial knowledge or financial self-efficacy). We then perform a bootstrap test of the indirect effects, as described in Preacher and Hayes (2008).

Studying these indirect channels is important in understanding the mechanisms driving the observed improvements in credit scores among the treatment group. If it's the case that the program improves either financial knowledge or self-efficacy or both, then this might explain why we continue to see improvements in credit scores even after the program ends at 12 months. Note that this part of the evaluation is more exploratory in nature—because although the treatment and control groups were randomly selected, those who chose to respond to the post-survey were not, even when offered a financial incentive of \$150 to participate. However, we feel that this analysis is still informative, if only suggestive, as to how the program achieves better credit outcomes for those who participate.

In addition, we can also contrast these two indirect effects to test whether they are equal in size as measured by the degree to which each accounts for the direct effect. Of course, contrasts represent comparisons of indirect effects only insofar as the mediators are themselves uncorrelated. We measure improvements in both financial knowledge and self-efficacy by comparing the pre- and post-survey responses using separately scored scales composed of multiple and dissimilar items. This has the advantage of ensuring that our measurement of these mediators is distinct from the dependent variable (e.g., credit score) as well as each other.

However, it might still be the case that financial knowledge and self-efficacy are positively correlated to some degree.

## RESULTS

We assess program impacts using both credit report and survey data in two ways. First, we estimate the Intent-to-Treat (ITT) by comparing outcomes for the entire treatment group relative to the control group, regardless of whether individuals in the treatment group actually participated (i.e., complied) with the program. We also estimate Treatment-on-the-Treated (TOT) by comparing study compliers in the treatment group relative to the control group, which provides an estimate of the program's impact, independent of the take-up rate.

### Assessing BYCBI Impacts on Credit Outcomes Using Administrative Data *Simple Comparison of Means Over Time*

In terms of increasing access to credit, the treatment group showed significant improvements relative to the control group, largely driven by the compliers. Within the first six months, the share of individuals in the treatment group with no credit score had fallen by 11 percentage points, compared to a decline of only 4 percentage points for the control group. This resulted in a significant difference between the two groups at the six-month mark, with 29 percent of the control group having no score, compared to only 25 percent of the treatment group and only 19 percent of the compliers (See Panel A of Figure 2). Most of the relative improvement among the treatment group was due to a greater share of credit invisibles, who had no record at all before the start of the program, gaining access to credit for the first time. Among the compliers, this group continued to show significant improvements in accessing credit through the 18-month mark, even after the program ended. Once they obtained credit at the 6-month mark, it's worth noting that the credit scores among the treatment and complier groups were higher than those of the control group over the next 12 months, although the number of



individuals (N=74) is too small to detect any significant differences (see Panel B of Figure 2).

In terms of boosting credit scores among those who initially had a credit file at baseline, the treatment group showed significant improvements relative to the control group—again largely driven by the compliers. Figure 3 shows a simple comparison of mean credit scores at six-month intervals for the control group versus the treatment group, as well as a separate line for treatment compliers. Panel A shows that for the sample of individuals with a credit file (N=226), the mean credit score for the treatment group increased significantly by 18 points during the first six months of the program, largely led by a rapid improvement of 22 points among the treatment compliers. The compliers continued to show steady improvements through the six months after the program ended, resulting in a mean score of 687 at the 18-month mark. These gains were large enough to significantly increase the mean score of the entire treatment group by 20 points relative to the control group, demonstrating that the program effects are large enough to show improvements among the population of young low-income adults whom the city intended to treat. Based on the trajectory of score improvements over time, it appears that the impact of the program is greatest during the first six months, when individuals receive the information from the workshop as well as their first coaching session to establish an individualized plan. When we limit the sample to just those individuals who initially had a credit score—not just a credit file—before the start of the program, the gains are much smaller, and relative improvements are detected only among those who complied with the program (see Panel B of Figure 3). By the 18-month mark, six months after the program’s end, the mean credit score of compliers in the treatment group who initially had a score outpaced the control group by 25 points.

Are the improvements in credit scores among the treatment group large enough to boost their credit ratings? Figure 4 shows the share of individuals with a credit file falling into each

credit rating category over time (i.e., poor credit through excellent credit), for the control group versus the treatment group, as well as for program compliers. While at baseline there were no significant differences in the distribution of credit ratings across the treatment and control groups, sizeable improvements were observed during the first six months of the program that largely persisted through the 18-month mark. During the first six months of the program, the compliers were 4.7 percentage points less likely to have poor credit and 10.3 percentage points more likely to have good credit relative to the control group. Although those in the control group also advanced their credit ratings over time, by the 18-month mark 57.4 percent of the treatment group had a “good” credit rating, compared to only 51.3 percent of the control group.

The observed improvements in credit scores and ratings over time for the treatment group are positively correlated with changes in the underlying factors that were discussed during the one-hour workshop and one-on-one coaching sessions. For example, Figure 5 indicates significant improvements over time in credit use among the treatment group versus the control group, such as using a mix of both installment and revolving credit. In addition, the treatment group was less likely to have any delinquent lines of credit or any outstanding negatives—items that typically decrease an individual’s credit score by 30 to 100 points. Other improvements appear to be more temporary, such as maintaining one to three open lines of credit or having a credit utilization ratio of less than 30 percent. Overall, by the 18-month mark, treatment compliers were able to significantly increase their available credit by \$2,881 compared to the control group.

Similarly, Figure 6 shows improvements in loan use over time that are also positively correlated with higher credit scores. For example, by the 18-month mark, both the compliers and the overall treatment group were significantly more likely than the control group to have a

history of sustained on-time payments. In addition, the compliers were 10 percentage points less likely to have a history of being 30-day delinquent compared to the control group. However, no significant differences were observed in terms of whether the individual has a car loan or a student loan, or the amount of the student loan.

Are the improved scores and credit ratings among the treatment group economically meaningful? One frequently touted benefit of having a better credit rating is the ability to get better borrowing terms. Figure 6 indicates that among individuals with a car loan, those in the treatment group had interest rates that were 3.6 percentage points (40 percent) lower than those in the control group—and this impact persisted through the 18-month mark. Compliers in the treatment group had even more favorable rates than the control group, resulting in a 6.4 percentage point (67 percent) difference by the end of the program. To put this into perspective, on a \$10,000 auto loan with a term of five years, the observed difference in interest rates would imply that individuals in the treatment group would save \$31.26 per month on average compared to individuals in the control group—enough to pay for an individual’s basic monthly cell phone bill or groceries for one week. This is a meaningful impact for this low-income population, of whom roughly 40 percent were on Medicaid—and several had indicated during the focus group discussion that they relied on Supplemental Nutrition Assistance Program (SNAP) benefits to make ends meet each month.

#### *ITT and TOT Regression Estimates at 18 Months*

Controlling for baseline measures of outcomes and demographic characteristics using a regression framework largely confirms the descriptive results presented above. By the 18-month mark, the percentage of individuals having no credit score fell by 10 percentage points among

the compliers relative to the control group (see Panel A of Table 3).<sup>11</sup> Moreover, the program's impact was not limited to helping individuals obtain credit—it also helped to improve their scores. Credit scores were 26.4 points higher among the treatment group versus the control group and 37.6 points higher among the compliers versus the control group (see Panel B of Table 3). These relative improvements in credit scores translated into higher credit ratings, with the share of individuals having good credit being 8.1 percentage points greater among the treatment compared to the control group and 13.0 percentage points greater among the compliers versus the control group. We find similar but smaller impacts among the compliers when we limit the sample to those who initially had a credit score at baseline (see Table A10).

Significant improvements also were observed among the underlying factors affecting the calculation of credit scores (see Table 4). Among the treatment group, these included having no lines of credit currently delinquent, no current outstanding negatives, and a history of sustained on-time payments. Among the compliers, positive impacts were also found for having a mix of revolving and installment lines of credit, and having no history of 30-day delinquency. By the end of the program, individuals in the treatment group had interest rates on car loans that were 3.5 percentage points less than those in the control group. Among compliers, the gap was even greater—6.8 percentage points—even when controlling for baseline interest rates and demographic characteristics.

We find some support for heterogeneous impacts, suggesting that the program might yield greater benefits for certain groups that could perhaps be targeted if policymakers wanted to make the most efficient use of limited resources. Based on prior studies from the literature, we hypothesized that the program would have a greater impact on younger individuals, African-

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<sup>11</sup> See Table A9 for a full listing of coefficients for all variables in the regression.

Americans, and females. To test these hypotheses, we initially stratified our random assignment across these subgroups to ensure that there would be sufficient representation to detect differential impacts. As would be consistent with human capital theory, younger individuals are more likely to benefit simply because they have had less exposure to financial knowledge and fewer opportunities to build credit (Atkinson et al. 2006, Taylor 2011). Similarly, researchers have documented that the racial wealth gap reflects lower participation in mainstream financial services among African-Americans, which may stem from a greater likelihood of growing up in a low-income household with less access to information and opportunities regarding finances, or from socioeconomic and political structure barriers that restrict access to financial services (Brevoort, Grimm, and Kambara 2015; Hamilton and Darity 2017).<sup>12</sup>

#### *Differential Impacts by Demographic Subgroup and Organization Type*

Table 5 confirms that the BYCBI did indeed have a greater impact on both younger individuals and African-Americans. Among both groups, the program expanded access, significantly decreasing the share of individuals with no credit score by 10 percentage points. However, significant improvements in credit scores were observed only among the younger individuals, with the program improving their scores by 25 points. In contrast, there was no significant difference in program impact between males and females in either improving access to credit or improving scores. This is despite a well-documented gender gap in financial literacy (Lusardi and Mitchell 2014). It could be the case that this gap widens over time, as do many other gender gaps, when men exceed women in terms of employment and earnings later in the

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<sup>12</sup> While we did not initially stratify our sample by household income, we tested the possibility that the observed heterogeneity by race could be driven by differences in household income, yet we found only partial support for this hypothesis. While individuals from households with incomes below the 2016 median for Greater Boston (\$71,992) did experience a larger increase in their credit score (64.7 points) compared to those from households above the median (42.5 points), this difference was not statistically significant. However, given that over 90 percent of individuals in both the treatment and control groups were from households below the median, this test lacks sufficient power to be meaningful.

life cycle.

Finally, we also stratified our sample by the type of organization from which the individuals were recruited to test the efficacy of the program's delivery model. We had initially hypothesized that individuals who were recruited through atypical organizations would be less likely to see improvements due to having neither regular contact with their employer nor an employment tenure that was guaranteed to last the duration of the program. We find no statistically significant difference in terms of program impact on increasing access to credit or improving credit scores for individuals recruited from organizations that do not fit the typical Working Credit model (see Table 5). This suggests that the BYCBI pilot could be expanded across these atypical sites to reach youth in nontraditional employment settings without a significant loss in terms of program efficacy. However, as we discussed above in the methodology section, the study non-compliers who applied but chose not to participate in the program were more likely to be recruited from atypical organizations. As such, policymakers need to investigate what barriers exist with regard to participation among this population before expansion to ensure program efficiency is maintained.

#### *Assessing the Take-up and Impact of the CW-3™ Product*

How many of the individuals made use of the CW-3™ secure loan product to help them build credit? Recall that the product was only offered to compliers in the treatment group who met certain criteria. Of the 101 program compliers, roughly half (53) were offered the program, of whom 60 percent decided to take it up. Those who were offered the CW-3™ product were more likely to be from a typical organization, have a high school degree, be a Medicaid recipient, and not have children (see Table A11). Compared to those who were offered the CW-3™ product, those who chose to make use of it were more likely to be Hispanic and to have children.

Females, those with a bachelor's degree, and individuals who did not own a home were less likely to take up the product when it was offered to them. Individuals in the latter two groups may not have wanted to take out another loan using the CW-3™ because they already had student loans or were planning to apply for a mortgage—both of which might make one reluctant to take on more debt. However, the gender difference is notable and might reflect the emergence of a gap in credit building among males versus females.

How much of the program's impact can be attributed to the use of the CW-3™ product versus just the workshop and financial counseling? Interestingly, Table 6 shows that among the compliers, just being offered the CW-3™ product can account for virtually all the gains in terms of access to credit as well as improving credit scores. This suggests that the product was well-targeted toward those who would benefit from it the most: largely, invisibles with no credit file and unscorables with thin credit files. However, because the use of the product was not randomly assigned, nor entirely at the discretion of the individual, we cannot distinguish the impact of the product from the characteristics of those to whom it was offered.

#### Exploring Program Mechanisms Using Survey and Focus Group Data

To explore the program's mechanisms, we make use of the responses from the pre- and post-program survey, as well as the insights gained from our focus groups to assess the individual's perceptions of their financial well-being as well as changes in their financial behavior, knowledge, and self-efficacy.

#### *ITT and TOT Regression Results from Self-Reported Survey Data*

Consistent with the administrative data, individuals in the treatment group reported being in better financial situations than those of the control group after the program had ended. Table 7 reports the ITT and TOT estimates for each self-reported outcome captured by the post-program

survey. Individuals in the treatment group were 7.3 percentage points less likely to report having a utility company currently holding a deposit, 9.5 percentage points less likely to be contacted by collection agencies about unsettled claims over the past three months, and 4.6 percentage points less likely to be evicted or in the process of eviction over the past year. In addition, the treatment group was 11.9 percentage points more likely than the control group to report having a credit-counseling or debt-management plan—likely the result of their one-on-one coaching. No significant differences were reported for having one’s wages garnished, utilities disconnected, or car repossessed, or for entering foreclosure or bankruptcy—although these events occurred with very low frequency even at baseline, and in some cases may take longer than a year to resolve.

One aspect of financial coaching that differs from other approaches is the continuous feedback loop that involves setting goals, establishing a concrete plan of action, and monitoring individual progress—with the objective of changing financial behaviors to improve long-term outcomes—rather than simply increasing knowledge of or providing access to financial products (Collins, Baker, and Gorey 2007). Indeed, we find that by the end of the program, the use of alternative financial services (e.g., using a check-casher, payday lender, or pawn shop, or borrowing from friends and family) was roughly 30 percent lower among the treatment group relative to the control group (see Table 7). This is consistent with our earlier finding that the program expanded access to formal credit and also increased the dollar value of available credit among the treatment group, potentially making it less likely that they would continue to rely on costly alternatives.

What are the mechanisms by which the BYCBI achieves better financial outcomes for individuals? Initially, we hypothesized that the financial coaching provided by the BYCBI could help individuals change their financial habits and achieve higher credit scores through two



primary channels: financial knowledge and/or financial self-efficacy. Using the standardized z-scores in our regressions, the bottom of Table 7 compares the magnitude of the program impact across each domain. While the BYCBI had a significant impact on both the financial knowledge and financial self-efficacy of the treatment group, the latter effect was twice as large. For example, column (2) of Table 7 shows that the BYCBI led to a 0.329 standard deviation increase in the financial literacy test score, compared to a 0.630 standard deviation increase in the overall self-efficacy score. These results suggest that both financial knowledge and financial self-efficacy have the potential to be mediators through which the BYCBI affects financial behaviors and improves credit scores.

### *Mediation Analysis*

We assess the indirect effects of both mediators using the framework developed by Preacher and Hayes (2008). Specifically, we use SEM to estimate both the indirect effects and the direct effect parameters simultaneously and then perform a bootstrap test of the indirect effects. The results are presented in Table 8 for two outcomes of interest: credit score (Panel A) and use of alternative financial services (Panel B). The credit score results show that the specific indirect effects are  $a1*b1=17.911$  (through financial literacy) and  $a2*b2=14.813$  (through self-efficacy). The normal SEs and critical ratios for these effects indicate that self-efficacy is likely an important mediator ( $Z=2.013$ ). Because the assumption of normality of the sampling distribution of the total and specific indirect effects is questionable, particularly in small samples, we bootstrapped the indirect effects on credit score and find that the estimates and 95% CIs (percentile, BC, and BCa) are in agreement with the results of the product-of-coefficients strategy, where self-efficacy is the only significant mediator of the BYCBI x credit score relationship. We come to a similar conclusion when testing the indirect effects on the use of

alternative financial services where only self-efficacy has a significant negative effect on the use of alternative financial services (Panel B).

We further test whether the contrast between financial literacy and self-efficacy is significant. For credit score, because zero is contained in the confidence interval of the contrast estimate, the two indirect effects cannot be distinguished in terms of magnitude, despite the fact that one is significantly different from zero and the other is not. According to Preacher and Hayes (2008), “Such apparent paradoxes can occur when one of the specific indirect effects involved in the contrast is not sufficiently far from zero.” In contrast, for the use of alternative financial services, it is clear that the indirect effect of self-efficacy is statistically distinguishable from the lack of an indirect effect of financial knowledge. Thus, based on our mediation analysis, it appears that the impact of financial coaching on changing behaviors and subsequently improving credit scores stems primarily from increasing financial self-efficacy among individuals to be able to act on the financial information and opportunities with which they are presented.

