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Financial Education Interventions Targeting Immigrants and Children of Immigrants: Results from a Randomized Control Trial

SILVIA HELENA BARCELLOS [Economist],

University of Southern California

LEANDRO S. CARVALHO [Economist],

University of Southern California

JAMES P. SMITH [Senior Economist],

RAND Corporation

JOANNE YOONG [Assistant Professor]

National University of Singapore

SILVIA HELENA BARCELLOS: sbarcell@usc.edu; LEANDRO S. CARVALHO: lcarvalh@usc.edu; JAMES P. SMITH: smith@rand.org; JOANNE YOONG: joanne_yoong@nuhs.edu.sg

Abstract

We document that immigrants in the United States differ from natives in several aspects relevant for their financial decision making. Based on these differences, we designed novel financial education materials targeted at US immigrants and their children and evaluated their effectiveness using a randomized control trial. To the best of our knowledge, this is the first rigorous evaluation of financial education programs targeted at this population. Compared to a control group, the groups that received the one-time educational intervention were more likely to correctly answer financial knowledge questions immediately after the intervention. The estimated effects of this one-time intervention on knowledge were large, but most of them faded away after six months. Moreover, we find little effect of the treatments on intended financial behavior measures, both immediately and six months later. Our results point to the efficacy of this type of educational material in informing immigrants and their children about important financial information that they are unfamiliar with, including information related to their immigrant status. However, they also suggest that a priority for future research should be to test whether repeated opportunities for learning can increase financial knowledge retention and lead to behavior change.

Today, one out of every eight individuals living in the United States was born abroad. Moreover, 82% of American population growth between 2005 and 2050 will be due to immigrants arriving during this period and their US-born descendants (Passel and Cohn 2008; Smith and Edmonson 1997).¹ As the number of foreign-born residents continues to grow, the extent to which this population can fully participate in the economic life of the nation is an increasingly important issue. However, when compared to natives, immigrants

¹-Such population estimates are heavily affected by immigration policy; policies such as President Obama's November 2014 Executive Action on Immigration mean that these numbers are probably an underestimate. Other policies that have aimed at legalizing previously undocumented immigrants (such as the 1986 IRCA) have been shown to increase subsequent immigration, see Durand, Massey, and Parrado (1999).

usually lag behind in financial participation. Overall, foreign-born individuals are much more likely to be unbanked compared to individuals who were born in the United States (Aizcorbe, Kennickell, and Moore 2003; Rhine and Greene 2006). This pattern holds for savings accounts, homeownership, and stock ownership as well, with the native–immigrant gap in financial participation increasing with the sophistication of the financial product (Osili and Paulson 2008a, 2008b). Moreover, immigrant financial participation rates decrease the higher the levels of ethnic concentration in the metropolitan area where they reside (Osili and Paulson 2008a, 2008b). In addition to having consequences for immigrants themselves, these gaps in financial behavior might affect second-generation Americans (the children of immigrants). It has been shown that parents are an important channel through which young adults in the United States acquire financial knowledge, with family financial sophistication being strongly related to financial literacy among this population (Lusardi, Mitchell, and Curto 2010).

Access to banking services is critical to wealth-building: they provide households with the means to conduct basic financial transactions, save for an emergency and long-term security needs, and access credit on affordable terms. Unbanked workers must find other ways to accommodate their financial needs such as patronizing (usually expensive) check-cashing businesses and/or paying in cash (Casey 1994; Dunham 2001; Rhine, Greene, and Toussaint-Comeau 2006). Immigrant households often save informally outside the banking sector, but such savings are likely to be at risk of appropriation and offer no return or inflation protection. Besides, they do not count for credit histories, possibly contributing to immigrant households' struggle to achieve financial independence. These native–immigrant gaps in financial participation can potentially explain why natives accumulate more wealth (Carroll, Rhee, and Rhee 1994; Cobb-Clark and Hildebrand 2006; Shamsuddin and DeVoretz 1998) and have higher precautionary savings than immigrants with similar characteristics (Amuedo-Dorantes and Pozo 2002).

While previous studies provide evidence that there are significant native–immigrant differences in wealth levels and accumulation, financial market participation, and portfolio choices, very little is known about what drives these differences and what can be done to eliminate them. In fact, a 2005 Organisation for Economic Co-Operation and Development (OECD) report reviewing the state of financial literacy around the world concluded that “there have been relatively few evaluations of financial education programmes to determine what has worked well and what has not” (OECD 2005, 15). Moreover, many of the evaluations that do exist use “self-reported measures, measure outcomes over short time periods and cannot rule out selection bias due to nonrandomized designs, all of which may bias results” (Collins and O’Rourke 2010, 483). In a meta-analysis, Fernandes, Lynch, and Netemeyer (2014) show that correlational studies of financial literacy find strong associations with behavior that diminish dramatically when controls for omitted variables are included. In another meta-analysis of 188 studies, Miller et al. (2015) show that only a small minority of papers use randomized control trials (RCTs) for which the impacts are more reliable. Moreover, Theodos et al. (2014) emphasize the need for more RCTs to evaluate the impacts of financial capabilities interventions, but also list the challenges in successfully implementing such studies.

