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## Selection into financial literacy programs: Evidence from a field study

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## **Discounting Financial Literacy: Time Preferences and Participation in Financial Education Programs**

**Stephan Meier and Charles Sprenger**

**Abstract:**

Many policymakers and economists argue that financial literacy is key to financial well-being. But why do many individuals remain financially illiterate despite the apparent importance of being financially informed?

This paper presents results of a field study linking individual decisions to acquire financial information to a critical, and normally unobservable, characteristic: time preferences. We offered a short, free credit counseling and information program to more than 870 individuals. About 55 percent chose to participate. Independently, we elicited time preferences using incentivized choice experiments both for individuals who selected into the program and those who did not. Our results show that the two groups differ sharply in their measured discount factors. Individuals who choose to acquire personal financial information through the credit counseling program discount the future less than individuals who choose not to participate.

Our results suggest that individual time preference may explain who will and who will not choose to become financially literate. This has implications for the validity of studies evaluating voluntary financial education programs and policy efforts focused on expanding financial education.

**JEL Classifications:** D14, D91, C93

**Keywords:** selection, credit scores, financial literacy, time preferences, field experiment

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The views expressed in this paper are solely those of the authors and not necessarily those of the Federal Reserve Bank of Boston or the Federal Reserve System.

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# 1 Introduction

In personal finance, as in other areas of decision-making, information is thought to be a good thing. Financially literate individuals make fewer mistakes and are in better financial condition than financial illiterates (for surveys, see Lusardi and Mitchell, 2007b; Bernanke, 2006). Based on the positive association between financial outcomes and financial knowledge, policymakers promote educational programs, such as credit counseling, homeownership classes, and retirement seminars. The fact that the new bankruptcy law in the U.S. requires individuals to undergo credit counseling illustrates the importance that policymakers place on providing information on personal finance.<sup>1</sup>

But who acquires information about personal finance and who selects into voluntary educational programs offering such information? An understanding of who decides to improve his financial literacy is crucial to assessing the effects of financial education on economic behavior. If individuals select into educational programs based on unobservable characteristics that are directly linked to financial outcomes, it remains unclear whether individuals “treated” with the information would have had different outcomes from “untreated” individuals even without the educational intervention. Additionally, those who voluntarily select not to participate in the educational programs may be those who need them the most.

Surprisingly, very little evidence exists on who chooses to learn about personal finance. In this paper, we show that an individual’s discount factor helps to explain the choice to acquire personal financial information. Participating in an educational/counseling session can be seen as investing in human capital, and individuals who discount the future highly are expected to be less likely than others to invest in financial education.

Our evidence comes from a field study that offered more than 870 individuals a short credit counseling session, including an overview of their credit report (with their credit score). Independently, we elicited the time preferences of all individuals to whom the program was offered, using incentivized choice experiments. Knowledge about credit scoring and one’s credit score is very important in the United States for financial decisions, since credit scores are used to determine not only whether one gets a loan, but also the interest rate. In addition, credit reports may legally be used by employers, landlords, and insurance companies in their decision-making (see 15 U.S.C. §1681b). The advantage of our field study is that the outcome variable (that is, whether individuals choose to receive the counseling session) is an objectively observable behavioral measure. The participation decision does not depend on cognitive or learning abilities (as some financial literacy tests do), and the program is designed to have very low costs for participants (described below). We are able to measure time preferences with incentivized choice experiments both for those individuals who select into credit counseling

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<sup>1</sup>A debtor may be eligible for bankruptcy under Chapter 7 law only if he or she has participated in an approved credit counseling session in the previous 180 days. See 11 U.S.C. §§ 109, 111 for details.

and for those who do not.

In the field study, only about 55 percent of individuals choose to receive a free credit score and participate in the short credit counseling session. We show that this participation decision is highly correlated with individual time preference; more patient individuals are more likely to opt into the program. This result survives after controlling for a number of socio-demographic characteristics, such as income and education.

Additionally, we show that time preference influences information acquisition prior to receipt of the offer of the small intervention in the field study. More patient individuals are more likely to know what a credit score is, and, conditional upon knowing what a credit score is, they are more likely to believe that credit scores are important to their lives. Controlling for this prior information acquisition, however, only partly explains the strong correlation between time preference and the participation decision.

Our results have at least three implications. First, research evaluating the effects of financial programs should take selection effects into account.<sup>2</sup> Previous research has found that, in general, more patient individuals have better financial outcomes, since they search longer for a good job (DellaVigna and Paserman, 2005), experience steeper wage growth (Munasinghe and Sicherman, 2006), take up welfare programs earlier (Fang and Silverman, 2006), have higher credit scores, and are less likely to default on their loans (Meier and Sprenger, 2006). This indicates that patient individuals who acquire financial information are already on the path to better outcomes. If the measured effects of financial information interventions do not rely on randomization, then their observed educational effects are most likely overestimated. Because few studies can solve this selection problem,<sup>3</sup> we believe there is very limited knowledge as to whether financial information interventions have a positive effect.

Second, an expansion of voluntary financial programs will continue to miss certain consumers. Voluntary programs are considered by some to be a helpful step in creating better financial outcomes for an increasing number of people (see, for example, Braunstein and Welch, 2002). We suggest not only that the expansion of these programs may reach only a limited number of individuals, but also that such an expansion will reach precisely those consumers who will become better off regardless of the intervention.

A final implication concerns work that links financial literacy to the propensity to plan (see, for example, Lusardi and Mitchell, 2007a, and the literature cited therein). Our result shows that a more traditional preference parameter may be able to explain information acquisition. Hence, what appears to be a propensity to plan may be a proxy for an

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<sup>2</sup>Selection effects received substantial attention in the evaluation of job training programs (see, for example, Heckman, Ichimura, and Todd, 1997).

<sup>3</sup>There are a few notable exceptions. For example, Bernheim, Garrett, and Maki (2001) use a change in the financial curriculum in high schools as an identification strategy and find that financial education does increase retirement savings. Duflo and Saez (2003) offer randomized incentives to participate in a savings seminar and show that the effects, although positive, are extremely small. See also Lusardi and Mitchell (2007b) for a survey.

individual’s discount factor.