#### *Insights from Focus Group Discussions*

The importance of self-efficacy was also a key theme that emerged from our focus group discussions both at the beginning and end of the program. Panel A of Figure 7 compares the most frequent themes that occurred for the treatment versus the control groups during the first set of focus group discussions.<sup>13</sup> In the treatment group, issues of credit history, lessons learned in the credit workshop and counseling, and strategies for dealing with credit dominated the discussion. In the control group, while approximately a third of the time was devoted to one’s credit history and strategies for dealing with credit, discussions of their dire financial situations and lack of

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<sup>13</sup> See the data appendix for a detailed discussion of how we coded the themes emerging from the focus groups.

financial guidance dominated the conversation.

While individuals in the treatment group expressed the same concerns as those in the control groups, they exhibited less anxiety, which appeared to stem from a greater internal locus of control. For example, whereas members of the treatment group knew specific things they had done that had damaged their credit, members of the control group still did not have a clear idea of their specific mistakes. Similarly, members of the treatment group referenced concrete steps they had learned through the credit workshop and/or counseling, and seemed confident that these steps would be beneficial. In contrast, members of the control group who described their financial strategies stated that they did not understand whether they were doing the “right” things and were still casting about for solutions. Finally, members of the control group mentioned financial anxiety much more frequently than the treatment group and reported feeling overwhelmed by paying off credit card debt. These feelings of anxiety were not reported in the treatment group, despite the treatment group also having large amounts of credit card debt.

When asked about how their lack of knowledge about specific areas contributed to their financial circumstances, both groups expressed frustration that practical financial guidance had not been taught in high school. During the course of the discussion, two distinct stories emerged around credit cards and student debt. One individual noted that he did not get a credit card until much later because he was scared of it, “but that also hurts you because then you don’t have any credit at all.” Three individuals talked about problems with student debt, including both loans and direct debt to the college. Two of them said that they took on student debt without understanding what it meant to pay it back and had to drop out of school before completing their degrees because of financial difficulty, making it even harder to pay back their loans.

When asked about how their credit history affected their current and/or future plans, a

range of answers was given. Almost all individuals reported having to rely on cash availability to meet expenses and none felt they could cover themselves in case of an emergency. One talked about being hesitant to get married and buy a home. Another talked about how she wants to buy a house but found it hard to save money because of credit card payments. A third talked about being forced to wait to buy a car because she would have to take a loan with a high interest rate unless she improved her credit score. A fourth reported she was unable to get a car and had to get two people to cosign for an apartment so she was “not even thinking about house, car, future planning, etc. for at least five years” until she got her finances in order.

Panel B of Figure 6 shows that many of these themes persisted or even strengthened over time. By the end of the program, the treatment group talked about specific information including how credit works, strategies for credit and financial planning, knowledge gained from the credit counseling and workshop, and a sense of personal responsibility, control, and confidence over their credit and finances. The control group still exhibited confusion around how to proceed to fix their finances, felt anxious and squeezed for money, and believed that essential information about credit and finances was inaccessible to the general public—revealing a feeling of a lack of control. As a result, much of the discussion in the control group reflected the emotional toll of constantly wondering whether one is making the right financial decisions and feeling that no help is available. While the treatment group demonstrated actual understanding of credit or financial planning and talked about specific strategies, the control group rarely did. In contrast, the control group appeared to add to their cognitive load by needing to try many different strategies without a system or framework for vetting them. Indeed, the control group displayed a notable amount of help-seeking/help-giving behavior during the focus group, such as asking questions and sharing specific information about financial tools, institutions, and cell phone apps with their peers.

## DISCUSSION

### Benchmarking and Interpreting the BYCBI Results

The BYCBI impacts on the administrative credit outcomes for the treatment group are sizeable and persist through the end of the program as well as six months afterward. Within the first six months of the program, individuals in the treatment group were 10 percentage points more likely to have established a credit score compared to the control group. By the end of the 18-month observation window, the average credit score among individuals with a credit file prior to the start of the program was 26 points higher for the treatment group relative to the control group, raising the likelihood of achieving a “good” credit rating by 8 percentage points.

These results stand somewhat in contrast to much of the existing literature on financial education interventions, which have produced mixed results to date, even when assessing programs that aim to develop financial capability by combining financial knowledge as well as access to financial products. Instead, our findings are largely consistent with more recent, albeit descriptive, studies specifically focused on financial coaching that find positive associations between coaching and client outcomes including greater confidence, changes in behaviors such as budgeting and saving, and improvements in credit building (Collins and O’Rourke 2012, Moulton et al. 2013, NeighborWorks America 2013, Center for Financial Security 2015). In addition, the experimental study most similar to ours found that financial coaching increased credit scores by as much as 20 points, although these gains were not consistent across the two sites due to low compliance rates (Theodos et al. 2015).

Based on our survey data, individuals in the treatment group reported being in better financial situations than those of the control group after the program had ended. In addition, the BYCBI had a significant impact on the financial behaviors of the treatment group by reducing

their use of alternative financial services. Finally, the program had a greater impact on financial self-efficacy, rather than financial knowledge, with the former driving both the change in financial behaviors and the improvement in credit scores. Again, the only comparable experimental study (Theodos et al. 2015) corroborates some of these findings, although not consistently across both sites studied. They find significant increases in satisfaction with one's current situation as well as a reduction in the use of alternative financial services at one coaching site, but not the other. In addition, they find significant decreases in financial stress at one of their sites that was similar in magnitude to our self-efficacy components, yet “no impact of financial coaching on factual financial knowledge as we measured it.”

The measurable effect size of the BYCBI on credit outcomes is perhaps unsurprising in hindsight. First, the BYCBI achieved a greater take-up rate compared to other experimental studies of financial coaching, making it more likely that we would be able to detect impacts. In addition, the intervention was longer in duration and greater in intensity, with credit pulls and coaching every six months to measure each individual's progress toward their goals. Finally, the BYCBI was focused solely on improving credit scores, whereas the aims of other financial coaching programs are more broadly encompassing, making it more difficult to detect impacts given that outcomes can vary considerably across individuals based on their goals.

#### Cost Comparisons

Working Credit typically charges employers \$10 per employee per month to deliver their program in an employer setting. This cost may be subsidized by the employer on behalf of the worker as an employee benefit, further reducing the cost to the individual. Recall that by the end of the program, individuals in the treatment group had interest rates on their car loans that were nearly half (3.5 percentage points lower) the rates paid by those in the control group—resulting

in savings of about \$30 per month on a five-year \$10,000 car loan. Thus, on an individual basis, the program is certainly “worth it”—even without the employer subsidy—especially when one factors in having lower interest rates on future loans going forward.

Yet is there a less costly alternative that could achieve the same benefits as financial coaching? At this point, it’s hard to say. On the one hand, Theodos et al. 2015 studied a lighter-touch intervention that included a workshop and on average one financial coaching session over a three-month period, but found weaker effects. On the other, the BYCBI was only a slightly more intensive program with just one in-person coaching session at the beginning and most subsequent six-month check-ins occurring over the phone or via email.

#### Validity of BYCBI Effect Estimates

Several factors suggest that our results are likely lower bound estimates among this population of young adults. The most obvious is that due to the experimental design of the evaluation, we deviated from the Working Credit model that typically begins with a mandatory workshop followed by the offer for financial coaching. This approach typically results in a take-up rate of over 90 percent for the financial coaching—similar to what we saw among compliers who had at least attended the workshop. It is likely that if youth workforce development programs made the workshop a mandatory part of training, the program’s overall compliance rate would be much closer to 90 percent.

Diminishing marginal returns among the treatment group attending more coaching sessions and/or any “John Henry” effects among control group members who seek to “catch up” will also serve to mitigate positive findings. However, the additional coaching after the initial in-person meeting was often conducted either on the phone or via email and included the individual’s credit report, which would be continually updated and tracked against their goals,

which is certainly valuable information for each session, even on the margin. And although our randomization was stratified within organizational groups, leaving open the possibility of cross-contamination, we did not see any indication of that in our focus group discussions.

Still, some external validity concerns suggest that these estimates may be difficult to replicate in other settings. These include having trained Working Credit staff to provide coaching to all individuals. In addition, the individuals were relatively young, new to the labor force, and often living alone for the first time. Finally, while individuals could not select into treatment, they did choose to apply to the program such that there may be some selection on unobservable characteristics, such as wanting a better financial future, thereby making these individuals “better compliers” than the average young adult in Greater Boston. As a result, our findings are most usefully applied to other groups of young workers, such as new public sector employees and those in workforce development, apprenticeship, or union programs.

## CONCLUSION

Access to credit can provide individuals with the liquidity necessary to maintain financial stability during an economic setback and to take advantage of opportunities that affect their long-term financial well-being. Despite the mixed results of earlier financial education programs, policymakers continue to employ financial education programs as part of their toolbox to improve the financial well-being of low- and moderate-income groups. Such efforts are reflected in a renewed focus on the financial capability of youth engaged in workforce development programs, as required by the Workforce Innovation and Opportunity Act of 2014.

Using an experimental design, we estimate the causal impact of a financial coaching program on low-income young adults currently working or enrolled in a workforce development program. The goal of the BYCBI was to help individuals either gain access to credit and/or



improve their credit scores by providing a one-hour workshop as well as financial coaching over the course of one year. Overall, our results demonstrate that the program affected the treatment group in many of the ways that it was designed to. The self-reported survey data show that the treatment group increased their financial knowledge, gained greater financial self-efficacy, and reduced their use of alternative financial services. The credit report data confirm that these behavioral changes improved credit scores by upward of 26 points and raised the likelihood of having a “good” credit rating by 8 percentage points.

Moreover, we find that financial coaching can have a meaningful impact on the financial circumstances of low-income young adults apart from simply increasing credit scores. By improving access to credit, individuals in the treatment group faced fewer financial barriers to establishing a household (e.g., costly utility deposits) and fewer negative financial shocks (e.g., eviction). In addition, they enjoyed greater access to credit as well as more favorable rates on car loans—outcomes that are quite impactful for this cash-strapped population.

We close by offering some policy-relevant insights for future program design. First, our analysis shows heterogeneous impacts by age and race, suggesting how cities with limited resources may want to target these programs. Second, we find that much of the impact of financial coaching is driven by improvements in financial self-efficacy, which may have been the missing ingredient in prior financial education programs. Finally, we show how the path toward better credit evolves over time and across different dimensions—even after the program ends—suggesting that financial coaching programs have the potential to affect behaviors beyond the short term. We hope these lessons can help cities and states as they move toward incorporating financial education into youth workforce development programs with an eye toward improving both financial and labor market outcomes.

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**FIGURE 1. Program Timeline**

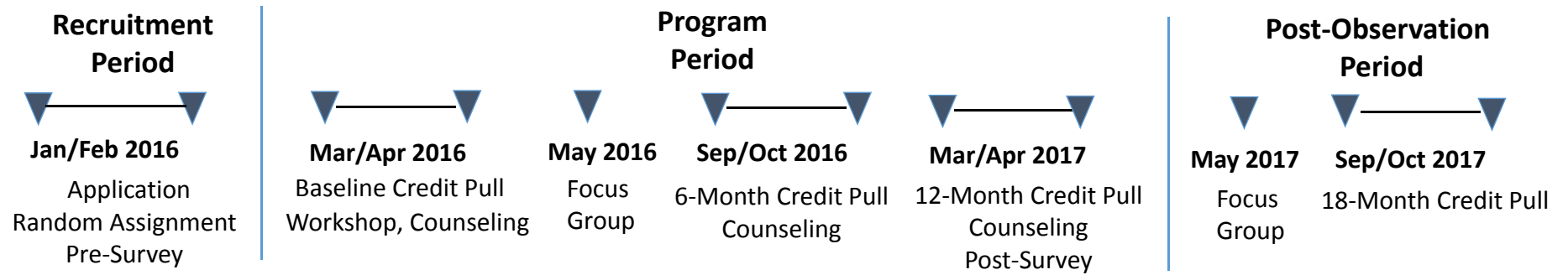
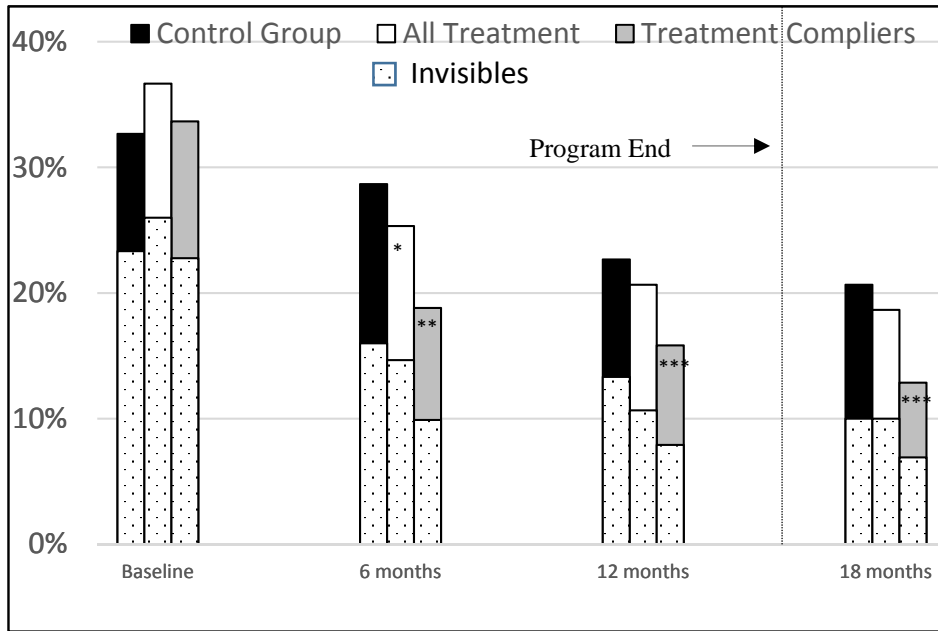
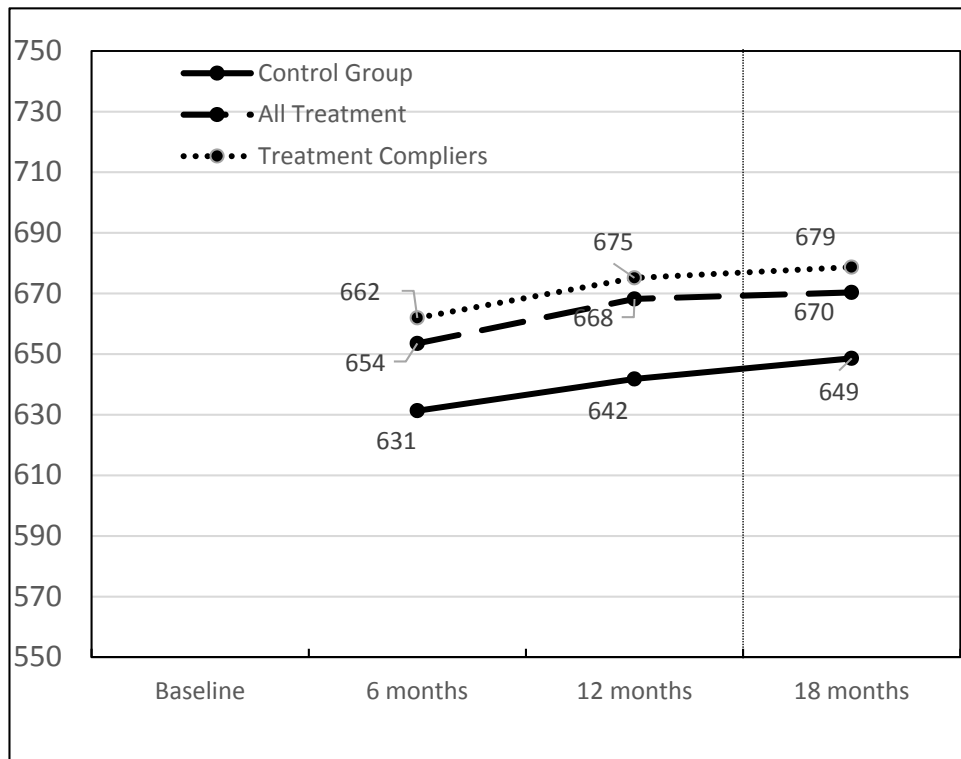