The main contribution of our study is to design novel financial education materials targeted at US immigrants and their children and to evaluate their effectiveness using an RCT. To the best of our knowledge, this is the first rigorous evaluation of financial education programs targeted at this population. We collected and analyzed primary data on key outcomes such as financial knowledge, participation, confidence, trust in financial institutions, and retirement plan participation using the RAND American Life Panel (ALP). Based on this analysis, we designed financial educational interventions to address low levels of knowledge, participation, confidence, and trust among first- and second-generation Americans.² Using an RCT, we evaluated the effectiveness of the proposed interventions in changing a wide range of financial knowledge and intended behavior measures among immigrants and their children, both immediately and in the longer term.

DATA

We collected three waves of primary data for this study using the ALP. The ALP is an Internet panel of respondents 18 years and older who agreed to participate in occasional online surveys. Respondents were recruited using a nationally representative sampling frame, and were provided with Internet access and a computer when necessary. Sample weights were calculated to make the distributions of age, sex, ethnicity, education, and income approximate the distributions in the Current Population Survey and to increase the generalizability of the results.³ Respondents participate in approximately two surveys per month and are compensated for their time. Since 2006, the ALP has included more than 350 surveys on a wide range of topics. Notably, it was used for the RAND Election Poll in 2012, providing one of the most accurate predictions of the results of the presidential election (Gutsche et al. 2014).

Our results are based on three different survey modules designed by the research team, as displayed in Figure 1. All the data used in this paper are freely available at <https://mmicdata.rand.org/alp/index.php?page=data>. All the ALP data collection efforts for this project were approved by RAND's Institutional Review Board (approval numbers: k0177-01-01-AM27 [MS189] and k0177-01-01-AM32 [MS215 and MS258] [Kapteyn]).

The first survey, MS189, was a baseline survey conducted in 2011 that includes a wide range of financial-related questions, such as knowledge, participation, confidence, access, trust in financial institutions as well as participation in retirement plans, and portfolio allocation within these plans. All panel respondents were invited to participate in this survey, which had a response rate of 85%. MS189 contains data on the respondent's country of birth as well as parents' and grandparents' country of birth. Using this information, we classified respondents into natives, first-generation (born abroad) or second-generation (at least one parent born abroad) Americans. According to this definition, 563 first- and second-generation Americans and 2,498 natives were surveyed in the baseline survey.

².Throughout the text, we will use the terms "first-generation Americans" and "immigrants," as well as "second-generation Americans" and "children of immigrants" interchangeably.

³.See Appendix S3 for a description of the weight construction. A ranking weighting method is used.

The second survey, MS215, was an experimental module with the main purpose to test new financial education materials targeted at immigrants and their descendants. Therefore, only first-, second-, and third-generation Americans (i.e., those with at least one grandparent born abroad) who completed the baseline survey were invited to take MS215. In this article, we focus on the 370 first- and second-generation respondents who completed MS215, as, conceptually, those who are grandchildren of immigrants most likely belong with the “native” group.⁴ As explained in detail below, each respondent who logged in to answer MS215 was randomly allocated to one of three groups: treatment 1, treatment 2, or control (see Figure 1).

The third survey module (MS258) was designed to measure whether our intervention had lasting effects—it was fielded six months after most of the respondents completed MS215⁵ and it included the same set of outcome measures, which are described in detail below. In MS258, we were able to reinterview 89% of the first- and second-generation respondents in MS215. We show below that this 11% attrition rate was not significantly different for the different experimental groups and did not affect the balance of observable characteristics across these groups.

FINANCIAL EDUCATION MATERIALS

We developed new financial education materials targeted at immigrants and their children using as a starting point the Federal Deposit Insurance Corporation’s (FDIC) Money Smart curriculum.⁶ The materials took the form of an easy-to-navigate website with information and questions to fixate content. Two versions were developed and tested: version 1 was based on general-purpose financial information about banking services, saving, and investing (in particular rules governing Individual Retirement Accounts or IRAs); version 2 had all the information in version 1 plus content directly addressing immigrant-specific rules. Appendix S1, Supporting Information, displays the screens presented on both versions of the educational material (1 and 2).

Version 1 materials provided information about reasons to keep money in a bank, types of financial institutions, accounts insured (and not insured) by the FDIC, saving options, “rule of 72” for interest rate calculation (a simple rule to determine the amount of time it takes for an investment to double in value at a constant interest rate), investment products (including bonds, mutual funds, retirement plans, stocks, and securities), and saving tips. The materials included pictures and straightforward questions about the content presented in order to increase participants’ engagement and interest. In version 2, additional information targeted at immigrants included procedures and documents needed to open a US bank account, FDIC insurance information, and retirement plan rules that apply to noncitizens. This additional information took the form of a frequently asked questions (FAQ) section.

⁴.Neidert and Farley (1985), Smith (2003), and Smith and Delaney (2015) report significant immigrant assimilation across generations, e.g., by the third-generation ethnic background is no longer an important predictor of most measures of socioeconomic achievement. In MS189, we had 638 respondents classified as third-generation Americans.

⁵.Most ALP respondents complete surveys within its first month in the field.

⁶.See <http://www.fdic.gov/consumers/consumer/moneysmart/index.html>

METHODS

In this article, we (1) collected primary data and measured immigrant-native gaps on a number of key outcomes such as financial knowledge, participation, confidence, trust in financial institutions, and retirement plan participation, and (2) designed and evaluated with an RCT, financial education materials targeted at immigrants and children of immigrants. The first empirical exercise consisted of documenting how financial outcomes differ for first-generation Americans (i.e., foreign-born respondents), second-generation Americans (respondents with at least one parent born abroad), and natives with American-born parents. We verified whether these differences were statistically significant by testing the null hypothesis of equality of means across the demographic groups.

We also conducted an RCT to test the effectiveness of new financial education interventions targeted at immigrants and their children. As described in Figure 1, first- and second-generation Americans were invited to participate in survey MS215 and were randomly divided in three groups: one was administered material version 1 (general content information), another was administered material version 2 (general content information plus immigrant-specific FAQ section), and a third group did not receive any financial education material (i.e., the control group). The random assignment of ALP participants into three groups facilitates impact evaluation of the materials. Random assignment implies that participants should have on average similar observable and unobservable characteristics across the three groups; therefore, any differences in postintervention outcomes between them can be attributed to the educational materials. In order to verify that the random assignment was properly implemented, we tested for statistical differences across experimental groups for a number of relevant outcomes measured in our baseline survey (i.e., preintervention), such as financial knowledge, confidence, and trust.

We tested the effect of both versions of our educational materials on financial knowledge and intended change in financial behavior both immediately after the intervention was delivered (using MS215) and in a six-month follow-up (MS258). We had a number of key financial knowledge and intended behavior measures, all listed in Table 1. To measure financial knowledge, we asked respondents ten questions on a wide range of topics such as the difference between saving and investing, interest rate calculations, risk diversification, instruments to save for retirement, and rules governing IRA accounts (see Appendix S2). Two of the six IRA questions asked about how specific rules apply to immigrants. The binary variables used in the analyses indicated whether the respondent correctly answered each one of these knowledge questions.

In addition to the knowledge questions, we asked questions designed to measure the likelihood that individuals would change their financial behavior, including the likelihood that an individual would enroll in a retirement plan (401k), and subjective expectations of wanting to learn more about IRAs and opening an IRA.⁷ In addition, respondents were given a hypothetical savings choice task.

⁷ While the two IRA questions were subjective expectation questions that ranged between 0% and 100%, the 401k question had four possible answers: “very likely,” “likely,” “not likely,” and “not sure” (see Table 1). In the analyses, we use a binary variable that indicates whether the respondent reports she is “very likely” or “likely” to enroll.

For our three experimental groups (treatment 1, treatment 2, and control group), we listed the means for each financial knowledge and behavior outcome and tested for statistically significant differences. Whenever outcomes in the treatment groups differed from outcomes in the control group, this was interpreted as an effect of the treatment intervention. We documented differences in outcomes measured both immediately after the intervention and six months later in order to test whether the effects persisted over time.

RESULTS

Immigrant–Native Gaps in Financial Knowledge, Confidence, Trust, and Behavior

We start by documenting how immigrants and their children differ from natives in terms of various financial outcomes using data from our baseline survey, collected before our educational intervention took place. Survey questions in MS189 that directly address or proxy for aspects of financial behavior include financial knowledge and participation, confidence about financial matters, retirement planning, access to financial products, and trust in financial institutions. The survey also contains information on enrollment in retirement plans and retirement portfolio allocation.

As a first step, we tabulate in Table 2 Panel A financial knowledge for first- and second-generation Americans as well as for natives with American-born parents. Figures in bold for immigrants and their children are statistically significantly different than corresponding averages for natives at the 10% confidence level or lower.⁸ Financial knowledge is low among all groups, but it is especially low among second-generation Americans, who are 9%–25% less likely than natives to get these questions right. Second-generation Americans are also less likely to have a checking/savings account and to hold assets such as stocks, real estate, and vehicles (Panel B). In addition, when compared to natives, first-generation Americans are more likely to have debts and less likely to own a home and vehicles.

Interestingly, Panel C in Table 2 does not show consistent differences in financial confidence across the three demographic groups. Even though levels of long-term preparedness are low for all groups—more than one third of all respondents report not planning for retirement and over 60% are not confident they will be able to meet their long-term financial needs—they do not differ across groups (Panel C). One exception is confidence in ability to meet short-term expenses, which is lower for second-generation Americans.

One possible explanation for the lower rates of financial market participation and asset ownership among immigrants is that they might have lower levels of trust in American financial institutions. Several factors could explain such lack of trust, such as language barriers, lack of knowledge about the system's operation, and lack of trust “inherited” from the home country, where institutions are possibly weaker, less efficient, and more vulnerable to corruption or mismanagement (Osili and Paulson 2008b). We look at differences in trust in institutions across demographic groups in Table 2 Panel D.⁹

⁸. Throughout the text, we will only discuss differences that are statistically significant.

⁹. Participants were asked to rate their trust in financial institutions in the United States and in the immigrant's country of origin using a 1 (I do not trust at all) to 5 (I trust completely) scale; respondents were considered to trust one institution if they answered 4 or 5.

The level of trust in US financial institutions is very low across all groups and types of institutions, with banks being on average the most trusted institution and stock brokers being the least trusted. Contrary to the notion that trust is a driving factor of differences in financial participation, for most institutions trust levels do not differ for natives and immigrants and their children. Curiously, when asked to rate trust in financial institutions in their country of origin, first-generation Americans report on average trusting institutions abroad significantly less than they trust US institutions; the lack of trust in country of origin institutions is especially true for banks, insurance companies, and financial advisors (results available upon request).¹⁰ Even though trust in financial institutions does not differ cross-native and immigrant groups, the focus here, the remarkably low levels of trust reported overall (and for stock brokers in particular), is intriguing and deserves future investigation.

Next, we look at savings behavior and outcomes in the context of employee-benefit plans. In our sample, a little over half of respondents are offered retirement plans by their employer—in most cases a Defined Contribution (DC) plan—and this fraction is similar for immigrants and natives. Table 3 shows that conditional on holding a DC plan, first- and second-generation Americans are as likely as natives to have contributed and borrowed against their plan in 2010 but that first-generation Americans are two times more likely to have made a preretirement withdrawal when compared to natives and second-generation Americans. This might reflect lack of knowledge regarding program rules (i.e., penalties for early withdrawal) or unwillingness to keep the money in a retirement account in the United States until retirement (e.g., in case they migrate back to their country of origin).

When it comes to portfolio allocation in DC plans, second-generation Americans behave clearly differently—when compared to natives they are less likely to hold stocks (54% vs. 64%) and more likely to hold bonds. This again might be related to the lack of familiarity with the American stock market.¹¹

Tables 2 and 3 show that immigrants and their children differ from natives in several aspects relevant for their financial decision making. When compared to natives, second-generation Americans have lower levels of financial knowledge, lower levels of financial participation and asset ownership, and are more likely to allocate their retirement savings in bonds relative to stocks. Similarly, first-generation Americans are more likely to have debts, less likely to be home and vehicle owners, and more likely to make preretirement withdrawals from their retirement accounts than are natives.

Our financial education materials, described above, were designed to address some of these differences in participation and knowledge. In the next session, we evaluate the different versions of our materials and test whether this type of intervention can help in closing the documented gaps in financial knowledge and behaviors.

¹⁰·We did not ask native-born respondents (including second-generation Americans) to rate their trust in foreign financial institutions, as they probably have limited experience with these institutions.

¹¹·Other possible explanations include differences in risk preferences, numeracy, or financial literacy.

Evaluation of Financial Education Materials

Testing Random Assignment—We start by testing whether the random assignment into the three experimental groups was successful. Random assignment implies that participants should have on average similar characteristics across the three groups; this in turn makes the evaluation of our educational materials easier because any postintervention differences in outcomes across groups can be attributed to the educational intervention. Table S1 shows that a number of characteristics measured in the baseline sample (i.e., preintervention) are balanced across the control group and treatment groups 1 and 2, which have 117, 135, and 118 observations, respectively.¹² These characteristics include not only socioeconomic status (SES) and immigrant-related variables but also financial knowledge, confidence, and trust, which are crucial for the question under study. Binary tests for significant differences across them cannot reject the null hypothesis that the means are the same (of 114 binary tests, we only reject the null six times at the 10% confidence level). Moreover, all the variables cannot jointly predict treatment assignment. Table S2 shows that even though we have 11% sample attrition between the experiment and the six-month follow-up survey, characteristics remain balanced across the different experimental groups once we restrict the sample to respondents who participated in both the first wave and the follow-up (we reject the null of equality seven times out of 114 tests). Moreover, treatment status does not significantly predict attrition (results available upon request).

Evaluating Financial Education Materials—We test the effect of two versions of our educational materials on financial knowledge and intended financial behavior (measures listed in Table 1), both immediately after the intervention was delivered and in a six-month follow-up. Table 4 shows the effects of the two versions of the educational material (treatments 1 and 2) on financial knowledge questions; it lists outcome means for each group (first three rows) and the *p*-value of equality tests across these means (last three rows).¹³ We mark in bold *p*-values where we reject the null hypothesis of equality. Because we randomly assigned respondents to the three groups (treatment 1, treatment 2, and control), respondents are on average similar across these groups and any significant difference in outcomes can be attributed to our educational interventions.

Panel A in Table 4 shows that respondents who were administered either version of the educational materials were more likely than respondents who did not (control group) to correctly differentiate savings and investment products, to correctly perform an interest rate calculation, to be more familiar with the purpose of a 401k and with the rules governing retirement accounts. The magnitudes of the effects are large, varying from 16% of the control's mean to 187% (this large effect is found for the interest rate calculation question, which had a low mean of 21% for the control group). When questions involved immigrant-specific information (i.e., columns eight and nine of Table 4), the effect was larger for the second treatment, which included the immigrant-specific FAQ screens. If we use the total

¹² Respondents were assigned to one of the groups at the time they first entered our survey and our records indicate that 97% of the respondents who started our experimental module finished it. This “finish” rate did not vary significantly across groups, therefore we believe that the variation in the size of the groups was naturally generated by the random assignment process. It is important to emphasize that, even though T1 is slightly larger, the characteristics are balanced across the three groups.

¹³ We do not include standard deviations in Table 5 because, since all variables are binary, the standard deviations can be easily computed using the formula $\sqrt{M(1-M)}$, where *M* is the variable mean.

number of correct answers as the outcome variable (ranging from 0 to 10), we find that relative to the control mean of 5.8 correct answers, respondents who got either treatment on average answered one extra question correctly (results available upon request). As expected given that the randomization was successfully done, results that test for differences controlling for observable characteristics are almost identical (see Table S3). Table S4 shows that the effects are similar in magnitude if we restrict the sample to foreign-born respondents only, however, results for this smaller sample are not always significant.

Table 4 Panel B presents a similar analysis using the same knowledge variables measured six months after the intervention was delivered. These results show that most of the gains in knowledge reported on Panel A are short lived.¹⁴ Interestingly, the one question for which the treatment 1 respondents appeared more knowledgeable six months after the experiment was the IRA taxation question, for which no difference was found immediately after the treatment was administered. This intriguing difference might be related to the fact that the follow-up survey was fielded during tax season (April 2012) when respondents might have been more attentive to taxation questions.

Overall, this analysis suggests that this type of educational material can increase immediate knowledge with respect to important financial information. However, we find that the effects of this one-time intervention are temporary: first- and second-generation Americans were not able to retain this knowledge six months after the intervention. Knowledge retention might require multiple educational interventions.¹⁵

Even more important than assessing the effects of educational interventions on knowledge is to understand how those affect behavior. Although, given our sample sizes, we would not have enough power to estimate how our intervention affects real-life financial decisions—which usually do not change very often—our outcome measures included a number of intended and hypothetical financial behavior measures (see Table 1 for a description of these measures). Table 5 investigates the effect of the treatments on these measures.

Despite the significant effects of the treatments on immediate knowledge, treatment respondents did not report being more likely to enroll in a 401(k) account,¹⁶ to want to learn more about IRAs, or to open an IRA. When analyzing the hypothetical choice task, we find that respondents in treatment group 1 choose to save or invest a higher fraction of money than the control group (an 11% effect relative to the control mean). However, treatment 2 has no effect on this hypothetical choice task and even for treatment 1 the effect disappears after six months (Panel B). These results are unaffected by the inclusion of controls (Table S5) and are qualitatively the same if we restrict the sample to foreign-born respondents only (Table S6).

¹⁴ Similarly, the average of the sum of correct answers in the follow-up survey is not statistically different across experimental groups (results available upon request).

¹⁵ The fact that multiple or extended interventions are needed for knowledge retention has been shown in the family life education literature, see e.g., Herz, Reis, and Barbera-Stein (1986).

¹⁶ Asked only of respondents who did not have a 401(k) to begin with.

DISCUSSION

This study investigated gaps in financial outcomes between natives and immigrants and their children and the potential of targeted education interventions to close those gaps. We collected primary data and measured gaps on a number of key outcomes such as financial knowledge, participation, confidence, trust in financial institution, and retirement plan participation; designed an educational intervention to address such differences; and evaluated with a random control trial the effectiveness of the proposed intervention.

Our results show that immigrants and their children are different than natives in several aspects relevant for financial decision making. When compared to natives, first-generation Americans are more likely to have debts, less likely to be home and vehicle owners, and more likely to make preretirement withdrawals from their retirement accounts. Similarly, second-generation Americans have lower levels of financial knowledge, lower levels of financial participation and asset ownership, and are more likely to allocate their retirement savings in bonds relative to stocks than are natives. Even though we find that trust in US financial institutions does not vary significantly for natives and first- and second-generation immigrants, the levels of trust are remarkably low, especially trust in stock brokers.

Keeping in mind these gaps in financial knowledge and behavior, we developed new educational materials targeted at immigrants and their children using as a benchmark FDIC's Money Smart curriculum. We designed two versions of this resource material: a general one and an immigrant-specific one. Using a RCT, we evaluated the effectiveness of the two versions of our educational materials. We found that one-time exposure to either version was effective in substantially increasing financial knowledge in the short term, but that these effects were short lived. Even though we document large increases in knowledge for the treatment groups immediately after the intervention is administered, a follow-up survey shows that most of these effects fade away after six months. Moreover, our analysis shows that the intervention had very limited effects on intended behavior change, measured either immediately after the intervention is administered or six months after.

This study has a number of limitations. First, the relatively small sample size in the randomized experiment limits our ability to investigate how treatment effects differ for potentially interesting subsamples such as Hispanic immigrants, and makes it necessary to pool together results for first- and second-generation Americans. First- and second-generation Americans are very different populations—in fact we documented above that they differ in terms of some key financial outcomes—and analyzing these populations separately would have been ideal. As a robustness check, we present in the Supporting Information the evaluation of our education materials restricting the sample to foreign-born respondents only and, even though the samples are much smaller in this case, the overall patterns are similar to the ones in the pooled sample. A second limitation is the attrition across survey waves (see Figure 1). This attrition is considerable, but it is not related to the experimental group assignment and therefore does not invalidate the design of the evaluation. Finally, we lack real financial behavior outcomes as changes in this type of behavior are somewhat infrequent, making them hard to capture in experiments with small samples such as the one described here. To remedy that, we investigated the measures of

intended and hypothetical financial behavior listed in Table 1 as a proxy for real behavior. However, such measures are an imperfect proxy which limits our ability to draw conclusions about financial behavior, the ultimate outcome of interest.

Overall, our results point to the efficacy of low-cost educational materials in informing respondents with respect to important financial information that they are not familiar with, including information related to their immigrant status. Furthermore, tailoring information to the specific conditions and needs of the target population can enhance effectiveness.

However, these effects are mostly temporary and are not accompanied by changes in intended behavior, results that are qualitatively consistent with recently published work on financial literacy training for migrant populations in New Zealand and Australia which finds that training-related gains in knowledge of remittance products failed to translate into improved, cost-saving, remitting behavior (Gibson, McKenzie, and Zia 2014). These new results add substantially to our developing understanding of consumer behavior in this population, as our study shows that this pattern also holds when considering migrants' longer term financial savings and investment decisions.

Our results are also consistent with the findings from the broader literature that evaluates financial education programs for the overall adult population. A review of 41 such studies, published in this journal, finds that most evaluations report positive impacts, but the findings are often small when compared with valid comparison groups (Collins and O'Rourke 2010). Fernandes, Lynch, and Netemeyer (2014) present a meta-analysis of studies of financial literacy and education and also find small effects on financial behaviors, effects that decay over time and vanish for even the most intensive interventions after 20 months or more. In addition, a financial education program targeted at very low-income families in the United States, evaluated using a randomized design, was found to lead to improvements in self-reported behaviors, but no measurable effects on savings or credit (Collins 2013). In fact, another meta-analysis of 188 studies (Miller et al. 2015) finds that financial literacy and capability can have a positive impact in some areas (such as increasing savings and promoting financial skills) but not others (credit default). All the review articles and meta-analyses mentioned above emphasize that most of the studies in this area do not use research methods that can credibly uncover the causal effects of financial literacy interventions, such as RCT. We contribute to this growing literature with a rigorous evaluation of novel educational interventions targeted at immigrants and their children.

Our findings hold significant implications for addressing the disparities in behavior and outcomes described earlier. A one-time dissemination of information alone may not be sufficient: while creating more informed consumers is in itself a valuable goal, policymakers seeking behavior change may have to look to other approaches. The relatively rapid decay of knowledge implies that repeated or reinforced opportunities for learning may be necessary to sustain levels of knowledge over time, allowing consumers to take advantage of opportunity as and when it arises. Another alternative is to adopt a pedagogical strategy that explicitly builds confidence at the same time as knowledge, based on the theoretical premise that self-efficacy (or the belief in one's ability to succeed, which was not directly targeted in our intervention) is also a necessary precondition for behavior change (Bandura 1977). Increased

knowledge and self-efficacy might, in turn, increase trust in financial institutions, which we show above to be very low. Finally, policymakers and practitioners may need to focus on interventions that directly relieve external constraints in the financial environment, such as increasing access to finance. Intervention approaches that combine the delivery of knowledge, confidence-building, and the provision of an immediate opportunity to take action may be most able to address these issues simultaneously. While these implications are beyond the scope of our paper, future research should test and compare the effects of different delivery methods, frequency of interventions, and designs of financial education materials not only on knowledge but also on knowledge retention and, most importantly, on actual behavior change.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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FIGURE 1.
American Life Panel (ALP) Surveys

TABLE 1

Key Financial Knowledge and Behavior Questions

Question Label	Question Text	Possible Answers
<u>Panel A: Financial Knowledge Questions</u>		
Differentiate between saving and investment	What is the major difference between saving and investing?	1 Most savings products are federally insured, while investment products are not 2 Savings products have a risk of loss and investment products do not 3 Investment products do not have as high a potential for growth as savings products 4 Savings and investment products are the same
Knows interest rate calculation ("Rule of 72")	If you want your savings account to double in value in 36 years, what fixed interest rate would the account need to have?	Open ended question Right answer: 2%
Diversification	When an investor spreads his or her money among different assets, does the risk of losing money:	1 Increase 2 Decrease 3 Stay the same
Knows 401(k)	Which of the following are ways you can save for retirement? (select all that apply)	1 Build home equity, then apply for a home equity loan 2 Invest in stocks, bonds, or mutual funds 3 Establish a 529 Plan 4 Enroll in a 401(k) or 403(b) plan
IRA taxation	A person who withdraws money from a standard 401(k) plan or IRA after he turns 59 1/2 does not pay taxes on the money that he withdraws	1 True 2 False
IRA withdrawal	After age 70 1/2, you have to withdraw at least some money from your 401(k) plan or IRA	1 True 2 False
IRA loan	If you lose your job while holding an outstanding loan from your 401(k) plan	1 You can still continue to pay for the loan as before 2 You are no longer responsible for paying back the loan 3 The loan will be treated as a withdrawal if not paid back promptly, resulting in taxes and penalties
IRA immigrant	An immigrant living abroad can hold a standard 401(k) plan in the United States if he or she has worked in this country in the past	1 True 2 False
Immigrant IRA taxation	If an immigrant withdraws money from a standard 401(k) plan or IRA held in the United States after he or she turns 59 1/2, he or she will have to pay taxes and the amount of taxes will depend on his or her country of residence	1 True 2 False
Employer retirement account match	If your employer increases the percentage of the money you invest in your 401(k) matched by the firm, you should:	1 Reduce your contribution since now you need to invest less money to reach your retirement savings target 2 Increase your contribution since this is free money your employer is depositing in your account 3 Not change your contribution
<u>Panel B: Financial Behavior Questions</u>		
How likely to enroll in 401k?	If your employer offered you this type of plan (401(k) or 403(b)) how likely would you be to enroll? (only asked of those whose employer does not offer DC plan)	1 Very likely 2 Likely 3 Not likely 4 Not sure
How likely learn about IRA?	What is the percent chance that you will want to learn more about IRAs?	0%–100%
How likely open IRA?	What is the percent chance that you will want to open an IRA?	0%–100%
Save or Invest	Suppose that you were to receive \$1,000 as an unexpected inheritance from a relative on condition that you use it for your retirement savings. How would you divide this money across the following assets?	Amount between \$0 and \$1,000, summing to \$1,000 across all categories

Question Label	Question Text	Possible Answers
Spend		Amount between \$0 and \$1,000, summing to \$1,000 across all categories
Give to friend or relative		Amount between \$0 and \$1,000, summing to \$1,000 across all categories
Other		Amount between \$0 and \$1,000, summing to \$1,000 across all categories

Notes: Summary of financial knowledge and behavior used in the experiment. Answers in bold are considered right.

TABLE 2

Financial Knowledge, Participation, Confidence, and Trust, by Immigrant Status

	# Obs	Interest Rate Calculation	Inflation Calculation	Diversification	Interest Rates/ Bond Prices	Asset with Highest Return
<u>Panel A: Financial Knowledge (% answering correct)</u>						
Natives, American-born parents	2,449	66%	69%	62%	28%	49%
First-generation Americans	208	62%	71%	63%	30%	46%
Second-generation Americans	340	61%	63%	50%	24%	41%
	# Obs	% Have Checkings or Savings Account	% Own Stocks	% Have Debts	% Home Owner	% Own Vehicles
<u>Panel B: Financial Participation and Asset Holdings</u>						
Natives, American-born parents	2,461	85%	33%	40%	64%	82%
First-generation Americans	208	88%	31%	52%	58%	75%
Second-generation Americans	343	78%	23%	42%	53%	72%
	# Obs	% Confident about Financial Matters	% Able to Access Financial Products	% Confident in Ability to Meet Unexpected Short-Term Expenses	% Report Thinking/ Planning for Retirement	% Confident in Ability to Meet Long-Term Needs
<u>Panel C: Financial Confidence/Access</u>						
Natives, American-born parents	2,496	84%	61%	64%	65%	34%
First-generation Americans	215	82%	64%	61%	67%	31%
Second-generation Americans	346	82%	62%	57%	61%	37%
	# Obs	% Trust the Stock Market	% Trust Banks	% Trust Insurance Companies	% Trust Stock Brokers	% Trust Investment Advisers
<u>Panel D: Trust in Financial Institutions in the United States</u>						
Natives, American-born parents	2,445	9%	44%	19%	6%	17%
First-generation Americans	208	10%	50%	23%	4%	16%
Second-generation Americans	338	8%	40%	20%	3%	15%

Notes: Authors' calculations using wave 189 of the American Life Panel. Averages calculated using survey weights. Figures in bold for first- and second-generation Americans are statistically significantly different (at the 10% confidence level or lower) than corresponding averages for natives with American-born parents. Trust measures were constructed from a question that asked participants to rank their trust in different financial institutions using a 1 (I do not trust at all) to 5 (I trust completely) scale. Respondents were considered to trust one institution if then answered 4 or 5; these are the fractions presented here.

TABLE 3

Retirement Plans and Portfolio Allocation

	If Offered DC Plan (N =1,005)		If Enrolled in DC Plan (N =837) in 2010		
	# Obs	Enrolled	Contributed	Made Withdrawal	Borrowed Against
Panel A: Defined Contribution Plans					
Natives, American-born parents	833	81%	82%	7%	4%
First-generation Americans	62	84%	80%	15%	2%
Second-generation Americans	110	79%	82%	7%	7%
	# Obs	Stocks	Bonds	Cash	Other
Panel B: Portfolio Allocation in Defined Contribution Plan (N =631)					
Natives, American-born parents	526	64%	18%	12%	5%
First-generation Americans	36	64%	14%	12%	10%
Second-generation Americans	69	54%	23%	9%	14%

Notes: Authors' calculations using wave 189 of the American Life Panel. Averages calculated using survey weights. Figures in bold for first- and second-generation Americans are statistically significantly different (at the 10% confidence level or lower) than corresponding averages for natives with American-born parents.

