Experimental Economics, 2:107–127 (1999)

Eliciting Individual Discount Rates

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Abstract

Controlled laboratory conditions using monetary incentives have been utilized in previous studies that examine individual discount rates, and researchers have found several apparently robust anomalies. We conjecture that subject behavior in these experiments may be affected by (uncontrolled) factors other than discount rates. We address some experimental design issues and report a new series of experiments designed to elicit individual discount rates. Our primary treatments include: (i) informing subjects of the annual and effective interest rates associated with alternative payment streams, and (ii) informing subjects of current market interest rates. We also test for the effect of real (vs. hypothetical) payments and for the effect of delaying both payment options (vs. offering an immediate payment option). The statistical analysis uses censored data techniques to account for the interactions between field and lab incentives. Each of the information treatments appears to reduce revealed discount rates. When both types of information are provided, annual rates in the interval of 15%–17.5% are revealed, whereas rates of 20%–25% are revealed in the control session. Each of the treatments also lowers the residual variance of subject responses.

Keywords: experimental economics, discount rates, censored dependent variable

JEL Classification: C91, D90

1. Introduction

Laboratory experiments have been utilized to examine individual discount rates (IDR), or the rates at which individuals are willing to trade current consumption for future consumption (see Thaler, 1981; Loewenstein, 1988; Benzion et al., 1989; Horowitz, 1991; Winston and Woodbury, 1991; Carlson and Johnson, 1992; Holcomb and Nelson, 1992; Lazo et al., 1992; Shelley, 1993; Pender, 1996). In these studies, subjects typically state their preferences over alternative money streams and an implicit IDR is inferred from their decisions. The results consistently reject the classical model of Fisher (1930). Among other paradoxes, (central tendencies of) the observed IDR are substantially higher than market interest rates.[1] Experimental studies have found discount rates ranging from 1% (Thaler, 1981) to well over 1000% (Holcomb and Nelson, 1992). For time horizons, dollar magnitudes, and question frames roughly comparable to those considered in this study,[2] discount rates range from approximately 40% (Winston and Woodbury, 1991; Benzion et al., 1989) to over 200% (Thaler, 1981).

This study reports a series of experiments incorporating design features that may result in a more controlled environment in which to elicit IDR. We do not attempt to test any particular model of individual intertemporal choice. Rather, we conjecture that subject behavior in prior experiments may have been affected by (uncontrolled) factors other than discount rates, and we focus on moving toward an improved method for data collection. Similar to previous studies, our basic experimental design entails providing subjects with fifteen scenarios involving a choice between \$500 payable in one month and \$500 + \$$x$ payable in three months, where \$$x$ is varied from \$1.67 to \$90.54 to reflect annual rates from 2% to 100%, respectively. Our two primary treatments are: (i) explicitly indicating the interest rate associated with each choice, and (ii) providing information on available market rates.

These treatments are motivated by the fact that there are field substitutes for the lab instrument we use to elicit IDR. In other words, field credit market instruments (or other investment opportunities) represent an opportunity cost to saving in the laboratory. Because subjects may be attempting to arbitrage between the lab and the field, the discount rate revealed in the lab may not reflect their time preference for money. Consider, for example, the following debriefing comment from a subject: "I could take the \$500 in one month from now and then turn it into well more than \$590.54 by investing." This subject was thus unwilling to save in the lab and revealed a rate of over 100%. This does not necessarily imply that his IDR is over 100%, but rather that he believes he has other more lucrative opportunities.

The problem of such *censored* responses (where rates revealed in the lab are influenced by subjects' field opportunities) is simple enough to correct for in the data analysis if we can determine what field opportunities the subject is considering. However, these opportunities, and thus the arbitrage possibilities, may be unclear to the subject himself. In particular, it may be difficult for the subject to compare the lab opportunity with his field opportunities for two reasons. First, he may simply be unfamiliar with his field opportunities. Second, even if he is aware of his field opportunities, he may be unable to make the comparison between field and lab opportunities because the different instruments are stated in different terms. The lab instrument in our control condition (and in most other studies) is stated in terms of a dollar return, whereas field instrument returns are typically stated in terms of interest rates. For example, the subject quoted above may simply be underestimating the rate associated with a \$90 return.[3]

Our experimental design thus recognizes that two conditions must be met if a subject wishes to successfully arbitrage between the lab and field. First, the subject must know what rate of interest is associated with the choice he is offered in the lab. Second, the subject must be aware of his field opportunities. If the subject either does not know (or cannot correctly calculate) the rate he faces in the lab or is not informed about field opportunities, he may reveal an erroneous IDR. The presence or absence of these two conditions (both independently and jointly) comprise four of our six experimental sessions. Additionally, we conduct sessions to evaluate the effects of removing the one- month front end delay in the payment options and of using hypothetical payoffs.

The prediction that subjects will arbitrage between the lab and field is then utilized in our data analysis. First, we assume that subject responses are censored with respect to (subject specific) field interest rates and use statistical methods to account for this.[4] Second, because our treatment conditions should reduce errors when subjects attempt to arbitrage, we expect one effect of the treatments to be a reduction in the residual variation of subject responses. This hypothesis can be examined by testing for heteroskedastic errors as a function of experimental treatment conditions.

Our results indicate that the median discount rate implied by subject choices for all experimental sessions combined is in the interval of 17.5%–20%, stated in annual terms.[5] The statistical analysis indicates that providing information on the rates implied by each choice lowers mean revealed discount rates, as does providing information on available market (field) rates. When both types of information are provided, median rates are in the 15%–17.5% range. These results are consistent with current market borrowing rates. Both information treatments lower the residual variance of subject responses. We also find that both removing the front end delay and the use of hypothetical payments have a positive effect on discount rates.

The following section presents further discussion of the issues that motivate our treatment conditions. Section 3 details the experimental design. Section 4 presents the statistical analysis of the results. Conclusions and discussion follow in Section 5.

2. Eliciting discount rates in the laboratory

Eliciting IDR over monetary outcomes in the laboratory involves asking the subject (implicitly or explicitly) to "invest" in a laboratory instrument. In our experiment, subjects may choose to receive \$500 on a given date or \$500 + \$$x$ two months later, where x implies a rate of return on "saving" the \$500 in the lab for two months. Given that most subjects will have access to credit markets *outside* the experimental laboratory (the field), market borrowing rates (r_B) and lending rates (r_L) should influence their decisions in the laboratory.

