Experimental Tests of the Endowment Effect and the Coase Theorem

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Contrary to theoretical expectations, measures of willingness to accept greatly exceed measures of willingness to pay. This paper reports several experiments that demonstrate that this "endowment effect" persists even in market settings with opportunities to learn. Consumption objects (e.g., coffee mugs) are randomly given to half the subjects in an experiment. Markets for the mugs are then conducted. The Coase theorem predicts that about half the mugs will trade, but observed volume is always significantly less. When markets for "induced-value" tokens are conducted, the predicted volume is observed, suggesting that transactions costs cannot explain the undertrading for consumption goods.

I. Introduction

The standard assumptions of economic theory imply that when income effects are small, differences between an individual's maximum

willingness to pay (WTP) for a good and minimum compensation demanded for the same entitlement (willingness to accept [WTA]) should be negligible (Willig 1976). Thus indifference curves are drawn without reference to current endowments; any difference between equivalent and compensating variation assessments of welfare changes is in practice ignored;[1] and there is wide acceptance of the Coase theorem assertion that, subject to income effects, the allocation of resources will be independent of the assignment of property rights when costless trades are possible.

The assumption that entitlements do not affect value contrasts sharply with empirical observations of significantly higher selling than buying prices. For example, Thaler (1980) found that the minimal compensation demanded for accepting a .001 risk of sudden death was higher by one or two orders of magnitude than the amount people were willing to pay to eliminate an identical existing risk. Other examples of similar reported findings are summarized in table 1. The disparities observed in these examples are clearly too large to be explained plausibly by income effects.

Several factors probably contribute to the discrepancies between the evaluations of buyers and sellers that are documented in table 1. The perceived illegitimacy of the transaction may, for example, contribute to the extraordinarily high demand for personal compensation for agreeing to the loss of a public good (e.g., Rowe, d'Arge, and Brookshire 1980). Standard bargaining habits may also contribute to a discrepancy between the stated reservation prices of buyers and sellers. Sellers are often rewarded for overstating their true value, and buyers for understating theirs (Knez, Smith, and Williams 1985). By force of habit they may misrepresent their true valuations even when such misrepresentation confers no advantage, as in answering hypothetical questions or one-shot or single transactions. In such situations the buying-selling discrepancy is simply a strategic mistake, which experienced traders will learn to avoid (Coursey, Hovis, and Schulze 1987; Brookshire and Coursey 1987).

The hypothesis of interest here is that many discrepancies between WTA and WTP, far from being a mistake, reflect a genuine effect of reference positions on preferences. Thaler (1980) labeled the increased value of a good to an individual when the good becomes part of the individual's endowment the "endowment effect." This effect is a manifestation of "loss aversion," the generalization that losses are weighted substantially more than objectively commensurate gains in

[1] For example, the conventional prescription for assessing environmental and other losses is that, "practically speaking, it does not appear to make much difference which definition is accepted" (Freeman 1979, p. 3).

TABLE 1

SUMMARY OF PAST TESTS OF EVALUATION DISPARITY

STUDY AND ENTITLEMENT	MEANS			MEDIANS		
	WTP	WTA	Ratio	WTP	WTA	Ratio
Hypothetical surveys:						
Hammack and Brown (1974): marshes	$247	$1,044	4.2			
Sinclair (1978): fishing				35	100	2.9
Banford et al. (1979):						
Fishing pier	43	120	2.8	47	129	2.7
Postal service	22	93	4.2	22	106	4.8
Bishop and Heberlein (1979): goose hunting permits	21	101	4.8			
Rowe et al. (1980): visibility	1.33	3.49	2.6			
Brookshire et al. (1980): elk hunting*	54	143	2.6			
Heberlein and Bishop (1985): deer hunting	31	513	16.5			
Real exchange experiments:						
Knetsch and Sinden (1984): lottery tickets	1.28	5.18	4.0			
Heberlein and Bishop (1985): deer hunting	25	172	6.9			
Coursey et al. (1987): taste of sucrose octa-acetate[†]	3.45	4.71	1.4	1.33	3.49	2.6
Brookshire and Coursey (1987): park trees[†‡]	10.12	56.60	5.6	6.30	12.96	2.1

* Middle-level change of several used in study.
† Final values after multiple iterations.
‡ Average of two levels of tree plantings.

the evaluation of prospects and trades (Kahneman and Tversky 1979; Tversky and Kahneman, in press). An implication of this asymmetry is that if a good is evaluated as a loss when it is given up and as a gain when it is acquired, loss aversion will, on average, induce a higher dollar value for owners than for potential buyers, reducing the set of mutually acceptable trades.

There are some cases in which no endowment effect would be expected, such as when goods are purchased for resale rather than for utilization. A particularly clear case of a good held exclusively for resale is the notional token typically traded in experimental markets commonly used to test the efficiency of market institutions (Plott 1982; Smith 1982). Such experiments employ the induced-value technique in which the objects of trade are tokens to which private redemption values that vary among individual participants have been assigned by the experimenter (Smith 1976). Subjects can obtain the prescribed value assigned for the tokens when redeeming them at the end of the trading period; the tokens are otherwise worthless.

