STEVEN R. BECKMAN
JOSHUA N. FOREMAN

An Experimental Test of the Baumol-Tobin Transactions Demand for Money

THE BAUMOL-TOBIN (B-T) TRANSACTIONS DEMAND for money is a cornerstone of macroeconomics and yet the theory has never been subjected to a statistically reliable test. Such a test based on historical data would require, at a minimum, identification of money demand, separation of the transactions demand from other motives, and inclusion of all important determinants of money demand as explanatory variables. No previous study of money demand satisfies these criteria. Experiments offer the opportunity to examine behavior when only a transactions motive for money demand exists; experimenters need not create a supply of money or other money-holding motives in order to create a transactions demand. The theoretically important determinants of the isolated transactions demand may be introduced as nonstochastic controls.

In addition, the experimental environment allows a test of bounded rationality. Simon (1982, p. 405) defines rationality as "a style of behavior (A) appropriate to the achievement of given goals, (B) within the limits imposed by given conditions and constraints." Objective rationality assumes all the conditions and constraints are "outside the skin of the rational actor" (Simon 1982, p. 409); bounded rationality allows the nature of the organism to impose its own condi-

The authors gratefully acknowledge the comments of William Baumol, Thomas Cooley, Milton Friedman, Elizabeth Hoffman, Glenn Harrison, Ron Harstad, David Laidler, Thomas Mayer, Jack Ochs, Charles Plott, discussants at the Winter 1984 Econometric Society meetings, and several anonymous referees. Mark Van Boening and Leonie Stone provided many hours of graduate assistance. The research was funded by a grant from Wichita State University.

STEVEN R. BECKMAN *is assistant professor of economics, Wichita State University.* JOSHUA N. FOREMAN *is president of Foreman Econometrics.*

Journal of Money, Credit, and Banking, Vol. 20, No. 3 (August 1988, Part 1)

tions and constraints. The B-T model assumes objective rationality but our subjects exhibit bounded rationality. Even so, the B-T model remains a powerful guide to their behavior.

1. THE BAUMOL-TOBIN MODEL

T. M. Whitin (1952) described an inventory control problem that Baumol (1952) adapted to the demand for money. Tobin's independent work (1956) is more readily adapted as an experiment. Tobin assumed the individual receives a lump-sum income payment at the beginning of the month. The income is spent at a constant rate so that income and the month run out together. Individuals have the option of buying bonds (which earn simple interest) during the month, but must eventually sell these bonds to meet transactions needs. The problem is that each cash-bond conversion costs a fixed fee: on one hand, individuals want to earn interest, but, on the other, want to avoid the conversion fee. Tobin developed four axioms of rational behavior in this situation:

(A1) Purchase bonds only on the first day of the month; delaying a bond purchase needlessly reduces interest earnings.

(A2) Sell bonds only when the inventory of cash is exhausted; early sales reduce interest earnings without benefit.

(A3) Never purchase and sell bonds on the same day; a single transaction can achieve the same net effect.

(A4) Cash inventories after bond sales (or immediately after bond purchases at the start of the month) should all be the same size. As a corollary, because cash inventories have the same initial size, are depleted at the same rate, and are replenished only when exhausted, all replenishments are equally separated in time; we call this time between cash-bond exchanges a period.

The optimal program for cash-bond management may be derived using these four axioms. First construct a candidate program with n periods; if income (and total transactions needs) $= T$, all periods will have equal initial cash inventories $= T/n$. If income is spent at rate t, then the duration of each period is $T/(nt)$. The revenue function for an n-period program is

$$r[\mathrm{T}/(nt)] \sum_{i=1}^{n} [T-(T/n)i] \ . \tag{1}$$

$T-(T/n)i$ is both the unspent income at the end of period i and the optimal bond holdings during period i. Therefore (1) is the interest opportunity cost, r, multiplied by the length of a period, $T/(nt)$, and the sum of bond holdings over the periods $i, \sum_{i=1}^{n} [T-(T/n)i]$. The next step is to use the revenue function to construct the table of marginal revenue as n increases. Marginal revenue is always positive and nonincreasing. Comparing marginal revenue with marginal cost (one additional brokerage fee) allows selection of the optimal n.