Consider, for example, a subject with an IDR of 3%. In the absence of field substitites for lab incentives, we would expect this subject to choose to invest in the lab instrument as long as it provides a return of 3% or higher. Now suppose that this subject can save in the field at a rate of 5%. Although she would be willing to save at 3%, at rates between 3% and 5%, she is better off investing in the field (and refusing to invest in the lab). The problem is symmetric for subjects with a discount rate above their field borrowing rate. Consider a subject with a true IDR of 30%. In the absence of field substitutes for lab incentives, we would expect this subject to choose to save in the lab when the lab provides a rate of return of 30% or higher. Now assume that this subject can borrow in the field at a rate of 14%. Although she demands at least 30% interest to delay consumption and save in the lab, at rates between 14% and 30% she is better off borrowing in the field at 14% (and not delaying consumption), leaving the money in the lab earning at least 14%, and repaying the debt at the time she collects from the experimenter. In this case, the subject should choose to invest in the lab when the lab instrument provides a rate of return of 14% or more.

For subjects to successfully make the type of comparisons discussed above, they must first be acquainted with their field credit options. Given the variety of borrowing and lending opportunities in the field, as well as the volatility and variability of the associated rates of return, it is plausible that many subjects (particularly those who are not active in credit markets) have imperfect knowledge of the rates that currently apply to them. If this is true, then variability in discount rates observed in the lab may be due in part to variability in subject's perceptions of market conditions. To test this hypothesis, we include a treatment in which subjects are provided with information on currently available field borrowing and lending rates.

In addition to knowing applicable field rates, the subject must also be able to compare the return offered in the lab to the return she can obtain in the field.[6] To do this accurately, she needs to either convert the dollar return on the lab instrument to a percent return, or convert the percent return on the field instrument into a dollar return. Because these calculations are difficult, particularly for time streams of less than a year, it seems unlikely that most experimental subjects can perform them accurately.[7] Hence, we hypothesize that the anomalous discount rates revealed by subjects in previous studies may be due in part to erroneous estimates or calculations. Specifically, subjects may tend to underestimate the rate of return associated with a given dollar interest (or overestimate the dollar interest associated with a given rate of return). We test this possibility by including a treatment in which subjects are provided with the interest rate implied by each choice they are given.[8]

The implications of these arbitrage opportunities for what we can expect to observe in the lab are clear. If the subject recognizes the opportunity cost of investing in the lab and acts to maximize her payoff, we should not observe true preferences over monetary streams when the field offers better terms than the lab. This issue is explicitly recognized by Loewenstein (1987, p. 688) and by Pender (1996, p. 283). As Pender states, "[If credit markets allow unlimited borrowing/saving at a fixed interest rate] and the rewards being offered are tradeable goods, intertemporal preferences are irrelevant to the choices being made in discount rate experiments." Pender's qualifying assumption, however, does not account for the difference between available borrowing and lending rates. For rates of return within this range, i.e., opportunities that are not available in the field, intertemporal preferences are relevant to the choice being made. In other words, only if a subject chooses to invest in the lab instrument at a rate not available in the field can we assume this subject is revealing his IDR.

For most individuals, field lending rates are lower than field borrowing rates, i.e., $r_L^i < r_B^i$ (where i represents the subject-specific rate). The subject i for whom $IDR^i < r_L^i$ will not save in the lab until the lab rate is at least as great as r_L^i. The rate she chooses in the lab will thus be r_L^i, not her true discount rate. Likewise, we should not observe the true discount rate for any subject i for whom $IDR^i > r_B^i$. Subject responses thus come from a distribution which is *censored* from the left with respect to field lending rates and from the right with respect to field borrowing rates, where these rates are *individual specific*. Our statistical analysis explicitly accounts for the effects of these individual specific field substitutes on subject responses, where we elicit subjects' perceptions of field rates as part of the experimental design.

3. Experimental design

3.1. General design

Each of six experimental sessions consisted of approximately 35 graduate and undergraduate students recruited from various School of Business classes at the University of South Carolina. For five of the six sessions, subjects were presented with the following initial information:[9]

> *One* person in this room will be randomly chosen to receive a large sum of money. If you are the individual chosen to receive this money (the "Assignee"), you will have a choice of two payment options; option A or option B. If you choose option B you will receive a sum of money 3 months from today. If you choose option A you will receive a sum of money 1 month from today, but this option (A) will pay a smaller amount than option B.

The remaining session differed only in that option A paid a sum of money on the day of the experiment, while option B paid the larger sum two months later. Research budget constraints dictated that only one person in each session could be paid. This person (the "Assignee") was chosen at random at the end of the experiment.[10] To ensure credibility of the payment instrument, a notarized payment certificate was given as a guarantee of payment.[11]

Option A was \$500 in all treatments. Option B paid \$500 + \$$x$ where \$$x$ ranged from \$1.67 (reflecting a 2% annual rate of return on the \$500 principal compounded daily) to \$90.54 (reflecting a 100% annual rate of return compounded daily). \$500 was chosen as the minimum payment in order to ensure that *all* subjects would have an opportunity to arbitrage in the field, regardless of whether or not they had an established investment vehicle.[12]

In five of the sessions, the earlier payment option (A) is not an immediate payment, but rather occurs in one month. This feature was chosen for the majority of the sessions in order to minimize the possibility of perceived differences between the two payoff options with respect to (i) transactions costs and (ii) risk associated with future payment. Regardless of the payment option chosen, the subject must keep track of a payment certificate for some time and, presumably, expend the same time and energy in redeeming the certificate. Moreover, if the participant's subjective probability of receiving the future payment is less than 100% despite our attempt to ensure credibility, then the fact that both payment options occur in the future should minimize any differences in perceived risk between the two payment options.[13]

Prior studies indicate that individuals appear to be more impatient about immediate delays than about future delays of the same length. Thus, although we view the front-end delay as an important experimental control feature, it may have the effect of lowering revealed rates overall. To examine the impact of the front-end delay, we conduct one session in which payment option A occurs immediately.

To determine the value of \$$x$ and the associated discount rate at which the subject is indifferent between the two payment options, we use a multiple price list (MPL) which is reproduced in Table 1. The MPL seves to expedite providing information on the annual and

Table 1. Multiple price list.