FIGURE 2. Change in Access to Credit Over Time

A. Percent with No Credit Score (N=300)



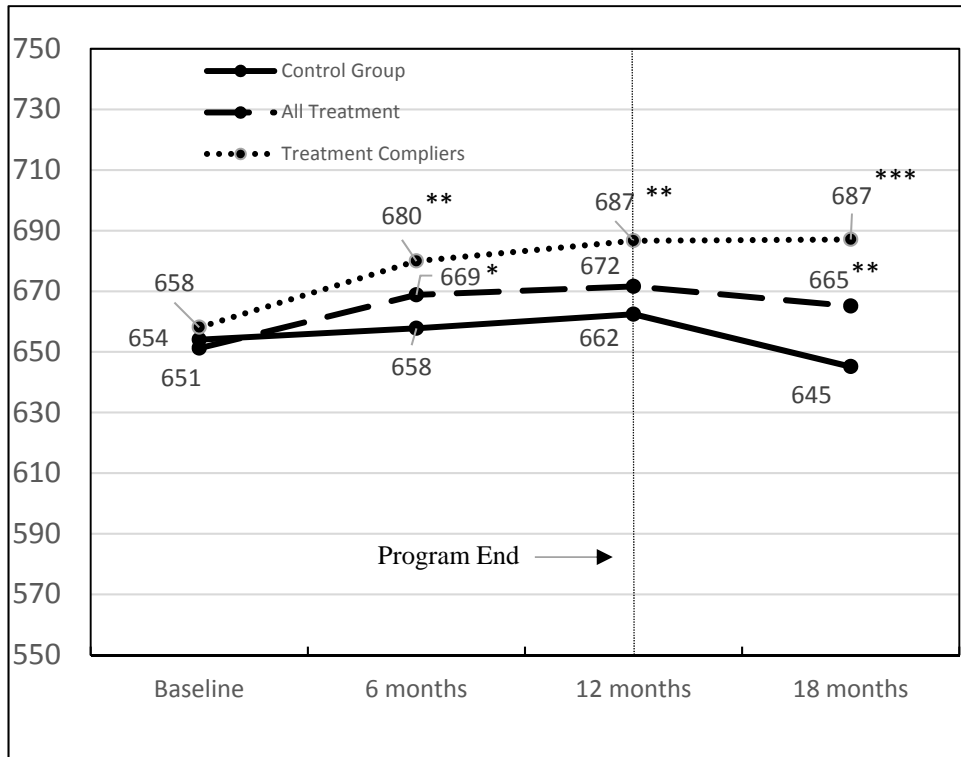
B. Mean Credit Score for Individuals Gaining Access to Credit at Six Months (N=74)



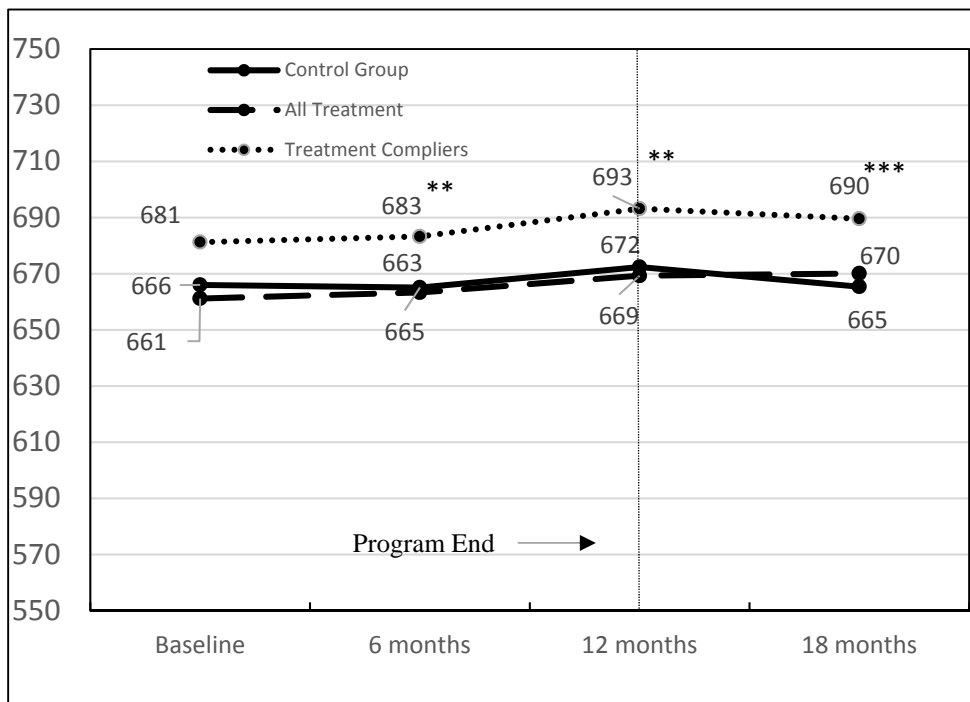
Note: Unadjusted means are reported for each group.  $p < 0.10^*$  percent level,  $p < 0.05^{**}$ , and  $p < 0.01^{***}$ .  
 Source: Authors' calculations based on data supplied by Working Credit.

FIGURE 3. Change in Credit Scores Over Time

A. All Individuals with Credit File at Baseline (N=226)

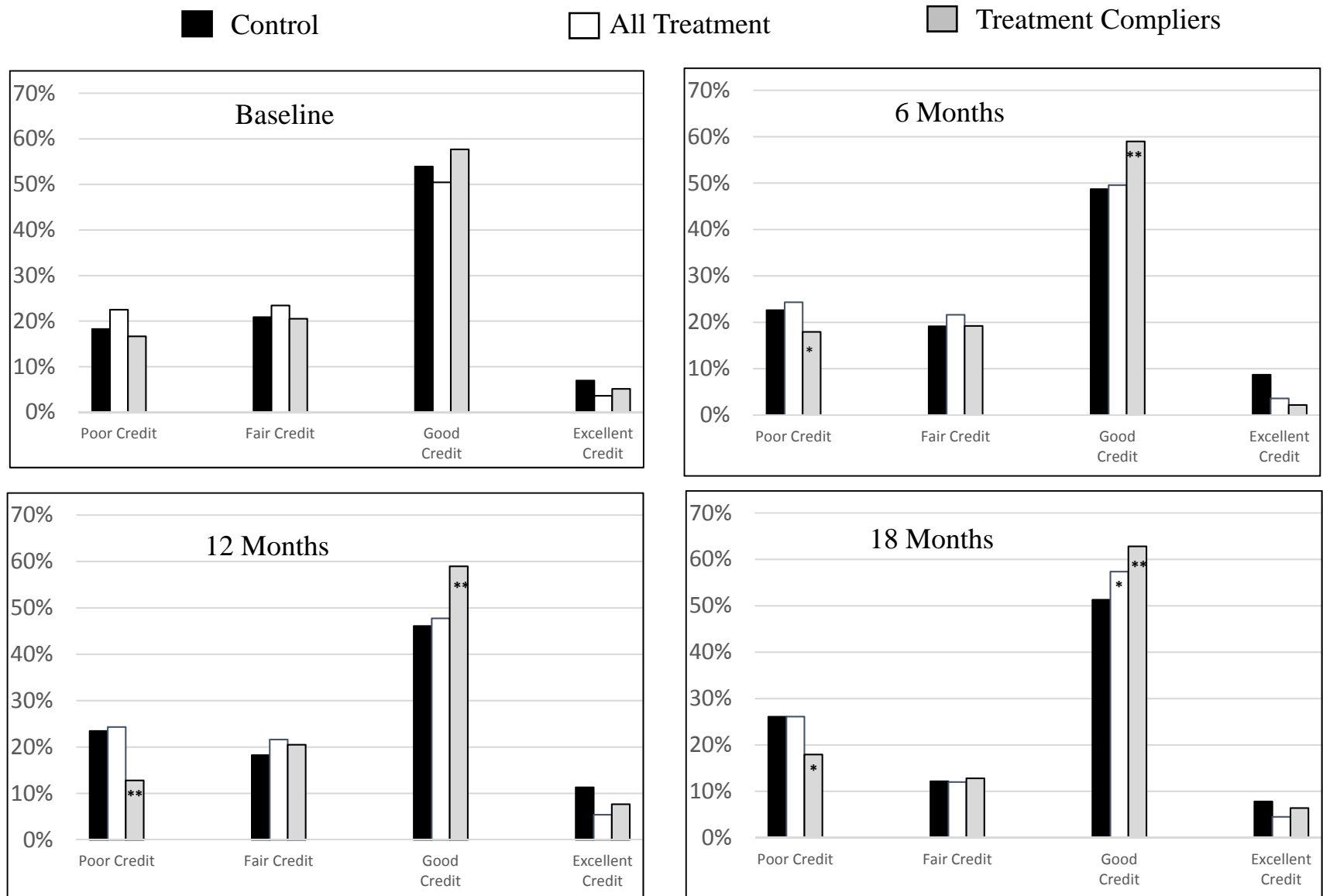


B. All Individuals with Credit Score at Baseline (N=196)



Note: Unadjusted means are reported for each group.  $p < 0.10^*$  percent level,  $p < 0.05^{**}$ , and  $p < 0.01^{***}$ .  
 Source: Authors' calculations based on data supplied by Working Credit.

**FIGURE 4. Credit Report Ratings: Comparison of Treatment versus Control Groups Over Time  
All Individuals with Credit File at Baseline (N=226)**

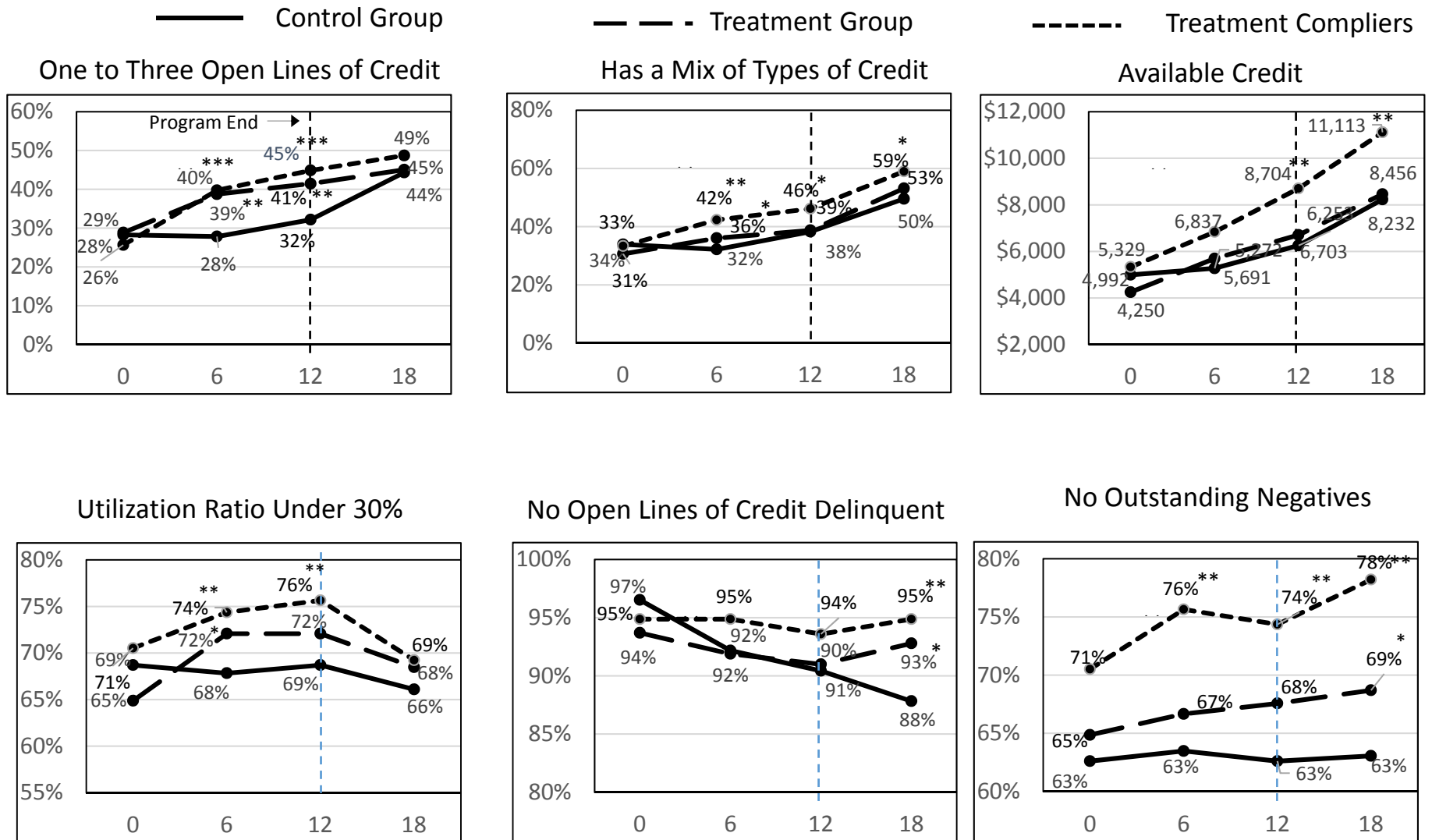


Note: Significance relative to the control group indicated at  $p < 0.10$ \* percent level and  $p < 0.05$ \*\*

Source: Authors' calculations based on data supplied by Working Credit.

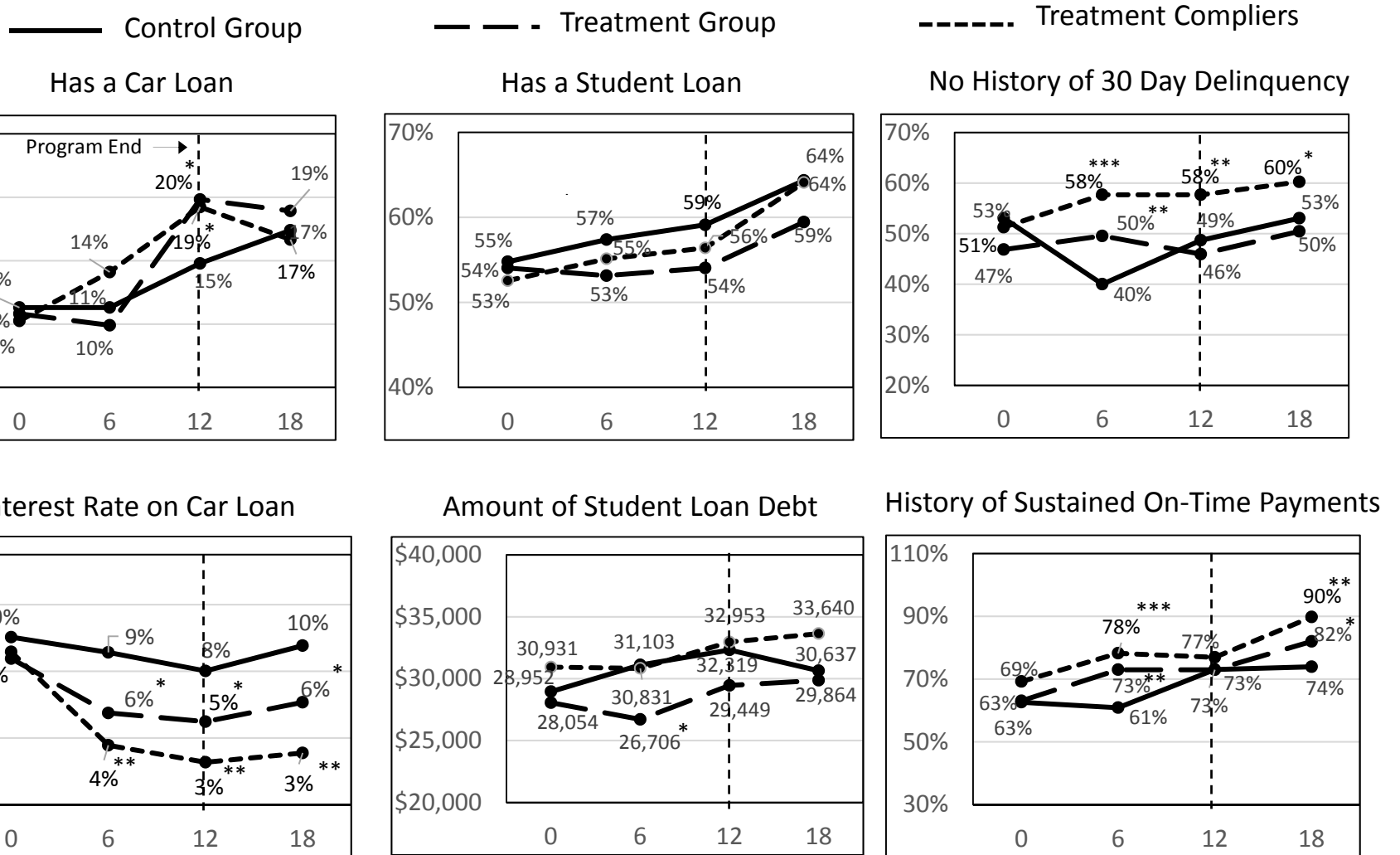


**FIGURE 5. Credit Use: Comparison of Treatment versus Control Groups Over Time**  
**All Individuals with Credit File at Baseline (N=226)**



Note: Significance relative to the control group indicated at \* $p < 0.10$  percent level,  $p < 0.05$ \*\*, and  $p < 0.01$ \*\*\*.  
 Source: Authors' calculations based on data supplied by Working Credit.