This is a difficult maze to negotiate; "real world" behavior in this environment is clearly an open question. Simon believes people stumped by a computational problem "satisfice"; satisficing is a strategy "that sets an aspiration level, searches until an alternative is found that is satisfactory by the aspiration level criterion, and selects that alternative" (Simon 1982, p. 415).

Experiments allow an indirect examination of the strategies employed by subjects. While the subjects' thoughts go unrecorded, an extensive record of cash-bond exchanges is available for examination. Do subjects conform to any of the four axioms of objectively rational behavior? Do they select the optimal program, or at least the correct number of periods? If they satisfice, can their aspiration levels be changed by altering incentives?

2. PREVIOUS TESTS OF THE BAUMOL-TOBIN MODEL

The most common method used to test the B-T model is to estimate a log-linear money demand function from historical data. This is based on Baumol's square root rule: if b is the fee for exchanging bonds and cash, then (ignoring integer contraints) optimal average money holdings are given by $\sqrt{\frac{bT}{2r}}$. Although the data reflect many other money-holding motives and Barro (1976) proved that the integer constraints can significantly alter these elasticities, if the estimated elasticities are close to the optimal values derived from the square root rule, the result is regarded as a confirmation of B-T theory.

However, these studies are not a test of any particular money demand hypothesis. Cooley and Leroy (1981) criticized this work by noting that the demand curve is not identified and that the choice of independent variables is arbitrary. Laidler (1982, p. 56) argued that the estimated relationship is a mixture of the long-run demand for money and the reduced form of a large macro model of the money-income relationship. Gordon (1984), following Laidler's hypothesis, concluded that the failure of Goldfeld's equation (1973) to track money demand after 1973 can be explained by supply shocks that altered the reduced form. Judd and Scadding (1982, p. 1014) conjectured that the Goldfeld (1976) problem can be explained by unobserved changes in the fee for exchanging bonds and cash. This fee probably reflects cost of inconvenience as well as restrictions on the use of funds; financial innovations in the 1970s, motivated by high interest rates, may have confounded the money-interest rate relationship.

These studies are interesting, but none tests the B-T theory under the theory's assumed conditions.

3. BASIC STRUCTURE OF THE EXPERIMENT

The experiment was conducted in a discrete time format. We were careful to avoid words like money, bonds, or interest in the instructions. We used a randomized block design and divided subjects into three groups with differing reward schemes. Each of these basic design decisions is explained in turn below.

An experiment is necessarily stylized; the experimenter does not attempt to create the world in miniature in his lab, but rather abstracts certain salient aspects. It would be a mistake to attempt to replicate the B-T environment by rescaling time, such as by setting 1 hour = 1 month. If we are to test the willingness of subjects to optimize, we must allow them to control their time. For this reason, it was convenient to design the experiment in discrete time. Subjects advanced from one discrete time unit to the next as rapidly or as slowly as they chose. They could stop at any time to make additional calculations, or they could choose not to stop. The basic unit of time is a month, with 12 divisions called days, because 12 is a small number evenly divisible by 1, 2, 3, 4, and 6; this abundance of integral divisors permits a wide variety of cash management programs. Parameters were selected so the optimal programs required two, four, or six periods; neither those integral constraints imposed by our experimental design nor those inherent in the B-T model were binding on the optimal programs.

In order to avoid any preset ideas about money management, money was called "mayers" and bonds were renamed "plotts"; the names honor our teachers, Thomas Mayer and Charles Plott. This renaming was essential to preserve the validity of induced value theory (Smith 1976, p. 228). Value is properly induced if the experimental rewards are the subjects' only motivation; familiar terms may evoke other motives. In this experiment, rewards (interest—brokerage fees) were denominated in points.

The renaming does impose real costs; it takes time to explain how points, and ultimately dollars, are earned if one is careful to avoid phrases such as "interest earnings" or "brokerage fees". Using the familiar terms would have enabled us to use shorter instructions, but would have cued subjects to extrapolate from past experiences which occurred in different environments; applying lessons learned there might have misled them in the experiment and would have been a needless complication for analysis of the results.