Payoff alternative	Payment option A (pays amount below in 1 month)	Payment option B (pays amount below in 3 months)	Annual interest rate (AR)	Annual effective interest rate (AER)	Preferred payment option (Circle A or B)	
1	\$500	\$501.67	2.00%	2.02%	A	B
2	\$500	\$502.51	3.00%	3.05%	A	B
3	\$500	\$503.34	4.00%	4.08%	A	B
4	\$500	\$504.18	5.00%	5.13%	A	B
5	\$500	\$506.29	7.50%	7.79%	A	B
6	\$500	\$508.40	10.00%	10.52%	A	B
7	\$500	\$510.52	12.50%	13.31%	A	B
8	\$500	\$512.65	15.00%	16.18%	A	B
9	\$500	\$514.79	17.50%	19.12%	A	B
10	\$500	\$516.94	20.00%	22.13%	A	B
11	\$500	\$521.27	25.00%	28.39%	A	B
12	\$500	\$530.02	35.00%	41.88%	A	B
13	\$500	\$543.42	50.00%	64.81%	A	B
14	\$500	\$566.50	75.00%	111.53%	A	B
15	\$500	\$590.54	100.00%	171.45%	A	B

effective interest rates associated with subject decisions. Although we lose some precision with this procedure (subject responses provide us with a discount rate interval rather than an exact value), the simple presentation is likely to minimize subject confusion and errors associated with more complicated alternatives.

The MPL presents subjects with 15 "payoff alternatives"; each payoff alternative pays \$500 in one month (today in the no front-end delay treatment) and $\$500 + \$x$ two months later (payment options A and B, respectively), where $\$x$ increases as one moves down the MPL. Subjects were asked to indicate which payment option they preferred for each payoff alternative. They were informed that after they made their decisions one of the payoff alternatives would be chosen at random and the Assignee would receive her preferred payment option under this alternative.

In order to illustrate the randomization devices used we conducted a simple trainer in which payments were in the form of chocolate candies. Payment options in the trainer paid 5 chocolate candies immediately or $5 + x$ chocolate candies at the end of the experiment. Subjects were given procedural instructions and an MPL in the same format as those used in the actual experiment (though the trainer incorporated only 6 payoff alternatives). After the trial payoff alternative and Assignee were chosen, the Trial Assignee was called to the front of the room and paid (if she chose option A) or the appropriate number of candies was set aside and the group was instructed that she would receive them at the end of the experiment. This served to illustrate the procedures used in the actual experiment and to emphasize that

Table 2. Experimental design.

Session	Front-end delay	Information on AR/AER	Information on market rates	Real payments
1	X			X
2	X	X		X
3	X		X	X
4	X	X	X	X
5				X
6	X	X	X	

the payment was real.[14] We then stressed to subjects that, although the Assignee would be identified at the end of the experiment, the choices made by the Assignee would be confidential.[15]

3.2. *Treatments*

The overall experimental design is summarized in Table 2. Session 1 is our control treatment; it incorporates the front-end delay and real payments, but no information on interest rates associated with each alternative or on market interest rates. Session 2 provides subjects with information on the annual interest rates implied by choosing payment option B over option A. These are described as interest rates that could be earned on the \$500 if the subject chose to postpone payment. The rates are in terms of annual effective rates (AER), computed using daily compounding, as well as annual rates (AR).[16]

Session 3 provides subjects with information on current market (field) interest rates and describes some consumption smoothing opportunities. Specifically, we point out that one possibility for subjects who would not wish to spend the money for at least three months is to choose option A and place the \$500 in a CD or passbook savings. Subjects are informed of the best local rates at the time of the experiment and which local banks provide these rates, and we explain minimum balance requirements and early withdrawal penalties. Similarly, we explain that one possibility for subjects who would like to choose option B but would like to spend the money sooner is to borrow the money via a credit card or line of credit, and repay the money (with interest) at the end of three months. We provide subjects with current rates on lines of credit and credit cards, informing them that the credit card rates correspond to those advertised on campus.

Session 4 incorporates both information treatments. The first four sessions thus comprise a complete 2×2 design. The treatments included in these sessions are our primary focus in determining whether the provision of information affects revealed discount rates.

Session 5 is included to test the effects of the front-end delay. It is identical to Session 1 (no information on AR/AER or on market rates is provided) with the exception that option A pays \$500 on the day of the experiment, and option B pays $\$500 + \$x$ two months later.

Finally, Session 6 is designed as a test of the effect on responses of real payments relative to hypothetical payments. Information is provided on both AR/AER and market rates, but no one actually receives \$500 (or \$500 + \$$x$). This treatment is included as a bridge to future research in which we will be interested in eliciting discount rates via mechanisms which do not allow for real money consequences. Previous studies have found that responses using hypothetical incentives are consistent with those using real monetary consequences, but no study has conducted a direct test of the effect of hypothetical vs. real payments. This treatment can also provide evidence on whether our subjects found our payoffs salient. If subjects respond differently when facing real (vs. hypothetical) payments, then we can infer that they were influenced by the possibility of actually receiving \$500 or more.[17]

4. Statistical analysis

4.1. *Raw data*

Our results consist of data on the socio-economic characteristics of subjects, answers to a set of debriefing questions including specific questions about borrowing and lending activities and associated rates, and subjects' choices over payment options A and B in each of the 15 payoff alternatives in the MPL.[18] These raw responses are coded as a $1, 2, \ldots,$ or 16 corresponding to the payoff alternative at which they first choose payment option B over payment option A; if a subject always chooses A his response is coded as a 16.[19] *Temporarily ignoring the issue of censored responses*, we interpret this payoff alternative as the discount rate interval for that subject. For example, if the subject first chooses option B over option A at payoff alternative 10, then his discount rate must lie above 19.12% but is no greater than 22.13%.

Descriptive statistics for the entire sample and for each treatment session are reported in Table 3. Because the ranges defining our elicitation intervals are not constant, and because

Table 3. Descriptive statistics for subject responses.

	Median						
		Interval (%)[b]					
Session	Raw responses[a]	AR	AER	Interquartile ranges[c]	% Within median interval	% Below median interval	N
All	10	17.5–20	19.1–22.1	7.8–41.2	4.00	48.7	199
1	11	20–25	22.1–28.4	7.8–41.2	17.1	42.9	35
2	9	15–17.5	16.2–19.1	7.8–28.4	7.7	48.7	39
3	10	17.5–20	19.1–22.1	10.5–41.2	10.3	41.4	29
4	9.5	15–17.5	16.2–19.1	5.1–41.2	6.7	43.3	30
5	12	25–35	28.4–41.9	16.2–171.5	16.1%	42.9	31
6	7	10–12.5	10.5–13.3	3.1–28.4	17.1	42.9	35

[a]Raw responses refer to the payoff alternative at which the subject first chooses to postpone payment. [b]Interval (%) refers to the discount rates defining the thresholds of the payoff alternative interval. [c]AER at the outer thresholds of 25th and 75th quartiles. This can be interpreted as a 50% confidence interval centered around the median.

the distribution of responses is skewed to the right, the median response is the appropriate measure of central tendency.[20] Median statistics are reported for the raw responses and their associated intervals.