**FIGURE 6. Loan History: Comparison of Treatment versus Control Groups Over Time**  
**All Individuals with Credit File at Baseline (N=226)**



Note: Significance relative to the control group indicated at \* $p < 0.10$  percent level,  $p < 0.05$ \*\*, and  $p < 0.01$ \*\*\*.

Source: Authors' calculations based on data supplied by Working Credit.

**FIGURE 7. Comparison of Themes from Focus Group Discussions Ranked by Frequency**

**Panel A. May 2016 (Start of Program)**

<b>Treatment Group</b>	<b>Control Group</b>
Made credit mistakes due to ignorance	Financially strapped
Feels regret over past credit mistakes	In a precarious financial position
Received credit in the past but did not understand it	Never learned about credit before
Learned concrete steps to improve credit score through the program	Never received guidance when making credit decisions
Uses a strategy for dealing with credit	Uses a strategy for dealing with credit
Schools or agencies should offer opportunity to learn about credit/ finances to younger kids	Schools or agencies should offer opportunity to learn about credit/ finances to younger kids

**Panel B. May 2017 (End of Program)**

<b>Treatment Group</b>	<b>Control Group</b>
Has strategy for dealing with credit	Shares information with group
Feels confident	Feels squeezed for money
Demonstrates understanding of credit and financial planning	Information inaccessible
Gained concrete knowledge from credit program	Confusion about what actions to take

Note: Themes ranked by frequency, with the most frequent theme listed first.

Source: Authors' analysis based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table 1. Baseline Demographic Characteristics: Treatment v Control Group**

	Treatment Group		Control Group		Difference
	(1)	(2)	(3)	(4)	(5)
<b>Type of Organization</b>					
Typical	37.3%	(0.040)	34.7%	(0.039)	2.7
Near-Typical	15.3%	(0.030)	16.7%	(0.031)	-1.3
Atypical	47.3%	(0.041)	48.7%	(0.041)	-1.3
<b>Age</b>					
Mean	23.64	(0.252)	23.75	(0.224)	-0.1
18-24	60.7%	(0.040)	58.0%	(0.040)	2.7
25-29	39.3%	(0.040)	42.0%	(0.040)	-2.7
<b>Gender</b>					
Female	58.7%	(0.040)	63.3%	(0.039)	-4.7
<b>Race</b>					
African American/Black	48.7%	(0.041)	50.0%	(0.041)	-1.3
American Indian/Native Alaskan	1.3%	(0.009)	1.3%	(0.009)	0.0
Asian/Hawaiian/Pacific Islander	8.3%	(0.018)	4.7%	(0.017)	3.7 *
Caucasian/White	16.0%	(0.030)	21.3%	(0.034)	-5.3
Two or more races	10.7%	(0.025)	8.7%	(0.023)	2.0
Other	15.3%	(0.034)	14.3%	(0.031)	1.0
<b>Ethnicity</b>					
Hispanic	24.7%	(0.035)	26.0%	(0.036)	-1.3
<b>Veteran status</b>					
Veteran	0.0%	(0.000)	1.3%	(0.009)	-1.3
<b>Marital status</b>					
Married	3.3%	(0.015)	6.7%	(0.020)	-3.3
<b>Household size</b>					
Number	2.92	(0.115)	2.98	(0.109)	-0.1
<b>Children</b>					
Has any children	17.3%	(0.031)	14.0%	(0.028)	3.3
<b>Education</b>					
Less than a high school diploma	8.0%	(0.022)	9.3%	(0.024)	-1.3
High school diploma or GED	28.0%	(0.037)	22.0%	(0.034)	6.0
Some college	22.7%	(0.034)	32.0%	(0.039)	-9.3 *
Associate's degree	3.3%	(0.015)	2.0%	(0.011)	1.3
Bachelor's degree	30.0%	(0.038)	25.3%	(0.036)	4.7
Advanced or professional degree	4.7%	(0.017)	5.3%	(0.018)	-0.7
Not reported	2.7%	(0.013)	2.7%	(0.007)	0.0
<b>Employment tenure</b>					
Less than one year	64.7%	(0.039)	60.7%	(0.040)	4.0
One to two years	16.7%	(0.031)	17.3%	(0.031)	-0.7
Two to five years	12.7%	(0.027)	14.0%	(0.028)	-1.3
More than five years	2.0%	(0.011)	2.7%	(0.013)	-0.7
Not reported	4.0%	(0.016)	5.3%	(0.018)	-1.3
<b>Health insurance</b>					
Private plan, through employer	29.3%	(0.037)	28.7%	(0.037)	0.7
Medicaid (MassHealth)	44.0%	(0.041)	36.7%	(0.039)	7.3
Other	19.3%	(0.032)	27.3%	(0.037)	-8.0
None	4.7%	(0.017)	4.0%	(0.016)	0.7
Not reported	2.7%	(0.013)	3.3%	(0.015)	-0.7
<b>Homeowner status</b>					
Own	6.0%	(0.019)	7.3%	(0.021)	-1.3
<b>Household income</b>					
Above \$71,991	10.0%	(0.025)	10.7%	(0.025)	-0.7
<b>Can save \$26 per month</b>					
Yes	94.7%	(0.018)	95.3%	(0.017)	-0.7
<b>Number of observations</b>					
	150		150		

Note: \*Significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table 2. Baseline Outcome Measures: Treatment v Control Groups**

	Control group		Treatment group		Difference
	(1)		(2)		(3)
<b>Panel A. Administrative Credit Report Measures</b>					
Access to credit (Full sample N=300)					
No credit score	32.7%	(0.038)	36.7%	(0.039)	-4.00
No credit score, no credit file	23.3%	(0.035)	26.0%	(0.036)	-2.67
No credit score, thin credit file	9.3%	(0.024)	10.7%	(0.025)	-1.33
Credit score (mean), excluding those with no credit file					
All individuals with credit score (N=196)	666.03	(8.227)	661.2	(8.689)	486.13
All individuals with credit file (N=226)	654.04	(7.338)	651.2	(7.623)	284.00
Credit rating (all individuals with credit file N=226)					
Poor: credit score >=300 and <=600	18.3%	(0.036)	22.5%	(0.040)	-4.26
Fair: credit score >=601 and <=660	20.9%	(0.038)	23.4%	(0.040)	-2.55
Good: credit score >=661 and <=780	53.9%	(0.047)	50.5%	(0.048)	3.46
Excellent: credit score >=780	7.0%	(0.024)	3.6%	(0.018)	3.35
Factors affecting credit score (all individuals with credit file N=226)					
At least one open line of credit but no more than three	28.3%	(0.046)	28.8%	(0.043)	-0.57
Has a mix of revolving and installment lines of credit	33.9%	(0.044)	30.6%	(0.044)	3.28
Utilization ratio under 30 percent	68.7%	(0.043)	64.9%	(0.046)	3.83
Amount of available credit	\$ 5,684.13	(1156.366)	\$ 4,933.75	(949.934)	750.38
No lines of credit that are currently delinquent (30 days currently past due)	96.5%	(0.017)	93.7%	(0.023)	2.83
No current outstanding negatives (collections, charge-offs, judgements)	62.6%	(0.045)	64.9%	(0.046)	-2.26
Has a car loan	11.3%	(0.030)	10.8%	(0.030)	0.49
Interest rate on car loan (for those with a car loan)	10.5%	(0.021)	9.0%	(0.022)	1.56
Has a student loan	54.8%	(0.047)	54.1%	(0.048)	0.73
Amount of student loan debt (for those with a student loan)	\$ 28,952.30	(3514.661)	\$28,054.07	(3877.413)	898.23
No history of 30-day delinquent	53.0%	(0.047)	46.8%	(0.048)	6.20
History of sustained on-time payments	62.6%	(0.045)	63.1%	(0.046)	-0.45
<b>Panel B. Self-Reported Survey Measures (Full sample N=300)</b>					
Financial well-being					
In credit counseling or debt management plan or working with one	4.0%	(0.016)	3.3%	(0.015)	0.67
Cell phone company currently holding a deposit	12.0%	(0.027)	12.0%	(0.027)	0.00
Utility company currently holding a deposit	5.3%	(0.018)	6.0%	(0.019)	-0.67
Wages garnished in the past year	7.3%	(0.021)	8.7%	(0.023)	-1.33
Utilities been disconnected in the past year or in danger of disconnection	10.7%	(0.025)	8.0%	(0.022)	2.67
Car been repossessed in past year or in danger of repossession	3.3%	(0.015)	1.3%	(0.009)	2.00
Been evicted in past year or in process of eviction	4.0%	(0.016)	1.3%	(0.009)	2.67
Foreclosure started or in danger of foreclosure	0.7%	(0.007)	0.7%	(0.007)	0.00
Contacted by collection agencies contacting about unsettled claims	20.0%	(0.033)	19.3%	(0.032)	0.67
In bankruptcy or in process of bankruptcy	2.0%	(0.011)	0.0%	(0.000)	2.00
Plan to apply for a mortgage or car loan in next three months	8.7%	(0.023)	8.0%	(0.022)	0.67
Financial behaviors (standardized to a scale of 0 to 1)					
Use of mainstream financial services	0.56	(0.022)	0.54	(0.022)	0.01
Use of alternative financial services	0.15	(0.012)	0.15	(0.012)	-0.01
Financial knowledge (based on 18 true/false questions)					
Mean score (percent right)	76.5%	(0.011)	74.9%	(0.011)	0.02
Share getting more than 75% correct	62.0%	(0.040)	58.0%	(0.040)	0.04
Financial self-efficacy (standardized to a scale of 0 to 1)					
Confidence in financial knowledge	0.59	(0.012)	0.61	(0.010)	-0.02
Confidence in financial skills	0.60	(0.013)	0.63	(0.016)	-0.03
Concern about financial situation	0.75	(0.014)	0.71	(0.017)	0.04 *
Overall self-efficacy score	0.57	(0.013)	0.59	(0.015)	-0.02
<b>Number of observations</b>	150		150		

Note: Standard errors in parentheses. See data appendix for construction of each outcome measure. \*indicates significance at the 10% level.

Source: Authors' calculations based on credit report data provided by Working Credit and survey data provided by supplied the Boston Mayor's Office of Financial Empowerment.

**Table 3. Regression Estimates of BYCBI Impact on Access to Credit and Credit Scores at 18 Months**

	Treatment versus Control Group (ITT)		Compliers versus Control Group (TOT)	
	(1)	(2)	(3)	(4)
<b>Panel A. Access to Credit: Full Sample (N=300)</b>				
Percent with no credit	0.004 (0.037)	0.001 (0.036)	-0.102 *** (0.032)	-0.101 *** (0.031)
<b>Panel B. Credit Score: All Individuals with a Credit File at Baseline (N=226)</b>				
Mean credit score	24.226 * (13.022)	26.405 ** (12.380)	39.343 ** (13.702)	37.554 ** (13.191)
<u>Credit rating:</u>				
Poor: credit score >=300 and <=600	-0.021 (0.047)	-0.029 (0.043)	-0.087 * (0.053)	-0.090 * (0.050)
Fair: credit score >=601 and <=660	0.020 (0.055)	0.032 (0.052)	-0.001 (0.050)	-0.028 (0.048)
Good: credit score >=661 and <=780	0.078 * (0.045)	0.081 * (0.042)	0.121 ** (0.060)	0.130 ** (0.058)
Excellent: credit score >=780	-0.014 (0.028)	-0.031 (0.033)	-0.009 (0.031)	0.005 (0.029)
Includes controls for baseline measures of outcomes	Yes	Yes	Yes	Yes
Includes controls for demographic characteristics	No	Yes	No	Yes

Note: Each entry is the estimated coefficient on the treatment dummy. Percent with no credit is estimated as a probability using a probit regression where the coefficients are reported as marginal effects. Credit score outcomes are estimated using OLS. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*indicates significance at the 5% level, and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by Working Credit.

**Table 4. Regression Estimates of BYCBI Impact on Underlying Factors Affecting Credit Score at 18 Months: All Individuals with a Credit File**

	Treatment versus Control Group (ITT)		Compliers versus Control Group (TOT)			
	(1)	(2)	(3)		(4)	
At least one open line of credit but no more than three	0.034 (0.064)	0.037 (0.062)	0.097 (0.068)		0.121 (0.071)	*
Has a mix of revolving and installment lines of credit	0.043 (0.058)	0.053 (0.055)	0.109 (0.061)	*	0.110 (0.063)	*
Utilization ratio under 30 percent	0.036 (0.058)	0.036 (0.055)	0.009 (0.062)		-0.010 (0.064)	
Amount of available credit	1167.471 (1142.126)	913.697 (1050.742)	2879.731 (1239.979)	**	2693.900 (1175.266)	**
No lines of credit that are currently delinquent	0.064 (0.038)	0.066 (0.036)	0.081 (0.041)	**	0.085 (0.042)	**
No current outstanding negatives	0.069 (0.039)	0.068 (0.035)	0.129 (0.066)	**	0.142 (0.054)	**
Has a car loan	0.020 (0.047)	0.019 (0.048)	-0.023 (0.054)		-0.036 (0.052)	
Interest rate on car loan	-0.034 (0.020)	-0.035 (0.019)	-0.056 (0.023)	**	-0.068 (0.018)	***
Has a student loan	-0.049 (0.045)	-0.028 (0.040)	0.053 (0.048)		0.068 (0.049)	
Amount of student loan debt	186.527 (2598.437)	1277.014 (3487.591)	1201.578 (2754.581)		2745.385 (3180.533)	
No history of 30-day delinquent on loans	0.010 (0.055)	0.032 (0.051)	0.118 (0.057)	**	0.114 (0.058)	**
History of sustained on-time payments on loans	0.076 (0.046)	0.106 (0.040)	0.139 (0.049)	**	0.117 (0.048)	**
Includes controls for baseline measures of outcomes	Yes	Yes	Yes		Yes	
Includes controls for demographic characteristics	No	Yes	No		Yes	
Number of observations	226	226	226		226	

Note: Each entry is the estimated coefficient on the treatment dummy. Outcomes are estimated as probabilities using probit regressions where the coefficients are reported as marginal effects. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*indicates significance at the 5% level, and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by Working Credit.

**Table 5. Regression Estimates of BYCBI Impact on Access to Credit and Credit Scores at 18 Months - By Subgroups**

	<b>All individuals</b>	<b>18-24 years</b>	<b>Female</b>	<b>African-American</b>	<b>Atypical Organization</b>
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>
<b>Percent with No Credit Score: Full Sample (N=300)</b>					
Treatment versus Control Group: ITT Estimates					
Treatment	0.001 (0.036)	0.023 (0.060)	0.028 (0.044)	0.034 (0.040)	0.042 (0.039)
Treatment * group dummy	-----	-0.105 * (0.055)	-0.047 (0.051)	-0.102 * (0.055)	-0.095 * (0.054)
Compliers versus Control Group: TOT Estimates					
Treatment	-0.101 *** (0.031)	-0.047 (0.045)	-0.062 (0.044)	-0.060 * (0.034)	-0.079 ** (0.042)
Treatment * group dummy	-----	-0.093 * (0.057)	-0.068 (0.052)	-0.194 *** (0.062)	-0.048 (0.062)
<b>Mean Credit Score: All Individuals with a Credit File at Baseline (N=226)</b>					
Treatment versus Control Group: ITT Estimates					
Treatment	26.405 ** (12.380)	9.517 (13.760)	21.791 (13.744)	18.355 (13.645)	24.302 * (13.068)
Treatment * group dummy	-----	25.226 * (14.358)	2.720 (11.289)	9.574 (16.586)	1.723 (11.265)
Compliers versus Control Group: TOT Estimates					
Treatment	37.554 ** (13.191)	6.556 (14.351)	26.247 (18.640)	23.109 (17.533)	31.497 ** (16.491)
Treatment * group dummy	-----	30.493 * (16.423)	5.456 (21.677)	2.752 (23.730)	6.006 (14.353)
Includes controls for baseline measures of outcomes	Yes	Yes	Yes	Yes	Yes
Includes controls for demographic characteristics	Yes	Yes	Yes	Yes	Yes

Note: Each entry is the estimated coefficient on the treatment dummy. Percent with no credit is estimated as a probability using a probit regression where the coefficients are reported as marginal effects. Credit score outcomes are estimated using OLS. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*indicates significance at the 5% level, and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by Working Credit.



**Table 6. Regression Estimates of BYCBI Impact at 18 Months for Compliers Offered the CW-3 Product**

	All individuals	Offered CW-3	Took Up CW-3
	(1)	(2)	(3)
<b>Percent with No Credit Score: Full Sample (N=300)</b>			
Compliers versus Control Group: TOT Estimates			
Treatment dummy	-0.101 *** (0.031)	0.063 (0.042)	-0.032 (0.046)
CW-3 dummy	-----	-0.140 ** (0.050)	-0.146 ** (0.072)
<b>Mean Credit Score: All Individuals with a Credit File at Baseline (N=226)</b>			
Compliers versus Control Group: TOT Estimates			
Treatment dummy	37.554 ** (13.191)	-1.712 (23.815)	28.459 (43.937)
CW-3 dummy	-----	97.564 ** (34.553)	68.257 ** (24.202)
Includes controls for baseline measures of outcomes	Yes	Yes	Yes
Includes controls for demographic characteristics	Yes	Yes	Yes

Note: Each entry is the estimated coefficient on the treatment dummy. Percent with no credit is estimated as a probability using a probit regression where the coefficients are reported as marginal effects. Credit score outcomes are estimated using OLS. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*Indicates significance at the 5% level and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by Working Credit.

**Table 7. Regression Estimates of BYCBI Impact on Self-Reported Survey Outcomes at 12 Months**

	Treatment versus Control Group (ITT)		Compliers versus Control Group (TOT)	
	(1)	(2)	(3)	(4)
<u>Financial well-being (dummy variable where yes=1, no=0)</u>				
In credit counseling or debt management plan or working with one	0.088 ** (0.045)	0.119 ** (0.051)	0.092 ** (0.044)	0.102 ** (0.046)
Cell phone company currently holding a deposit	-0.019 (0.037)	0.005 (0.037)	-0.026 (0.037)	-0.009 (0.039)
Utility company currently holding a deposit	-0.059 * (0.033)	-0.073 ** (0.034)	-0.070 ** (0.034)	-0.075 ** (0.036)
Wages garnished in the past year	0.012 (0.036)	0.022 (0.041)	0.005 (0.036)	0.019 (0.038)
Utilities been disconnected in the past year or in danger of disconnection	-0.024 (0.041)	-0.011 (0.045)	-0.065 (0.042)	-0.046 (0.044)
Car been repossessed in past year or in danger of repossession	-0.005 (0.019)	0.001 (0.019)	-0.003 (0.017)	-0.002 (0.018)
Been evicted in past year or in process of eviction	-0.032 * (0.018)	-0.046 ** (0.022)	-0.037 * (0.020)	-0.049 ** (0.021)
Foreclosure started or in danger of foreclosure	-0.021 (0.015)	-0.025 (0.018)	-0.019 (0.015)	-0.022 (0.016)
Contacted by collection agencies contacting about unsettled claims	-0.076 (0.051)	-0.095 * (0.050)	-0.098 * (0.052)	-0.110 ** (0.055)
In bankruptcy or in process of bankruptcy	-0.010 (0.018)	-0.018 (0.020)	-0.007 (0.018)	-0.015 (0.019)
Plan to apply for a mortgage or car loan in next three months	0.013 (0.049)	0.014 (0.053)	0.022 (0.049)	0.020 (0.052)
<u>Financial behaviors</u>				
Mainstream financial services z-score	0.019 (0.076)	0.020 (0.076)	0.027 (0.077)	0.013 (0.080)
Alternative financial services z-score	-0.328 ** (0.147)	-0.395 ** (0.145)	-0.443 *** (0.145)	-0.419 *** (0.150)
<u>Financial knowledge</u>				
Financial knowledge z-score	0.315 ** (0.131)	0.329 ** (0.136)	0.306 ** (0.132)	0.285 ** (0.136)
Dummy variable for getting more than 75% correct	0.117 ** (0.057)	0.125 ** (0.058)	0.154 ** (0.063)	0.176 ** (0.061)
<u>Financial self-efficacy</u>				
Confidence in financial knowledge z-score	0.676 *** (0.130)	0.640 *** (0.125)	0.684 *** (0.128)	0.639 *** (0.127)
Confidence in financial skills z-score	0.532 *** (0.139)	0.558 *** (0.143)	0.559 *** (0.140)	0.566 *** (0.143)
Confidence in financial situation z-score	0.186 (0.142)	0.202 (0.156)	0.170 (0.143)	0.211 (0.152)
Overall self-efficacy z-score	0.618 *** (0.135)	0.630 *** (0.138)	0.635 *** (0.134)	0.641 *** (0.135)
Includes controls for baseline measures of outcomes	Yes	Yes	Yes	Yes
Includes controls for demographic characteristics	No	Yes	No	Yes
Number of observations	194	194	194	194

Note: The sample of individuals answering both the pre-and post-survey included 96 individuals in the treatment group and 98 individuals in the control group. Each entry is the estimated coefficient on the treatment dummy. Scored outcomes are estimated using OLS. Other outcomes are estimated as probabilities using probit regressions where the coefficients are reported as marginal effects. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*indicates significance at the 5% level, and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by the Boston Mayor's Office of Financial Empowerment.

**Table 8. Mediation of the Impact of BYCBI on Credit Outcomes through Participants Gain in Financial Literacy and Self-Efficacy**

	Point Estimate	Product of Coefficients		Bootstrapping					
		SE	Z	Percentile 95% CI		BC 95% CI		BCa 95% CI	
				Lower	Upper	Lower	Upper	Lower	Upper
Panel A. Dependent Variable: 12-Month Credit Score									
				<u>Indirect Effects</u>					
Financial literacy	17.911	11.974	1.500	-2.576	40.966	-1.095	42.633	-1.045	42.877
Self-efficacy	14.813	7.360	2.013	0.516	37.718	2.023	40.804	2.425	42.349
TOTAL	32.724	14.570	2.250	7.775	63.507	9.259	66.389	9.616	67.297
				<u>Contrast</u>					
Financial literacy versus self-efficacy	3.098	14.638	0.210	-28.435	35.745	-27.481	36.109	-----	-----
Panel B. Dependent Variable: 12-Month Alternative Financial Services Score									
				<u>Indirect Effects</u>					
Financial literacy	-0.004	0.004	-1.210	-0.013	0.002	-0.015	0.001	-0.015	0.001
Self-efficacy	-0.028	0.009	-3.170	-0.048	-0.012	-0.050	-0.013	-0.051	-0.013
TOTAL	-0.032	0.010	-3.400	-0.055	-0.014	-0.056	-0.015	-0.057	-0.015
				<u>Contrast</u>					
Financial literacy versus self-efficacy	0.024	0.010	2.490	0.008	0.045	0.009	0.046	-----	-----

Note: Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. BC refers to bias corrected, BCa refers to bias corrected and accelerated using 5,000 bootstrap samples. No BCa reported for the contrasts due to insufficient number of observations to compute jackknife standard errors.

Source: Authors' calculations based on credit report data provided by Working Credit and survey data provided by the Boston Mayor's Office of Financial Empowerment.

## APPENDIX TABLES AND MATERIALS

This appendix includes supplementary tables as well as material describing:

1. Boston Youth Credit Building Initiative Intervention and Experimental Design
2. Construction of Measures using Administrative Credit Report Data
3. Construction of Measures using Self-Reported Survey Data
4. Focus Group Analysis
5. Mediation Analysis
6. Robustness checks

### 1. Boston Youth Credit Building Initiative (BYCBI) Intervention and Experimental Design

The BYCBI was developed by the Boston Mayor's Office of Financial Empowerment (OFE) and implemented by Working Credit NFP over the course of one year from March/April of 2016 through March/April of 2017. OFE recruited participants for the study during the two months prior to the start of the program, targeting low-income young adults, age 18-29, who were currently working or in a workforce development program. Most of the study participants were recruited from various organizations at a pre-arranged meeting where the program was explained in a five-minute presentation and application forms were distributed. Additional individuals were also recruited by OFE directly via a marketing campaign.

#### A. The BYCBI Intervention

The goal of the program was to help individuals build strong credit scores by increasing their knowledge of credit building, supplying them with credit building and saving products, and providing individualized advice through coaching over the course of one year. The treatment included the following program components:

**Financial literacy workshop.** A one-hour session was delivered at or near the individual's worksite, or as part of a mandatory staff meeting or a previously-scheduled training. The content focused on the information contained in a credit report, how the credit reporting system works, the consequences of having no or poor credit, and how to use different financial products to improve one's credit score. In addition to making payments on time, specific rules of thumb were given such as keeping one to four open lines of credit, having a mix of installment and revolving credit, having a sufficient amount of available credit for emergencies, and keeping the utilization ratio for each line of credit below 30 percent. At the end of the workshop, participants were urged to sign up for a one-on-one coaching session with a credit building counselor, either immediately after the workshop or at a later date.

**One-on-one coaching.** The initial coaching session was a one-hour in-person meeting that included a review of the participant's credit report and the development of an individualized budget and credit action plan focused on increasing the participant's credit score. The plan was put on paper during the session and also emailed to the participant afterwards. The counselor also assessed the participant's eligibility for the CW-3™ product. If eligible, the counselor enrolled

the participant immediately. If not yet eligible, the participant received clear direction about what he/she needed to do to qualify. Regardless of whether a person was enrolled in the CW-3 product, the counselor continued to support participants with credit coaching following the first appointment. At a minimum, the counselor pulled individual credit reports at six-month intervals and shared the results, along with additional credit building guidance, in person or by email.

**Enrollment in CW-3™ matched savings account.** The CW-3™ product is a “locked” savings account where the individual opens a 12-month \$300 Installment loan but does not take the loan proceeds; instead they are kept by the lender in an account until the loan is paid off. The individual makes 12 monthly payments of \$26 that is reported by the lender to the credit bureaus, building a positive track record for the participant. At the end of the loan term, the individual has accumulated \$300 in savings as well as an improved credit score. There is no risk of delinquency or default. If an individual fails to make a loan payment, Working Credit pays off the loan with the money from the “locked” savings account. To be eligible to enroll in the CW-3™ product the counselor must confirm that (1) the individual has a budget that shows they can afford to save \$26 per month and (2) that taking the product would be the best way to increase the individual’s credit score aside from other possible courses of action such as paying down debt.

## **2. Experimental Design**

To evaluate the impact of the BYCBI, we compared the outcomes of randomly selected individuals in the treatment group to those in the control group over time. Since the number of individuals applying for the program exceeded the number ultimately selected for participation, we were able to randomly assign participation in the program so that those individuals who applied but were not randomly selected to participate were used as a control group for the evaluation. Individuals in both the treatment and control groups received a \$150 financial incentive to participate in the study for one year. The control group received a \$25 gift card when notified that they were not selected for the program and a \$125 incentive for completing the post-survey. The treatment group received \$150 at the end of the program after completing the post-survey.

Working Credit’s program is typically delivered to a group of employees within a large firm where individuals have both a steady income for the duration of the program as well as regular and strong attachment to their employer, which helps ensure a high take-up rate. However, large firms serve only a small share of the low-income young adult population targeted for this intervention. In addition, there was interest in delivering the BYCBI to individuals in the context of a workforce development program to pilot the use of such interventions under the new WIOA requirements. As a result, it was necessary to cast a wider net for recruitment, with a total of 18 different organizations participating in the study (see Table A1). While these educational and community-based organizations serve low-income young adults, they do not conform to the typical Working Credit delivery model. To account for this, we categorized organizations as “typical,” “near-typical,” and “atypical” based on having: (1) regular/strong contact with individuals, and (2) an employment tenure that covered the duration of the BYCBI.

A total of 171 individuals were recruited from “typical” or “near-typical” organizations, accounting for roughly half (53 percent) of the sample. The remainder were recruited from “atypical” organizations, primarily from a local community college. Although somewhat complicated, this recruitment method allowed us to test the delivery model of the program. Due to concerns about fairness, we were required to randomize individuals into both treatment and control groups within each organization. This had the advantage of ensuring that program impacts were not driven by a particular site, given the different settings in which the program was delivered. Yet it also created the opportunity for cross-contamination among the control group, since many of these organizations are small, and individuals in the treatment and control groups could interact with one another. As such, our estimates may be biased downward, as individuals randomized into receiving no treatment may have been unintentionally exposed to treatment through peer relationships. However, we think that this is unlikely. During our focus group discussions, we did not receive any indication that information was shared across the treatment and control groups, although we did not ask about cross-contamination directly.

As part of the application process, individuals supplied information to assess their basic eligibility, which required that they be at least 18 years of age and currently working or enrolled in a workforce development program. Individuals also were required to provide a written request to perform the baseline credit check as well as for subsequent credit pulls at 6, 12, and 18 months. Of the 300 individuals eligible to participate in the study, we randomly assigned applicants to one of the following two groups:

- Treatment Group: This group of 150 individuals were assigned to receive the financial workshop and the one-on-one coaching. They were also offered the CW-3™ product if they were eligible based on the criteria discussion above.
- Control Group: This group of 150 individuals received no intervention at all.

We also stratified our random assignment by age (18-24 versus 25-29), race (African-American versus non-African-American), and gender (male versus female) to test for heterogeneity in treatment effects, which have been shown to be important (Kaiser and Menkhoff 2017). For example, consistent with human capital theory, previous studies in the literature (Taylor 2011) have reported a negative relationship between financial capability and age. However, this relationship between score and age does not hold uniformly across racial and ethnic groups. Among African-American and Hispanic adults, growing older does not make them more likely to obtain a credit score, because these groups are less likely to participate in the mainstream economy as they age (Brevoort, Grimm, and Kambara 2015). Finally, a gender gap in financial literacy has been treated as a stylized fact in the literature (Lusardi and Mitchell 2014), which may also translate into gender differences in program impacts across individuals in the treatment group.

While we chose to stratify our sample by the characteristics discussed above, the distribution of the remaining demographic factors across the treatment and control groups was left to chance, as is the case with random assignment. The treatment and control groups were roughly equivalent across almost all other observable characteristics, including ethnicity, employment tenure, marital status, household size, number of children, health insurance, homeowner status,

household income, and confidence in their ability to save \$26 per month for the CW-3™ product (see Table 1). The only significant differences at baseline were that the treatment group had a higher share of individuals who were Asian and a lower share of individuals with just “some college.” We note that having two statistically significant differences at the  $p < 0.10$  level would be expected by random chance when testing 15 different categories of characteristics. As such, given the randomization design, we do not expect these small differences to affect the program outcomes we observe across the treatment and control groups.

Despite having applied for the program, only two-thirds (67 percent) of the individuals assigned to the treatment group attended at least the workshop or one coaching session. We call these individuals “study compliers” because they complied with the requirements of the program after being assigned to treatment. In contrast, the remaining one-third of the individuals assigned to the treatment group did not participate at all, choosing not to attend the workshop nor sign up for any coaching sessions. We call these individuals “study non-compliers,” because despite being assigned to receive the program, they did not comply with the requirements and chose not to participate. This is not uncommon among randomized control treatment studies of financial coaching programs where half to two-thirds of individuals drop out even when services are offered for free (Theodos et al. 2015).

As one can imagine, it is typically lower-income and underserved populations that have “second thoughts” after applying and choose not to participate (Rothwell and Han 2010). In our study, the non-compliers were about one year younger on average and one-third less likely to have a college degree (see Table A2). In addition, non-compliers were twice as likely to have children, suggesting that perhaps scheduling constraints made it difficult to attend the workshop sessions, even though several make-up sessions were held. Despite having longer tenure with their employers, they were less likely to have employer-provided health insurance. Finally, non-compliers were more likely to have been recruited from an atypical organization and less likely to indicate that they would be able to save \$26 per month.

We should emphasize that our study non-compliers did not receive any services whatsoever—they applied and then failed to show up at the first workshop. Under the standard Working Credit model, individuals usually apply to the program after the workshop, which typically yields a participation rate of over 90 percent. Indeed, of those in the study treatment group who attended a workshop, 96 percent signed up for the one-on-one coaching, suggesting that policymakers might be able to boost take-up rates using this alternative approach. However, because of the high positive correlation between those who attended the workshop and those who received coaching, we cannot disentangle the impacts of these two program components.

## **2. Construction of Measures using Administrative Credit Report Data**

All of the measures from the administrative credit report data were collected by Working Credit from TransUnion and shared with the authors under a sub-contract data use agreement as the evaluator for the program.