The experiment employed a randomized block design: each subject received three parameter sets (treatments) but the order of the parameter sets was randomized. This allows analysis of effects (both subject- and parameter-specific) without any systematic bias due to any unknown order effects, such as learning or fatigue. For each subject, either the interest rate or the brokerage fee was varied but not both. Interest rates and brokerage fees were coordinated so optimal programs with two, four, and six periods were available to each subject.

There were three different reward structures: high-pay, low-pay, and course credit. The ultimate dollar rewards earned by high-pay subjects were four times as great as those received by low-pay subjects for equal performance.

Course credit subjects could earn up to 50 of the 400 points that determined their grades in a money and banking course; each letter grade above F occupied a range of roughly 60 points. These subjects received a fraction of 50 points equal to the ratio of their hypothetical dollar reward to the maximum dollar reward available. We believe this use of course credit was ethical because money demand was a routine part of this course.

The randomized block design coupled with the three reward structures allow some tests of Simon's satisficing hypothesis. If subjects search through strategy space until an aspiration level is achieved, then individuals may attempt to transfer previously discovered strategies across treatments; such transmissions should be related to the reward structure.

4. PARAMETERS

The parameter sets and optimal number of periods are listed in Table 1; each subject received either the first three or the last three sets. Two additional numbers presented in Table 1 were used to calculate subjects' payments. The first is the minimum number of points needed; subjects were paid for points in excess of the minimum. We did this to maximize the difference between rewards for different behavior while keeping our costs low. This minimum point value was always about half the maximum number of points available. For example, with interest rate = 1 and brokerage fee = 1,500, maximum point earnings = 600. Six hundred points minus 300 (the minimum) leaves 300 points to be converted at the stated rate, $0.25 per 25 points. Therefore, the maximum reward for a low-pay subject under this treatment is $3.00; a high-pay subject could receive up to $12.00 with these parameters.

Table 2 displays the payments a low-pay subject earned if he used programs with equal-length periods (but not necessarily the optimal number of periods); this table gives some indication of how rewards varied with behavior. (For high-pay subjects, quadruple these payments.) Most subjects never learned to use periods of equal length, so we observed dozens of possibilities not listed here. Each column of the table applies to some optimal number of periods (given variations in either the interest rate or the brokerage fee); rows pertain to the subject's hypothesized behavior. For example, the entry of $1.00 in the last column of the first row means that, if a subject had the parameters for the six-period brokerage fee treatment but used the best two-period program, he earned $1.00.

The gradient of rewards with respect to decisions is very steep under two-period treatments; when the optimal $n = 2$, it is very difficult for a subject to earn any positive reward without at least approximately optimizing. This caused

TABLE 1
PARAMETER VALUES

Income	Daily interest	Brokerage fee	Minimum points	$.25 = ___ pts.	Optimum n
1,200	1	1,500	300	25	2
1,200	3	1,500	5,000	425	4
1,200	6	1,500	13,000	1,225	6
1,200	2	3,000	600	50	2
1,200	2	900	3,600	300	4
1,200	2	400	4,800	400	6

TABLE 2

REWARDS FOR EVEN-PERIOD PROGRAMS

Behavior	Two-period interest	Two-period fee	Four-period interest	Four-period fee	Six-period interest	Six-period fee
	Treatment					
Two-period	$3	$3	$1.75	$1.50	$1.25	$1.00
Four-period	$0	$0	$3.25	$3.00	$2.75	$2.75
Six-period	$0	$0	$3.00	$2.50	$3.00	$3.00

subject frustration during the experiment but permitted a test of satisficing; possibly a harsher environment compels behavior that is more nearly optimal. In contrast, the four- and six-period treatments generate reward structures with flatter gradients; satisficing may generate heteroskedasticity associated with treatment if subjects' behavior is closer to the optimal program under two-period treatments. Further, a subject using a four- (or six-) period program when six (or four) periods are optimal does not suffer the dramatic loss in efficiency that results from using this program when the optimal $n = 2$; this could lead to errors correlated across the four- and six-period treatments as subjects transfer nonoptimal strategies across environments.