Comparing Sessions 2–4 to the control (Session 1), the central tendencies of the raw responses suggest that the effect of the information conditions is to lower revealed discount rates. The front-end delay appears to have a negative effect on responses, as indicated by the increase in mean responses in Session 5 relative to the control. Finally, the mean revealed discount rate is lower in Session 6, in which payments are hypothetical.

Our statistical inferences should account for the likelihood that subjects are censoring their responses, as well as the possibility that the sample is not randomized across treatment conditions. Under the assumption that variability in the realized sample is adequately explained by the socio-economic characteristics of our subjects, we use parametric regression procedures to test the null hypotheses that our treatment conditions have no effect on subject responses.

4.2. *Statistical models of the data*

Our general model is[21]

$$y_i^* = \beta x_i + \epsilon_i$$

in which y_i^* is the subject's individual discount rate and is not directly observed, x_i is a vector of explanatory variables (including socio-economic characteristics and treatment variables), and ϵ_i is an error term. The observed counterpart to y_i^* is a variable y_i which is either an interval around y_i^* or censored at some limit. In general, we observe

$$\begin{aligned} y_i &= 1 \quad \textit{if } y_i^* \leq \eta_1 \\ y_i &= 2 \quad \textit{if } \eta_1 < y_i^* \leq \eta_2 \\ &\ \ \vdots \\ y_i &= J \quad \textit{if } y_i^* \geq \eta_J, \end{aligned}$$

where the threshold values η_j are known. If we ignore the issue of censoring at $r_{\rm L}^i$ and $r_{\rm B}^i$ then the η_j correspond to the interval limits in our MPL: $\eta_1 = 2.02\%$, $\eta_2 = 3.05\%, \ldots$, and $\eta_{J-1} = 171.45\%$.

Two econometric issues arise in our analysis: censoring and heteroskedasticity. Each is important for the correct interpretation of our results.

4.2.1. Censoring. If we have subjects arbitraging between lab and field incentives, then we should allow for censoring at field interest rates. In the debriefing questionnaire, we elicit from subjects the interest rates they currently face on their own savings accounts, CD's, "other" investment accounts, credit cards, lines of credit, and student loans. The descriptive statistics for these data are reported in Table 4.

Table 4. Descriptive statistics for subjects' reported interest rates.

Instrument	Mean	Std. dev.	Minimum	Maximum	N[a]
Credit card	14.79	3.84	4.90	22.00	111
Line of credit	13.98	5.83	3.00	22.00	26
Student loan	7.65	0.84	6.00	10.37	39
Savings account	3.34	2.85	0.5	23.00	57
Certificates of deposit	4.94	1.27	3.5	7.50	13
Other investments	11.35	8.78	0.00	28.00	20

[a]Refers to the number of subjects reporting that they possessed the credit market instrument and reporting the associated interest rate.

A. Censoring at borrowing rates. Because our design utilizes a two month time horizon, we believe that short-term borrowing instruments are the field substitutes subjects are most likely to consider. Hence, short-term rates are used for censoring purposes. The subject's borrowing rate (r_{B}^{i}) was calculated by taking the lowest of his *effective* credit card rate or line of credit rate (we found that banks typically charge simple interest on lines of credit while credit cards are typically subject to monthly compounding). We assume that subjects reported their annual rates. The effective credit card rate was calculated by first adding a 3% premium consisting of a crude average of the interest premium most banks charge on cash advances over purchases, then compounding monthly, and finally adding the 2% transactions fee banks generally charge for cash advances. If the subject did not have a credit card, or did not know her rate, we set her annual credit card rate equal to 17%. This is a crude average of credit card rates offered to students via advertising brochures distributed on campus. If the subject stated that she had a line of credit but did not know her rate, we set that rate equal to the current market rate of 18%.[22]

If the subject did not have a line of credit or credit card, our assumptions regarding that subject's borrowing rate depend on the treatment condition. Because the availability of credit cards is heavily advertised, while lines of credit are not, if the subject did not receive information on current market rates we assumed she would censor her response at the effective market credit card rate. When we provided information on market rates we explained that it was relatively easy to obtain a line of credit. Therefore, we assumed that subjects in those treatment conditions would censor their responses relative to the market line of credit rate.

Now, consider our previous example of an individual with a true IDR of 30% and a field borrowing rate of 14%. If she could not borrow in the field, we would expect this subject to switch to option B at interval 12, the first point at which AER is 30% or higher. If the subject arbitrages between the lab and field, however, she should switch to option B at interval 8, where discount rates are greater than 13.31% but no greater than 16.18%. Because this subject's r_{B}^{i} of 14% falls within this interval, if we assume she is cognizant of her field credit options and is acting rationally then all we can infer from her raw response is that her IDR is at least 14%. Thus, all responses which fall in the interval containing the subject's r_{B}^{i} are right censored *at* r_{B}^{i}.

While responses *at* r_B^i can be interpreted as right-censored observations, economic theory does not explain responses strictly *in excess of* r_B^i. One way to explain these higher responses is to recognize that credit market frictions do not allow us to observe the true cost of borrowing for every subject. For example, some subjects may be credit constrained at current (formal) credit market rates.[23] In other words, the subject may not currently be able to borrow additional funds at the r_B^i he provides in the debriefing questionnaire. Yet (informal) credit markets of a sort do exist for this type of individual, provided he is willing to pay the price. For example, he may obtain a loan at a pawn shop at a rate higher than those in formal credit markets. Alternatively, many subjects may borrow from their relatives or friends, but this often carries an implicit and subjective price as well. Furthermore, trading in credit markets carries transactions costs which are individual specific and unobservable. Thus, if we maintain the assumption that subjects are rational intertemporal utility maximizers, all we can infer about a subject whose raw response falls above his individual (observed) borrowing rate, r_B^i, is that his rate of time preference lies somewhere above the lower threshold of the interval corresponding to his raw response. In other words, we assume the subject's true (unobserved) borrowing rate falls within that interval and that her response is censored at this unobserved borrowing rate. Thus, all responses *above* r_B^i are right censored at the lower threshold of the IDR interval corresponding to the raw response.