No endowment effect would be expected for such tokens, which are valued only because they can be redeemed for cash. Thus both buyers and sellers should value tokens at the induced value they have been assigned. Markets for induced-value tokens can therefore be used as a control condition to determine whether differences between the values of buyers and sellers in other markets could be attributable to transaction costs, misunderstandings, or habitual strategies of bargaining. Any discrepancy between the buying and selling values can be isolated in an experiment by comparing the outcomes of markets for real goods with those of otherwise identical markets for induced-value tokens. If no differences in values are observed for the induced-value tokens, then economic theory predicts that no differences between buying and selling values will be observed for consumption goods evaluated and traded under the same conditions.

The results from a series of experiments involving real exchanges of tokens and of various consumption goods are reported in this paper. In each case, a random allocation design was used to test for the presence of an endowment effect. Half of the subjects were endowed with a good and became potential sellers in each market; the other half of the subjects were potential buyers. Conventional economic analysis yields the simple prediction that one-half of the goods should be traded in voluntary exchanges. If value is unaffected by ownership, then the distribution of values in the two groups should be the same except for sampling variation. The supply and demand curves should therefore be mirror images of each other, intersecting at their common median. The null hypothesis is, therefore, that half of the goods provided should change hands. Label this predicted

volume V^*. If there is an endowment effect, the value of the good will be higher for sellers than for buyers, and observed volume V will be less than V^*. The ratio V/V^* provides a unit-free measure of the undertrading that is produced by the effect of ownership on value. To test the hypothesis that market experience eliminates undertrading, the markets were repeated several times.

A test for the possibility that observed undertrading was due to transaction costs was provided by a comparison of the results from a series of induced-value markets with those from the subsequent goods markets carried out with identical trading rules. Notice that this comparison can also be used to eliminate numerous other possible explanations of the observed undertrading. For example, if the instructions to the subjects are confusing or misleading, the effects should show up in both the induced-value markets and the experimental markets for real goods. Section II describes studies of trading volume in induced-value markets and in consumption goods markets. Section III provides a further test for strategic behavior and demonstrates that the disparity findings are not likely caused by this. Section IV investigates the extent to which the undertrading of goods is produced by reluctance to buy and reluctance to sell. Section V examines undertrading in bilateral negotiations and provides a test of the Coase theorem. Section VI describes an experiment that rules out income effects and a trophy effect as explanations of the observed valuation disparity. Implications of the observed effects are discussed in Section VII.

II. Repeated Market Experiments

In experiment 1, 44 students in an advanced undergraduate law and economics class at Cornell University received a packet of general instructions plus 11 forms, one for each of the markets that were conducted in the experiment. (The instructions for all experiments are available from the authors.) The first three markets were conducted for induced-value tokens. Sellers received the following instructions (with differences for buyers in brackets):

> In this market the objects being traded are tokens. You are an owner, so you now own a token [You are a buyer, so you have an opportunity to buy a token] which has a value to you of $x. It has this value to you because the experimenter will give you this much money for it. The value of the token is different for different individuals. A price for the tokens will be determined later. For each of the prices listed below, please indicate whether you prefer to: (1) Sell your token at

this price and receive the market price. [Buy a token at this price and cash it in for the sum of money indicated above.] (2) Keep your token and cash it in for the sum of money indicated above. [Not buy a token at this price.] For each price indicate your decision by marking an X in the appropriate column.

Part of the response form for sellers follows:

At a price of $8.75 I will sell _____ I will not sell _____

At a price of $8.25 I will sell _____ I will not sell _____

The same rectangular distribution of values—ranging from $0.25 to $8.75 in steps of $0.50—was prepared for both buyers and sellers. Because not all the forms were actually distributed, however, the induced supply and demand curves were not always precisely symmetrical. Subjects alternated between the buyer and seller role in the three successive markets and were assigned a different individual redemption value in each trial.

Experimenters collected the forms from all participants after each market period and immediately calculated and announced the market-clearing price,[2] the number of trades, and the presence or absence of excess demand or supply at the market-clearing price.[3] Three buyers and three sellers were selected at random after each of the induced markets and were paid off according to the preferences stated on their forms and the market-clearing price for that period.

Immediately after the three induced-value markets, subjects on alternating seats were given Cornell coffee mugs, which sell for $6.00 each at the bookstore. The experimenter asked all participants to examine a mug, either their own or their neighbor's. The experimenter then informed the subjects that four markets for mugs would be conducted using the same procedures as the prior induced markets with two exceptions: (1) One of the four market trials would subsequently be selected at random, and only the trades made on this

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[2] The instructions stated that "*it is in your best interest to answer these questions truthfully.* For any question, treat the price as fixed. (In economics jargon, you should act as 'price takers'.)" All the subjects were junior and senior economics majors, so they were familiar with the terms used. If subjects asked how the market prices were determined, they were told, truthfully, that the market price was the point at which the elicited supply and demand curves intersected. The uniformity of the results across many different experiments suggests that this information had no discernible effect on behavior. Furthermore, the responses of the subjects in the induced-value portion of the experiments indicate that nearly all understood and accepted their role as price takers. See also experiment 5, in which a random price procedure was used.