5. PROCEDURES

Each group of (about twelve) subjects worked in a room with forty microcomputers; photographs of experimental sessions are available from the authors. Each microcomputer had its own keyboard, printer, and monitor. A set of instructions was read establishing the B-T environment and familiarizing subjects with the computer program[1] by providing running commentary as subjects completed a sample month. Long pauses as subjects completed tasks accounted for part of the instructions' 50-minute length. The sample month scrupulously avoids teaching strategies, and the instructions were read by four different experimenters to minimize any bias that might be introduced from this source.

After the instructions were read and questions answered, each subject received his first set of parameters, entered them into his computer, and was then free to try different programs managing his cash and bonds over the month. At the end of the month, his computer printed out a record of his decisions that month and the resulting reward; Figure 1 displays a sample printout. The subject could choose to turn in a printout and proceed to the next treatment, or to repeat the month, attempting to improve his reward; subjects could leave after they completed the third treatment. Only the record for the best month determined the subject's reward.[2]

[1]The program and instructions are available from the authors.

[2]The "best" month was defined as the one with the highest earnings; ties were broken by choosing the month with the highest number of points; any further ties were broken arbitrarily.

Jonathan R. Subject 570-34-1234

Today is 11-01-1987
The time is 10:02:49
The earnings rate is 3 and the point charge is 1500
Each set of 400 points beyond 5000 earns $.25

Your Account History

day	mayers (start)	plotts sold	daily charge	plotts bought	mayers (end)
1	1200	0	100	900	200
2	200	0	100	0	100
3	100	0	100	0	0
4	0	300	100	0	200
5	200	0	100	0	100
6	100	0	100	0	0
7	0	300	100	0	200
8	200	0	100	0	100
9	100	0	100	0	0
10	0	300	100	0	200
11	200	0	100	0	100
12	100	0	100	0	0

Your Point Earnings and Charges

				points charged for:	
day	plotts (start)	plotts end	point earnings	plotts sold	plotts bought
1	0	900	2700	0	1500
2	900	900	2700	0	0
3	900	900	2700	0	0
4	900	600	1800	1500	0
5	600	600	1800	0	0
6	600	600	1800	0	0
7	600	300	900	1500	0
8	300	300	900	0	0
9	300	300	900	0	0
10	300	0	0	1500	0
11	0	0	0	0	0
12	0	0	0	0	0

Point earnings of 16200 less point charges of 6000
leaves you with 10200 total points through day 12 .
You have earned $ 3.25

FIG. 1. A Sample Printout

While there was no limit to the number of times a subject could repeat a month, there was an overall time limit of two hours and ten minutes after the instructions were finished, so experiments lasted about three hours. Subjects completed from 6 to 30 months and used between 38 minutes and the full 130 minutes allowed. The average subject completed 16 months in 102 minutes; 25 subjects (20 percent of the total) were still working five minutes before time was called.

The experimental month begins with day 1; the subject is initially endowed with 1,200 units of cash and no bonds. Each of the 12 days of the experimental month is the same. First, the computer asks how many bonds the subject wants to sell, while displaying the allowable range of bond sales. Short sales of bonds are not allowed, so the maximum bond sale equals the number of bonds held.

The subject must pay a 100-unit daily charge (representing transactions) and so must begin each day with at least 100 units of cash. Thus the minimum bond sale is the number needed to bring the cash account up to at least 100.

Any entry outside the allowable range triggers an error message and a repeat of the prompt to sell bonds. After an allowable bond sale occurs, the computer deducts 100 units from the cash account. Next, each subject is asked how many bonds he wants to buy. Again, the computer displays the allowable range; the maximum number equals the number of units in the cash account because cash and bonds exchange one for one; the minimum number is always zero. Thus the subject makes two cash-bond exchange decisions per day until the end of the month, when the final 100-unit charge reduces his stock of both assets to zero.