TABLE 4

The Effect of the Intervention on Knowledge

	Knowledge Questions									
	Differentiate between Saving and Investment	Knows Interest Rate Calculation (Rule of 72)	Diversification	Knows 401 (k)	IRA Taxation	IRA Withdrawal	IRA Loan	IRA Immigrant	Immigrant IRA Taxation	Employer Retirement Account Match
Panel A: First Wave Respondents (MS215)										
Control mean	0.77	0.21	0.75	0.43	0.72	0.67	0.46	0.60	0.49	0.68
Treatment 1 mean	0.93	0.61	0.78	0.55	0.72	0.84	0.39	0.77	0.48	0.80
Treatment 2 mean	0.90	0.41	0.74	0.52	0.74	0.76	0.40	0.90	0.62	0.76
Observations	363	352	360	361	359	360	359	358	359	359
p-value value C = T1	.000	.000	.623	.062	.930	.002	.266	.004	.866	.034
p-value C = T2	.010	.001	.766	.211	.759	.106	.308	.000	.051	.183
p-value T1 = T2	.361	.002	.422	.555	.821	.152	.942	.007	.030	.444
Panel B: Follow-up Respondents (MS258)										
Control mean	0.86	0.22	0.75	0.42	0.65	0.58	0.46	0.70	0.49	0.78
Treatment 1 mean	0.88	0.21	0.71	0.41	0.78	0.69	0.38	0.77	0.48	0.84
Treatment 2 mean	0.90	0.18	0.68	0.47	0.70	0.71	0.41	0.78	0.51	0.84
Observations	329	315	328	327	327	327	326	326	325	325
p-value C = T1	.664	.824	.588	.864	.036	.115	.232	.246	.816	.248
p-value C = T2	.364	.439	.303	.514	.455	.061	.452	.183	.837	.332
p-value T1 = T2	.607	.565	.595	.400	.195	.700	.689	.815	.660	.901

Notes: Authors' calculations using waves 215 and 258 of the American Life Panel. All knowledge variables are binary. Panel A presents results for first wave respondents, Panel B for follow-up respondents. First three rows in each panel show averages for each experimental group, last three rows show *p*-values for tests of equality across group averages. Tests where the null hypothesis is rejected at 10% confidence level or lower are marked in bold.

TABLE 5

The Effect of the Intervention on Intended Behavior Change

	Probability Expectations			Choice Task (\$1,000)			
	How Likely to Enroll in 401K?	How Likely Learn About IRA?	How Likely Open IRA?	Save or Invest	Spend	Give to Friend or Relative	Other
<u>Panel A: First Wave Respondents (MS215)</u>							
Control mean (SD)	0.71 (0.46)	0.57 (0.33)	0.46 (0.28)	646 (273)	201 (192)	57 (119)	97 (206)
Treatment 1 mean (SD)	0.68 (0.47)	0.54 (0.34)	0.49 (0.33)	715 (294)	204 (258)	34 (82)	46 (163)
Treatment 2 mean (SD)	0.79 (0.41)	0.49 (0.33)	0.46 (0.32)	653 (307)	186 (228)	68 (156)	93 (202)
Observations	205	358	229	353	353	353	353
<i>p</i> -value C=T1	.714	.492	.650	.061	.916	.088	.038
<i>p</i> -value C=T2	.279	.063	.959	.847	.602	.564	.896
<i>p</i> -value T1=T2	.149	.234	.635	.109	.574	.037	.048
<u>Panel B: Follow-up Respondents (MS258)</u>							
Control mean (SD)	0.69 (0.47)	0.55 (0.35)	0.48 (0.32)	658 (264)	189 (170)	57 (122)	96 (208)
Treatment 1 mean (SD)	0.61 (0.49)	0.49 (0.33)	0.37 (0.30)	713 (301)	209 (266)	33 (83)	45 (167)
Treatment 2 mean (SD)	0.77 (0.42)	0.47 (0.33)	0.39 (0.33)	657 (310)	176 (214)	72 (165)	96 (209)
Observations	197	322	201	318	318	318	318
<i>p</i> -value C=T1	.305	.158	.053	.153	.508	.082	.047
<i>p</i> -value C=T2	.302	.091	.137	.970	.636	.478	.999
<i>p</i> -value T1=T2	.039	.720	.733	.178	.325	.028	.050

Notes: Authors' calculations using waves 215 and 258 of the American Life Panel. Panel A presents results for first wave respondents, Panel B for follow-up respondents. First three rows in each panel show averages and standard deviations for each experimental group, last three rows show *p*-values for tests of equality across group averages. Tests where the null hypothesis is rejected at 10% confidence level or lower are marked in bold. While the two IRA questions were subjective expectation questions that ranged between 0% and 100%, the 401k question had four possible answers: "very likely," "likely," "not likely," and "not sure" (see Table 1). In the analyses, we use a binary variable that indicates whether the respondent reports she is "very likely" or "likely" to enroll.