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Yes, Wall Street, There Is A January Effect! Evidence from Laboratory Auctions

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There is a large literature using financial market data on the causes of a "January effect," which produces higher stock prices in January than in other months of the year. We present the first experimental study of this phenomenon in the context of two well-known auction experiments. After controlling for variables that could influence subject bids, such as differences in private values, cumulative earnings, and learning effects, the prices in the January markets were systematically higher than those in December, a difference that is economically large and statistically significant. The results provide support for the conjecture that psychological factors may contribute to the well-documented January effect in empirical stock market data.

Introduction

Over sixty years ago, Wachtel [1942] described a "January effect" in stock prices. This phenomenon occurs when there is a pattern of higher returns in January relative to the rest of the year after controlling for standard variables. Many studies have explored non-psychological factors that might explain the observation, but these economic variables have not fully explained the January effect. Furthermore, these variables cannot be controlled for in naturally occurring markets. Hence, we present a set of laboratory experiments to investigate this phenomenon in settings that rule out the non-psychological explanations discussed below.

Our experiments generated an economically large and statistically significant January effect among student subjects who participated in auctions. After controlling for variables that could influence subject bids such as differences in private values, cumulative earnings, and learning effects, the prices in the January markets were systematically higher than those in December. Our results suggest that subconscious psychological factors influence behavior in laboratory markets, and thus could be a source of bias in naturally occurring markets.

The second section reviews empirical studies of the January effect in financial markets. The third section describes the experimental environments used to investigate the January effect and the fourth section is the conclusion.

The January Effect

Rozeff and Kinney [1976] find a 3.5% average return during January on an equal-weighted index of NYSE prices from 1904 through 1974, and about 0.5% during the other months. Keim [1983] shows that nearly half the excess returns for small firms occurred during January. Moreover, half the January returns came during the first five days of the month, particularly on the first trading day. Gultekin and Gultekin [1983] document evidence of seasonality, mainly a January effect, in stock returns in thirteen of seventeen countries studied. More recently, Schwert [2003] has concluded that the January effect weakened from 1980–2001, but that it still existed.

Explanations for the January effect range from tax-loss selling, window dressing, increased year-end liquidity, and market microstructure effects, to real economic changes like macroeconomic news or changes in risk premia and investor psychology. The tax-loss selling hypothesis states that sales in mid-December conducted for the purpose of establishing tax losses tend to drive security prices below what they would otherwise be in light of earnings. Reinganum [1983], Jones, Lee, and Apenbrink [1991], Sias and Starks [1997], and Poterba and Weisbenner [2001] provide evidence consistent with this hypothesis.

Roll [1983] also finds some evidence consistent with tax-loss selling, but he calls the explanation "patently absurd." Even if investors sold stocks for tax reasons, other investors would buy those stocks in anticipation of the January price increase, thereby eliminating the January effect. In addition, Brown et al.

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[1983], Berges, McConnell, and Schlarbaum [1984], and Kato and Schallheim [1985] show that the January effect can occur even in international markets where there are no capital gains taxes and where the tax year does not begin in January. And DeBondt and Thaler [1985] find that portfolios consisting of stocks with relatively poor returns over a five-year period had unusually large January returns, long after any tax benefits gained from selling the stocks at the end of the year.

The window-dressing hypothesis refers to fund managers selling shares that have declined sharply in value at the end of the year and buying them back at the beginning of the new year. Lakonishok et al. [1991] find that funds sell poorly performing stocks every quarter, with this pattern accelerating during the fourth quarter. Thus, as Chen and Singal [2004] note, if window dressing drives the January effect, a similar pattern should exist during other quarters. They study the June through July period and conclude that window dressing does not cause the January effect.

Ogden [1990] argues that the substantial increase in business activity near the end of the calendar year results in greater profits in December. Thus the corresponding liquidity increase in January puts upward pressure on stock prices. This liquidity hypothesis does not explain why the January effect exists primarily among small stocks, however, as greater profits would presumably cause the entire market to increase. Furthermore, both the liquidity and window-dressing hypotheses are subject to Roll's [1983] critique that the market should exploit such obvious mispricings.

Keim [1989] shows there are systematic tendencies for December closing prices to be recorded at the bid and for January closing prices to be recorded at the ask, a pattern that may contribute to the January effect. However, later studies, such as Jones, Lee, and Apenbrink [1991], Poterba and Weisbenner [2001], and Chen and Singal [2004], explicitly account for this market microstructure issue and still find a January effect.

Seyhun [1988] investigates the possibility that higher January returns represent compensation for the increased risk of trading against informed investors. He concludes that increased risk does not cause the January effect. Seyhun [1993] argues that the January puzzle is greater than previously thought because omitted risk factors cannot explain it.

Christie-David and Chaudhry [2000] find that returns on interest rate instruments respond differently to macroeconomic announcements in January than in other months, and conclude this is due to either the tax-loss selling or window-dressing explanations. Lu and Ma [2004] show that positive earnings news partially accounts for the January effect during the second half of the month, but cannot explain the effect during the first half.

Some analysts argue that investor psychology may cause the January effect. Shiller [1999], for example, links the January effect to the tendency of individuals to place particular events into mental compartments: "If people view the year end as a time of reckoning and a new year as a new beginning, they may be inclined to behave differently at the turn of the year, and this may explain the January effect."[1] Economics experiments are an ideal environment to test whether psychological effects alone can generate higher prices in January than in December because the fundamental explanations of the January effect can be controlled for in the laboratory.[2]

Experimental Analysis

We explore the existence of a January effect in two very different auction environments spanning three calendar years. To study the effect with naturally occurring data, a much longer time horizon is necessary. Many factors, such as macroeconomic news and earnings announcements, affect asset prices. Hence, we cannot really identify a January effect with only four months of empirical data, because we would not be able to determine whether it was caused by some common shock to market participants.

The laboratory setting, on the other hand, allows us to control for factors that might otherwise influence subject behavior. If groups of people take part in identical laboratory markets in two consecutive December and January periods, and the groups bid significantly higher in the January experiments, the only possible explanations are that the December and January groups differ in some relevant way, or that a psychological effect explains the results. Although we cannot completely rule out the possibility that the subjects differed along an unobservable dimension, these groups were virtually identical in every observable respect, such as age, gender, race, proportion of economics majors, household income, employment status, and country of origin. We also performed difference of means and difference of proportions tests as appropriate, and found no significant differences between subjects in either experiment. For the double-auction experiment, we further conclude that our January and December subjects did not differ significantly in their risk preferences, as measured by the Holt and Laury [2002] lottery choice survey.

It is unlikely that a common unobserved shock affecting all the subjects created the bidding pattern that we observed. Such a shock would have had to occur in the December and January periods of both 2003–2004 and 2004–2005, and systematically bias the bids upward in January of both periods. That type of shock would presumably affect U.S. equity markets, and we have no empirical evidence that anything like that occurred. The S&P 500 index rose 19.21 points from

December 2003 through the end of January 2004, and fell 30.65 points from December 2004 through the end of January 2005. Average returns in December 2003 and December 2004 do not differ significantly from those in January 2004 and January 2005, respectively.

Returns on the CRSP equal-weighted index during those periods show similar patterns, except that the January 2005 returns are significantly lower than the December 2004 returns. Thus, U.S. equity prices made relatively small moves in opposite directions during the two time periods, a pattern that suggests no common shocks generated our results.

Common Value Auction

We based one series of experiments on the common value auction design developed by Holt and Sherman [2000]. Two bidders receive private signals about the value of a prize. The actual value is the average of the two signals. Each bidder knows her own signal and knows the range of possible values for the other bidder's signal before making a bid. The two bids are placed simultaneously, and the higher bidder wins the prize amount minus her bid.

In studying the possibility of a January effect, this design offers two important advantages. First, the analytical solution is relatively simple. Second, the auction shares important similarities with the financial markets studied in the context of the January effect. Both, for example, involve a "prize" of uncertain value and private signals of that value.

Holt and Sherman [2000] derive two game theoretic models of bidding behavior in common value auctions: "rational" and "naïve" bidding. Naïve bidders tend to overbid because they fail to realize that winning the auction puts an upper limit on the other player's private value signal. Rational bidders do not fall prey to this error. Holt and Sherman [2000] show it is rational for players in this two-person auction to bid one-half their own private value signal. The naïve bidding strategy is more complicated and depends on the bidder's degree of risk aversion.[3]

Holt and Sherman [2000] report bids that are significantly higher than the rational bid, and, in many cases, higher than the naïve bid. Kagel and Levin [2002] review the well-developed experimental literature on common value auctions and find that, in general, other research confirms Holt and Sherman's [2000] results: Bidders frequently fall prey to the winner's curse, which results in significant overbidding relative to the rational Nash equilibrium.

Procedures. Our subjects in this experiment consisted of 76 undergraduate students from a variety of classes at the College of William and Mary. Each group of ten participated in one hourlong session encompassing two treatments, with fifteen decision-making rounds per treatment.[4] Four sessions were conducted in December 2003, and four in January 2004.[5]

The experimental conditions were virtually identical across sessions; only the calendar date, time of day, and subject group differed.[6] At the beginning of each session, subjects were read the instructions and given the opportunity to ask questions.[7] The experiment was conducted over a computer network in the Experimental Economics Laboratory at the College of William and Mary using the *V*econlab software.[8]

At the beginning of each decision-making round, we randomly paired the subjects. Pairings were anonymous, and subjects were separated by dividers preventing them from making eye contact or looking at another person's computer screen. Each pairing represented a distinct first-price auction, with a single prize awarded to one member of the pair. Once subjects were paired, each person saw a private value signal, drawn independently from a uniform distribution between 0 and 10 for treatment A, and between 0 and 5 for treatment B. The prize value for each pair was the average of the two value signals shown to the subjects in that pair. Each subject knew her own private value and the probability distribution of the value signals, but not the other member's value signal.

After both subjects placed their bids, the prize was awarded to the high bidder in each pair,[9] who earned the difference between the true prize value and the bid. Earnings were cumulative across rounds and treatments, and we subtracted negative earnings from cumulative earnings. To reduce the probability of negative cumulative earnings, each subject received an initial, one-time endowment of $7 at the beginning of the session. Cumulative earnings were paid in cash at the end of each session, averaging $6.99 for the December group and $7.22 for the January group.

Results. Our data consist of a total of 2,130 observations from 76 experimental subjects: 38 in December and 38 in January.[10] Consistent with previous experimental research, we find significant overbidding relative to the rational Nash prediction. Overall, subject bids were approximately 82% of their signals, relative to the Nash prediction of 50%. Other summary statistics appear in Table 1.

The mean bid for the month of January is approximately $0.24 higher than for December, with approximately equal variances in the two months. An unconditional difference of means test allows us to reject the null hypothesis of equal means at the 1% level. Note, however, that the average private value signal in January is higher than in December (4.145 versus 3.859), which probably explains part of the mean bid disparity between the two months. Hence, we estimate econometric models that explain bidding behavior and control for the subject's signal,[11] cumulative earnings, round, gender,[12] day of the week, and month of the year.

Table 1. *Descriptive Statistics for 2003–2004 Common Value Auction Experiment*

	December 2003		January 2004	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Bid	3.15	1.71	3.39	1.73
Signal	3.86	2.66	4.15	2.72
Cumulative Earnings	6.99	3.34	7.22	4.33
Participants	38		38	
Male Participants	19		16	
Monday Experiments	2		0	
Difference of Means Test For Bids in December and January (t)	3.16***			

Note: Results based on 990 observations in December and 1,140 observations in January. *Bid* is a given subject's bid for the common value "prize," *signal* is that subject's private value signal, and *cumulative earnings* are her cumulative earnings up to the bid in question. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

We estimate our model under three different regimes: robust ordinary least squares, random effects, and clustered ordinary least squares. Under ordinary least squares, a Breusch-Pagan test allows us to reject the null hypothesis of homescedasticity at the 1% level. We therefore use robust, heteroscedasticity-consistent standard errors. In both the random effects model and the pooled model with clustering, we group by individual subject (results appear in Table 2).

The coefficient on *January*, a dummy variable that takes a value of 1 if the corresponding bid was placed in January, is about 0.20 in all three regressions. This is statistically significant at the 10% or higher level. Thus, *ceteris paribus*, January bids are about $0.20 higher than December bids, an amount equal to more than 6% of the mean December bid. Furthermore, the estimates of this coefficient are stable across specifications, suggesting any individual effects are small.

The variable *signal* is positive and significant at the 1% level in all regressions. In addition, *cumulative earnings*, *male*, and *round* are negative and significant at the 1% level in all regressions. Finally, the variable *Monday* is positive and significant at the 10% level in the pooled model without clustering.

In summary, consistent with other studies, we find a general pattern of overbidding relative to the rational Nash prediction in a common value auction experiment. We find the degree of overbidding is significantly higher in January than in December. Bids decrease as subjects accumulate more money and experience in the auction, but they do not fall to the predicted level over twenty rounds of play. Note that in this auction environment, higher bids result in lower earnings for subjects, *ceteris paribus*, since all subjects are potential buyers. We next describe an auction environment with buyers and sellers, so higher bids are better for some subjects and worse for others.

Double Auction Experiment

The double auction experiment was invented by Nobel laureate Vernon Smith [1962]. Market participants are designated as either buyers or sellers. Buyers are assigned a dollar value, and they earn the difference between this value and the price they negotiate for a trade. Sellers are assigned a dollar cost, and they earn the difference between the price they negotiate and their cost. To negotiate trades, buyers make "bids" and sellers make "asks." Bids and asks are displayed in a queue that is updated as new prices are proposed. At

Table 2. *Regression Results for 2003–2004 Common Value Auction Experiment*

	Robust OLS		GLS, Random Effects		OLS, Cluster	
Bid	**Coefficient**	**t**	**Coefficient**	**z**	**Coefficient**	**t**
Signal	0.41	37.20***	0.40	43.71***	0.41	19.24***
Cumulative Earnings	−0.07	−8.01***	−0.06	−7.73***	−0.07	−4.96***
Monday	0.12	1.67*	0.12	0.91	0.12	0.89
January	0.20	3.61***	0.21	1.76*	0.20	1.66*
Round	−0.04	−13.44***	−0.04	−15.30***	−0.04	−7.96***
Male	−0.31	−6.18***	−0.32	−3.31***	−0.31	−3.09***
Constant	2.77	24.14***	2.75	20.04***	2.77	13.04***
R^2	0.588		0.588		0.588	
F	464.34***				140.91***	
Wald Chi-Square			3,049.20***			
Number of Obs.	2,130		2,130		2,130	
Number of Groups			76		76	

Note: Bid is a given subject's bid for the common value "prize," *January* is a dummy variable with the value of 1 if the corresponding bid was placed during the month of January, *signal* is a subject's private value signal, *cumulative earnings* is a continuous variable to test for a cumulative earnings effect, *round* is included to test for a time trend, the dummy variable *Monday* is included to test for a weekday effect, and the dummy variable *male* is included to test for a gender-specific effect. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

any point during a trading period, a buyer can accept an outstanding ask or a seller can accept an outstanding bid.

We use the box design version of the double auction market with multiple equilibria: All buyers have the same value ($7), and all sellers have the same cost ($5), so the supply and demand curves form a box. Further, we have five buyers and five sellers in each market, so all prices between seller cost and buyer value are consistent with theory. Market rules prohibit subjects from trading at a loss, so using this setup, with a box design and multiple equilibria, market prices are always consistent with theory.

In general, Holt and Davis [1993] report no consistent pattern of results with this design. There is some evidence that prices in the initial round anchor prices for the subsequent rounds, and there is additional evidence that psychological factors influence the division of surplus in this setting.

Specifically, Ball et al. [2001] used this design to examine the effect of laboratory-induced status on earnings. They induced status by awarding gold stars to certain subjects. In all cases, the stars were awarded randomly, but in some sessions subjects were told that the stars were awarded based on the results of an economics trivia quiz. Ball et al. [2001] found that status resulted in higher prices when the sellers had status and lower prices when the buyers had status, regardless of whether the traders perceived the status as real or random.

The evidence that psychological factors can influence market outcomes makes this particular double auction design appealing for studying the January effect. An additional advantage is that subjects face a very simple decision making problem, so decision error should play little or no role in outcomes. Finally, behavior in this market should not be affected by risk preferences, since there is no uncertainty about the value of the good being traded.

Procedures. Our subjects in the second experiment were 118 undergraduate students from a variety of classes at the College of William and Mary. Each group of ten subjects participated in one session consisting of a short lottery choice survey, followed by a ten-round market experiment. Each session lasted approximately one hour and fifteen minutes. Six sessions were conducted in December 2004, and six in January 2005. Experimental conditions were virtually identical across sessions; only the calendar date, time of day, and subject group differed.[13] At the beginning of the market trading, subjects were read the instructions and given the opportunity to ask questions.[14] The experiment was again conducted over a computer network in the Experimental Economics Laboratory at the College of William and Mary using the *V*econlab software.

Each subject was randomly assigned to be a seller or a buyer for all ten trading rounds. Value and cost information were revealed privately on each subject's computer screen. Subjects were separated by dividers preventing them from making eye contact or looking at another person's computer screen.

Subjects were told that values and costs may vary from person to person, but they would remain the same for all ten rounds. Each trading round lasted three minutes. Cumulative earnings, which averaged $5.07 for the December group and $5.20 for the January group, were added to a $7.50 show-up fee and additional earnings from a lottery choice game. All winnings were paid in cash at the end of each session.

Results. Our data consist of 1,076 observations from 118 experimental subjects: 58 in December and 60 in January. Summary statistics appear in Table 3. Prices in the first round of the December auctions averaged $5.97, just below the midpoint of the equilibrium price range. Prices in the first round of the January auctions were significantly higher, averaging $6.21. There is a general downward price trend across rounds in

Table 3. *Descriptive Statistics for 2004–2005 Double Auction Experiment*

	Pooled Data				First Round			
	December 2004		January 2005		December 2004		January 2005	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Market Price	5.91	0.53	5.98	0.55	5.97	0.56	6.21	0.56
Cumulative Earnings	5.07	4.00	5.20	4.07				
Participants	58		60		58		60	
Male Participants	32		31		32		31	
Monday Experiments	3		3		3		3	
Difference of Means Test for Market Prices in December and January (t)	2.24**				2.19**			

Note: Pooled results are based on 514 observations in December and 562 observations in January. First round results are based on fifty-two observations in December and fifty-six in January. Market price is the price at which a given subject agreed to buy or sell a unit, and cumulative earnings is the subject's cumulative earnings up to the time of the transaction. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

both months, with prices falling more in January than in December. However, the mean price for January was still significantly higher (by about $0.07) in the pooled results.

We established earlier that first round prices influence prices in subsequent rounds. Thus the only truly independent observations are those from the first round. We therefore estimate two market price models, one using first round prices only, and one using all trading rounds. The regression models use the same controls as the common value auctions. We include a dummy variable in all regression models to control for the fact that we had only eight subjects in one session.[15]

We omit the variables for cumulative earnings and round in the first round estimation model, because they are no longer relevant. We use ordinary least squares with heteroscedasticity-consistent standard errors to estimate the model. Our results appear in Table 4.

As with the common value auction, we find higher prices in January than in December. For the regressions using only the first round results, the variable *January* is positive and significant at the 1% level, with a coefficient of 0.370. Thus, *ceteris paribus*, prices in January are $0.37 higher than in December, which equals more than 6% of the mean December bid. The variables *eight participants* and *Monday* are also positive and significant at the 1% and 10% levels, respectively.[16] We do not note the presence of any gender-specific effects in this experiment.

Table 4. *Regression Results for 2004–2005 Double Auction Experiment*

	Pooled Data		First Round Only	
Market Price	**Coefficient**	**t**	**Coefficient**	**T**
Cumulative Earnings	0	−0.11		
January	0.23	8.76***	0.37	3.48***
Monday	0.59	21.48***	0.16	1.81*
Male	−0.01	−0.37	0.05	0.50
Eight Participants	1.07	45.64***	0.87	8.46***
Round	−0.02	−2.59***		
Constant	5.56	137.80***	5.71	49.61***
R^2	0.40		0.19	
F	418.30***		22.87***	
Number of Obs.	1,076		108	

Note: Market price is the *price* at which a given subject agreed to buy or sell a unit, *January* is a dummy variable with the value of 1 if the corresponding bid was placed during the month of January, *cumulative earnings* is a continuous variable to test for a cumulative earnings effect, *round* is included to test for a time trend, the dummy variable *Monday* is included to test for a weekday effect, and the dummy variable *male* is included to test for a gender-specific effect. *Eight participants* is a dummy variable included to control for the effect of fewer market participants in one session of our experiment. We use ordinary least squares with heteroscedasticity-consistent standard errors to estimate the model. Statistical significance at the 10%, 5%, and 1% levels is indicated by *, **, and ***, respectively.

In summary, prices in a double auction market start higher, but they decline faster in January than in December. Even considering price dynamics, January prices were still significantly higher. Prices were also higher on Mondays than on Wednesdays or Fridays, which is inconsistent with a "blue Monday" effect.

Conclusion

In the first experimental test of the January effect, we find an economically large and statistically significant effect in two very different auction environments with two distinct subject groups. The experiments spanned three different calendar years, with one pair of auctions conducted in December 2003 and January 2004, and another pair conducted in December 2004 and January 2005. Even after controlling for a wide variety of auxiliary effects, we find the same results. Barring a major unobservable difference between our January and December subjects in *both* study years, it appears that a psychological effect makes subjects willing to pay higher prices in January. Although caution must be used in applying laboratory results directly to non-experimental settings, there are reasons to believe our results can be generalized to naturally occurring situations, including financial markets.[17]

First, the break between fall and spring semesters for students is analogous to the break that many market participants take between Christmas and New Year's Day. U.S. financial markets are closed on Christmas and New Year's Day, and typically close early for Christmas Eve and New Year's Eve. During the remaining days between the two holidays, trading volume is much lower than during the rest of December and January, and many market participants take holidays at the same time students have winter break. If the January effect in these laboratory auction market experiments is attributable to a change in student mindsets across the winter holidays, there may be a similar effect among professional traders whose industry has a similar, albeit shorter, winter break.

A second reason why we may be able to extrapolate these results from the laboratory to Wall Street is that many William and Mary students go on to work in finance-related fields. In fact, it is quite likely that some of our experimental subjects have now become Wall Street traders. Hence any year-end (or holiday) psychological effect they feel as students is likely to carry over into the work force.

In addition to contributing to a large empirical literature on the January effect, our results are important for behavioral economics in general. While economics experiments have revealed that non-theoretical factors like envy and concerns over fairness play an important role in economic decision making, few have explored subconscious psychological effects like the January

effect. We have established that non-rational considerations can significantly alter economic behavior, even if only in a laboratory setting.

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Notes

1. See Shiller [1999, p. 1317].
2. In the only other experimental study of stock market calendar effects of which we are aware, Pettengill [1993] examined the "Blue Monday" effect, which attributes variations in equity returns across weekdays to investor mood shifts. He conducted a simulation in which subjects divided their portfolios among T-bills, blue-chip stocks, and small stocks. He found that some subject groups invested significantly more money in stocks on Friday, and significantly more in T-bills on Monday.
3. Specifically, the naïve bidding rule is: $b_i = 0.25L + (\frac{1}{4-2r})v_i$, where b_i is a given bidder's bid, v_i is the private value signal, L is the range of signal values, and r is the bidder's coefficient of risk aversion.
4. Session 1 only included one treatment.
5. A more comprehensive study of monthly effects in experimental auctions would include auctions in every month to determine how January differs from all other months. Budget constraints, however, forced us to limit the control to one month, and December was the logical choice.
6. We used a between-subjects design to avoid learning effects associated with having the same group of subjects participate twice in the same experiment. To control for learning effects using a within-subjects design would have required that half the subjects participate in December and then again in January, and the other half would participate first in January and then return in December of the same year. Potential learning effects with a twelve-month delay would likely be different from learning effects with a one-month delay. Using different subjects in December and January allowed us to avoid this complication.
7. The instructions are available on the *V*econlab web site at http://veconlab.econ.virginia.edu/admin.htm.
8. The *V*econlab software was developed by Charles Holt of the Department of Economics at the University of Virginia, and is available at http://veconlab.econ.virginia.edu/admin.htm.
9. In the event of a tie bid, the winner was determined by a random draw.
10. A computer problem required us to drop the observations of two of our original seventy-eight subjects.
11. Table 2 reports results from linear specifications, but we also explored the possibility of non-linear bidding behavior by including a squared term for the variable *Signal*. This term was found to be insignificant, and had no effect on the significance of our variables of interest.
12. Eckel and Grossman [2005] discuss gender effects in experiments.
13. As described in endnote 6, we used a between-subjects design to avoid potential learning effects associated with having the same group of subjects participate in two sessions of the same experiment.
14. The instructions are available on the *V*econlab web site at http://veconlab.econ.virginia.edu/admin.htm.
15. This smaller number of traders was not part of the original experimental design, but was the result of unusually low attendance for one session of the experiment.
16. We note a particularly strong weekday effect in the pooled regression results. The variable *Monday* is significant at the 1% level, with a coefficient of 0.592. However, the coefficient and the level of significance fall in the one-round estimation, suggesting that interround dependency might be affecting this coefficient.
17. An obvious, but costly, extension of this study is to replicate the experiments with a subject pool of professional traders. The study of subject pool effects is a relatively new but rapidly growing area of experimental economics and such an extension would be a valuable addition to this literature.

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