After the day has been completed, the computer displays the history of the month to date: a day-by-day record of cash, bonds, bond sales and purchases, interest earnings (called point earnings), and brokerage charges (called point charges). Finally, the computer converts the point earnings and charges into this month's cash earned to date; this dollar reward (or equivalent course credit) is paid if the subject turns in this particlar record of the month's decisions.

When a subject turned in his best monthly printout for a set of parameters, all printouts for that month were collected before he was given the next parameter set. Therefore, subjects had access to these records only as long as they repeated the current month, and they could not return to a previous month once they had gone on to the next treatment.

All scratch paper was collected and examined after each experimental session; we discovered only one clear example of a marginal revenue schedule.

The subjects were drawn primarily from the authors' upper division economics courses at Wichita State University. No subject had participated in an economics experiment before, nor did any repeat this experiment. The largest group of subjects came from two money and banking courses, but none had seen the square root rule in class prior to the experiment.[3] There were 126 subjects in total: 50 low-pay, 27 high pay, and 49 received course credit. We include the data from ten sessions in our analysis below: four for course credit, four low-pay, and two high-pay.

We also conducted eleven pilot experiments. Although we did not include the pilots in our data set, they were similar to the data included. During these pilots we tinkered with the computer program, reduced the number of subjects per

[3]To avoid communication about the experiment, the class period between experimental sessions was canceled. Unfortunately, this procedure was adopted after some contamination occurred, so data from these two experiments were disregarded and these sessions were repeated with new subjects drawn from other upper division courses.

session, adjusted the parameters, trained assistants, and more than doubled the length of the instructions. The instructions, assistants, computer program, and parameters remained unchanged for all the ten experiments analyzed in this paper. The instructions were lengthened because we found that short instructions led to endless questions, and we wanted all subjects to receive the same information.

6. VIOLATIONS OF TOBIN'S AXIOMS

Table 3 displays the number of best months violating one of Tobin's axioms of rationality or with a nonoptimal number of periods. The table is disaggregated by treatment and set; set 1 means this treatment was given first. This permits us to check for learning effects, including interactions with treatment.

Subjects rarely violated three of the axioms; they generally turned in best months with (A1) no bond purchases after the first day, (A2) bond sales only when the cash account was exhausted, and (A3) no purchases and sales on the same day.[4] However, they usually used programs with unequal initial cash inventories and the incorrect number of periods.

Examining the frequency of A(4) violations for the same treatment but a different set uncovers little evidence of learning, and there is no indication that choice of n improved over time, but both failures were correlated with treatment.

In the first two sets, A(4) violations occurred significantly less often under the two- than the six-period treatment. In the first set under the two-period treatment, violations of A(4) occur 16 (out of 41) times; assuming such violations are

TABLE 3

NUMBER OF BEST MONTHS WITH NONOPTIMAL n OR THAT VIOLATE AN AXIOM (disaggregated by set and treatment)

Set	Treatment	A(1)	A(2)	A(3)	A(4)	Wrong n	Sample size
1	two-period	1	4	3	16	14	41
1	four-period	2	5	1	32	29	45
1	six-period	4	5	2	30	33	40
2	two-period	0	5	2	11	10	40
2	four-period	1	3	1	28	26	40
2	six-period	1	3	2	31	39	46
3	two-period	2	1	0	17	10	45
3	four-period	2	5	0	20	26	41
3	six-period	1	1	1	26	33	40
	Totals	14	32	12	211	220	378

NOTES: A(1): Buy bonds only on day 1.
A(2): Sell bonds only when cash is exhausted.
A(3): Never buy and sell on the same day.
A(4): Initial cash inventories should be the same size.
Set: indicates whether the treatment was given first, second, or third.

[4]To distinguish between violations of A(1) and A(3), violations of A(1) are defined as bond purchases after day 1 unaccompanied by a bond sale.

distributed as a binomial, the 95 percent confidence interval for their probability is .39 ± .15. The same 95 percent interval for set 1 and the six-period treatment is .75 ± .13; these two confidence intervals do not overlap.