The rest of the paper is organized as follows: Section 2 presents the design of the field study and of the choice experiments, Section 3 presents the results, and Section 4 concludes by elaborating on the paper’s policy implications.

## 2 Design of Field Study and Sample

Our results are based on a unique field study that measures an individual’s discount factor regardless of his or her decision to participate in a financial education program, and investigates who selects into the educational program.

### 2.1 Design of the Field Study

The study took place at a Volunteer Income Tax Assistance (VITA) site in Roxbury, a neighborhood in Boston, Massachusetts.<sup>4</sup> We offered 872 individuals a free TransUnion & Co. credit report (including their Fair Issac Corporation (FICO) score), a short overview of how to read a credit report (for information about credit reports, see Avery, Bostic, Calem, and Canner (2003)), and several key steps they could take to improve or maintain their credit rating. All tax filers entering the VITA site were offered the short credit session.

The study focuses on low-to-moderate-income (LMI) individuals. This non-standard subject pool is of particular interest for the research question at hand, because their relatively insecure financial position puts them at great financial risk to health and income shocks (see Bertrand, Mullainathan, and Shafir, 2004). As (Campbell, 2006, p. 1554) notes “. . . for many households, the discrepancies between observed and ideal behavior have relatively minor consequences . . .; for a minority of households, particularly poorer and less educated households, there are larger discrepancies with potentially serious consequences.” Also, while LMI families may or may not differ significantly from wealthy families in their preferences, the behavior exhibited by this cohort sheds light on the general effects of selection in the acquisition of personal financial information.

The exact procedure in the field study was as follows: first, all taxpayers entering the Roxbury VITA site during the times we conducted the study were asked whether they wanted to receive a free, short credit counseling session (including their credit score) while waiting for a volunteer to help them with their taxes. Participation was free in the sense that we paid for the monetary cost of the credit report, individuals were explicitly informed that the credit report involved was a so-called “soft inquiry” that would not affect their score, and

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<sup>4</sup>There are currently 23 VITA sites in and around Boston, MA. Coordinated by a city-wide coalition of government and business leaders, VITA sites provide free tax preparation assistance to low-to-moderate income (LMI) households. Taxes are prepared by volunteers throughout tax season, from late January to mid-April each year.

most individuals already had to wait quite a long time at the tax sites for their taxes to be prepared.<sup>5</sup> Furthermore, concerns about identity theft are very unlikely to affect the decision to receive counseling, since individuals come to the VITA site voluntarily and entrust all their personal information to the volunteers at the site for tax filing purposes. Individuals could choose to receive the short counseling session at any point in the process.

Second, independent of whether they opted to receive the counseling session, individuals received a preparatory packet with forms for their taxes, a survey with some socio-demographic and other information, and a set of multiple price lists to enable us to measure time preferences.

Third, we explained to all individuals how to fill in the price lists and how the payment mechanism for the price lists would work (more details about the price lists are given below).

Fourth, individuals filled out the surveys and completed the price lists. Those who elected to participate then received a short credit counseling session. The session lasted no more than 15 minutes, and individuals had been informed of this fact before deciding to participate. Upon completion of the credit counseling session, a tax preparation volunteer helped the individual to file his or her taxes.<sup>6</sup>

We thus acquired the following information for 872 individuals: (*i*) whether they participated in the short educational program, (*ii*) a measure of their time preferences, and (*iii*) their income situation, from their tax filing. For a substantial number of the individuals in our study, we also have a complete record of the socio-demographic characteristics reported from the preparatory survey mentioned above. This survey includes questions concerning whether the individual has at least one credit card, the outstanding balances on all credit cards, information on knowledge of English, whether the individual expects to move in the next seven months, and the individual's willingness to take risks.

The survey also includes questions on prior financial knowledge. Individuals were asked: "*Do you know what a credit score is?*" Individuals had the option to answer: "*No, I don't know; Yes, I know but it is not important for me; Yes, I know and it is important for me.*" The answers to this question are used to determine whether time preferences influenced information acquisition prior to the field study, and whether inclusion of prior knowledge can explain the association between time preferences and the decision to acquire personal financial information.

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<sup>5</sup>To control for waiting time and also for potential social interaction effects, we control for the day when individuals enter the VITA site.

<sup>6</sup>Individuals who decided to receive the free credit counseling did not lose their place in the line for tax preparation, and all individuals were informed of this in advance.

## 2.2 Measuring Time Preferences

Participants' time preferences were measured with incentivized choice experiments (for similar approaches, see Harrison, Lau, and Williams, 2002; McClure, Laibson, Loewenstein, and Cohen, 2004; Dohmen, Falk, Huffman, and Sunde, 2006; Meier and Sprenger, 2006; Benjamin, Choi, and Strickland, 2007, and for a survey on measuring time preferences, see Frederick, Loewenstein, and O'Donoghue (2002)). Individuals were asked to make 22 choices between a smaller reward ( $\$X < \$50$ ) in period  $t$  and a larger reward ( $\$50$ ) in period  $t + \tau > t$ . The amount  $X$  varied from  $\$49$  to  $\$14$ . We use three time sets. In two of the price lists,  $t$  is the present ( $t = 0$ ) and  $\tau$ , the delay, is either one month ( $\tau = 1$ ) or six months ( $\tau = 6$ ). In the third price list,  $t$  is delayed six months ( $t = 6$ ) and  $\tau$  is one month ( $\tau = 1$ ). (See the appendix for the instructions and the multiple price lists.)

The multiple price list setup (see, for example, Harrison, Lau, Rutstrom, and Williams, 2005) enables us to measure an individual's discount factor ( $IDF$ )<sup>7</sup> for three different time frames by looking at the point,  $X^*$ , at which individuals switch from opting for the smaller, sooner payment to the larger, later payment, in a given price list. That is, a discount factor is taken from the last point at which an individual prefers the sooner, smaller payment. For example, if an individual prefers  $\$45$  today over  $\$50$  in one month, but prefers  $\$50$  in one month over  $\$40$  today, we take  $\$45$  as the switching point and the corresponding monthly discount factor as 0.9.<sup>8</sup> We use the average across the three choice sets of the calculated monthly discount factors,  $\overline{IDF}$ , in the main analysis.<sup>9</sup> We also test for the fact that we elicit an interval and not a point estimation of  $IDFs$  (see below). Importantly, the research question at hand needs only a reliable measure of the heterogeneity in  $IDFs$  across individuals and not necessarily precise point estimates of the level of the  $IDFs$ .