[3] When this occurred, a random draw determined which buyers and sellers were accommodated.

trial would be executed. (2) In the binding market trial, *all* trades would be implemented, unlike the subset implemented in the induced-value markets.[4] The initial assignment of buyer and seller roles was maintained for all four trading periods. The clearing price and the number of trades were announced after each period. The market that "counted" was indicated after the fourth period, and transactions were executed immediately. All sellers who had indicated that they would give up their mugs for a sum at the market-clearing price exchanged their mugs for cash, and successful buyers paid this same price and received their mugs. This design was used to permit learning to take place over successive trials and yet make each trial potentially binding. The same procedure was then followed for four more successive markets using boxed ballpoint pens with a visible bookstore price tag of $3.98, which were distributed to the subjects who had been buyers in the mug markets.

For each goods market, subjects completed a form similar to that used for the induced-value tokens, with the following instructions:

> You now own the object in your possession. [You do not own the object that you see in the possession of some of your neighbors.] You have the option of selling it [buying one] if a price, which will be determined later, is acceptable to you. For each of the possible prices below indicate whether you wish to: (1) Sell your object and receive this price [Pay this price and receive an object to take home with you] or (2) Keep your object and take it home with you. [Not buy an object at this price.] For each price indicate your decision by marking an X in the appropriate column.

The buyers and sellers in the consumption goods markets faced the same incentives that they had experienced in the induced-value markets. Buyers maximized their potential gain by agreeing to buy at all prices below the value they ascribed to the good, and sellers maximized their welfare by agreeing to sell at all prices above the good's worth to them. As in the induced-value markets, it was in the best interest of the participants to act as price takers.

As shown in table 2, the markets for induced-value tokens and consumption goods yielded sharply different results. In the induced-value markets, as expected, the median buying and selling prices were identical. The ratio of actual to predicted volume (V/V^*) was 1.0,

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[4] The experimental design was intended to give the markets for consumption goods every possible chance to be efficient. While in the induced-value markets not everyone was paid, in the consumption goods markets everyone was paid. Also, the consumption goods markets were conducted after the induced-value markets and were repeated four times each, to allow the subjects the maximum opportunity for learning.

TABLE 2

RESULTS OF EXPERIMENT 1

INDUCED-VALUE MARKETS

Trial	Actual Trades	Expected Trades	Price	Expected Price
1	12	11	3.75	3.75
2	11	11	4.75	4.75
3	10	11	4.25	4.25

CONSUMPTION GOODS MARKETS

Trial	Trades	Price	Median Buyer Reservation Price	Median Seller Reservation Price
			Mugs (Expected Trades = 11)	
4	4	4.25	2.75	5.25
5	1	4.75	2.25	5.25
6	2	4.50	2.25	5.25
7	2	4.25	2.25	5.25
			Pens (Expected Trades = 11)	
8	4	1.25	.75	2.50
9	5	1.25	.75	1.75
10	4	1.25	.75	2.25
11	5	1.25	.75	1.75

aggregating over the three periods. In contrast, the median selling prices in the mug and pen markets were more than twice the median buying prices, and the V/V^* ratio was only .20 for mugs and .41 for pens. Observed volume did not increase over successive periods in either the mug or the pen markets, providing no indication that subjects learned to adopt equal buying and selling prices.

The results of the first and last markets for coffee mugs are also displayed in figure 1. There are five features to notice in this figure: (1) Both buyers and sellers display a wide range of values, indicating that in the absence of an endowment effect there would be enough rents to produce gains from trade. Indeed, the range of values is similar to that used in the induced-value markets, which had near-perfect market efficiency. (2) The distribution of selling prices has a single mode, unlike some recent results in which an evaluation discrepancy could be explained by a bimodal distribution of compensation demanded (Boyce et al. 1990). (3) The payment of a small commission for trading, such as $0.25 per trade, would not significantly alter the results. (4) The mugs were desirable. Every subject assigned a positive value to the mug, and the lowest value assigned by a seller
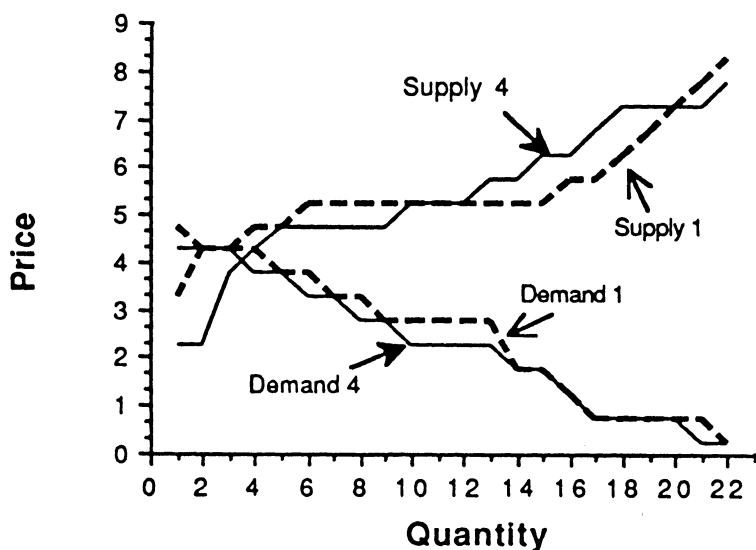
FIG. 1.—Supply and demand curves, markets 1 and 4

was $2.25. (5) Neither demand nor supply changed much between the first and last markets.

Experiment 2 was conducted in an undergraduate microeconomics class at Cornell (N = 38). The procedure was identical to that of experiment 1, except that the second consumption good was a pair of folding binoculars in a cardboard frame, available at the bookstore for $4.00. The results are reported in table 3.