The 95 percent confidence intervals for probability of incorrect n under the two- and six-period treatments also do not overlap regardless of the set. Apparently decision making is closer to the optimum under the two-period treatment, which exacts more severe penalties for nonoptimal decisions; this agrees well with Simon's view of people as satisficers.

The frequency of A(4) violations and the poor selection of n raise serious questions for monetary theory. Do these failures bias money holdings? Do they cause comparative statics responses differing from those predicted by B-T theory?

Average money holdings under the two-period treatment optimally are $300 but in fact are (by set) $306, $338, and $321. Under the four-period treatment, money holdings should average $150 but are (by set) $176, $174, and $190. Money holdings under the six-period treatment should average $100 but are (by set) $129, $154, and $111. These distributions tend to be highly non-Gaussian and so we analyze them with the sign test rather than by considering their standard errors. Average cash holdings for 189 best months are too high and 101 are too low; the resulting z statistic is 6.0, decisively refuting the null hypothesis of no bias; cash holdings tended to be too high.

Two possible sources of this bias are violating A(4) and selecting the wrong n; the frequency of bias rules out the rare violations of A(1), A(2), and A(3) as major factors. Excess money holdings could be explained by systematic failures to choose the optimal number of periods if subjects tended to set n too low, but this did not occur. There were 220 nonoptimal choices of n; 105 best months used too few n and 115 used too many. Choice of n is not the source of excess money holdings.

The remaining source of the bias is violations of A(4), and indeed such failures compel average cash holdings that are higher than the optimum for that n. (For simplicity, we always assume that it is possible to satisfy A(4) exactly.) When periods are not equally long, there is at least one (L) that is longer than the average and one (S) that is shorter. Suppose S has length s. Assuming that cash is always exhausted at the end of every period, cash holdings are identical during S and the last s days of L. Cash holdings during the first two days of L must be at least $s+2$ and $s+1$; so, if L were shortened by one day (eliminating at least $s+2$) and if S is lengthened by one day (adding $s+1$), there would be at least a 1-unit net reduction in the sum of daily cash holdings over the two periods combined. Thus, A(4) is seen to be equivalent to the minimization of average cash holdings (given n).

The excess average cash holdings (given n) due to violations of A(4) are reinforced by the effect of choosing the wrong n when n is too low; when n is too high, the two effects bias holdings in opposite directions and so the total effect of both failures simultaneously is ambiguous. For our subjects, n typically was near enough to the optimum for the A(4) effect to predominate.

7. COMPARATIVE STATICS RESULTS

Although we have refuted the naive application of B-T theory by demonstrating that, even in the simple environment of our lab, very few people are willing and able to solve for the optimal cash-bond management program, the comparative statics predictions of optimization theory were strongly supported by our data.

Figure 2 displays the percentage of subjects who used x or fewer periods under the two-, four-, and six-period treatments. The cumulative distributions are quite different; clearly subjects responded in the predicted direction to changed incentives.

The Kolmogorov-Smirnov statistic confirms that the differences in these cumulative distributions are highly significant; we can reject the hypothesis that two distributions are the result of random samplings from the same underlying population if the largest difference between cumulative distributions is greater than a critical value. The 1 percent value is 0.205 for 126 observations; the differences between cumulative distributions in Figure 2 are more than twice this value.

8. REGRESSIONS

There have been many appeals to Baumol's square root formula for average cash balances in econometric studies to justify a log-linear functional form for money demand; analysis of our experiment would not be complete without fit-