The order of the three time frames was randomized, and the same three experimenters explained the choices to the subjects. The researchers told the subjects that they were required to choose either the smaller, earlier reward or the larger, later reward for each decision pair. The experimenters also explained that 10 percent of individuals would be randomly paid one of their choices. This was done by giving subjects, at the end of their tax filing, raffle tickets that indicated which choice, if any, would be effective. To ensure credibility of the payments, we filled out money orders for the winning amounts on the spot in the presence of the participants, put them in labeled, pre-stamped envelopes, and sealed the envelopes. The payments were guaranteed by the Federal Reserve Bank of Boston, and individuals were informed that they could always return to the heads of the VITA sites where

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<sup>7</sup>In the paper, we use the individual discount factor ( $IDF$ ) instead of the individual discount rate ( $IDR$ ):  $IDF = 1/(1 + IDR)$ .

<sup>8</sup>Therefore, individuals' discount factors are calculated:  $IDF^\tau = X^*/50$ .

<sup>9</sup>We also test whether  $IDFs$  measured in each of the three price lists are associated with becoming informed, and indeed they are.

the experiments were run to report any problems receiving the payments.<sup>10</sup> Money orders were sent by mail to the winners' home addresses, either on that day (if  $t = 0$ ) or in one, six, or seven months, depending on the winner's choice. The payment procedure therefore mimicked a front-end-delay design (Harrison, Lau, Rutstrom, and Williams, 2005).<sup>11</sup>

Measuring time preferences with incentivized multiple price lists has many advantages over other approaches (Frederick, Loewenstein, and O'Donoghue, 2002), but the method also has challenges that must be addressed.

First, the price lists do not elicit point estimates of the *IDFs* but rather ranges of where the *IDFs* lie (see Coller and Williams, 1999, for details). Especially for individuals who accept the smaller, earlier payment in all choices, the interval will be relatively large, as the subjects might have accepted even lower amounts than offered at the earlier time. To address this issue, we show that the results are robust to estimation with interval regressions (as suggested by Harrison, Lau, Rutstrom, and Williams, 2005).

Second, individuals' decisions concerning the price lists may be affected by either their outside lending or their outside borrowing opportunities (see Harrison, Lau, Rutstrom, and Williams, 2005). On the one hand, an individual who can lend at an interest rate higher than the implied interest rate offered in the multiple price list should arbitrage the experiment by taking earlier payments. The lowest implied interest rate offered in the choice experiment for  $\tau = 1$  was 27 percent per year, which is difficult to match in the real world. Some of the interest rates for  $\tau = 6$  were substantially lower, making it is easier to find more favorable investment opportunities outside the experiment. If outside investment opportunities play a role, individuals should appear more impatient if  $\tau = 6$  than if  $\tau = 1$ . But individuals exhibit higher (not lower) *IDFs* if  $\tau = 6$  than if  $\tau = 1$  ( $p < 0.001$ ). Outside investment opportunities, therefore, do not seem to drive the experimental results. On the other hand, a person who can borrow at a rate lower than the experimentally offered rate should arbitrage the experiment by waiting for later payments. The individual may appear patient while actually arbitraging the experiment by borrowing externally at a lower rate and repaying later with earnings from the experiment. Because the implied interest rates in the experiment are large (especially in the case of  $\tau = 1$ ), this is relatively easy to do.<sup>12</sup> However, not many individuals consistently choose the later, larger payments to take advantage of the apparent arbitrage opportunity.

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<sup>10</sup>In fact, one participant returned to his VITA site, a community health center, almost seven months after the experiment to ask about his payments. He was, however, three days too early, and he received the payment on time.

<sup>11</sup>If individuals expect to move in the next seven months, they may question the likelihood of their mail being forwarded to their new address in a timely manner. As movers might therefore prefer payments in the present for logistical reasons and not for reasons related to their underlying time preference, we ask individuals "Do you expect to move in the next 7 months?" Including the answer to this question does not affect our results.

<sup>12</sup>In a companion survey, we asked "How many people do you know that would loan you \$100 if you asked?" Eighty percent of the people who answered the question ( $N = 772$ ) indicated that they have at least one friend who would do so.



Since the implied *IDFs* are rather small and significantly less than one ( $p < 0.001$ ), outside borrowing opportunities do not seem to drive the experimental results.

Third, measuring *IDFs* by observing individuals' switching points in price lists assumes that utility is linear over the payments in question. While some argue that this is a reasonable assumption (Rabin, 2000), others have argued that price lists might also measure the degree of curvature of the utility function (Anderson, Harrison, Lau, and Rutstrom, 2005). We therefore test whether differences in risk aversion affect our results, using a question on general risk attitudes previously validated with a large, representative sample (Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner, 2005). The question reads as follows: “*How willing are you to take risks in general (on a scale from 0 ‘unwilling’ to 10 ‘fully prepared’)?*” While risk aversion is correlated with measured time preferences, it does not affect the results of this paper.

Fourth, in order to measure an *IDF*, an individual must exhibit a unique switching point in each choice set. Around 11 percent do not exhibit a unique switching point in one or more price lists. In the main analysis we focus on the 778 individuals who show a unique switching point in all choice sets. Individuals who have multiple switching points within a choice set are neither more nor less likely to sign up for the counseling sessions. When we include these individuals in a robustness test by taking their first switching point, the results do not change.