## Credit Score and Rating

The credit score is as reported on the individuals' credit report using the FICO4 credit score based on reporting by TransUnion. Based on the individual's credit score, we determined their credit rating based on the following established standard ranges used by Working Credit when coaching participants:

Credit Score	Rating	Percent of People	Impact
300-600	Poor (Subprime)	17%	Credit applicants may be required to pay a fee or deposit, and applicants with this rating may not be approved for credit at all.
601-660	Fair (Nonprime)	20.2%	Applicants with scores in this range are considered to be subprime borrowers.
661-780	Good (Prime)	39.7%	Applicants with scores here are likely to receive better than average rates from lenders.
780 and above	Excellent (Superprime)	19.9%	Applicants with scores in this range are at the top of the list for the best rates from lenders.

Source: Credit Builders Alliance

## Predicting Credit Scores from "Thin" Credit Files

About one-third of the individuals in both the treatment and control groups have no credit score at baseline. However, not all individuals without a credit score are completely lacking a credit record. According to the CFPB, consumers with limited credit histories can be placed into two groups. The first group is comprised of consumers without NCRA credit records. They refer to this group as "credit invisibles." The second group includes consumers who, while they have NCRA credit records, they are considered "unscorable," meaning they have a "thin" file, mostly due to accounts that have become inactive. This latter group is the population that was studied in the Vantage report accessed by the link that you provided above. Vantage found that the unscorables "are in many ways very similar to conventional credit users" but there was also a "marked disparity in the amounts of revolving credit extended to each group, with the traditionally unscorable group receiving significantly less credit than traditionally scoreable peers."



The majority (71.2) of the individuals in our sample with no credit score are in fact invisibles, meaning they have no credit record whatsoever. This is likely because they are young adults from low income households, who have not had any experience with credit. Thus, for 74 of the 104 individuals without a credit score we have no prior data to be able to predict their score because they have no credit record on file.

Yet the remaining 30 percent or so are unscorables who do have some credit history. Rather than assign a score of zero or drop the unscorables entirely from the sample, we used a Heckman selection correction model to predict a score for those with thin credit profiles (e.g. the unscorables). To do this, we used the baseline information in our data including payment history on an old trade line, a public record, or an external collection that can be used to determine their credit worthiness using a model developed by VantageScore, one of the industry leaders in credit scoring models.

Among those individuals with a credit score before the start of the program, the average score was roughly 660, with no significant difference between the treatment and control groups. We also followed industry guidelines to predict a score for those with “thin” credit files (VantageScore, 2016) using a Heckman selection correction model, which yielded a slightly lower average score of about 650—again with no significant difference between the treatment and control groups. This method allows us to categorize most of the individuals in our sample (N=226) into the typical credit ratings prior to the start of the program, with the majority of individuals falling into the “Fair” to “Good” range.

### Factors Affecting Credit Score

Working Credit also collected measures related to the factors affecting an individual’s credit score including the number and types of open lines of credit, whether the individual had a car loan and the interest rate on that loan, and whether the individual had a student loan and the amount of student loan debt. Working Credit also reported the utilization ratio, the amount of available credit, the number of lines of credit that are currently delinquent (30 days currently past due), the number of current outstanding negatives (collections, chargeoffs, judgements), and whether the individual had a history of 30-day delinquency or a history of sustained on-time payments.

### **3. Construction of Measures using Self-Reported Survey Data**

All individuals in the treatment and control groups were asked to complete both a pre-and post-program survey that captured their current financial situation as well as their knowledge and behaviors related to credit building. Individuals were asked to complete the pre-survey when they applied for the program and were given a small monetary incentive (e.g., a \$5 gift card plus a raffle to win one of 10 iPads) to incentivize completion. The post-survey was deployed via email to both the treatment and control groups, and completion was required to receive the final installment of the \$150 financial incentive for participating in the program.

Surprisingly, individuals in the treatment group who responded to the survey exhibited characteristics that indicate they were less positively selected compared to survey responders in the control group, setting a high bar for detecting improvements over time. Treatment responders were more likely to have only a high school diploma, receive health insurance through Medicaid, and rent rather than own their home (see Table A3). Note that the direction of the bias goes against the detection of program impacts for the survey responders in the treatment versus the control groups. Nonetheless, we control for both demographic characteristics and baseline outcome measures to minimize the bias.

Questions on the pre- and post-survey regarding one's financial situation, behaviors, knowledge, and self-efficacy come from the "Keys to Your Financial Future Pre-Training Assessment" developed by the Annie E. Casey Foundation for their Opportunity Passport Program.<sup>1</sup> All summary measures that were constructed from the individual questions were converted to z-scores with a mean of zero and a standard deviation of one to be able to compare magnitudes across domains. See below for a listing of questions and responses for each component.

### Financial Situation

To get a more complete picture of their financial situation, individuals were asked about events that had happened over the past year that are not typically covered by a credit report (see Table A4). This included if they were in a credit counseling or debt management program, if a cell phone or utility company were holding a deposit, if their wages had been garnished, if their utilities been disconnected, if their car had been repossessed, if they had been evicted, if they had been foreclosed upon, if they have been contacted by collection agencies contacting about unsettled claims, and if they were in bankruptcy or in process of bankruptcy.

### Financial Behaviors

Individuals were asked to indicate how often they engaged in particular financial habits over the past three months (e.g. 0 times, 1-3 times, 4 or more times). From this set of questions, we constructed two measures of financial behaviors scaled them so that they each fell between 0 and 1 (see Table A5). We constructed a mainstream financial habits measure by summing the answers to five questions related to using direct deposit, depositing money into a savings or checking account, paying a bill using online bill pay, and using a credit card. We constructed an alternative financial habits measure by summing the answers to five questions related to borrowing money from a friend, using a payday lender, using a pawn shop, and using a check cashing service.

### Financial Knowledge

Individuals were asked to respond "true" or "false" to a series of 18 questions related to budgeting, saving, borrowing, and use of credit—including what is reported on a credit report

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<sup>1</sup> See <http://www.aecf.org/work/child-welfare/jim-casey-youth-opportunities-initiative/the-keys-to-your-financial-future-curriculum/> for more information.

and how that information is used.<sup>2</sup> From this set of questions we constructed a measure for each individual equal to the percent right as well as a dummy variable indicating whether they achieved a score of at least 75 percent (see Table A6).

### Financial Self-Efficacy

Individuals were asked to rate a series of questions related to their confidence and concerns using a Likert scale (1=Strongly Disagree, 2=Disagree, 3= Agree, and 4= Strongly Agree).<sup>3</sup> From this set of questions we constructed several measures of financial capability and scaled them so that they each fell between 0 and 1 (see Table A7).

First, we created a confidence in financial knowledge score by summing the answers to four questions related to understanding how to build assets, use credit, read a credit report, and make a budget, and then divided by the total number of possible points (16).

Second, we created a confidence in financial skills score by summing the answers to four questions related to feeling confident about managing finances, feeling comfortable making financial decisions, feeling they have all the skills to plan for their financial future, and feeling that they have the skills needed to succeed, and then divided by the total number of possible points (16).

Third, we created a concern about financial situation score by summing the answers to three questions related to concern over student debt, concern over meeting expenses, and being satisfied with their saving, and then divided by the total number of possible points (12).

Finally, we created an overall self-efficacy score that can be thought of as a summary across the first three domains. Although there are several widely accepted psychological measures of general self-efficacy, no reliable and valid measure specific to financial behavior exists (Dietz, Carrozza, & Ritchey, 2003). We follow Lown (2011) and measure self-efficacy using a combination of the statements discussed above that measure an individual's confidence in their ability and knowledge to manage their finances as well as their satisfaction with their ability to save. Specifically, our self-efficacy measure is constructed by summing the answers to five questions related to feeling confident about managing finances, feeling they have the skills to succeed, feeling they have the resources to plan for the future, being satisfied with their saving, and knowing where to get help. We then divided by the total number of possible points (20).

### **Focus Group Analysis**

We held two sets of focus groups, at the beginning and at the end of the program, separately for individuals in the treatment and the control group. The first set of focus groups were held in May 2016, shortly after the treatment group had participated in the workshop and the initial one-on-one coaching provided by Working Credit. The goal was to get an early assessment of how the program was going, as well as to uncover additional insights about take-up among the treatment group. In addition, we wanted to learn more about the barriers faced by individuals in both the

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<sup>2</sup> See Part C of the survey entitled "Tell us About Your Views on Money and Finances."

<sup>3</sup> See Part D of the survey entitled "Tell Us About Your Concerns."

treatment and control groups when it came to accessing and building credit. The second set of focus groups was held in May 2017, just after the program had ended, with the aim of developing a better understanding of the program’s overall impacts and mechanisms.

Each group was composed of five to seven young adults selected at random from the treatment and the control groups. Individuals were offered a modest financial incentive (a \$50 gift card) to encourage participation and compensate individuals for their time. Focus group members were fairly representative of the full cohort in terms of observable characteristics such as age, gender, race, and type of organization from which they were recruited. Comparing their credit histories and baseline survey responses, there was no evidence that focus group members had more difficult or extreme financial circumstances compared to the full study sample. If anything, focus group members were slightly more highly educated and slightly less likely to be experiencing problems with credit. We attempted to recruit the same individuals for both sets of focus groups, but only half were able to attend both sessions, so we recruited additional members with similar demographic characteristics as replacements.

All focus group sessions were transcribed and coded using NVivo software. Using standard methods, we initially coded the responses into major categories of information using open coding. These included obvious categories such as: Financial situation, Credit mistakes, Feelings about using credit, Strategies for using credit, Skills needed, Lack of knowledge, Impact on future plans.

From this initial open coding, we identified several open coding categories to focus on (e.g., “core” phenomenon) and then went back to the data and created categories around these core phenomena consisting of causal conditions (what factors caused the core phenomenon), strategies (actions taken in response to the core phenomenon), contextual and intervening conditions (broad and specific situational factors that influence the strategies), and consequences (outcomes from using the strategies). These categories were further refined and expanded in an iterative process as we conducted additional interviews to arrive at a final coding structure (see Table A8).

## 5. Analysis Methods

### ITT and TOT Estimates

Because participation is randomly assigned, we obtain causal estimates using a simple comparison of means on the outcome of interest. This Intent to Treat (ITT) estimate measures the impact of offering the program on the outcome. In many cases, this is the policy-relevant estimate because program administrators often want to account for program take-up in assessing the degree to which financial coaching could improve outcomes among the pool applicants, not just those who choose to participate (e.g., study compliers). Note that although covariates are not necessary to derive unbiased impact estimates when treatment is randomly assigned (Bloom, 2006), we also use a regression framework to include baseline characteristics, including pre-program measures of outcomes, to improve the precision of our estimates (see Table A9):

$$Y_{it} = \alpha_1 + \pi_1 TREAT_i + \beta_1 Y_{i0} + \gamma_1 X_{i0} + \mu_{itl} \quad (1)$$

where  $Y_{it}$  is the post-program outcome for individual  $i$  during post-randomization period  $t$ ,  $TREAT_i$  is a dummy variable indicating the individual received an offer to participate,  $Y_{i0}$  is the pre-program measure of the same outcome,  $X_{i0}$  is a set of pre-existing baseline characteristics collected when the individual applied to the program, and  $\mu_{it}$  is a stochastic error term.

Nonetheless, because not all individuals accept the offer, the ITT estimate will understate the effects of the program for those youth who choose to participate. As such, we also provide Treatment-on-the-Treated (TOT) estimates, which assess the program's impact, independent of the take-up rate. Under the usual relevance and exogeneity assumptions for instrumental variables, this latter set of effects can be recovered from the experimental data. We perform this estimation through a two-stage least squares strategy, in which random assignment ( $TREAT_i$ ) is an instrument for actual participation ( $P_{it}$ ), and  $P'_{it}$  is the predicted probability of participation from equation (2):

$$P_{it} = \alpha_2 + \pi_2 TREAT_i + \beta_2 Y_{i0} + \gamma_2 X_{i0} + \mu_{it2} \quad (2)$$

$$Y_{it} = \alpha_3 + \pi_3 P'_{it} + \beta_3 Y_{i0} + \gamma_3 X_{i0} + \mu_{it3} \quad (3)$$

If all individuals respond the same way to the program (i.e., if treatment effects are constant across youth), then equations (2) and (3) also yield an estimate of the average treatment effect (ATE) across this population of low-income young adults. Given that treatment effects are likely to be heterogeneous across young adults, then the coefficient  $\pi_3$  estimates the local average treatment effect (LATE)—the effect of participation on those who comply with random assignment. As long as there is no control crossover (no always-takers) in this setting,  $\pi_3$  provides an estimate of the treatment-on-the-treated.

### Mediation Analysis

Finally, we explore the program's mechanisms by conducting a mediation analysis that relates individual outcomes such as the credit score and the use of alternative financial services to our two components of financial literacy: financial knowledge and self-efficacy. In the case of assessing the effectiveness of the BYCBI program, we theorize that there could be both a direct and indirect effect. The direct effect of the program could arise from the compliers simply following the advice of the coach, which would directly improve their credit score. However, the literature also suggests two potential indirect effects that could also be at work. The first is that attending the financial workshop increases financial knowledge, which would help individuals in the treatment make better financial decisions and improve their credit scores. The second is that the financial coaching increases self-efficacy to address one's own financial situation, which would also help individuals make better financial decisions and improve their credit scores.

To conduct the mediation analysis, we draw on the model developed in Preacher and Hayes (2008) and further described in Zhao, Lynch, and Chen (2010). Using this model, the only requirement to establish mediation is that the indirect effect ( $a \times b$ ) in the following equation is significant:

$$c' = (a \times b) + c \quad (4)$$

where  $c'$  = total effect,  $(a \times b)$  = indirect path, and  $c$  = direct path.

Although it is not necessary for there to be a statistically significant direct effect,  $c$ , to be mediated, the presence of the direct effect  $c$  can inform theorizing about other mediators. For example, if the direct path  $c$  and the indirect path  $(a \times b)$  are both significant and of the same sign, the  $c'$  will also have the same sign, reflecting complementary mediation. As discussed earlier, we would expect to observe complementary mediation for both the indirect effects of financial knowledge and self-efficacy on credit score if both the indirect path  $(a \times b)$  and the direct path  $c$  are significant (Zhao, Lynch, and Chen 2010). Similarly, because both the direct and indirect effects on the use of alternative financial services is likely to be negative, we would again expect to see complementary mediation for that outcome as well.

To assess the indirect effects using this framework, we use the following system of Structural Equation Models (SEM) to estimate both the direct and indirect effect parameters simultaneously:

$$M_{it} = \alpha_4 + \pi_4 TREAT_i + \eta_4 M_{i0} + \gamma_4 X_{i0} + \mu_{it4} \quad (5)$$

$$Y_{it} = \alpha_5 + \pi_5 TREAT_i + \eta_5 M_{it} + \beta_5 Y_{i0} + \gamma_5 X_{i0} + \mu_{it5} \quad (6)$$

where  $M_{it}$  = mediating variable (e.g., financial knowledge or financial self-efficacy). We then perform a bootstrap test of the indirect effects, as described in Preacher and Hayes (2008).

Studying these indirect channels is important in understanding the mechanisms driving the observed improvements in credit scores among the treatment group. If it's the case that the program improves either financial knowledge or self-efficacy or both, then this might explain why we continue to see improvements in credit scores even after the program ends at 12 months. Note that this part of the evaluation is more exploratory in nature—because although the treatment and control groups were randomly selected, those who chose to respond to the post-survey were not, even when offered a financial incentive of \$150 to participate. However, we feel that this analysis is still informative, if only suggestive, as to how the program achieves better credit outcomes for those who participate.

In addition, we can also contrast these two indirect effects to test whether they are equal in size as measured by the degree to which each accounts for the effect of X on Y. Of course, contrasts represent comparisons of indirect effects only insofar as the mediators are themselves uncorrelated. We measure improvements in both financial knowledge and self-efficacy by comparing the pre- and post-survey responses using separately scored scales composed of multiple and dissimilar items. This has the advantage of ensuring that our measurement of these mediators is distinct from the dependent variable (e.g., credit score) as well as each other. However, it might still be the case that financial knowledge and self-efficacy are positively correlated to some degree.