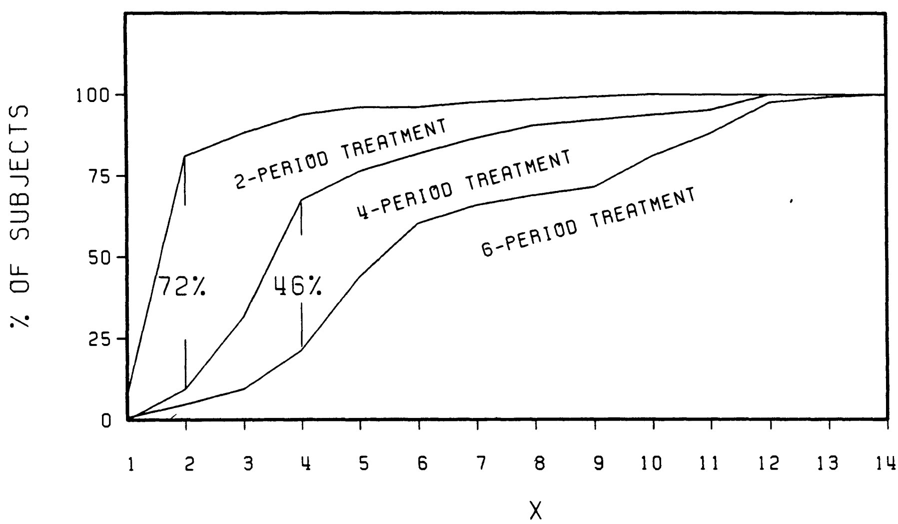

FIG. 2. Percentage of Subjects Selecting $n \leqslant X$

ting such a regression to our data.[5] The regression coefficients that would have been obtained if decisions had been optimal, are

$$LAVCASH = 3.343 - .624\ LMRATE + .538\ LFEE\ . \quad (2)$$

The variables are the logs of average cash holdings (*LAVCASH*), the monthly interest rate (*LMRATE*), and the brokerage fee (*LFEE*).

The naive application of OLS to our data would be inappropriate because the variance of behavior was smaller under the two-period treatment; this heteroskedasticity indicates the need to employ a weighted regression technique. We calculated the unweighted regression residuals' variance under each treatment; the regressions reported in Table 4 are weighted according to the inverse of these estimated residual variances (disaggregated by treatment). Glejser tests failed to indicate heteroskedasticity associated with any other variable.

Satisficing behavior may be the cause of this heteroskedasticity, and satisficing subjects may have decision-making strategies that introduce similar errors across tasks. The differences between actual and optimal money holdings for a given subject were correlated across the four- and six-period treatments; the Spearman's rank correlation coefficient of 0.378 (with 126 observations) is significant at the 0.0001 level. These two treatments pay nearly maximum amounts when the

TABLE 4

LOG-LINEAR MONEY DEMAND REGRESSIONS (Subdivided by reward scheme)

	Dependent = ln(average cash holdings)					
Independents	High pay		Low pay		Course credit	
X_0 :intercept	3.5** (5.8)	3.3** (5.5)	3.8** (8.0)	3.5** (8.2)	2.1** (4.7)	2.0** (4.8)
X_1 :ln(rate)	−.47** (−5.2)	−.54** (−5.8)	−.52** (−8.2)	−.59** (−9.8)	−.49** (−8.2)	−.50** (−8.9)
X_2 :ln(fee)	.47** (6.4)	.53** (6.9)	.44** (7.4)	.49** (9.1)	.67** (12.1)	.68** (13.0)
X_3 :$D2$*ln(error4)		.06 (.40)		.15 (1.7)		−.09 (−1.1)
X_4 :$D6$*ln(error4)		.69* (2.7)		.89** (4.1)		.42** (3.1)
X_5 :position*X_4		−.01 (−.09)		.36* (2.3)		.19* (2.1)
adj. R^2	.55	.59	.57	.64	.68	.72
F:$\beta_1 = -.62, \beta_2 = .54$ D.F.	1.9 (2,51)	.37 (2,48)	2.6 (2,97)	.51 (2,94)	5.13** (2,95)	5.5** (2,92)

NOTES: * indicates significance at the 5 percent level
** indicates significance at the 1 percent level
Error4 is excess cash held for the four-period treatment.
D2 is 1 for two-period treatment data, 0 otherwise.
D6 is 1 for six-period treatment data, 0 otherwise.
Position is the set of the four-period treatment minus the set of the six-period treatment. This measures learning effects.