### 2.3 Summary Statistics and Samples

Table 1 shows the characteristics of individuals in the field study. The table divides the sample into those who choose to receive the short counseling session and those who decline the offer. *T*-tests show whether the two groups differ in any of the characteristic variables. For 751 individuals, we have all basic demographic variables (age, race, and gender). The table shows that the average individual in the field study is around 38 years of age, African-American, and female. For 670 individuals, we have all the basic demographic variables, plus education, income, and the number of dependents. The average individual has no college experience, annual disposable income of around \$16,500, and 0.5 dependents. As these additional socio-demographic control variables may be simultaneously determined with a person's *IDF*, they are controlled for separately from the basic demographic variables. Controlling for education is important, since it could be a proxy for cognitive abilities, which might influence *IDFs* (Benjamin, Brown, and Shapiro, 2006). The number of observations with non-missing variables drops further if we add all the additional control variables (credit card holder-ship, volume of credit card debt, English language proficiency, risk aversion, expectations of moving). The analysis will be based on four basic samples: all observations ( $N = 872$ ), non-missing basic demographic controls ( $N = 751$ ), non-missing basic demographic and ad-

ditional socio-demographic variables ( $N = 670$ ), and observations with non-missing variables for the full set of control variables ( $N = 346$ ). Importantly, the various samples show the same effect of time preferences on acquiring information.

The comparison of the two self-selected groups shows that they do not differ much in observable basic demographic characteristics. The two groups, however, differ starkly in some additional socio-demographic and control variables. The two groups differ most extremely in both educational level and whether they know what a credit score is. Both variables are potentially correlated with time preferences (because their levels reflect investments in human capital) and appear to be correlated with the participation decision. To avoid omitted variable bias in our analysis, we control both for education and for knowing what a credit score is. The summary statistics show that the two groups also differ in their experience with credit cards. Individuals who choose to become informed are more likely to have a credit card, and are more likely to have a substantial amount of outstanding debt. These will also be crucial control variables, since credit behavior is influenced by time preferences (Meier and Sprenger, 2006). Interestingly, the variables regularly controlled for in evaluation studies of educational programs (for example, demographics and income), do not seem to differ between the two groups.

### 3 Results

Of all individuals offered the short credit counseling session, only 55 percent ( $N = 479$ ) opted to acquire financial information, while the rest ( $N = 393$ ) declined. The role of individuals' time preferences in this decision is presented in two steps. First, we present the direct association between time preferences and the decision to participate in the credit counseling program. Second, we present the association between time preferences and prior financial knowledge and the joint effect of time preferences and prior knowledge on the decision to participate in the program.

#### 3.1 The Effect of Time Preferences

There is a clear difference in time preferences between individuals who select into acquiring information and those who decide not to become informed. Individuals who choose to participate in the counseling session have an  $\overline{IDF}$  of 0.85, compared with an  $\overline{IDF}$  of 0.78 for individuals who choose not to participate ( $p < 0.001$ ;  $t$ -test).<sup>13</sup> Figure 1 shows the cumulative distribution of the  $\overline{IDF}$ s for the group who opted to acquire information and for the group who chose not to. The figure illustrates that individuals who select to acquire information have substantially higher  $\overline{IDF}$ s. To put it differently, individuals who selected

<sup>13</sup>The analysis is based on the 778 individuals who have unique switching points in the choice sets. We use  $t$ -tests to compare means unless otherwise noted.

into credit counseling chose the \$50 in the future an average of 12 times out of 22 choices, while individuals who selected not to receive counseling chose the larger, later reward only 9 times.

Table 2 presents a logit regression where the dependent variable is 1 if the individual participated in the credit counseling program and 0 otherwise. Column (1) shows the association between  $\overline{IDF}$  and the counseling participation decision. The marginal effect is 0.59 for a change in the  $\overline{IDF}$  from 0 to 1. This means that a change in the  $\overline{IDF}$  of one standard deviation increases the probability that a person acquires information by about 10 percentage points. Column (2) replicates this result for the sample with non-missing basic demographic control variables. The reduction of the number of observations and the slightly different sample do not affect the coefficient of  $\overline{IDF}$ . In fact, all the samples used in this study produce the same basic association between  $\overline{IDF}$  and the decision to become informed. The coefficients range from 2.4 to 2.7, and the respective marginal effects range from 0.59 to 0.66. Column (3) shows that the effect of  $\overline{IDF}$  on the decision to participate in the counseling program holds when controlling for basic demographic characteristics. The control variables show that the probability of acquiring information increases with age and then decreases again. African-Americans are more likely than others to choose to become informed. Column (4) adds additional socio-demographic variables. The results show that, among these additional variables, only education is significantly associated with the decision to acquire information. The relationship between time preferences and counseling participation is little changed by the inclusion of education and income variables.

Column (5) in Table 2 extends the list of control variables. The regression controls for whether the individual is a native English speaker, whether he expects to move in the next seven months, his attitudes toward risk (on a scale from 0, “unwilling,” to 10, “fully prepared”),<sup>14</sup> whether he holds at least one credit card, and whether his current outstanding balance is greater than \$1,000. The results in column (5) show that none of the newly added variables is significant.

This result—that individuals who elect to participate in the credit counseling program differ substantially in their time preferences from those who choose not to participate—is also supported in an interval regression (Stewart, 1983) with time preferences as a dependent variable. The interval regression technique takes into account the fact that the method used to elicit time preferences produces an interval rather than a point estimate of the  $IDF$  (see Table A1 in the appendix). The result is also robust to various other tests. First, the association between  $IDF$  and self-selection into credit counseling does not depend on which price list we use to measure individual discount factors. In the main analysis, we take the average,  $\overline{IDF}$ , but all three separate measures of  $IDF$  are strongly associated with the

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<sup>14</sup>The risk measure is treated as a continuous variable. The result does not change, however, if we use dummies for each of the points on the 11-point scale.

decision to acquire personal financial information (see Table A2 in the appendix). Second, instead of using  $\overline{IDF}$  to measure time preferences, we use the number of patient decisions (for example, out of 22 choices, the number of times that the individual opts for \$50 later) and still find a strong association between information acquisition and individuals' patience (see Table A3 in the appendix). Third, taking individuals' first switching point, the results are robust to including individuals who do not exhibit unique switching points (see Table A4 in the appendix).<sup>15</sup>

In sum, the results show that choosing to attend an educational seminar is strongly associated with individuals' discount factors. This association holds when controlling for demographic variables and for further characteristics potentially correlated with time preferences. In the following subsection we analyze whether time preferences are also associated with prior financial information acquisition and whether this prior knowledge can explain the relationship between time preference and the decision to acquire information in our field study.