B. Censoring at lending rates. Subjects who wish to substitute a field instrument for the lab instrument can add the $500 to an existing investment account or open a new investment account. We asked subjects to provide information on their savings accounts, CDs and money market or "other" investment accounts. Under the assumption that subjects provided annual rates, we converted the savings and CD rates to effective interest rates using daily compounding (consistent with general banking practices on these instruments). If a subject had a savings account or CD but did not know her rate, we set these rates equal to the current market (effective) rate of 3.0% and 3.76%, respectively. We then use the subject's highest rate of return as her lending rate (r_L^i). If a subject reported that she did not have any type of investment account, we assume she would consider a CD to be the closest substitute for the lab instrument (more lucrative investments typically require a minimum initial balance greater than $500) and set her r_L^i equal to the current market rate.

Consider our previous example of an individual with a true IDR of 3% and a field (effective) lending rate of 5%. If she could not save in the field, we would expect her to switch to option B at payoff alternative 2, the first point where AER is 3% or higher. If the subject is aware of her field opportunities, however, she increases her payoff by taking payment option A at any rate less than 5% and depositing the $500 in her field investment account earning 5%. We would not expect her to postpone payment in the lab until the lab instrument earns *at least* 5%. In this case, the arbitraging subject will switch to option B at interval 4, where discount rates are greater than 4.08% and no greater than 5.13%. Because her r_L^i of 5% falls within this interval, all we can infer from her response is that her IDR is no greater than 5% (although it could be lower). Thus, all responses which fall in the interval containing the subject's r_L^i are censored from below *at* r_L^i.

How do we interpret responses strictly below r_L^i? Field investment accounts carry transactions costs which lower the return on the investment. Because these transactions costs are unobservable and individual specific, we may not observe the true lending rate for all subjects. If we assume subjects recognize and act on arbitrage opportunities, all we can infer about a subject whose raw response lies below his observed lending rate is that his discount rate lies somewhere below the upper threshold of the interval corresponding to his true response. That is, we assume that the subject's true but unobservable lending rate falls within that interval and his response is censored at this unobserved lending rate. Thus, all responses *below* r_{L}^{i} are left-censored at the upper threshold of the interval corresponding to the raw response.

C. Summary. To summarize, the subject's response is defined as the interval in the MPL at which she first chooses payment option B (the later payment) over option A (the earlier payment). Denote the lower threshold of this interval as $\eta_{y_{i-1}}$ and the upper threshold as η_{y_i}. For example, if the subject's response is 10, then $\eta_{y_{i-1}} = 19.12\%$ and $\eta_{y_i} = 22.13\%$. Responses can be categorized as one of five types:

(i) If $\eta_{y_{i-1}} \leq r_{\mathrm{B}}^{i} \leq \eta_{y_i}$, the subject's response is censored from above at r_{B}^{i} (11% of all responses).
(ii) If $r_{\mathrm{B}}^{i} \leq \eta_{y_{i-1}}$, the subject's response is censored from above at $\eta_{y_{i-1}}$ (41% of all responses).
(iii) If $\eta_{y_{i-1}} \leq r_{\mathrm{L}}^{i} \leq \eta_{y_i}$, the subject's response is censored from below at r_{L}^{i} (4.5% of all responses).
(iv) If $\eta_{y_i} \leq r_{\mathrm{L}}^{i}$, the subject's response is censored from below at η_{y_i} (10.7% of all responses). Finally, if the subject's raw response falls strictly *above* r_{L}^{i} and strictly *below* r_{B}^{i}, then we assume her true discount rate falls within the interval corresponding to her raw response. That is:
(v) If $r_{\mathrm{L}}^{i} \leq \eta_{y_{i-1}}$ and $\eta_{y_i} \leq r_{\mathrm{B}}^{i}$, then we assume $\eta_{y_{i-1}} \leq \mathrm{IDR}_i \leq \eta_{y_i}$ (32.8% of all responses).

4.2.2. Heteroskedasticity. Heteroskedasticity is generally seen as a problem with model specification. Rutström (1998) suggests, however, that explicitly correcting for it can reveal significant effects of treatment conditions on the residual variances of responses. This is distinct from their direct effects on the mean response.

We expect *a priori* that the effect of our treatment conditions will be to reduce the residual variation in subject responses. If subjects are attempting to arbitrage with respect to field rates and have inadequate information with respect to market interest rates and/or the interest rates implied by the payoff alternatives, then they are likely to make (random) errors in choosing their best response. Providing information on implied rates and market rates would reduce these errors and hence the unexplained variability around market rates.

In addition, if incentives to research preferences are reduced when payments are hypothetical, then more subjects may respond randomly in the hypothetical session, and we may see an increase in unexplained variance. Alternatively, if incentives to research preferences are reduced, then we expect that subjects would be more likely to anchor on some focal rate of return, where the market rates supplied in the experimental instructions are the most likely focal rates. This would imply a reduction in the residual variance of responses when payments are hypothetical relative to when they are real.

We expect that the residual variance of responses may be affected by the socio-economic characteristics of our sample. To account for heteroskedasticity, we assume the variance of the error term for subject i can be expressed in multiplicative form as $\sigma_i = \exp(\gamma' z_i)$, where z_i is a matrix of explanatory variables and γ is a vector of parameters to be estimated (Greene, 1993, pp. 405–407).

4.2.3. ***Estimation results.*** Table 5 presents equation estimates assuming a censored dependent variable and corrected for multiplicative heteroskedasticity. The model is estimated using maximum likelihood estimation assuming the errors are normally distributed. Due to missing values for some observations, the estimates are based on a sample size of 177. Each treatment condition enters the equation as a binary variable. ARAER is a dummy variable equal to one if information on interest rates implied by choosing payoff option B over option A is provided. MKT is a dummy variable equal to one if information on current market rates is provided. ARMKT is the interaction between ARAER and MKT (equal to ARAER $\times$ MKT). REAL is a dummy variable equal to one if the treatment incorporates real money payments. FED is a dummy variable equal to one if the session includes a front-end delay. Socio-economic characteristics are defined as follows: AGE is in years, SEX is a dummy equal to one for males, RACE is a dummy equal to one for non-whites, HH is the number of household members (HH2 is equal to HH squared included to capture any non-linear relationship with household size), and HHY and PARY are household and parents' income, respectively, in thousands of dollars.[24] Because our subject pool consists of students, who tend to have low incomes *while in school* and often rely on parents for financial support, parental income may be a better measure of income for our purposes. Parental income may also be a proxy for expected future income.

The overall model is significant at all conventional levels. A likelihood ratio test rejects the hypothesis that the errors are homoskedastic at all conventional levels.

The results in Table 5 indicate that providing information on interest rates associated with payment option B (ARAER) has a statistically significant negative effect on mean responses. This is consistent with subjects in the control session *underestimating* the interest rates implied by their decisions. This treatment also has the effect of significantly reducing the variance of revealed discount rates, consistent with a decline in subject errors in formulating a best response.

Providing information on available market rates (MKT) also has a statistically significant negative effect on mean responses. This is consistent with subjects in the control session *overestimating* the investment opportunities available to them in the field. This treatment also has the effect of significantly reducing the variance of revealed discount rates, suggesting that providing this information serves to reduce subject errors in formulating a best response.

An alternative explanation for the statistical effects of MKT is that subjects in sessions where market rates are provided are anchoring on the rates we provide (rather than considering arbitrage possibilities with respect to their own opportunities). To the extent possible, we investigate this explanation by considering likely effects of such anchoring behavior. In particular, if real payments are salient (as the evidence reported below suggests), then we would expect anchoring behavior to be more pronounced in the hypothetical session (where market rates are provided). This would then cause the residual variance of responses and/or

Table 5. Maximum likelihood estimates of the IDR model.

Variable	Coefficient	Std. error	t-ratio	P-value	Mean of X	Std. Dev. of X
Effects on mean response						
Constant	133.861	85.082	1.573	0.11564		
AGE	1.342	1.431	0.938	0.34829	21.757	3.526
SEX	13.494	7.290	1.851	0.06417	0.559	0.498
RACE	20.814	9.989	2.084	0.03719	0.283	0.452
HHY	0.788	0.327	2.413	0.01583	22.062	25.920
PARY	0.254	0.091	2.786	0.00533	65.283	39.018
HH	64.283	54.778	1.174	0.24059	1.458	0.941
HH2	−14.181	11.095	−1.278	0.20120	3.006	4.546
ARAER	−65.764	23.946	−2.746	0.00603	0.531	0.500
MKT	−70.742	23.751	−2.979	0.00290	0.480	0.501
REAL	−53.394	27.317	−1.955	0.05063	0.831	0.376
ARMKT	63.353	25.695	2.466	0.01368	0.164	0.371
FED	−96.001	69.631	−1.379	0.16798	0.853	0.355
Effects on residual variance						
Constant	6.374	2.097	3.039	0.00238		
AGE	0.057	0.079	0.722	0.47030		
SEX	0.564	0.477	1.182	0.23705		
RACE	1.291	0.588	2.196	0.02810		
HHY	0.025	0.014	1.763	0.07796		
PARY	0.006	0.006	0.863	0.38833		
HH	4.718	1.943	2.429	0.01515		
HH2	−1.086	0.450	−2.416	0.01569		
ARAER	−2.346	0.759	−3.093	0.00198		
MKT	−2.525	0.825	−3.059	0.00222		
REAL	−1.784	1.193	−1.495	0.13487		
ARMKT	1.449	1.174	1.235	0.21679		
FED	−1.193	1.196	−0.998	0.31841		

Log-Likelihood: −318.5192
Restricted (Slopes = 0) Log−L.: −366.1344
Chi-squared (20): 95.2304
Significance Level: 0.0000001
Number of observations: 177

mean responses to be *lower* in the hypothetical treatment. However, hypothetical payments have a marginally significant *positive* effect ($p = 0.13$, two-tailed) on the residual variance and a significantly *positive* effect ($p = 0.05$, two-tailed) on mean responses.

To provide further evidence on a possible anchoring effect, in Session 3 (where market rates are provided) and in Session 5 (where market rates are not provided), we ask subjects to explain why they preferred the delayed payment in those cases where they chose payment option B. Although subject responses were in general quite similar in the two sessions, the subjects in Session 3 (where market rates and arbitrage opportunities were explicitly discussed) mentioned *more* diverse field investment opportunities in their responses. Three of the subjects in this session did explicitly mention a comparison to the CD rate (CDs were presented in the instructions as one arbitrage possibility). Further investigation of these three subjects indicates that they did not have any investments at the time of the experiment. Because they were likely not well informed about credit markets, these subjects may have focused on the market instruments we suggested. It does not appear that everyone in the session was similarly affected (subjects with other credit market activities mentioned arbitrage considerations with respect to their own opportunities). Thus, while the arbitrage possibilities we suggested likely caused some subjects to focus on those instruments, it appears that those who did so had no prior focal point. In other words, subjects without information on credit market possibilities focused on the suggested instruments, but subjects with prior knowledge of their opportunities were not induced to *shift* their focus. It thus appears that while the provision of market rates was successful in providing information to those who were uncertain of their field opportunities, it did not cause a significant anchoring effect.

The interaction between ARAER and MKT (ARMKT) has a statistically significant *positive* effect on mean responses. A simple interpretation of this result is that while providing information on *either* interest rates associated with payment option B *or* on available market rates lowers mean IDR, providing information on *both* does not further significantly reduce mean IDR. The magnitude of the coefficients on ARAER, MKT, and ARMKT supports this interpretation. Providing information on the rates associated with payment option B lowers revealed discount rates by over 65 percentage points, while providing information on market rates lowers revealed discount rates by over 70 percentage points. However, when both types of information are provided, revealed rates are lowered by just over 73 percentage points (-65.764 coefficient on ARAER; -70.742 coefficient on MKT; $+63.353$ coefficient on ARMKT).

Providing real (vs. hypothetical) payments also has a statistically significant negative effect on mean responses (REAL). This is contrary to the effect apparent from the raw data reported in Table 3. This is likely due to the fact that subjects were not adequately randomized into treatment cells, so that subject demographics are correlated with the treatment. Indeed, we find that the session with hypothetical payments contained a substantially higher proportion of non-white participants and somewhat lower mean household and parental incomes. Once these effects are controlled for in the regression, we find that revealed IDR are higher when payments are hypothetical, not lower as suggested by the raw data.

The significant difference in behavior between payment conditions suggests that subjects were influenced by the possibility of actually receiving one of the payoff alternatives.

Moreover, we find that real payments result in a marginally significant reduction (p-value $= 0.13$) in the unexplained variance of responses, suggesting that subjects respond in a more random manner when payments are hypothetical. Further evidence on the perceived salience of the payoffs is provided by responses to debriefing questions. In Sessions 3 and 5, subjects were asked two questions regarding their decisions. The first asked, "(If) you are chosen to be the Assignee, ... what do you plan to do with the money you receive?" The second asked, "(if you chose payment option B for any of the payoff alternatives), ... why did you choose to delay receiving your payment?" These open-ended questions were asked (in a non-leading way) in order to determine how seriously the subjects took their decisions and whether they mentally placed themselves in the position of being the Assignee as they made their decisions. All subjects listed specific uses for the money in response to the first question. Common responses included paying off (or paying down) credit cards or other debt, savings, or the purchase of specific items. Notably, no subject answered "don't know" or otherwise indicated that they were unable to visualize receiving the money. In response to the second question, subjects also had specific reasons for preferring option B. Although some simply said they preferred "more money," many mentioned the return being greater than the interest they could earn in the market or greater than the interest that would accrue on their outstanding debt. Taken together, these results support that subjects were influenced by the real monetary payoffs being offered and took the decisions they made between payoff options seriously.

Table 5 indicates that the effect of the front-end delay is significant at the 92% confidence level in a one-tailed test (the table reports p-values for two-tailed tests). While the expected effect was to eliminate a "bird-in-the-hand" influence on responses, the results are also consistent with the hyperbolic discount rates found in previous studies.[25] If the latter is true, our results are not directly comparable to previous studies which elicit spot rates. Although our hypothesis tests remain valid with respect to our treatments, our results may only strictly apply to "forward decisions." Even this strict interpretation is relevant to many intertemporal decisions of interest; while consumer purchase decisions may be considered spot decisions, private investment (e.g., retirement, education, etc.) and public policy decisions are most often forward decisions in the sense that some period of time is typically involved between the time when the decision is made and the investment takes place.

We also note that mean IDR appears to be affected by some socio-economic characteristics. The results indicate that males have marginally significantly higher discount rates than females; males on average reveal discount rates that are over 13 percentage points higher than those revealed by females. This is consistent with the gender effect reported in Kirby and Maraković (1998). Race has a significant direct effect on discount rates; non-whites on average reveal discount rates that are nearly 21 percentage points higher than those revealed by whites. This effect is consistent with results reported in Lawrance (1991). In addition, the variance of revealed IDR is significantly higher for non-whites. The estimates on HHY and PARY indicate that income and discount rates are positively correlated. This is inconsistent with previous results; Hausman (1979) and Lawrance (1991) find that discount rates and income are inversely related. Horowitz (1991) and Pender (1996) find that discount rates and wealth are inversely related. It is possible, however, that a positive relationship between income and discount rates is an artifact of irregular income flows and expectations of future income increases that are unique to student subjects. Finally, there is a convex relationship between household size and the variance of revealed IDR. The results indicate that the variance of responses is highest at a household size of 2. This suggests that subjects with a spouse (or the equivalent) may be more uncertain of the optimal choice for the household unit.

5. Discussion

The primary goal of this study was to elicit IDR using a more controlled experimental design. We explicitly recognize that, while the existence of field substitutes for the laboratory instrument is likely to influence subject responses, subjects may have difficulty evaluating the lab investment relative to their field opportunities. Our main treatments involve providing information which should help subjects make this comparison and better evaluate their options. We also recognize that if subjects are substituting field instruments for lab instruments, then their responses will be censored with respect to field instruments, and we account for this within the data analysis.

Median AER across all experimental sessions is in the 19.1%–22.1% range (implying an overall median AR in the 17.5%–20% range). When information on both the rates associated with future payment options and on available market rates is provided, median AER is in the 16.2%–19.1% range (AR is in the 15%–17.5% range). These median rates are substantially lower than those found in prior studies and are consistent with prevailing market borrowing rates.

We find significant direct effects of each of our information treatments. Providing information on *either* the AR (and AER) associated with future payments or on available market rates significantly reduces revealed IDR. However, providing *both* types of information does not reduce IDR substantially beyond the levels achieved by providing either type of information. It thus appears that providing either type of information served to lower revealed IDR to levels comparable with market (field) borrowing rates. These results are consistent with the conjecture that (some) subjects are attempting to arbitrage with respect to field opportunities, but have difficulty doing so because they are unfamiliar with field opportunities and/or they are unable to determine rates implied by payment options. Each treatment also reduces the residual variance of the observed discount rates. This is consistent with subjects making fewer random errors when either type of information is available.

We find that both the mean and unexplained variance of revealed discount rates are lower when incentives are real relative to when they are hypothetical. To our knowledge, this is the first direct test of the effect of real vs. hypothetical payments on revealed discount rates. While empirical regularities have been observed in the extant literature across experiments using both real and hypothetical payments, our results suggest that the use of real incentives can affect behavior, at least in some contexts.

We also find that the individual discount rates we elicit are sensitive to some socio-demographic characteristics of individuals and often differ from generally observed market rates. This suggests that care must be taken when applying market rates as substitutes for individual discount rates.

Finally, we note that the existence of field substitutes for the laboratory commodity is a general problem for experimental economics. The methods we use to address the issue of censored responses may appear complicated in this study. However, the complexity is due to difficulties in ascertaining subjects' perceptions of their field opportunities. In other experiments, where field substitutes are better defined, these methods would be quite simple to employ. They should thus be applicable to a wide range of experimental studies.

Acknowledgment

This paper is based upon William's Ph.D. dissertation from the University of South Carolina, Columbia, South Carolina. All of the opinions and conclusions are those of the authors, and have not been endorsed by their employers or the funding agency. We are grateful to Richland County South Carolina Government and to the University of South Carolina Friends of the Accounting Department for funding. We thank McKinley Blackburn, Jim Cox, Ron Cummings, Phillip Grossman, Ron Harstad, Glenn Harrison, Peter Morgan, John Pender, Nat Wilcox, and participants at seminars in Wilmington, NC, the University of South Carolina and the Economic Science Association meetings (Orlando and Tucson) for helpful comments.

Notes

1. Field studies have also documented high IDRs, ranging from 17% to 300%, in examining consumer purchases of home weatherization and electrical appliances and the associated tradeoffs between purchase prices and delayed energy payment (see, for example, Gately, 1980; Hartman and Doane, 1986; Hausman 1979; Ruderman et al., 1986).
2. Other studies have found implied discount rates that decline with time horizon and magnitudes of rewards (see Thaler, 1981; Ainslie and Haendel, 1982; Benzion et al., 1989; Carlson and Johnson, 1992; Shelley, 1993; Kirby and Maraković, 1996; Pender, 1996). In many previous studies, subjects face real economic incentives (Ainslie and Haendel, 1982; Horowitz, 1991; Carlson and Johnson, 1992; Holcomb and Nelson, 1992; Lazo et al., 1992; Kirby and Maraković, 1996; Pender, 1996). However, similar patterns of behavior have been observed in experiments incorporating hypothetical payoffs (Thaler, 1981; Ainslie and Haendel, 1983; Benzion et al., 1989; Winston and Woodbury, 1991). Using hypothetical incentives, Loewenstein (1988), Benzion et al. (1989), and Shelley (1993) find that question frames appear to systematically affect revealed discount rates, e.g., discount rates tend to be higher for delayed receipts than for expedited payments.
3. This same subject reported that he earned 28% on a stock account and reported no other more profitable investments. One possible explanation for his discount rate choice in light of his stated field investments is that he was unable to accurately compare the lab and field instruments (i.e., he felt that his stock account would yield more than a $90 return on $500 invested for two months).
4. The problem of censored responses is a general problem when "homegrown" values are elicited in the experimental laboratory (as distinct from induced values).
5. As will be detailed further, the experiment design allows us to elicit individual discount rates only within some interval.
6. The classical discounting model predicts that individuals will equate their discount rate, at the margin, with the market interest rate. Though we are not attempting to test the classical model, note that both information conditions discussed in this study would be necessary for subject behavior to conform with its predictions.
7. Wagenaar and Sagaria (1975) report that individuals tend to underestimate exponential functions and that this downward bias can be substantial. Thaler (1981) suggests that arithmetic errors may contributed to the negative effects of time frame on revealed discount rates. Our own experiences in teaching time value concepts support this hypothesis. We also tested this notion in an informal focus group. Upper level undergraduates in accounting were given a short "pop quiz" involving present and future value calculations consistent with those

required in experiments found in the literature. Though we could not provide the focus group participants with substantial incentives for correct answers, we did observe that most participants seemed to take the task seriously: on average, participants took 10 minutes to complete a three question test and most used calculators. Although these students had been trained in time value of money concepts and applications, a surprising number of (good) students were unable to answer the questions correctly. This strongly suggests that people in general may be unable to perform such calculations "off the cuff."

8. Note that there is an important field counterpart to this treatment condition; Truth in Lending/Savings laws require disclosure of both the annual and effective interest rates associated with credit market instruments. Hence, this treatment serves to provide information in the laboratory that is consistent with the information available to individuals in formal credit markets.
9. All instructions and information were provided in written form. Whenever possible, questions were answered by repeating the appropriate section of the written instruction packet. The full instruction packet is available upon request.
10. Such a procedure has also been utilized in prior experimental studies (Camerer and Ho, 1994; Kirby and Maraković, 1996). While it does allow us to use high monetary stakes, it also reduces the expected value of the payoff. Possible effects on salience are further discussed in Section 4.2.
11. This certificate was signed by a faculty member and was redeemable for a university check or cashier's check (on or after the appropriate date) in the office of the departmental Administrative Assistant.
12. $500 is a large enough sum to establish various types of investments. For example, most area banks require a minimum deposit of $500 for new Certificates of Deposit (which typically provide a higher rate of return than passbook savings accounts).
13. Another way to ensure credibility and equalize transactions costs might be to issue post-dated checks on the day of the experiment. However, we found that banks could not guarantee that post-dated checks would not be honored before the payment date. If the subject was aware of this fact, then she should always choose payment option B regardless of her discount rate since she could immediately obtain the highest payoff. Attempts to find alternative instruments that could be issued the day of the experiments met with similar deficiencies.
14. Our design contains a hypothetical treatment, in which case the trainer payoff was hypothetical as well.
15. Ideally, we would prefer to preserve the Assignee's anonymity, but in the interests of future credibility we felt that participants should actually observe that someone was chosen to receive payment.
16. Daily compounding is consistent with general banking practices on Certificates of Deposit (CDs), the instrument which most closely resembles option B. Subjects could choose payment option A and place the $500 in a 2–3 month CD. This CD requires a minimum deposit of $500 and has a penalty of three months interest for early withdrawal. Provided the subject finds this penalty binding, the CD resembles payment option B in that it "locks" the Assignee's funds into the two month investment. When alternative field instruments are compared, the AER is the appropriate rate to consider. This is the rate that determines the dollar amount of interest, as it includes the effects of compounding. Thus, subjective discount rates correspond to an AER. Because market rates are stated in annual terms by convention, we also report the AR implied by our subjects decisions in Section 4.
17. Nevertheless, we cannot reject the salience of payoffs in the absence of a significant effect. Subjects could respond similarly in both situations for two reasons: (i) they respond in the hypothetical treatment just as if the payoff were real, or (ii) they respond in the real payoff treatments just as in the hypothetical treatment because the possibility of receiving the real payoff is not salient.
18. The debriefing questionnaire, coding methods, and raw data are presented in appendices (available on request).
19. Four out of 199 subjects failed to respond in the expected monotonic fashion. For example, one subject switched from payment option A to payment option B at payoff alternative 10, then switched back to A for payoff alternatives 13 through 15. In such a case we chose the most conservative interval as his "true" response. This particular individual's response is coded as a 16, indicating his IDR is greater than 171%.
20. Because some subjects always choose immediate payment, calculating the mean requires choosing a trunction point for the right tail of the distribution of discount rates. The choice of a truncation point is arbitrary, and we find that the mean is sensitive to the particular point chosen.
21. Greene (1993) provides a standard exposition of this model as well as subsequent variations.
22. This procedure assumes that subjects who arbitrage with respect to their credit cards will use a cash advance. An alternative assumption is that subjects will make direct charges, in which case their base annual rate

(compounded monthly) will apply. We find that the estimated model is robust across alternative assumptions.

23. The debriefing questionnaire includes a question designed to elicit subjects' perceptions of their creditworthiness. Responses indicate that 53% of our sample believe they are currently less than 90% likely to be approved for credit, and 13.6% claim they are less than 50% likely to be approved for credit.
24. Income is elicited in the form of intervals. We recorded these responses using the mid-point of the interval and $125,000 for those reporting income over $100,000.
25. While several prior studies find discount rates that decline over increasing time horizons, the effects of a front-end delay remain unclear. Winston and Woodbury (1991) manipulate the length of the front-end delay and find that subjects are more impatient with respect to more immediate delays. Alternatively, Holcomb and Nelson (1992) find no significant effect in a direct test of a front-end delay. In a pilot study, Pender also tested for the effect of a front-end delay and found no effect (personal correspondence, dated November 1997).

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