[5]The integer constraints complicate application of the square root rule; however, regressing on the optima yielded an R^2 of 0.997, indicating that the log-linear form is an excellent approximation.

subject transfers the optimal strategy from one treatment to the other, so this high level of association is not surprising, given the costs of decision making (neglected by microeconomic optimizing theory generally and the B-T analysis in particular). Subjects' decision-making errors under the two-period treatment were not significantly correlated with their errors under the other treatments; this agrees with our model of satisficing subjects who search for cash-bond management strategies only until desired aspiration levels are achieved.

If the errors under all treatments had been correlated across subjects, then an error-components model would be appropriate. However, the data do not indicate a different stochastic intercept for each subject; instead, errors were transmitted across tasks according to the reward function. We handled these problems by considering the four-period treatment error as an explanatory variable instead of using the four-period treatment observations; the four-period, rather than the six-period, observations were omitted to preserve the range of the observations. The remaining two- and six-period treatment errors are independent as required, and we used dummy variables to model the dependence between the four- and six-period errors. One advantage of this analytic strategy is that regressions can be run separately on the data collected under different reward schemes and then standard pooling tests can be conducted to see how the reward schemes affected behavior.

Table 4 presents log-linear regressions run on the two- and six-period treatment data disaggregated by reward scheme; the X_i and β_i refer to independent variables and their coefficients. The F-test reported compares β_1 and β_2 to the optimal elasticities from (2). X_3 and X_4 were obtained by multiplying the four-period excess money holdings by dummies for the two- and six-period treatments respectively; these allow the four-period treatment error to be applied to the two- and six-period data independently.

If the subjects learned over time, the error correlation between the four- and six-period treatments should be affected by temporal proximity; if the six-period treatment were before (or after) the four-period treatment, the error in the six-period data could be relatively large (or small). X_5 examines this possibility, multiplying X_4 by the four-period set (monthly order) minus the six-period set; for example, if the four-period treatment were first and the six-period month were last, $X_5 = (1-3)X_4 = -2X_4$. If learning occurred, the later position of the six-period data should be associated with less error transmission.

The B-T comparative statics predictions were strongly supported regardless of the reward structure; the interest rate and brokerage fee coefficients had the right signs and approximately the predicted magnitudes. Disaggregating by reward schemes was indicated because the increase in the error sum of squares caused by pooling the data across reward schemes is significant at the 5 percent level: $F_{12,234} = 1.85$. Only the course credit data rejected an F-test restricting β_1 and β_2 to their predicted values. The course credit reactions to interest rates were about the same as the paid responses (and the B-T model), but these subjects overreacted to brokerage fees; apparently subjects pursuing a grade were overly sensitive to negative influences on rewards.

Our data strongly support the satisficing model. The four-period treatment error is transferred to the six- but not the two-period data; β_4 is significant but β_3 is not. The transmission is strongest in the low-pay data, perhaps because a smaller reward means less motivation. The β_5 coefficient, which captures learning effects, also indicates satisficing. No learning effects were detected in the high-pay data; apparently high-pay subjects learned what they could earlier than other subjects, perhaps because they were more motivated.

9. CONCLUSIONS

Economics experiments can be used to test existing theories and to suggest new possibilities for theorists; our experiment has done both. It is evident that the simple B-T theory, which neglects the cost of decision making, is not universally applicable; however, three of the four axioms of rational cash-bond management were generally discovered, and our subjects demonstrated that people with average intelligence, no prior training, and modest monetary incentives can respond to changing interest rates and brokerage fees as predicted, confirming the relevance of microeconomic optimization models to analysis of the transactions demand for money.

Our data also support Simon's satisficing model of behavior. Evidently subjects searched until aspiration levels were achieved, working harder when the gradient of rewards to decisions was steeper; error transmission between treatments and error size both increased when this gradient was flatter. Learning effects were detected in the low-pay but not the high-pay data, confirming the costs of learning and the relevance of motivation.

There are many exciting opportunities for more sophisticated models of cash management and for further experimental analysis of money demand; we hope our work will contribute to this effort.

Data for this paper are available from the JMCB *editorial office.*

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