### 3.2 Time Preferences and Prior Knowledge

Time preferences should not only explain who will select into becoming informed in our field study, but also influence the acquisition of personal financial information prior to the field study. To test this claim, we analyze responses to a simple question, in which individuals self-reported their knowledge about credit scoring. We create two dummies based on the question of knowledge about credit scores. The first dummy, *Know what score is*, is set equal to 1 for individuals' reporting that they know what a credit score is, and 0 otherwise. The second dummy, *Important for me*, is set equal to 1 if, conditional on knowing what a score is, respondents think that credit scores are important for them. Of the 568 individuals who answered the question, had non-missing basic control variables, and exhibited a unique switching point in the choice experiments, a substantial share (42 percent) did not know what a credit score is. Of those who did know, 80 percent thought that credit scores are important for them.

Table 3 presents the association between time preferences and prior financial knowledge, that is, whether individuals know what a credit score is. Column (1) shows results of a regression in which *Know what score is* is the dependent variable. The results show that  $\overline{IDF}$  is associated with prior knowledge. The more patient an individual, that is, the higher an individual's  $\overline{IDF}$ , the more likely it is that she knows what a credit score is. Not surprisingly, education is also a substantial predictor of whether individuals know what a score is. Better-educated individuals are more likely to know what a score is. The model in column (2) has as the dependent variable whether individuals perceive the credit score to be important,

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<sup>15</sup>Some studies even force people to have only one switching point, by assuming that once they switch, they will continue to prefer the larger, later payment.

conditional on knowing what a score is (*Important for me*). The result shows that, conditional upon knowing what a credit score is, impatient individuals (low  $\overline{IDF}$ ) are more likely to think that credit scores are not important for them. The results, therefore, indicate that more patient individuals are more knowledgeable about personal finance prior to the field study.

Columns (3) and (4) in Table 3 show the results of regressions in which the dependent variable is the decision to participate in the counseling program. Prior knowledge and whether an individual also thinks that credit scores are personally important are two of the independent variables. The results show that prior knowledge of credit scores is not sufficient to explain participation in the educational program. However, if an individual cares about credit scores and views them as important, then participation is much more likely. Controlling for whether a consumer knows about the importance of credit scores (along with all additional controls), however, does not substantially change the relationship between time preferences and the decision to acquire information in the field study. The relationship between time preferences and the decision to participate in the credit counseling program is maintained when controlling for whether or not individuals have prior knowledge of credit scores.

In sum, patient individuals are more likely to invest in information about personal finance in the field study and are more knowledgeable prior to the field study. And, given one's prior financial knowledge, time preferences are still important for the decision to acquire personal financial information.

## 4 Discussion and Conclusions

This paper presents evidence that time preferences strongly matter for whether or not individuals choose to inform themselves about personal finance. In a unique field study, we offered more than 870 individuals a short credit counseling session including their credit report and score. Only about half of the individuals chose to participate in the credit counseling session, despite the importance of credit scoring for most individuals and households. Independently, we measured individual time preferences using incentivized choice experiments to test whether selection into credit counseling is based on (normally) unobservable time preferences. The results show a very strong selection effect. The less individuals care about the future, the lower the probability that they select into acquiring information on a crucial aspect of personal finance. The result holds when controlling for prior investment in human capital—both general (for example, education) and specific to financial knowledge (for example, knowledge of credit scores).

These results have important policy implications for financial education programs and educational programs in general (for example, in the health domain). Self-selection on time preferences in attending financial education programs will affect the results of most evaluations of these programs. For example, evidence on the positive effect of credit counseling

programs (for example, Eliehausen, Lundquist, and Staten, 2007; Hartarska and Gonzalez-Vega, 2006) are most likely biased upwards. Our results indicate that individuals entering voluntary financial education programs probably care more about the future than those who decide not to enter. Previous research has shown that these more patient, “treated” individuals are more likely to have improved financial outcomes regardless of whether they participate in education programs. Measured effects of “treatment” are therefore biased and the direction of the bias is towards overestimation of positive effects.

Unbiased evidence on the effect of financial educational programs therefore requires randomized treatment. The estimated effects are then expected to be much smaller. In fact, Duflo and Saez (2003) find very small effects in a randomized study. Additional, unbiased studies such as this one are needed to evaluate whether promotion of educational programs will indeed have positive effects on individual financial decision-making.

A second implication is that efforts to improve financial decisions through educational programs are unlikely to reach people who need help the most. Making financial education mandatory, however, risks both irritating responsible consumers and having little effect on individuals who would have ignored the program had it been voluntary. In any case, there is very little evidence as to how, and whether, mandatory programs, like the one introduced in the new U.S. bankruptcy law, work. One of the few convincing findings is from Bernheim, Garrett, and Maki (2001), who show that mandatory financial education for high school children does increase future retirement savings.

Our results also raise questions about whether time preferences are fixed or can potentially be influenced. Becker and Mulligan (1997) argue not only that time preferences influence investment in education, but also that education can influence time preferences by making it easier for individuals to imagine the future. Evidence from our field study shows that, even controlling for education and prior financial knowledge, time preferences influence the acquisition of new information. Borghans and Golsteyn (2005) show that discount factors are heavily correlated with imagination, which might be important for future-oriented behavior. Similarly, time preferences might be correlated with the propensity to plan, which is also associated with financial literacy (Lusardi and Mitchell, 2007a). However, Ameriks, Caplin, and Leahy (2003) seem not to find an association between time preferences and the propensity to plan. Future research should investigate the relationship between time preferences and abilities like planning, imagination, and motivation in general. This will be crucial in order to think about how time preferences are formed and, in turn, how to increase financial information acquisition for all consumers.