## 6. Robustness checks

### Individuals with a Credit File versus Individuals with a Credit Score at Baseline

We show the impact of the program on credit scores for both individuals initially with credit files (e.g., including predicted scores for unscorables N=226) as well as individuals initially with credit scores (e.g., excluding anyone with a zero score at baseline N=196) in Figure 3. In terms of boosting credit scores among those who initially had a credit file at baseline, the treatment group showed significant improvements relative to the control group—again largely driven by the compliers. Figure 3 shows a simple comparison of mean credit scores at six-month intervals for the control group versus the treatment group, as well as a separate line for treatment compliers. Panel A shows that for the sample of individuals with a credit file (N=226), the mean credit score for the treatment group increased significantly by 18 points during the first six months of the program, largely led by a rapid improvement of 22 points among the treatment compliers. The compliers continued to show steady improvements through the six months after the program ended, resulting in a mean score of 687 at the 18-month mark. These gains were large enough to significantly increase the mean score of the entire treatment group by 20 points relative to the control group, demonstrating that the program effects are large enough to show improvements among the population of young low-income adults whom the city intended to treat. Based on the trajectory of score improvements over time, it appears that the impact of the program is greatest during the first six months, when participants receive the information from the workshop as well as their first coaching session to establish an individualized plan. When we limit the sample to just those individuals who initially had a credit score—not just a credit file—before the start of the program, the gains are much smaller, and relative improvements are detected only among those who complied with the program (see Panel B of Figure 3). By the 18-month mark, six months after the program’s end, the mean credit score of compliers in the treatment group who initially had a score outpaced the control group by 25 points.”

We then provide regression estimates of these effects at the 18-month mark. Credit scores were 26.4 points higher among the treatment group versus the control group and 37.6 points higher among the compliers versus the control group (see Panel B of Table 3). We find similar but smaller impacts among the compliers when we limit the sample to those who initially had a credit score at baseline (see Table A10).

### Assessing the Take-up and Impact of the CW-3™ Product

How many of the individuals made use of the CW-3™ secure loan product to help them build credit? Recall that the product was only offered to compliers in the treatment group who met the following criteria: (1) had fewer than three open revolving accounts or no installment account, (2) could afford to save \$26 per month, and (3) were not currently past due on any account. Of the 101 program compliers, roughly half (53) were offered the program, of whom 60 percent decided to take it up. Those who were offered the CW-3™ product were more likely to be from a typical organization, have a high school degree, be a Medicaid recipient, and not have children (see Table A11). Compared to those who were offered the CW-3™ product, those who chose to make use of it were more likely to be Hispanic and to have children. Females, those with a

bachelor's degree, and individuals who did not own a home were less likely to take up the product when it was offered to them. Individuals in the latter two groups may not have wanted to take out another loan using the CW-3™ because they already had student loans or were planning to apply for a mortgage—both of which might make one reluctant to take on more debt. However, the gender difference is notable and might reflect the emergence of a gap in credit building among males versus females.

How much of the program's impact can be attributed to the use of the CW-3™ product versus just the workshop and financial counseling? Interestingly, Table 6 shows that among the compliers, just being offered the CW-3™ product can account for virtually all the gains in terms of access to credit as well as improving credit scores. This suggests that the product was well-targeted toward those who would benefit from it the most: largely, invisibles with no credit file and unscorables with thin credit files. However, because the use of the product was not randomly assigned, nor entirely at the discretion of the individual, we cannot distinguish the impact of the product from the characteristics of those to whom it was offered.



**Table A1. Recruitment from Organizations: Number of Applicants**

	Age of Population	Employment/ Program Duration	Regular / Strong Contact?	Number of Applicants	
				Original	Share of Total
	(1)	(2)	(3)	(4)	(5)
<b>Typical Organizations</b>					
BEST Corp Hospitality Training Center	21-28	Year round	Yes	10	3.1%
Boston Housing Authority	26-27	Year round	Yes	5	1.6%
BPHC	23-29	Year round	Yes	6	1.9%
Catholic Charities	24-27	Year round	Yes	5	1.6%
OFE Boston	21-29	Year round	Yes	15	4.7%
ROCA	23-30	Year round	Yes	14	4.4%
YearUp	19-27	Year round	Yes	59	18.6%
<b>Near-Typical Organizations</b>					
Boston Day & Evening Academy	24-27	School year	Yes	2	0.6%
CityYear	19-27	6 months	Yes	18	5.7%
LISC Americorps	23-29	6 months	Yes	6	1.9%
Hyde Park YCD	20-26	6 months	Yes	3	0.9%
Madison Park Housing Development	18-24	School year	Yes	20	6.3%
<b>Not-Typical Organizations</b>					
Boston Cares	22-27	No formal program	No	3	0.9%
Roxbury Community College	18-29	School year	No	60	18.9%
Roxbury YouthWorks	25-28	Year round	No	2	0.6%
Youth Employment & Engagement	19-29	6 months	No	29	9.1%
<b>TOTAL</b>					
Total Number of Applicants				315	100%
Eligible Organizations				114	36%
Near-Eligible Organizations				49	16%
Not-Eligible Organizations				152	48%

*Note:* Number of applicants = applicants recruited prior to random assignment. Applicants as share of total = Applicants (Treatments + Controls) for a given organization / Total Applicants across all organizations.

*Source:* Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A2. Baseline Demographic Characteristics: Compliers v Non-Compliers in the Treatment Group**

	Treatment Group				Difference (3)
	Compliers		Non-Compliers		
	(1)	(2)	(1)	(2)	
<b>Type of Organization</b>					
Typical	41.6%	(0.049)	28.6%	(0.065)	13.0
Near-Typical	16.8%	(0.037)	12.2%	(0.047)	4.6
Atypical	41.6%	(0.049)	59.2%	(0.071)	-17.6 **
<b>Age</b>					
Mean	24.02	(0.307)	22.86	(0.424)	1.2 **
18-24	55.4%	(0.050)	71.4%	(0.065)	-16.0 *
25-30	44.6%	(0.050)	28.6%	(0.065)	16.0 *
<b>Gender</b>					
Female	59.4%	(0.049)	57.1%	(0.071)	2.3
<b>Race</b>					
African American/Black	46.5%	(0.050)	44.9%	(0.072)	1.6
American Indian / Native Alaskan	2.0%	(0.014)	0.0%	0.000	2.0
Asian/Hawaiin/Pacific Islander	11.9%	(0.032)	16.3%	(0.053)	-4.4
Caucasian / White	8.8%	(0.039)	10.2%	(0.044)	-1.4
Two or more races	10.9%	(0.031)	10.2%	(0.044)	0.7
Other	20.8%	(0.041)	18.4%	(0.060)	2.3
<b>Ethnicity</b>					
Hispanic	21.8%	(0.041)	30.6%	(0.067)	-8.8
<b>Veteran status</b>					
Veteran	0.0%	(0.000)	0.0%	(0.000)	0.0
<b>Marital status</b>					
Married	5.0%	(0.022)	0.0%	(0.000)	5.0
<b>Household size</b>					
Number	2.97	(0.151)	2.82	(0.162)	0.2
<b>Number of children</b>					
Has any children	12.9%	(0.033)	26.5%	(0.064)	-13.7 **
<b>Education</b>					
Less than a high school diploma	5.9%	(0.024)	12.2%	(0.047)	-6.3
High school diploma or GED	23.8%	(0.043)	36.7%	(0.070)	-13.0
Some college	20.8%	(0.041)	26.5%	(0.064)	-5.7
Associate's degree	3.0%	(0.017)	4.1%	(0.029)	-1.1
Bachelor's degree	38.6%	(0.049)	12.2%	(0.047)	26.4 ***
Advanced or professional degree	6.9%	(0.025)	0.0%	0.000	6.9
Not reported	0.0%	0.000	8.2%	(0.040)	-8.2
<b>Employment tenure</b>					
Less than one year	70.3%	(0.046)	53.1%	(0.072)	17.2 **
One to two years	16.8%	(0.037)	16.3%	(0.053)	0.5
Two to five years	9.9%	(0.030)	18.4%	(0.056)	-8.5
More than five years	1.0%	(0.010)	4.1%	(0.029)	-3.1
Not reported	2.0%	(0.014)	8.2%	(0.040)	-6.2 *
<b>Health insurance</b>					
Private plan, through employer	36.6%	(0.048)	14.3%	(0.051)	22.3 **
Medicaid (MassHealth)	41.6%	(0.049)	49.0%	(0.072)	-7.4
Other	15.8%	(0.037)	26.5%	(0.064)	-10.7
None	3.0%	(0.017)	8.2%	(0.040)	-5.2
Not reported	3.0%	(0.017)	2.0%	(0.020)	0.9
<b>Homeowner status</b>					
Own	6.9%	(0.025)	4.1%	(0.029)	2.8
<b>Household income</b>					
Above \$71,991	10.9%	(0.031)	8.2%	(0.040)	2.7
<b>Can save \$26 per month</b>					
Yes	97.0%	(0.017)	89.8%	(0.044)	7.2 *
<b>Number of observations</b>	101		49		52

Note: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Non-compliers have completed neither. \*\*\*Significance at the 1% level. \*\*Significance at the 5% level. \*Significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A3. Baseline Demographic Characteristics: Treatment v Control Group Survey Responders**

	Treatment Group		Control Group		Difference
	(1)		(2)		(3)
<b>Type of Organization</b>					
Typical	39.6%	(0.050)	35.7%	(0.049)	3.9
Near-Typical	15.6%	(0.037)	17.3%	(0.038)	-1.7
Atypical	44.8%	(0.051)	46.9%	(0.051)	-2.1
<b>Age</b>					
Mean	24.29	(0.319)	24.31	(0.260)	0.0
18-24	53.1%	(0.051)	51.0%	(0.051)	2.1
25-30	46.9%	(0.051)	49.0%	(0.051)	-2.1
<b>Gender</b>					
Female	64.6%	(0.049)	71.4%	(0.046)	-6.8
<b>Race</b>					
African American/Black	43.8%	(0.051)	45.9%	(0.051)	-2.2
American Indian / Native Alaskan	2.1%	(0.015)	1.0%	(0.010)	1.1
Asian/Hawaiin/Pacific Islander	9.6%	(0.022)	5.1%	(0.064)	4.5 *
Caucasian / White	21.9%	(0.042)	27.6%	(0.045)	-5.7
Two or more races	10.9%	(0.043)	13.4%	(0.017)	-2.5
Other	12.5%	(0.034)	7.1%	(0.026)	5.4
<b>Ethnicity</b>					
Hispanic	25.0%	(0.044)	27.6%	(0.045)	-2.6
<b>Veteran status</b>					
Veteran	0.0%	(0.000)	2.0%	(0.014)	-2.0
<b>Marital status</b>					
Married	5.2%	(0.023)	6.1%	(0.024)	-0.9
<b>Household size</b>					
Number	2.93	(0.137)	2.90	(0.155)	0.0
<b>Children</b>					
Has any children	14.3%	(0.036)	14.6%	(0.036)	-0.3
<b>Education</b>					
Less than a high school diploma	1.0%	(0.010)	6.1%	(0.024)	-5.1
High school diploma or GED	27.1%	(0.046)	13.3%	(0.034)	13.8 **
Some college	19.8%	(0.041)	36.7%	(0.049)	-16.9 **
Associate's degree	3.1%	(0.018)	1.0%	(0.010)	2.1
Bachelor's degree	41.7%	(0.051)	32.7%	(0.048)	9.0
Advanced or professional degree	6.3%	(0.025)	8.2%	(0.028)	-1.9
Not reported	0.0%	(0.000)	0.0%	(0.000)	0.0
<b>Employment tenure</b>					
Less than one year	67.7%	(0.048)	58.2%	(0.050)	9.5
One to two years	15.6%	(0.037)	17.3%	(0.038)	-1.7
Two to five years	13.5%	(0.035)	15.3%	(0.037)	-1.8
More than five years	1.0%	(0.010)	4.1%	(0.020)	-3.0
Not reported	2.1%	(0.015)	5.1%	(0.022)	-3.0
<b>Health insurance</b>					
Private plan, through employer	37.5%	(0.050)	38.8%	(0.049)	-1.3
Medicaid (MassHealth)	40.6%	(0.050)	27.6%	(0.045)	13.1 *
Other	15.6%	(0.037)	25.8%	(0.045)	-10.2 *
None	3.1%	(0.018)	2.0%	(0.014)	1.1
Not reported	3.1%	(0.018)	4.1%	(0.020)	-1.0
<b>Homeowner status</b>					
Own	3.1%	(0.018)	9.2%	(0.029)	-6.1 *
<b>Household income</b>					
Above \$71,991	11.5%	(0.033)	12.2%	(0.033)	-0.8
<b>Can save \$26 per month</b>					
Yes	96.9%	(0.018)	98.0%	(0.014)	-1.1
<b>Number of observations</b>					
	96		98		-2

Note: \*\*\*Significance at the 1% level. \*\*Significance at the 5% level. \*Significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A4. Financial Situation: Comparison of Treatment and Control Groups at 12 Months**

	Control Group	Treatment Group			Differences (Percentage Point)	
		All	Study Compliers	Study Non-compliers	All - Control	Study Compliers - Study Non Compliers
<b>Number</b>	150	150	101	49		
<b><u>Percent Responding Yes in Each Group</u></b>						
Have a checking account	84.7%	87.3%	93.1%	75.5%	2.7	17.6 **
Have a savings account	73.2%	63.5%	67.7%	55.1%	-9.6 *	12.6 *
Set aside money regularly for saving	49.3%	43.3%	43.6%	42.9%	-6.0	0.7
Participate in employer 401K or 403B	20.0%	21.3%	19.8%	24.5%	1.3	-4.7
Collection agencies contacting about unsettled claims	20.1%	19.3%	22.8%	12.2%	-0.8	10.5 *
Cell phone company holding a deposit	12.0%	12.0%	10.9%	14.3%	0.0	-3.4
Utilities been disconnected in past year or in danger of disconnection	10.7%	8.0%	9.9%	4.1%	-2.7	5.8
Wages been garnished in past year	7.3%	8.7%	8.9%	8.2%	1.3	0.7
Utility company holding a deposit	5.3%	6.0%	5.0%	8.2%	0.7	-3.2
In credit counseling or debt management plan or working with one	4.0%	3.3%	3.0%	4.1%	-0.7	-1.1
Been evicted in past year or in process of eviction	4.0%	1.3%	0.0%	4.1%	-2.7	-4.1
Car been repossessed in past year or in danger of repossession	3.3%	1.3%	1.0%	2.0%	-2.0	-1.1
In bankruptcy or in process of bankruptcy	2.0%	0.0%	0.0%	0.0%	-2.0	0.0
Foreclosure started or in danger of foreclosure	0.7%	0.7%	0.0%	2.0%	0.0	-2.0
Filed a tax return last year	80.7%	66.0%	69.3%	59.2%	-14.7 **	10.1 *
Have one or more credit cards	49.3%	46.0%	50.5%	36.7%	-3.3	13.8 *
If you don't have a credit card, ever had one	24.8%	21.3%	18.8%	26.5%	-3.5	-7.7 *
Will be applying for a mortgage or car loan in next three months	8.7%	8.0%	7.9%	8.2%	-0.7	-0.2

Notes: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Non-compliers have completed neither. \*\*Indicates significance at the 5% level and \*indicates significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A5. Financial Behaviors: Comparison of Treatment and Control Groups at 12 Months**

	Control Group	Treatment Group			Difference (Percentage Point)	
		All	Study Compliers	Study Non-compliers	All - Control	Study Compliers - Study Non Compliers
<b>Number</b>	150	150	101	49		
<b><u>Percent Responding More than Four Times</u></b>						
<b><u>Mainstream Financial Services</u></b>						
Used direct deposit.	62.7%	54.0%	57.4%	46.9%	-8.7	10.5 *
Deposited money into a savings or checking account.	58.0%	59.3%	57.4%	63.3%	1.3	-5.8
Paid a bill using online bill pay.	40.7%	38.9%	39.0%	38.8%	-1.7	0.2
Used a credit card.	32.7%	34.7%	40.6%	22.5%	2.0	18.1 *
<b><u>Alternative Financial Services</u></b>						
Used a payday lender.	0.7%	0.7%	0.0%	2.0%	0.0	-2.0
Used a pawn shop.	1.3%	1.3%	1.0%	2.0%	0.0	-1.1
Borrowed money from a friend.	3.3%	4.0%	3.0%	6.3%	0.7	-3.3
Used a check cashing service.	13.3%	13.3%	10.9%	18.4%	0.0	-7.5 *

Notes: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Non-compliers have completed neither. \*\*Indicates significance at the 5% level and \*indicates significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A6. Financial Knowledge: Comparison of Treatment and Control Groups at 12 Months**

	ANSWER KEY	Control Group	Treatment Group			Difference (Percentage Points)	
			All	Study Compliers	Study Non-compliers	All - Control	Study Compliers- Study Non Compliers
<b>Number</b>		150	150	101	49		
<b>Percent Responding True in Each Group</b>							
Vision and goals have nothing to do with managing your money.	FALSE	8.7%	12.7%	6.9%	24.5%	4.0	-17.6 **
Contingency planning is thinking about what could go wrong and making alternative plans.	TRUE	89.3%	82.7%	86.1%	75.5%	-6.6	10.6
An asset is something you own that always increases in value.	FALSE	65.1%	61.3%	63.4%	57.1%	-3.8	6.2
Saving is setting aside money now for use at some future time.	TRUE	96.0%	95.3%	99.0%	87.8%	-0.7	11.3 **
Having positive credit reports, high credit scores and affordable credit are productive assets	TRUE	96.0%	90.7%	92.1%	87.8%	-5.3	4.3
A credit report is a document that contains only some of your bill paying history.	TRUE	43.3%	41.3%	40.6%	42.9%	-2.0	-2.3
You have the right to get your credit reports from each of the credit reporting agencies each year.	TRUE	92.7%	88.0%	90.1%	83.7%	-4.7	6.4
Credit reports are completely accurate; you never need to check for mistakes.	FALSE	6.0%	14.7%	10.9%	22.5%	8.7 **	-11.6 *
A poor credit history can prevent you from getting insurance coverage, an apartment, or a job.	TRUE	83.3%	86.0%	88.1%	81.6%	2.7	6.5
If you are under 18 and have a credit report, you may have been the victim of identity theft.	TRUE	61.3%	64.0%	61.4%	69.4%	2.7	-8.0
Credit is money you owe.	FALSE	48.7%	46.7%	43.6%	53.1%	-2.0	-9.5
When you use credit, you are obligating future income.	TRUE	68.0%	64.0%	67.3%	57.1%	-4.0	10.2
Your credit score is calculated from your income, your assets, your age, and where you live.	FALSE	33.3%	32.0%	28.7%	38.8%	-1.3	-10.1
There is nothing you can do to change your credit score.	FALSE	3.3%	8.7%	6.9%	12.2%	5.3	-5.3
Using direct deposit for your paycheck can save you money and time.	TRUE	91.3%	88.7%	92.1%	81.6%	-2.7	10.5 *
A bank or credit union with FDIC or NCUA insurance means the money in your account is insured.	TRUE	69.3%	72.0%	77.2%	61.2%	2.7	16.0 **
If you bounce checks, you could be listed in a database that may keep you from opening accounts.	TRUE	74.7%	67.3%	69.3%	63.3%	-7.3	6.0
The best ways to find money to save in your budget is to cut spending or increase income.	TRUE	90.0%	90.0%	92.1%	85.7%	0.0	6.4

Notes: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Non-compliers have completed neither. \*\*Indicates significance at the 5% level and \*indicates significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A7. Financial Self-Efficacy: Comparison of Treatment and Control Groups at 12 Months**

	Control Group	Treatment Group		Difference	
		All	Compliers	Treatment-Control	Compliers-Control
Number responding to the post-survey	98	96	85		
<b><u>Percent Responding Agree or Strongly Agree in Each Group</u></b>					
<b><u>Confidence in skills and resources</u></b>					
I feel confident about managing my money and personal finances.	65.3%	83.3%	83.5%	18.0 ***	18.2 ***
I am comfortable making financial decisions.	67.3%	85.4%	85.9%	18.1 ***	18.5 ***
I have the skills to plan for my financial future.	56.1%	85.4%	87.1%	29.3 ***	30.9 ***
I feel I have all the resources I need to succeed with my goals.	39.8%	59.4%	61.2%	19.6 ***	21.4 ***
<b><u>Confidence in level of understanding</u></b>					
I know how to build assets.	31.6%	54.2%	54.1%	22.5 ***	22.5 ***
I understand how credit works.	58.2%	87.5%	89.4%	29.3 ***	31.2 ***
I can read a credit report.	50.0%	85.4%	87.1%	35.4 ***	37.1 ***
I know how to make a budget.	71.4%	83.3%	85.9%	11.9 **	14.5 **
I know where to get help with money matters.	44.9%	74.0%	75.3%	29.1 ***	30.4 ***
<b><u>Concerns regarding financial situation</u></b>					
I worry about being able to pay monthly living expenses once I am on my own.	46.9%	42.7%	41.2%	-4.2	-5.8
I feel concern about how much money I will owe after college.	55.1%	49.0%	47.1%	-6.1	-8.0
I am satisfied with the amount of money I am able to save.	31.6%	39.6%	40.0%	8.0	8.4
Average: Confidence measures	2.3	3.1	3.2	0.8 ***	0.9 ***
Average: Knowledge measures	2.6	3.8	3.9	1.3 ***	1.4 ***
Average: Concern measures	1.3	1.3	1.3	0.0	-0.1
Measure of Self-Efficacy	0.48	0.68	0.69	0.2 ***	0.2 ***

Notes: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Non-compliers have completed neither. \*\*\*Indicates significance at the 1% level, \*\*indicates significance at the 5% level, and \*indicates significance at the 10% level.

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.

**Table A8. Focus Group Nodes Clustered by Word Similarity**

	Dealing with credit/has strategy for dealing with credit	Dealing with credit/demonstrates understanding of credit or financial planning	Dealing with credit/overwhelmed by paying off debt	Dealing with credit/try to only use credit when I have the money to pay it off	Feelings about finances and credit/feel hopeless	Feelings about finances and credit/I am good at managing my cash and credit	Feelings about finances and credit/worried about new debt commitments due to past experience	Need additional skills/don't know how to plan	Need additional skills/don't trust myself with credit card	Need additional skills/have credit but still do not understand it	Need additional skills/need additional skills to manage money or credit	Need additional skills/need additional skills to set/fin financial goals	Need additional skills/never learned about credit before	Need additional skills/never received	Need additional skills/not able to budget	Need additional skills/worried about developing or continuing bad habits	
Dealing with credit/has strategy for dealing with credit	1.00																
Dealing with credit/demonstrates understanding of credit or financial planning	0.75	1.00															
Dealing with credit/overwhelmed by paying off debt	0.46	0.38	1.00														
Dealing with credit/try to only use credit when I have the money to pay it off	0.43	0.44	0.29	1.00													
Feelings about finances and credit/feel hopeless	0.43	0.27	0.26	0.23	1.00												
Feelings about finances and credit/I am good at managing my cash and credit	0.38	0.38	0.21	0.23	0.18	1.00											
Feelings about finances and credit/worried about new debt commitments due to past experience	0.64	0.33	0.29	0.32	0.39	0.30	1.00										
Need additional skills/don't know how to plan	0.38	0.24	0.24	0.16	0.16	0.12	0.27	0.25	1.00								
Need additional skills/don't trust myself with credit card	0.58	0.37	0.28	0.38	0.45	0.23	0.47	0.33	0.31	1.00							
Need additional skills/have credit but still do not understand it	0.38	0.25	0.25	0.30	0.41	0.20	0.31	0.24	0.22	0.41	1.00						
Need additional skills/need additional skills to manage money or credit	0.41	0.32	0.39	0.23	0.23	0.25	0.24	0.75	0.27	0.26	0.30	1.00					
Need additional skills/need additional skills to set/fin financial goals	0.45	0.34	0.38	0.27	0.32	0.30	0.30	0.75	0.23	0.34	0.30	0.95	1.00				
Need additional skills/never learned about credit before	0.41	0.34	0.42	0.30	0.32	0.28	0.27	0.75	0.26	0.27	0.32	0.74	0.75	1.00			
Need additional skills/never received	0.56	0.42	0.43	0.30	0.42	0.29	0.38	0.75	0.39	0.44	0.42	0.75	0.75	0.92	1.00		
Need additional skills/not able to budget	0.40	0.24	0.35	0.34	0.46	0.23	0.34	0.19	0.21	0.33	0.34	0.22	0.27	0.29	0.37	1.00	
Need additional skills/worried about developing or continuing bad habits	0.39	0.34	0.35	0.31	0.29	0.26	0.25	0.87	0.25	0.30	0.21	0.75	0.75	0.70	0.72	0.14	1.00

Source: Authors' calculations based on data supplied by the Boston Mayor's Office of Financial Empowerment.



**Table A9. Regression Estimates of BYCBI Impact on Credit Score: All Individuals with a Credit File - 18 Months**

	ITT	TOT
	(1)	(2)
Treatment dummy	26.405 ** (12.380)	37.6 *** (13.191)
Credit score at baseline	0.889 *** (0.045)	0.802 *** (0.093)
Age	-2.412 ** (1.220)	-2.485 (1.997)
Male	-11.189 (15.371)	-10.755 (14.328)
Black	-12.364 (23.590)	-11.387 (16.863)
Hispanic	-8.357 (28.629)	-9.692 (24.822)
Married	27.897 * (16.604)	25.227 (27.759)
Children	-7.239 (20.636)	-6.023 (18.128)
Household size	-7.313 (20.636)	-7.609 (4.735)
High school degree	-36.584 (24.946)	-40.403 (27.743)
Some college	-1.383 (17.403)	-7.815 (26.597)
Associate's degree	15.735 (29.397)	13.080 (42.202)
Bachelor's degree	29.961 (19.279)	23.655 (29.937)
Advanced degree	41.391 * (21.845)	33.052 (37.040)
Tenure with employer less than one year	0.604 (12.434)	-0.856 (13.402)
Employer provided healthcare	-7.546 (12.578)	-8.784 (16.336)
Houshold income above median (\$71,992)	4.247 (13.791)	6.042 (20.888)
Own home	16.397 (21.304)	15.829 (42.309)
Able to save \$26	0.055 (22.953)	-3.024 (34.459)
Recruited from "atypical" organization	1.442 (11.208)	2.963 (13.528)
Constant	181.969 (62.857)	206.723 (90.731)
Number of observations	226	226
R-squared	0.428	0.428

Note: Standard errors in parentheses.

Source: Authors' calculations based on data provided by Working Credit.

**Table A10. Regression Estimates of BYCBI Impact on Credit Scores and Ratings: ITT and TOT Estimates - 18 Months**

	All individuals with credit file		All individuals with credit score	
	(1)	(2)	(3)	(4)
<b><u>Treatment versus Control Group: ITT Estimates</u></b>				
Mean credit score	24.226 (13.022)	* 26.405 (12.380)	** 6.158 (9.002)	8.843 (8.415)
<b><u>Credit rating:</u></b>				
Poor: credit score >=300 and <=600	-0.021 (0.047)	-0.029 (0.043)	-0.024 (0.047)	-0.030 (0.044)
Fair: credit score >=601 and <=660	0.020 (0.055)	0.032 (0.052)	0.057 (0.043)	0.062 (0.039)
Good: credit score >=661 and <=780	0.078 (0.045)	* 0.081 (0.042)	* 0.011 (0.056)	0.030 (0.053)
Excellent: credit score >=780	-0.014 (0.028)	-0.031 (0.033)	-0.034 (0.033)	-0.031 (0.033)
<b><u>Compliers versus Control Group: TOT Estimates</u></b>				
Mean credit score	39.343 (13.702)	** 37.554 (13.191)	** 24.267 -8.758	** 20.252 -9.388
<b><u>Credit rating:</u></b>				
Poor: credit score >=300 and <=600	-0.087 (0.053)	* -0.090 (0.050)	* -0.062 (0.050)	-0.055 (0.053)
Fair: credit score >=601 and <=660	-0.001 (0.050)	-0.028 (0.048)	-0.020 (0.045)	0.001 (0.048)
Good: credit score >=661 and <=780	0.121 (0.060)	** 0.130 (0.058)	** 0.151 (0.059)	** 0.129 (0.061)
Excellent: credit score >=780	-0.009 (0.031)	0.005 (0.029)	0.005 (0.029)	-0.009 (0.031)
Includes controls for baseline measures of outcomes	Yes	Yes	Yes	Yes
Includes controls for demographic characteristics	No	Yes	No	Yes
Number of observations	226	226	196	196

Note: Each entry is the estimated coefficient on the treatment dummy. Credit score outcomes are estimated using OLS. Other outcomes are estimated as probabilities using probit regressions where the coefficients are reported as marginal effects. Controls for demographic characteristics include age, gender, race, ethnicity, marital status, presence of children, household size, education, employment tenure, health insurance, household income, homeownership, ability to save \$26 per month, type of organization that the individual was recruited from. TOT estimates are from a two-stage least squares regression in which random assignment is an instrument for actual participation (compliance). Standard errors in parentheses. \*\*\*indicates significance at the 1% level, \*\*Indicates significance at the 5% level and \*indicates significance at the 10% level.

Source: Authors' calculations based on data provided by Working Credit.

**Table A11. Baseline Demographic Characteristics: Compliers v CW3 Recipients in the Treatment Group**

	Treatment Group						
	Compliers		Offered CW3		Took up CW3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Type of Organization</b>							
Typical	41.6%	(0.049)	50.9%	(0.069)	**	53.1%	(0.090)
Near-Typical	16.8%	(0.037)	13.2%	(0.047)		12.5%	(0.059)
Atypical	41.6%	(0.049)	35.8%	(0.067)		34.4%	(0.085)
<b>Age</b>							
Mean	24.02	(0.307)	23.151	(0.422)		22.97	(0.576)
18-24	55.4%	(0.050)	67.9%	(0.065)		71.9%	(0.081)
25-30	44.6%	(0.050)	32.1%	(0.065)		28.1%	(0.081)
<b>Gender</b>							
Female	59.4%	(0.049)	52.8%	(0.069)		37.5%	(0.087) **
<b>Race</b>							
African American/Black	46.5%	(0.050)	43.4%	(0.069)		43.8%	(0.089)
American Indian / Native Alaskan	2.0%	(0.014)	1.9%	(0.019)		3.1%	(0.031)
Asian/Hawaiian/Pacific Islander	11.9%	(0.032)	13.2%	(0.047)		9.4%	(0.052)
Caucasian / White	8.8%	(0.039)	18.9%	(0.054)		15.6%	(0.065)
Two or more races	10.9%	(0.031)	11.3%	(0.044)		15.6%	(0.065)
Other	20.8%	(0.041)	30.2%	(0.064)	**	40.6%	(0.088) **
<b>Ethnicity</b>							
Hispanic	21.8%	(0.041)	26.4%	(0.061)		34.4%	(0.085) *
<b>Marital status</b>							
Married	5.0%	(0.022)	3.8%	(0.026)		3.1%	(0.031)
<b>Household size</b>							
Number	2.97	(0.151)	2.981	(0.208)		2.94	(0.258)
<b>Number of children</b>							
Has any children	12.9%	(0.033)	7.5%	(0.037)	*	12.5%	(0.059) *
<b>Education</b>							
Less than a high school diploma	5.9%	(0.024)	7.5%	(0.037)		9.4%	(0.052)
High school diploma or GED	23.8%	(0.043)	35.8%	(0.067)	**	40.6%	(0.088)
Some college	20.8%	(0.041)	32.1%	(0.065)		37.5%	(0.087)
Associate's degree	3.0%	(0.017)	0.0%	0.000		0.0%	0.000
Bachelor's degree	38.6%	(0.049)	20.8%	(0.056)		9.4%	(0.052) **
Advanced or professional degree	6.9%	(0.025)	1.9%	(0.019)		0.0%	0.000
Not reported	0.0%	0.000	0.0%	0.000		0.0%	0.000
<b>Employment tenure</b>							
Less than one year	70.3%	(0.046)	69.8%	(0.064)		65.6%	(0.085)
One to two years	16.8%	(0.037)	18.9%	(0.054)		25.0%	(0.078)
Two to five years	9.9%	(0.030)	9.4%	(0.041)		6.3%	(0.043)
More than five years	1.0%	(0.010)	0.0%	0.000		0.0%	0.000
Not reported*	2.0%	(0.014)	1.9%	(0.019)		3.1%	(0.031)
<b>Health insurance</b>							
Private plan, through employer	36.6%	(0.048)	24.5%	(0.060)		18.8%	(0.070)
Medicaid (MassHealth)	41.6%	(0.049)	50.9%	(0.069)	*	59.4%	(0.088)
Other	15.8%	(0.037)	15.1%	(0.050)		15.6%	(0.065)
None	3.0%	(0.017)	3.8%	(0.026)		3.1%	(0.031)
Not reported	3.0%	(0.017)	5.7%	(0.032)		3.1%	(0.031)
<b>Homeowner status</b>							
Own	6.9%	(0.025)	9.4%	(0.041)		3.1%	(0.031) **
<b>Household income</b>							
Above \$71,991	10.9%	(0.031)	5.7%	(0.032)		3.1%	(0.031)
<b>Can save \$26 per month</b>							
Yes	97.0%	(0.017)	96.2%	(0.026)		100.0%	0.000 *
<b>Number of individuals</b>	101		53		32		

Note: Compliers refer to those that have at least attended a workshop or one-on-one coaching session. Significance is calculated based on comparisons of compliers versus those offered the CW-3 product and those offered the CW-3 product versus those who took it up. \*\*\*Significance at the 1% level. \*\*Significance at the 5% level. \*Significance at the 10% level.

Source: Authors' calculations based on data supplied by the Office of Financial Empowerment.