In experiments 3 and 4, conducted in Simon Fraser University undergraduate economics classes, the subjects were asked to provide minimum selling prices or maximum buying prices rather than to answer the series of yes or no questions used in experiments 1 and 2. The induced-value markets were conducted with no monetary pay-offs and were followed by four markets for pens in experiment 3 and five markets for mugs in experiment 4. In experiment 3, subjects were told that the first three markets for pens would be used for practice, so only the fourth and final market would be binding. In experiment 4, one of the five markets was selected at random to count, as in experiments 1 and 2. Other procedures were unchanged. The results are shown in table 4.

Experiments 2–4 all yielded results similar to those obtained in experiment 1. Summing over the induced-value markets in all four experiments produced a $V/V*$ index of .91. This excellent perfor-mance was achieved even though the participants did not have the

TABLE 3

RESULTS OF EXPERIMENT 2

INDUCED-VALUE MARKETS

Trial	Actual Trades	Expected Trades	Price	Expected Price
1	10	10	3.75	4.25
2	9	10	4.75	4.25
3	7	8	4.25	4.75

CONSUMPTION GOODS MARKETS

Trial	Trades	Price	Median Buyer Reservation Price	Median Seller Reservation Price
		Mugs (Expected Trades = 9.5)		
4	3	3.75	1.75	4.75
5	3	3.25	2.25	4.75
6	2	3.25	2.25	4.75
7	2	3.25	2.25	4.25
		Binoculars (Expected Trades = 9.5)		
8	4	1.25	.75	1.25
9	4	.75	.75	1.25
10	3	.75	.75	1.75
11	3	.75	.75	1.75

benefit of experience with the trading rules, there were limited monetary incentives in experiments 1 and 2, and there were no monetary incentives in experiments 3 and 4. In the markets for consumption goods, in which all participants faced monetary incentives and experience with the market rules gained from the induced-value markets, $V/V*$ averaged .31, and median selling prices were more than double the corresponding buying prices. Trading procedures were precisely identical in markets for goods and for induced-value tokens. The high volume of trade in money tokens therefore eliminates transaction costs (or any other feature that was present in both types of markets) as an explanation of the observed undertrading of consumption goods.

It should be noted that subjects in the position of buyers were not given money to use for purchases, but rather had to make transactions using their own money. (Subjects were told to bring money to class and that credit and change would be available if necessary. Some subjects borrowed from friends to make payments.) The aim was to study transactions in a realistic setting. While the present design makes potential sellers slightly wealthier, at least in the first market, the magnitude of the possible income effect is trivial. In one of the

TABLE 4

RESULTS OF EXPERIMENTS 3 AND 4

Trial	N	Object	Actual Trades	Expected Trades	Ratio of Seller Median Value to Buyer Median Value
			Experiment 3		
1	26	Induced	5	6.5	
2	26	Pen	2	6.5	6.0
3	26	Pen	2	6.5	6.0
4	26	Pen	2	6.5	5.0
5	26	Pen	1	6.5	5.0
			Experiment 4		
1	74	Induced	15	18.5	
2	74	Induced	16	18.5	
3	74	Mug	6	18.5	3.8
4	74	Mug	4	18.5	2.8
5	72	Mug	4	18	2.2
6	73	Mug	8	18	1.8
7	74	Mug	8	18.5	1.8

markets the equilibrium price was only $0.75, and the prices in other markets were never above a few dollars. Also, as shown in experiments 7 and 8 below, equal undertrading was found in designs that eliminated the possibility of an income effect or cash constraint.

As shown in tables 2–4, subjects showed almost no undertrading even in their first trial in an induced-value market. Evidently neither bargaining habits nor any transaction costs impede trading in money tokens. On the other hand, there is no indication that participants in the markets for goods learned to make valuations independent of their entitlements. The discrepant evaluations of buyers and sellers remained stable over four, and in one case five, successive markets for the same good and did not change systematically over repeated markets for successive goods.

A difference in procedure probably explains the apparent conflict between these results and the conclusion reached in some other studies, that the WTA-WTP discrepancy is greatly reduced by market experience. The studies that reported a disciplinary effect of market experience assessed this effect by comparing the responses of buyers and sellers in preliminary hypothetical questions or nonbinding market trials to their behavior in a subsequent binding trial with real monetary payoffs (Knez et al. 1985; Brookshire and Coursey 1987; Coursey et al. 1987). In the present experiments, the markets for consumption goods were real and potentially binding from the first trial, and the WTA-WTP discrepancy was found to be stable over a series of such binding trials.

It should be stressed that previous research did not actually demon-
strate that the discrepancy between buyers and sellers is eliminated in
markets. Although the discrepancy between the final selling and buy-
ing prices in the sucrose octa-acetate experiment of Coursey et al.
(1987) was not statistically significant, the ratio of median prices of
sellers and buyers was still 2.6.[5] If the buyers and sellers had been
allowed to trade according to their final bids, a total of nine advanta-
geous exchanges would have occurred between the two groups, com-
pared to the theoretical expectation of 16 trades (for details, see
Knetsch and Sinden [1987]). This V/V^* ratio of .56 is quite similar to
the ratios observed in experiments 1–4. In the study by Brookshire
and Coursey (1987), the ratio of mean prices was indeed reduced by
experience, from a high of 77 for initial hypothetical survey responses
to 6.1 in the first potentially binding auction conducted in a labora-
tory. However, the ratio remained at 5.6 in the final auction.