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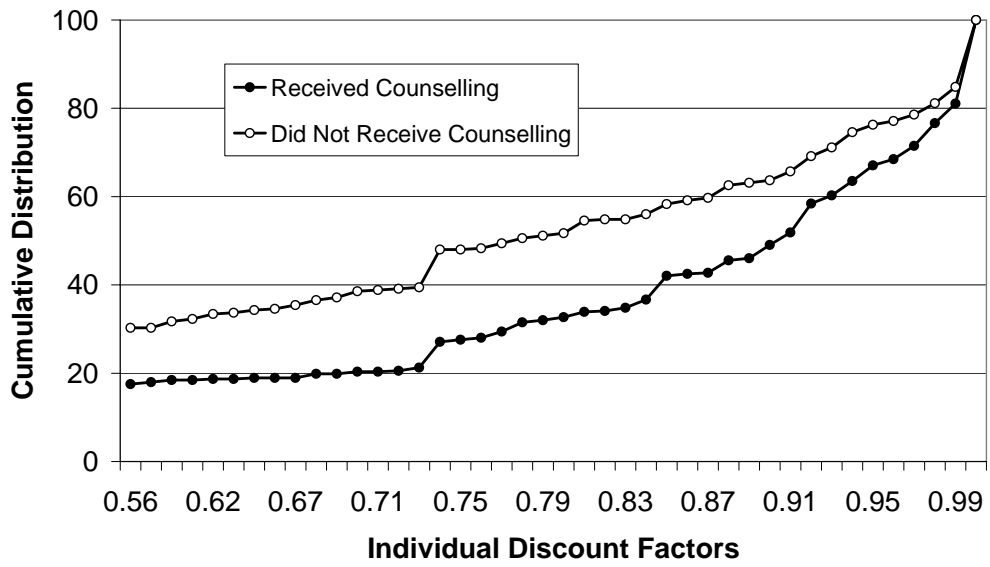


Figure 1: Cumulative Distribution of  $\overline{IDF}$

Table 1: Summary Statistics

	Total		Non-Participation		Participation		<i>t</i> -tests
	N	Mean (s.d.)	N	Mean (s.d.)	N	Mean (s.d.)	p-value
<i>1. Basic demographic variables</i>							
Age	751	37.82 (14.99)	344	38.69 (16.02)	407	37.07 (14.05)	$p = 0.14$
Race (African-American=1)	751	0.77 (0.42)	344	0.74 (0.44)	407	0.80 (0.40)	$p < 0.10$
Gender (Female = 1)	751	0.63 (0.48)	344	0.60 (0.49)	407	0.65 (0.48)	$p = 0.24$
<i>2. Additional socio-demographic variables</i>							
College Experience (=1)	670	0.44 (0.50)	306	0.37 (0.48)	364	0.51 (0.50)	$p < 0.01$
Adjusted Gross Income	670	16,623 (13,672)	306	15,975 (13,827)	364	17,169 (15,774)	$p = 0.26$
Number of Dependents	670	0.47 (0.81)	306	0.45 (0.79)	364	0.49 (0.83)	$p = 0.49$
<i>3. Additional control variables</i>							
Hold Credit Card (=1)	346	0.34 (0.47)	141	0.27 (0.45)	205	0.39 (0.49)	$p < 0.05$
CC Debt >1000 (=1)	346	0.15 (0.36)	141	0.09 (0.29)	205	0.20 (0.40)	$p < 0.01$
Native English (=1)	346	0.89 (0.31)	141	0.87 (0.34)	205	0.91 (0.29)	$p = 0.22$
Risk Attitudes	346	6.03 (2.73)	141	5.87 (2.99)	205	6.14 (2.53)	$p = 0.36$
Expect to Move (=1)	346	0.23 (0.42)	141	0.21 (0.41)	205	0.24 (0.43)	$p = 0.41$
<i>4. Prior financial knowledge</i>							
Know What Score Is (=1)	339	0.63 (0.48)	139	0.53 (0.50)	200	0.69 (0.46)	$p < 0.01$

*Notes:* Summary statistics for sample who selected not to participate in counseling program and those who participated. Last column shows  $p$ -value of whether means of the two samples differ in a  $t$ -test. Based on observations with non-missing variables for the respective block of variables plus non-missing variables in the blocks above.

Table 2: Time Preferences and Participation in Counseling Program

	(1)	(2)	(3)	(4)	(5)
$\overline{IDF}$	2.4***	2.48***	2.48***	2.39***	2.79***
	(.437)	(.472)	(.495)	(.607)	(.999)
Age			.118***	.081**	.172**
			(.033)	(.039)	(.078)
Age Squared			-.002***	-.001**	-.002**
			(.000)	(.001)	(.001)
Race			.407**	.439**	.642**
			(.19)	(.216)	(.327)
Gender (Female=1)			.066	.078	.372
			(.169)	(.203)	(.33)
College Experience (=1)				.414**	.118
				(.198)	(.312)
Ln(Income)				.007	.003
				(.093)	(.146)
# of Dependents				.091	.033
				(.13)	(.212)
Hold Credit Card (=1)					.209
					(.345)
CC Debt >1000 (=1)					.778
					(.477)
Native English (=1)					-.052
					(.475)
Risk Attitudes					-.008
					(.055)
Expect to Move (=1)					.307
					(.342)
Constant	-1.75***	-1.86***	-4.21***	-3.69***	-5.08**
	(.363)	(.394)	(.787)	(1.16)	(2.06)
Day dummies	No	No	No	Yes	Yes
Log Likelihood	-519.93	-452.67	-442.18	-361.68	-172.93
N	778	677	677	602	311

Notes: Dependent variable: Participated in credit counseling program (=1). Logit regressions. Standard errors in parentheses.

Level of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 3: Time Preferences, Prior Knowledge, and Participation in Counseling Program

	(1)	(2)	(3)	(4)
Dependent variable	a)	b)	c)	c)
$\overline{IDF}$	1.13** (.567)	2.21** (.918)	1.99*** (.647)	2.59** (1.01)
Age	.003 (.038)	.088 (.061)	.068 (.045)	.166** (.081)
Age Squared	.000 (.000)	-.001 (.001)	-.001 (.001)	-.002** (.001)
Race	.033 (.21)	.315 (.332)	.439** (.224)	.536 (.333)
Gender (Female=1)	-.632*** (.197)	-.13 (.337)	-.08 (.217)	-.255 (.338)
College Experience (=1)	.981*** (.194)	.486 (.307)	.218 (.215)	.115 (.33)
Ln(Income)	.18** (.09)	.33** (.142)	.001 (.098)	-.059 (.153)
# of Dependents	-.055 (.121)	.024 (.208)	.11 (.139)	.03 (.216)
Know What Score Is (=1)			-.313 (.32)	-.506 (.462)
Important for Me (=1)			.845*** (.323)	1.02** (.464)
Constant	-2.57** (1.04)	-5.43*** (1.71)	-2.94** (1.25)	-3.82* (2.07)
Additional control variables	No	No	No	Yes
Day dummies	No	No	Yes	Yes
Log Likelihood	-353.82	-149.5	-329.31	-167.69
N	568	326	555	305

Notes: Logit regressions. Standard errors in parentheses.

Dependent variables: a) *Know what score is*: Dummy equals 1 if individual knows what a credit score is; b) *Important for me*: Dummy equals 1 if individual thinks that credit score is important for them (conditional on knowing what a score is), and c) Dummy equals 1 if individual chooses to receive counseling session.

Additional control variables include ‘Hold Credit Card (=1),’ ‘CC Debt >1000,’ ‘Native English (=1),’ ‘Risk Attitudes,’ and ‘Expected to Move (=1).’

Level of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

# A Appendix

## A.1 Instructions

As a tax filer at this Volunteer Income Tax Assistance site you are automatically entered in a raffle in which you could win up to \$50. Just follow the directions below:

How It Works: In the boxes below you are asked to choose between smaller payments closer to today and larger payments further in the future. For each row, choose one payment: either the smaller, sooner payment or the later, larger payment. When you return this completed form, you will receive a raffle ticket. If you are a winner, the raffle ticket will have a number on it from 1 to 22. These numbers correspond to the numbered choices below. You will be paid your chosen payment. The choices you make could mean a difference in payment of more than \$35, so **CHOOSE CAREFULLY!!!**

RED BLOCK (Numbers 1 through 7): Decide between payment **today** and payment in **one month**

BLACK BLOCK (Numbers 8 through 15): Decide between payment **today** and payment in **six months**

BLUE BLOCK (Numbers 16 through 22): Decide between payment in **six months** and payment in **seven months**

Rules and Eligibility: For each possible number below, state whether you would like the earlier, smaller payment or the later, larger payment. Only completed raffle forms are eligible for the raffle. All prizes will be sent to you by normal mail and will be paid by money order. One out of ten raffle tickets will be a winner. You can obtain your raffle ticket as soon as your tax filing is complete. You may not participate in the raffle if you are associated with the EITC campaign (volunteer, business associate, etc.) or an employee (or relative of an employee) of the Federal Reserve Bank of Boston or the Federal Reserve System.

[Red Block;  $t = 0$ ,  $\tau = 1$ ]

TODAY VS. ONE MONTH FROM TODAY WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 1 AND 7? Decide for **each** possible number if you would like the smaller payment for sure **today** or the larger payment for sure in **one month**? Please answer for each possible number (1) through (7) by filling in one box for each possible number.

Example: If you prefer \$49 today in Question 1 mark as follows: ✓ \$49 today or \$50 in one month

If you prefer \$50 in one month in Question 1, mark as follows: \$49 today or ✓ \$50 in one month

If you get number (1): Would you like to receive \$49 **today** or \$50 in **one month**

If you get number (2): Would you like to receive \$47 **today** or \$50 in **one month**

If you get number (3): Would you like to receive \$44 **today** or \$50 in **one month**

If you get number (4): Would you like to receive \$40 **today** or \$50 in **one month**

If you get number (5): Would you like to receive \$35 **today** or \$50 in **one month**

If you get number (6): Would you like to receive \$29 **today** or \$50 in **one month**

If you get number (7): Would you like to receive \$22 **today** or \$50 in **one month**

[Black Block;  $t = 0$ ,  $\tau = 6$ ]

TODAY VS. SIX MONTHS FROM TODAY WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 8 AND 15? Now, decide for **each** possible number if you would like the smaller payment for sure **today** or the larger payment for sure in **six months**? Please answer each possible number (8) through (15) by filling in one box for each possible number.

If you get number (8): Would you like to receive \$49 **today** or \$50 in **six months**

If you get number (9): Would you like to receive \$47 **today** or \$50 in **six months**

If you get number (10): Would you like to receive \$44 **today** or \$50 in **six months**

If you get number (11): Would you like to receive \$40 **today** or \$50 in **six months**

If you get number (12): Would you like to receive \$35 **today** or \$50 in **six months**

If you get number (13): Would you like to receive \$29 **today** or \$50 in **six months**

If you get number (14): Would you like to receive \$22 **today** or \$50 in **six months**

If you get number (15): Would you like to receive \$14 **today** or \$50 in **six months**

[Blue Block;  $t = 6$ ,  $\tau = 1$ ]

SIX MONTHS FROM TODAY VS. SEVEN MONTHS FROM TODAY WHAT WILL YOU DO IF YOU GET A NUMBER BETWEEN 16 AND 22? Decide for **each** possible number if you would like the smaller payment for sure in **six months** or the larger payment for sure in **seven months**? Please answer for each possible number (16) through (22) by filling in one box for each possible number.

If you get number (16): Would you like to receive \$49 in **six months** or \$50 in **seven months**

If you get number (17): Would you like to receive \$47 in **six months** or \$50 in **seven months**

If you get number (18): Would you like to receive \$44 in **six months** or \$50 in **seven months**

If you get number (19): Would you like to receive \$40 in **six months** or \$50 in **seven months**

If you get number (20): Would you like to receive \$35 in **six months** or \$50 in **seven months**

If you get number (21): Would you like to receive \$29 in **six months** or \$50 in **seven months**

If you get number (22): Would you like to receive \$22 in **six months** or \$50 in **seven months**

## A.2 Appendix tables

Table A1: Time Preferences and Participation in Counseling Program (Interval Regressions)