III. Testing for Misrepresentation

As previously stated, subjects faced identical incentives in the in-
duced-value and consumption goods phases of experiments 1–4.
Therefore, it seems safe to attribute the difference in observed trad-
ing to the endowment effect. However, some readers of early drafts
of this paper have suggested that because of the way market prices
were determined, subjects might have felt that they had an incentive
to misstate their true values in order to influence the price, and per-
haps this incentive was perceived to be greater in the consumption
goods markets. To eliminate this possible interpretation of the previ-
ous results, experiment 5 was carried out in a manner similar to the
first four experiments, except that subjects were told that the price
would be selected at random. As is well known, this is an incentive-
compatible procedure for eliciting values (see Becker, DeGroot, and
Marschak 1964).
    Each participant received the following instructions (with appropri-
ate alternative wording in the buyers' forms):

> After you have finished, one of the prices listed below will
> be selected at random and any exchanges will take place at
> that price. If you have indicated you will sell at this price you
> will receive this amount of money and will give up the mug;
> if you have indicated that you will keep the mug at this price

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[5] The ratio of the mean selling and buying prices is 1.4 if all subjects are included.
However, if one buyer and one seller with extreme valuations are excluded, the ratio is
1.9. These numbers were reported in an earlier version of Coursey et al. (1987).

then no exchange will be made and you can take the mug
home with you.

. . . Your decision can have no effect on the price actually
used because the price will be selected at random.

The experiment was conducted in a series of six tutorial groups of a
business statistics class at Simon Fraser University. The use of small
groups helped assure complete understanding of the instructions,
and the exercises were conducted over the course of a single day
to minimize opportunities for communication between participants.
Each group was divided equally: half of the subjects were designated
as sellers by random selection, and the other half became buyers. A
total of 59 people took part.

Two induced-value markets for hypothetical payoffs and a subse-
quent third real exchange market for money and mugs were con-
ducted with identical trading rules used in all three. All participants
maintained the same role as either buyers or sellers for the three
markets. As in experiments 1 and 2, the prices that individuals chose
to buy or to sell were selected from possible prices ranging from $0.00
to $9.50 listed by increments of $0.50.

The results of this experiment were nearly identical to the earlier
ones in which the actual exchanges were based on the market-clearing
price. Even though possibly less motivating hypothetical values were
used in the two induced-value markets, nearly all participants pur-
sued a profit-maximizing selection of prices to buy or sell the assets.
Fourteen exchanges at a price of $4.75 were expected in the first
induced-value market on the basis of the randomly distributed values
written on the forms. Thirteen trades at this price were indicated
by the prices actually selected by the participants. The results of the
second hypothetical induced-value market were equally convincing,
with 16 of the 17 expected exchanges made at the expected price of
$5.75. The procedures and incentives were apparently well under-
stood by the participants.

Mugs, comparable to those used in other experiments, were distrib-
uted to the potential sellers after the induced-value markets were
completed. A mug was also shown to all the potential buyers. The
following form with instructions, nearly identical to the ones used in
the induced-value markets, was then distributed (with the alternative
wording for buyers in brackets):

You now [do not] have, and own a mug which you can
keep and take home. You also have the option of selling it
and receiving [buying one to take home by paying] money
for it.

For each of the possible prices listed below, please indicate

whether you wish to: (1) Receive [pay] that amount of money and sell your [buy a] mug, or (2) Not sell your [buy a] mug at this price.

After you have finished, one of the prices listed below will be selected at random and any exchanges will take place at that price. If you have indicated you will sell [buy] at this price you will receive this amount of money [a mug] and will give up the mug [pay this amount of money]; if you have indicated that you will keep the [not buy a] mug at this price then no exchange will be made and you can take the mug home with you [do not pay anything].

Notice the following two things: (1) Your decision can have no effect on the price actually used because the price will be selected at random. (2) It is in your interest to indicate your true preferences at each of the possible prices listed below.

For each price indicate your decision by marking an X in the appropriate column.

	I Will Sell [Buy]	I Will Keep [Not Buy] the Mug
If the price is $0	_____	_____
If the price is $0.50	_____	_____
⋮		
If the price is $9.50	_____	_____

After the instructions were read and reviewed by the experimenter and questions were answered, participants completed the forms indicating either their lowest selling price or their highest buying price. A random price, from among the list from $0.00 to $9.50, was then drawn, and exchanges based on this price were completed.

The results again showed a large and significant endowment effect. Given the 29 potential buyers, 30 potential sellers, and the random distribution of the mugs, 14.5 exchanges would be expected if entitlements did not influence valuations. Instead, only six were indicated on the basis of the values actually selected by the potential buyers and sellers ($V/V^* = .41$). The median selling price of $5.75 was over twice the median buying price of $2.25, and the means were $5.78 and $2.21, respectively.

IV. Reluctance to Buy versus Reluctance to Sell

Exchanges of money and a good (or between two goods) offer the possibilities of four comparisons: a choice of gaining either the good or money, a choice of losing one or the other, buying (giving up

money for the good), and selling (giving up the good for money) (Tversky and Kahneman, in press). The endowment effect results from a difference between the relative preferences for the good and money. The comparison of buying and selling to simple choices between gains permits an analysis of the discrepancy between WTA and WTP into two components: reluctance to sell (exchanging the good for money) and reluctance to buy (exchanging money for the good).

Experiments 6 and 7 were carried out to assess the weight of reluctance to buy and reluctance to sell in undertrading of a good similar to the goods used in the earlier experiments. The subjects in experiment 6 were 77 Simon Fraser students, randomly assigned to three groups. Members of one group, designated sellers, were given a coffee mug and were asked to indicate whether or not they would sell the mug at a series of prices ranging from $0.00 to $9.25. A group of buyers indicated whether they were willing to buy a mug at each of these prices. Finally, choosers were asked to choose, for each of the possible prices, between a mug and cash.

The results again reveal substantial undertrading: While 12.5 trades were expected between buyers and sellers, only three trades took place ($V/V^* = .24$). The median valuations were $7.12 for sellers, $3.12 for choosers, and $2.87 for buyers. The close similarity of results for buyers and choosers indicates that there was relatively little reluctance to pay for the mug.

Experiment 7 was carried out with 117 students at the University of British Columbia. It used an identical design except that price tags were left on the mugs. The results were consistent with those in experiment 6. Nineteen trades were expected on the basis of valuation equivalence, but only one was concluded on the basis of actual valuations ($V/V^* = .05$). The median valuations were $7.00 for sellers, $3.50 for choosers, and $2.00 for buyers.

It is worth noting that these results eliminate any form of income effect as an explanation of the discrepant valuations since the positions of sellers and choosers were strictly identical. The allocation of a particular mug to each seller evidently induced a sense of endowment that the choosers did not share: the median value of the mug to the sellers was more than double the value indicated by the choosers even though their choices were objectively the same. The results imply that the observed undertrading of consumption goods may be largely due to a reluctance to part with entitlements.

V. Bilateral Bargaining and the Coase Theorem

According to the Coase theorem, the allocation of resources to individuals who can bargain and transact at no cost should be indepen-

dent of initial property rights. However, if the marginal rate of sub-
stitution between one good and another is affected by endowment,
then the individual who is assigned the property right to a good will
be more likely to retain it. A bilateral bargaining experiment (experi-
ment 8) was carried out to test this implication of the endowment
effect.

The subjects were 35 pairs of students in seven small tutorials at
Simon Fraser University. The students were enrolled in either a be-
ginning economics course or an English class. Each student was ran-
domly paired with another student in the same tutorial group, with
care taken to assure that students entering the tutorial together were
not assigned as a pair. A game of Nim, a simple game easily ex-
plained, was played by each pair of participants. The winners of the
game were each given a 400-gram Swiss chocolate bar and told it was
theirs to keep.

An induced-value bargaining session was then conducted. The
member of each pair who did not win the Nim game, and therefore
did not receive the chocolate bar, was given a ticket and an instruction
sheet that indicated that the ticket was worth $3.00 because it could be
redeemed for that sum. The ticket owners were also told that they
could sell the ticket to their partner if mutually agreeable terms could
be reached. The partners (the chocolate bar owners) received instruc-
tions indicating that they could receive $5.00 for the ticket if they
could successfully buy it from the owner. Thus there was a $2.00
surplus available to any pair completing a trade.

The pairs were then given an unlimited amount of time to bargain.
Subjects were told that both credit and change were available from
the experimenter. Results of the bargaining sessions were recorded
on their instruction sheets.

Of the 35 pairs of participants, 29 agreed to an exchange ($V/V^* =$
.83). The average price paid for the 29 tickets was $4.09, with 12 of
the exchange prices being exactly $4.00. Payments of the redemption
values of the tickets were made as soon as the exchanges were com-
pleted. These payments were made in single dollar bills to facilitate
trading in the subsequent bargaining session. After the ticket ex-
changes were completed, owners of the chocolate bars were told that
they could sell them to their partners if a mutually agreeable price
could be determined. The procedures used for the tickets were once
again applied to these bargaining sessions.

An important effect of the preliminary induced-value ticket bar-
gains was to provide the ticket owners with some cash. The average
gain to the ticket owners (including the six who did not sell their
tickets) was $3.90. The average gain to their partners (the chocolate
bar owners) was only $0.76. Thus the potential chocolate bar buyers

were endowed with an average of $3.14 more than the owners, creating a small income effect toward the buyers. Also, to the extent that a windfall gain such as this is spent more casually by subjects than other money (for evidence on such a "house money effect," see Thaler and Johnson [1990]), trading of chocolate bars should be facilitated.

Results of the chocolate bar bargains once again suggest reluctance to trade. Rather than the 17.5 trades expected from the random allocations, only seven were observed ($V/V^* = .4$). The average price paid in those exchanges that did occur was $2.69 (the actual prices were $6.00, $3.10, $3.00, $2.75, $2.00, $1.00, and $1.00). If the six pairs of subjects who did not successfully complete bargains in the first stage are omitted from the sample on the grounds that they did not understand the task or procedures, then six trades are observed where 14.5 would be expected ($V/V^* = .414$). Similarly, if two more pairs are dropped because the prices at which they exchanged tickets were outside the range $3.00–$5.00, then the number of trades falls to four, and V/V^* falls to .296. (No significant differences between the students in the English and economics classes were observed.)[6]

To be sure that the chocolate bars were valued by the subjects and that these valuations would vary enough to yield mutually beneficial trades, the same chocolate bars were distributed to half the members of another class at Simon Fraser. Those who received chocolate bars were asked the minimum price they would accept to sell their bar, while those without the bars were asked the maximum price they would pay to acquire a bar. The valuations of the bars varied from $0.50 to $8.00. The average value ascribed by sellers was $3.98, while the buyers' average valuation was $1.25. (The median values were $3.50 and $1.25.)

VI. The Endowment Effect in Choices between Goods

The previous experiments documented undertrading in exchanges of money and consumption goods. A separate experiment (Knetsch 1989) establishes the same effect in exchanges between two goods. Participants in three classes were offered a choice between the same two goods. All students in one class were given a coffee mug at the

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[6] We conducted two similar bargaining experiments that yielded comparable results. Twenty-six pairs of subjects negotiated the sale of mugs and then envelopes containing an uncertain amount of money. Buyers had not been given any cash endowment. These sessions yielded six and five trades, respectively, where 13 would be expected. Also, some induced-value bilateral negotiation sessions were conducted in which only $0.50 of surplus was available (the seller's valuation was $1.50 and the buyer's was $2.00). Nevertheless, 21 of a possible 26 trades were completed.

beginning of the session as compensation for completing a short questionnaire. At the completion of the task, the experimenters showed the students a bar of Swiss chocolate that they could immediately receive in exchange for the mug. The students in another class were offered an opportunity to make the opposite exchange after first being given the chocolate bar. The students in a third class were simply offered a choice, at the beginning of the session, between a chocolate bar and a mug. The proportion of students selecting the mug was 89 percent in the class originally endowed with mugs ($N = 76$), 56 percent in the class offered a choice ($N = 55$), and only 10 percent in the class originally endowed with chocolate bars ($N = 87$). For most participants a mug was more valuable than the chocolate when the mug had to be given up but less valuable when the chocolate had to be given up. This experiment confirms that undertrading can occur even when income effects are ruled out. It also demonstrates an endowment effect for a good that was distributed to everyone in the class and therefore did not have the appeal of a prize or trophy.

VII.   Discussion

The evidence presented in this paper supports what may be called an instant endowment effect: the value that an individual assigns to such objects as mugs, pens, binoculars, and chocolate bars appears to increase substantially as soon as that individual is given the object.[7] The apparently instantaneous nature of the reference point shift and consequent value change induced by giving a person possession of a good goes beyond previous discussions of the endowment effect, which focused on goods that have been in the individual's possession for some time. While long-term endowment effects could be explained by sentimental attachment or by an improved technology of consumption in the Stigler-Becker (1977) sense, the differences in preference or taste demonstrated by more than 700 participants in the experiments reported in this paper cannot be explained in this fashion.

The endowment effect is one explanation for the systematic differences between buying and selling prices that have been observed so often in past work. One of the objectives of this study was to examine an alternative explanation for this buying-selling discrepancy, namely that it reflects a general bargaining strategy (Knez and Smith 1987) that would be eliminated by experience in the market (Brookshire

[7] The impression gained from informal pilot experiments is that the act of giving the participant physical possession of the good results in a more consistent endowment effect. Assigning subjects a chance to receive a good, or a property right to a good to be received at a later time, seemed to produce weaker effects.

and Coursey 1987; Coursey et al. 1987). Our results do not support this alternative view. The trading institution used in experiments 1–7 encouraged participants to be price takers (especially in experiment 5), and the rules provided no incentive to conceal true preferences. Furthermore, the results of the induced-value markets indicate that the subjects understood the demand-revealing nature of the questions they were asked and acted accordingly. Substantial undertrading was nevertheless observed in markets for consumption goods. As for learning and market discipline, there was no indication that buying and selling prices converged over repeated market trials, though full feedback was provided at the end of each trial. The undertrading observed in these experiments appears to reflect a true difference in preferences between the potential buyers and sellers. The robustness of this result reduces the risk that the outcome is produced by an experimental artifact. In short, the present findings indicate that the endowment effect can persist in genuine market settings.

The contrast between the induced-value markets and the consumption goods markets lends support to Heiner's (1985) conjecture that the results of induced-value experiments may not generalize to all market settings. The defining characteristic of the induced-value markets is that the values of the tokens are unequivocally defined by the amount the experimenter will pay for them. Loss aversion is irrelevant with such objects because transactions are evaluated simply on the basis of net gain or loss. (If someone is offered $6.00 for a $5.00 bill, there is no sense of loss associated with the trade.) Some markets may share this feature of induced-value markets, especially when the conditions of pure arbitrage are approached. However, the computation of net gain and loss is not possible in other situations, for example, in markets in which risky prospects are traded for cash or in markets in which people sell goods that they also value for their use. In these conditions, the cancellation of the loss of the object against the dollars received is not possible because the good and money are not strictly commensurate. The valuation ambiguity produced by this lack of commensurability is necessary, although not sufficient, for both loss aversion and a buying-selling discrepancy.

The results of the experimental demonstrations of the endowment effect have direct implications for economic theory and economic predictions. Contrary to the assumptions of standard economic theory that preferences are independent of entitlements,[8] the evidence

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[8] Although ownership can affect taste in the manner suggested by Stigler and Becker (1977), in the absence of income effects, it is traditional to assume that the indifference curves in an Edgeworth box diagram do not depend on the location of the endowment point.

presented here indicates that people's preferences depend on their reference positions. Consequently, preference orderings are not defined independently of endowments: good A may be preferred to B when A is part of an original endowment, but the reverse may be true when initial reference positions are changed. Indifference curves will have a kink at the endowment or reference point (see Tversky and Kahneman, in press), and an indifference curve tracing acceptable trades in one direction may even cross another indifference curve that plots the acceptable exchanges in the opposite direction (Knetsch 1989).

The existence of endowment effects reduces the gains from trade. In comparison with a world in which preferences are independent of endowment, the existence of loss aversion produces an inertia in the economy because potential traders are more reluctant to trade than is conventionally assumed. This is not to say that Pareto-optimal trades will not take place. Rather, there are simply fewer mutually advantageous exchanges possible, and so the volume of trade is lower than it otherwise would be.

To assess the practical significance of the endowment effect, it is important to consider first some necessary conditions for the effect to be observed. Experiments 6 and 7 suggest that the endowment effect is primarily a problem for sellers; we observed little reluctance to buy but much reluctance to sell. Furthermore, not all sellers are afflicted by an endowment effect. The effect did not appear in the markets for money tokens, and there is no reason in general to expect reluctance to resell goods that are held especially for that purpose. An owner will not be reluctant to sell an item at a given price if a perfect substitute is readily available at a lower price. This reasoning suggests that endowment effects will almost certainly occur when owners are faced with an opportunity to sell an item purchased for use that is not easily replaceable. Examples might include tickets to a sold-out event, hunting licenses in limited supply (Bishop and Heberlein 1979), works of art, or a pleasant view.

While the conditions necessary for an endowment effect to be observed may appear to limit its applicability in economic settings, in fact these conditions are very often satisfied, and especially so in the bargaining contexts to which the Coase theorem is applied. For example, tickets to Wimbledon are allocated by means of a lottery. A standard Coasean analysis would imply that in the presence of an efficient ticket brokerage market, winners of the lottery would be no more likely to attend the matches than other tennis fans who had won a similar cash prize in an unrelated lottery. In contrast, the experimental results presented in this paper predict that many winners of Wimbledon tickets will attend the event, turning down opportunities

to sell their tickets that exceed their reservation price for buying them.

Endowment effects can also be observed for firms and other organizations. Endowment effects are predicted for property rights acquired by historic accident or fortuitous circumstances, such as government licenses, landing rights, or transferable pollution permits. Owing to endowment effects, firms will be reluctant to divest themselves of divisions, plants, and product lines even though they would never consider buying the same assets; indeed, stock prices often rise when firms do give them up. Again, the prediction is not an absence of trade, just a reduction in the volume of trade.

Isolating the influence of endowment effects from those of transaction costs as causes of low trading volumes is, of course, difficult in actual market settings. Demonstrations of endowment effects are most persuasive where transaction costs are very small. By design, this was the case in the experimental markets, where the efficiency of the induced-value markets demonstrated the minimal effect of transaction costs, or other impediments, on exchange decisions, leaving the great reluctance to trade mugs and other goods to be attributable to endowment effects.

Endowment effects are not limited to cases involving physical goods or to legal entitlements. The reference position of individuals and firms often includes terms of previous transactions or expectations of continuation of present, often informal, arrangements. There is clear evidence of dramatically asymmetric reactions to improvements and deteriorations of these terms and a willingness to make sacrifices to avoid unfair treatment (Kahneman, Knetsch, and Thaler 1986). The reluctance to sell at a loss, owing to a perceived entitlement to a formerly prevailing price, can explain two observations of apparent undertrading. The first pertains to housing markets. It is often observed that when housing prices fall, volume also falls. When house prices are falling, houses remain on the market longer than when prices are rising. Similarly, the volume for stocks that have declined in price is lower than the volume for stocks that have increased in value (Shefrin and Statman 1985; Ferris, Haugen, and Makhija 1988), although tax considerations would lead to the opposite prediction.

Another manifestation of loss aversion in the context of multiattribute negotiations is what might be termed "concession aversion": a reluctance to accept a loss on any dimension of an agreement. A straightforward and common instance of this is the downward stickiness of wages. A somewhat more subtle implication of concession aversion is that it can produce inefficient contract terms owing to historic precedents. Old firms may have more inefficient arrange-

ments than new ones because new companies can negotiate without the reference positions created by prior agreements. Some airlines, for example, are required to carry three pilots on some planes while others—newer ones—operate with two.

Loss aversion implies a marked asymmetry in the treatment of losses and forgone gains, which plays an essential role in judgments of fairness (Kahneman et al. 1986). Accordingly, disputes in which concessions are viewed as losses are often much less tractable than disputes in which concessions involve forgone gains. Court decisions recognize the asymmetry of losses and forgone gains by favoring possessors of goods over other claimants, by limiting recovery of lost profits relative to compensation for actual expenditures, and by failing to enforce gratuitous promises that are coded as forgone gains to the injured party (Cohen and Knetsch 1989).

To conclude, the evidence reported here offers no support for the contention that observations of loss aversion and the consequential evaluation disparities are artifacts; nor should they be interpreted as mistakes likely to be eliminated by experience, training, or "market discipline." Instead, the findings support an alternative view of endowment effects and loss aversion as fundamental characteristics of preferences.

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