	(1)	(2)	(3)	(4)	(5)
Participate in Program (=1)	.096*** (.017)	.097*** (.018)	.091*** (.018)	.073*** (.019)	.066*** (.023)
Delta Time ( $\tau=6$ )	.033*** (.001)	.033*** (.001)	.033*** (.001)	.033*** (.001)	.03*** (.002)
Has Present (t=0)	-.074*** (.009)	-.078*** (.009)	-.078*** (.009)	-.082*** (.01)	-.075*** (.013)
Age			-.005 (.004)	-.011*** (.004)	-.024*** (.006)
Age Squared			.000 (.000)	.000* (.000)	.000*** (.000)
Race			-.003 (.02)	-.012 (.019)	-.066*** (.022)
Gender (Female=1)			-.086*** (.018)	-.066*** (.019)	-.054** (.026)
College Experience (=1)				.052*** (.019)	.086*** (.024)
Ln(Income)				.022** (.009)	.022* (.012)
# of Dependents				.022* (.012)	.046*** (.017)
Constant	.663*** (.016)	.67*** (.017)	.853*** (.066)	.701*** (.103)	.837*** (.158)
Additional control variables	No	No	No	No	Yes
Day dummies	No	No	No	Yes	Yes
Log Pseudolikelihood	-6556.46	-5742.09	-5692.29	-4904.5	-2522
# of Observations	2334	2031	2031	1806	933
# of Individuals	778	677	677	602	311

*Notes:* Interval regressions (Stewart, 1983). Standard errors clustered on individual level in parentheses.

*Dependent variable:* Interval of *IDF*. The interval of *IDF* is calculated as follows: For each of the three price lists and for each individual, we observe a range of possible *IDFs* from  $IDF_{low}$  to  $IDF_{high}$ . This results in three interval observations per individual. We control for the differences in  $t$  and  $\tau$  of the underlying price lists using the variables *Delta Time* and *Has Present* which indicate whether the price list involves a time delay of six months and whether the price list involves the present, respectively. Interval regression of the range of *IDFs* on the counseling participation decision explores the correlation between time preferences and the decision to acquire information allowing for the reality that a precise *IDF* is not measured but rather a range of where the *IDF* may lie. Additional control variables include ‘Hold Credit Card (=1),’ ‘CC Debt >1000,’ ‘Native English (=1),’ ‘Risk Attitudes,’ and ‘Expected to Move (=1).’

*Level of significance:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A2: *IDF* from Each Time Frame and Participation in Counseling Program

	(1)	(2)	(3)
<i>IDF</i> <sub><i>t=0,τ=1</i></sub>	1.35*** (.413)		
<i>IDF</i> <sub><i>t=0,τ=6</i></sub>		3.64*** (1.19)	
<i>IDF</i> <sub><i>t=6,τ=1</i></sub>			1.68*** (.428)
Age	.074* (.039)	.076* (.039)	.081** (.039)
Age Squared	-.001** (.000)	-.001** (.000)	-.001** (.000)
Race	.451** (.216)	.405* (.215)	.425** (.216)
Gender (Female=1)	-.099 (.201)	-.11 (.201)	-.106 (.202)
College Experience (=1)	.445** (.197)	.458** (.196)	.413** (.198)
Ln(Income)	.016 (.092)	.021 (.092)	.014 (.092)
# of Dependents	.119 (.129)	.104 (.129)	.074 (.131)
Day dummies	Yes	Yes	Yes
Constant	-2.65** (1.08)	-5.02*** (1.51)	-3.02*** (1.1)
Log Likelihood	-364.18	-364.89	-361.71
N	602	602	602

*Notes:* Dependent variable: Participation in credit counseling program (=1). Logit regressions. Standard errors in parentheses.

*Level of significance:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .



Table A3: Participation in Counseling Program and # of Patient Choices

	(1)	(2)	(3)	(4)	(5)
# of Patient Choices	.044*** (.009)	.046*** (.01)	.045*** (.01)	.041*** (.013)	.05** (.021)
Age			.114*** (.033)	.076** (.039)	.161** (.078)
Age Squared			-.001*** (.000)	-.001** (.000)	-.002** (.001)
Race			.407** (.189)	.429** (.215)	.6* (.325)
Gender (Female=1)			-.092 (.168)	-.099 (.202)	-.383 (.328)
College Experience (=1)				.444** (.197)	.155 (.311)
Ln(Income)				.01 (.093)	.006 (.146)
# of Dependents				.098 (.129)	.042 (.211)
Constant	-.261** (.121)	-.324** (.132)	-2.48*** (.642)	-2.02* (1.04)	-2.83 (1.82)
Additional control variables	No	No	No	No	Yes
Day dummies	No	No	No	Yes	Yes
Log Likelihood	-524.02	-456.02	-445.33	-364.17	-173.97
N	778	677	677	602	311

*Notes:* Dependent variable: Participation in credit counseling program (=1). Logit regressions. Standard errors in parentheses. Additional control variables include 'Hold Credit Card (=1),' 'CC Debt >1000,' 'Native English (=1),' 'Risk Attitudes,' and 'Expected to Move (=1).'

*Level of significance:* \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table A4:  $\overline{IDF}$  and Participation in Counseling Program (Including Multiple Switchers)

	(1)	(2)	(3)	(4)	(5)
$\overline{IDF}$	2.18*** (.418)	2.23*** (.453)	2.21*** (.469)	2.25*** (.563)	2.51*** (.923)
Age			.076*** (.028)	.045 (.034)	.091 (.059)
Age Squared			-.001*** (.000)	-.001 (.000)	-.001* (.001)
Race			.372** (.18)	.422** (.206)	.653** (.306)
Gender (Female=1)			-.068 (.158)	-.077 (.189)	-.267 (.301)
College Experience (=1)				.442** (.186)	.183 (.288)
Ln(Income)				.007 (.084)	.032 (.129)
# of Dependents				.114 (.121)	.191 (.192)
Constant	-1.61*** (.352)	-1.69*** (.383)	-3.2*** (.709)	-3.07*** (1.07)	-4.08** (1.76)
Additional control variables	No	No	No	No	Yes
Day dummies	No	No	No	Yes	Yes
Log Likelihood	-586.24	-505.41	-498.66	-410.58	-196.47
N	872	751	751	670	346

Notes: Dependent variable: Participation in credit counseling program (=1). Logit regressions. Standard errors in parentheses. Additional control variables include 'Hold Credit Card (=1),' 'CC Debt >1000,' 'Native English (=1),' 'Risk Attitudes,' and 'Expected to Move (=1).'

Level of significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .