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

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Abstract

In the first experimental test of the January effect, we find an economically large and statistically significant result in two very different auction environments. After controlling for variables that could influence subjects' 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. The results suggest that psychological factors may contribute to the well-documented January effect in empirical stock market data, a conclusion that clearly violates the efficient markets hypothesis.

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Over six decades ago, Wachtel (1942) described a "January effect" in stock prices. After controlling for standard variables that are known to influence prices, there remains an unexplained component to a pattern of higher prices in January relative to the rest of the year. Many studies have explored a large number of non-psychological factors that might explain this observation, but economic variables cannot fully explain the January effect. We present a set of laboratory experiments to investigate this phenomenon in settings that rule out these non-psychological explanations.

The auction experiments described in this paper generate an economically large and statistically significant January effect. After controlling for variables that could influence subjects' 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. The results suggest that psychological factors may contribute to the well-documented January effect in empirical stock market data, a conclusion that violates the efficient markets hypothesis. Section I reviews empirical studies of the January effect in financial markets. Section II describes the experimental environments we used to investigate the January effect and section III concludes.

I. The January Effect

Wachtel (1942), who first described a January effect in financial markets, found that the Dow-Jones Industrial Average from 1927 to 1942 showed "frequent bullish tendencies" from December to January. Rozeff and Kinney (1976) found that the average return on an equal-weighted index of New York Stock Exchange prices from 1904 through 1974 was 3.5 percent during January and only about 0.5 percent during the other months. Banz (1981) showed that small firms had higher expected returns and Keim (1983) found that nearly half of the excess returns for small firms occurred during January. Moreover, half of the January returns came during the first five days of the month, particularly on the first trading day. Gultekin and Gultekin (1983) documented evidence of seasonality, mainly a January effect, in stock returns in 13 of 17 countries studies. Their results are particularly strong given that they used value-weighted indices that give less weight to small firms, which drive the January effect in U.S. data. Schwert (2003) concluded that the January effect weakened in the period from 1980 to 2001, but that it still existed.

A number of explanations of the January effect have been proposed and tested empirically. The explanation that has been most widely studied is the “tax-loss selling hypothesis” which was first described by Wachtel (1942). Wachtel proposed that heavy sales in mid-December to establish tax losses tend to drive security prices below what they should be in light of earnings. The corresponding rise in prices in January is simply a recovery from depressed levels in December. Wachtel showed that stocks with high yields in December, which are those investors are most likely to sell for tax purposes, have a greater price reaction in January than the overall market.

Reinganum (1983) measured potential tax-loss selling (PTS) and found that small stocks with high PTS had unusually high returns. He concluded that tax-loss selling explains some of the January effect, but also noted that the effect still existed even after the data were purged of PTS effects. Sias and Starks (1997) found that loser stocks dominated by individual investors yield lower average returns during December and higher average returns during January than loser stocks dominated by institutional investors. That result is consistent with the tax-loss selling hypothesis, but they also found a substantial January effect for winner stocks dominated by individuals, which contradicts the tax-loss selling hypothesis. Poterba and Weisbenner (2001) studied the January effect across different capital gains tax regimes in the U.S. and found evidence consistent with the tax-loss selling hypothesis. Jones, Lee, and Apenbrink (1991) concluded that the January effect intensified after the introduction of the income tax in the U.S. in 1917, thus lending credence to the tax-loss selling hypothesis.

Roll (1983) also found some evidence consistent with tax-loss selling, but called the explanation "patently absurd." Even if investors sell stocks for tax reasons, other investors would buy those stocks in anticipation of the price increase in January, thus eliminating the January effect. Further, the January effect occurs in countries in which there are no capital gains taxes and in which the tax year does not begin in January. Kato and Schallheim (1985) found that although there is no tax on capital gains for investors in Japan nor a tax benefit for losses, there is still a strong January effect in that country. Berges, McConnell, and Schlarbaum (1984) noted that Canada did not introduce a capital gains tax until 1973, but a January effect existed there both before and after the introduction of the tax. Despite the fact that Australia has a tax year that ends in June, Brown, Keim, Kleidon, and Marsh (1983) documented a January effect in Australian stock returns. They concluded that the relationship between the U.S. tax year and the January effect may be more correlation than causation.

A further problem with the tax-loss selling hypothesis is that the unusual returns in January seem to persist over a period of several years. DeBondt and Thaler (1985) found that the firms whose stocks were the biggest losers over a period of five years had unusually large returns over the next several years. In particular, the excess returns for the loser portfolios were concentrated in January. Notably, the unusual January returns existed for several years after portfolio formation, long after any tax benefits gained from selling the stocks at the end of the year.

Another potential explanation for the January effect is the "window-dressing explanation" which holds that fund managers do not want their annual reports to list shares that have declined sharply in value during the previous year even if they would otherwise prefer to hold those stocks. The managers sell losers at the end of the year and buy them at the beginning of the year, thus generating the January effect. Lakonishok, Shleifer, Thaler, and Vishny (1991) found that in every quarter, funds sell poorly performing stocks and that this pattern accelerates in the fourth quarter. Chen and Singal (2004) argued that if window dressing drives the January effect, a similar pattern should exist during other quarters. They studied the June through July period, found few similarities to the December through January period, and concluded that window dressing does not cause the January effect.

Ogden (1990) argued that the substantial increase in business activity near the end of the calendar year results in greater profits in December and the corresponding increase in liquidity in January puts upward pressure on stock prices. This liquidity hypothesis does not explain why the January effect exists primarily among small stocks as greater profits would presumably cause the entire market to increase. Further, both the liquidity and window-dressing hypotheses are subject to Roll's critique that the market should exploit such obvious mispricing, particularly as it still exists more than a half century after Wachtel (1942) documented the January effect.

Keim (1989) argued that market microstructure issues may contribute to the January effect. His work showed systematic tendencies for December closing prices to be recorded at the bid and January closing prices to be recorded at the ask, which means that stock returns over this period could appear high even if the bid and ask prices did not change. Later studies, though, such as Jones, Lee, and Apenbrink (1991), Poterba and Weisbenner (2001), and Chen and Singal

(2004), explicitly accounted for this critique by using alternative return measures and still found a January effect.

The January effect could also be driven by real economic changes that occur at the end of the year such as macroeconomic news or changes in risk premia. Seyhun (1988) investigated the possibility that the large returns for small firms in January represent compensation for the increased risk of trading against informed investors, who are more likely to have private information at the end of the year. Based on an analysis of insider trading in small firms, Seyhun concluded that the January effect cannot be interpreted as compensation for trading against informed traders. Barry and Brown (1984) presented evidence that firms for which investors have less information are riskier and hence, have higher expected returns. As investors presumably have less information about small firms, their results help explain why small-firm stocks react differently from large-firm stocks to information that becomes available in January.

Seyhun (1993) used a stochastic dominance approach and found that January returns in small firms dominate all other size-based portfolios. He concluded that the January puzzle is greater than previously thought because omitted risk factors cannot explain the January effect. Christie-David and Chaudhry (2000) found that returns on interest-rate instruments respond differently to macroeconomic announcements in January compared to other months. They concluded that their results are consistent with either the tax-loss selling or window-dressing explanations. Lu and Ma (2004) showed that positive earnings news partially accounts for the January effect in the second half of the month, but cannot explain the effect in the first half of the month.

There is also the possibility that the January effect is simply the result of data mining. Sullivan, Timmermann, and White (2001) used a bootstrap method that involved considering a large universe of plausible calendar rules, and concluded that the evidence supporting calendar anomalies is weak when viewed in that context. However, the fact that the January effect is well-documented in so many markets and over such a long period of time even after its discovery suggests that data mining is not a full explanation of this phenomenon.

The fact that fundamental economic variables do not appear to fully explain the January effect has caused some to attribute it to non-fundamental causes. In his original paper, Wachtel (1942) argued that investor psychology may contribute to the January effect. The unusual return for stocks at the end of the year may arise from the "general feeling of good fellowship and cheer existing throughout the Christmas holidays…" and the "…widespread hope that the new year will prove better than the old."[1] More recently, Shiller (1999) linked the January effect to the tendency of individuals to place particular events into mental compartments. According to Shiller, "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."[2] Psychological explanations are consistent with the fact that the January effect is a small-firm phenomenon. If relatively subtle psychological factors were present, one would expect them to have less impact on large, heavily traded shares that attract a great deal of analyst attention compared to thinly traded shares. The fact that the January effect is strongest among firms that have performed poorly during the previous year is also consistent with the psychological explanations. If, as Shiller conjectures, investors view the new year as a new beginning, they may rethink their assessments of stocks that experienced sharp price declines.

In sum, the causes of the January effect are not fully understood. There is evidence that tax-loss selling contributes to the higher average returns of small stocks in January. However, the January effect exists in markets with no capital gains taxes and in markets with different tax years, which implies that the tax-loss selling hypothesis cannot be a complete explanation. Research seems to rule out window dressing and increased liquidity as the sources of the January effect while information made available at the end of the year offers a partial explanation at best. Market microstructure issues can impact January returns, but the January effect exists even after accounting for those issues. Given that the alternatives do not fully explain the January effect, one cannot rule out psychological explanations. Since the market-related explanations discussed above can be completely controlled in the laboratory, economic experiments are an ideal environment to test whether psychological effects alone can generate higher prices in January than in December. Therefore, we present the first experimental test of the January effect.[3]

II. Experimental Analysis

We explore the existence of a January effect in two very different auction environments spanning three calendar years. In a *common value auction*, subjects do not know the value of the good they are bidding on and they compete against one other bidder. In this environment, a large body of experimental results reveals that behavior deviates from the Nash equilibrium prediction in the form of overbidding. In a *double auction market*, subjects know exactly how much money they will earn when they engage in a trade and they compete in a market with five buyers and five sellers. We use a "box design" where all buyers have the same value for making a trade and all sellers have the same cost, which generates a range of equilibrium prices. In addition, subjects are not allowed to trade at a loss, so all transactions are in the equilibrium price range. Other researchers have found no clear pattern of results in using this box design with multiple equilibria. These two experiments are described in greater detail below.

A. 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 value of the prize 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 to this game is relatively simple and second, the auction shares important similarities with the financial markets that others have studied in the context of the January effect. Both, for example, involve a "prize" with an uncertain future 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 are those who fail to realize that winning the auction puts an upper limit on the other player's private value signal, and who consequently overbid. Rational bidders do not fall prey to this error. Holt and Sherman (2000) show that it is rational for players in this two-person auction to bid half of their private value signal. The naïve bidding strategy is more complicated and depends on a bidder's degree of risk aversion.[4]

Holt and Sherman (2000) report bids that are significantly higher than the rational bid and in many cases, higher than the naïve bid. There is a well developed experimental literature on common value auctions which is reviewed in Kagel and Levin (2002). In general, other research has confirmed the results reported in Holt and Sherman; Bidders frequently fall prey to the winner's curse which results in significant overbidding relative to the rational Nash equilibrium prediction.

A.1. Procedures

Eighty undergraduate students from the College of William and Mary were recruited from a variety of classes to serve as subjects in this experiment. Each group of 10 people participated in one session consisting of two treatments with 15 decision-making rounds per treatment.[5] Each session lasted approximately one hour. Four sessions were conducted in December 2003 and four were conducted in January 2004. Experimental conditions were virtually identical across sessions; only the calendar date and the subject group differed.[6] At the beginning of each session, subjects were read the instructions in Appendix A and offered the opportunity to ask questions. 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 developed by Charles Holt of the Department of Economics at the University of Virginia.

At the beginning of each decision-making round, subjects were randomly paired. Pairings were anonymous and subjects were separated by dividers that prevented 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 to be 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 value signal of the other member of the pair.

After both subjects placed their bids, the prize was awarded to the high bidder in each pair.[7] The winner earned the difference between the true prize value and her bid. Negative earnings were subtracted from a subject's cumulative earnings. Earnings were cumulative across rounds and treatments. 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 and averaged $6.99 for the December group and $7.22 for the January group.

A. 2. Results

The data for this experiment consist of a total of 2130 observations from 76 experimental subjects: 38 in December and 38 in January.[8] Consistent with previous experimental research, we find significant overbidding relative to the rational Nash prediction. The bid to value ratio ranged from 1.6 to 2.6, depending on the month and the range of possible value signals. Other summary statistics appear in Table I. The mean bid for the month of January is approximately $0.24 higher than that of 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 one percent level. Note however that the average private value signal in the month of January is higher than that of December (4.145 versus 3.859), which likely 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, cumulative earnings, round, Monday, gender,[9] and January.

TABLE I ABOUT HERE

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 homeskedasticity at the one percent level. We therefore use robust, heteroskedasticity-consistent standard errors. In both the random effects model and the pooled model with clustering, we group by individual subject. The results appear in Table II.

TABLE II ABOUT HERE

In all three regressions the coefficient on *January*, a dummy variable that takes a value of one if the corresponding bid was placed in January, is positive and statistically significant at the ten percent level or higher. Further, the estimates of this coefficient are stable across the specifications, suggesting that any individual effects, if present, are small. The variable *Signal* is positive and significant at the one percent level in all regressions. In addition, *Cumulative Earnings*, *Male*, and *Round* are negative and significant at the one percent level in all regressions. Finally, the variable *Monday* is positive and significant at the ten percent level in the pooled model without clustering.[10]

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. In addition, the degree of overbidding is significantly higher (more than 20 cents on average) 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 in 30 rounds of play.

B. Double Auction Experiment

The double auction experiment was invented by Nobel laureate Vernon Smith (1962). Market participants are designated to be 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 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. In this case market prices in a double auction market with a box design and multiple equilibria 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 of the experiment anchor prices for the subsequent rounds. There is additional evidence that psychological factors influence the division of surplus in this setting. For example, 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) report that status results in higher prices when the sellers have status and it results in lower prices when the buyers have status, regardless of whether the traders perceive the status as real or random.

This evidence that psychological factors can influence market outcomes makes this particular double auction design an appealing setting to study the January effect. An additional advantage of this design is that subjects face a very simple decision making problem, so decision error should play little to 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.

B.1. Procedures

One hundred and twenty undergraduate students from the College of William and Mary were recruited from a variety of classes to serve as subjects in this experiment. Each group of ten people participated in one session consisting of a short lottery choice survey[11] 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 were conducted in January 2005. Experimental conditions were virtually identical across sessions; only the calendar date and the subject group differed.[12] At the beginning of the market trading, subjects were read the instructions in Appendix B, and offered the opportunity to ask questions. 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 developed by Charles Holt of the Department of Economics at the University of Virginia.

Subjects were randomly assigned to be a seller or a buyer for all ten trading rounds. Value and cost information were privately revealed on each subject's computer screen. In addition, subjects were separated by dividers that prevented 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 and would remain the same for all rounds of the experiment. Each trading round lasted three minutes. Earnings for the double auction experiment averaged $5.07 for the December group and $5.20 for the January group. These earnings were added to a $7.50 show up fee and additional earnings from the lottery choice game and paid in cash at the end of each session.

B.2. Results

The data for this experiment consist of 1076 observations from 116 experimental subjects: 58 in December and 60 in January. Summary Statistics appear in Table III. Contract prices in the first round of the December auctions averaged $5.97, which is just below the midpoint of the equilibrium price range. Contract prices in the first round of the January auctions were significantly higher, averaging $6.21. There is a general downward trend in prices across rounds in both months, with prices falling more in January than in December. However, the mean price for the month of January was still significantly higher (by about $0.07) than that of December in the pooled results.

TABLE III ABOUT HERE

As noted above, the one empirical regularity that has been established with the multiple equilibria box design is that first round prices influence prices in subsequent rounds. As a consequence, the only truly independent observations are those from the first round of each session. We therefore estimate two models of market prices, one using first round prices only and one using all trading rounds. The regression models use the same controls as those in the common value auctions. In all regression models, a dummy variable is included to control for the fact that we had only eight subjects in one session of the experiment.[13] In the first round estimation model, the variables for cumulative earnings and round are omitted as they are no longer relevant. We use ordinary least squares with heteroskedasticity-consistent standard errors to estimate the model and the results appear in Table IV.

TABLE IV ABOUT HERE

As with the common value auction, we find, *ceteris paribus*, a statistically significant increase in prices during the month of January. The variable *January* is positive and significant at the one percent level with a coefficient of 0.370. The variables *Eight Participants* and *Monday* are also positive and significant at the one and ten percent levels respectively.[14] A gender-specific effect does not appear to be present in this experiment.

In summary, prices in a double auction market started higher, but declined faster in January than in December. Even when price dynamics are taken into account, prices were still significantly higher (by about 37 cents) in January. Prices were also higher on Mondays than on Wednesdays or Fridays, which is inconsistent with a "blue Monday" effect.

III. 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. Further, the experiments spanned three different calendar years, with one pair of auctions conducted in December 2003 and January 2004 and another pair of auctions conducted in December 2004 and January 2005. Even after controlling for a wide variety of auxiliary effects, we find the same result. The January effect is present in laboratory auctions, and the most plausible explanation is a psychological effect that makes people willing to pay higher prices in January than in December. In addition to contributing to a large empirical literature on the January effect, this is an important result for behavioral economics in general. While economics experiments have revealed that non-theoretical factors such as envy and concerns for fairness play an important role in economic decision making, few have explored subconscious psychological effects like the one we document here.

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Endnotes

[1] See Wachtel (1942), p. 186.

[2] See Shiller (1999), p. 1317.

[3] Pettengill (1993) used experimental techniques to examine 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 and found that some subject groups invested significantly more money in stocks on Friday and significantly more in T-bills on Monday. He concluded that this pattern of shifting assets from equities to T-bills is consistent with the Blue Monday effect. However, the weekend effect generally refers to systematically lower returns in financial markets on Mondays. Gibbons and Hess (1981), for example, found abnormally low returns for both stocks and treasury bills. Instead of generally lower returns on Mondays, Pettengill's results suggest that stocks should have relatively low returns on Mondays while T-bills should have relatively high returns on Mondays as investors shift their money from higher-risk to lower-risk assets. That pattern is not consistent with the empirical evidence that established a weekday effect. Pettengill's study also has some methodological differences with our study. First, he did not include controls for standard variables such as gender or round of play. Second, his instructions contained some phrases like "How lucky do you feel today?" while our instructions do not refer to the subject's mood or psychology in any way. Third, Pettengill studies changes in risk preferences across different time periods and, because no prices are generated in those experiments, he must infer how the changes in risk preferences lead to changes in returns. Our experimental markets directly generate prices, which means that we can directly observe price differences across time periods.

[4] Specifically, the naïve bidding rule is: $b_i = 0.25L + \left(\frac{1}{4-2r}\right)v_i$,where b_i is a given bidder's bid, v_i is her private value signal, L is the range of signal values and r is the bidder's coefficient of risk aversion.

[5] Session 1 did not include treatment B.

[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 require that half of the subjects participate in December and then again in January and the other half of the subjects would first participate in January and then return in December of the same year. Potential learning effects with a 12-month delay would likely be different from learning effects with a one-month delay. Hence, using different subjects in December and January avoided this complication.

[7] In the event of a tie bid, the winner was determined by a random draw.

[8] A computer problem required us to drop the observations of two of our original 78 subjects.

[9] Gender effects in experiments are discussed in Eckel and Grossman (2005).

[10] This is inconsistent with the finding of a "Blue Monday" effect by Pettengill (1993), albeit his experiments were conducted in a very different experimental environment.

[11] The lottery choice survey revealed no significant difference in risk preferences across December and January.

[12] As described in footnote 6, we used a between-subjects design to avoid potential learning effects associated with having a group of subjects participate in two session of the same experiment.

[13] 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.

[14] We note a particularly strong weekday effect in the pooled regression results. The Variable *Monday* is significant at the one percent level with a coefficient of 0.592. However, the coefficient and the level of significance fall in the one-round estimation, suggesting that inter-round dependency might be affecting this coefficient.

Table I

Descriptive Statistics for the Common Value Auction Experiment

Table I presents descriptive statistics for our 2003-2004 Common Value Auction Experiment. Results are based on 990 observations in December and 1140 observations in January. *Bid* is a given subject's bid for the common value "prize," *Signal* her private value signal, and *Cumulative Earnings* her cumulative earnings up to the point of the bid in question. Means and standard deviations are rounded to the nearest thousandth, test statistics to the nearest hundredth. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

	December 2003		January 2004	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Bid	3.154	1.711	3.39	1.725
Signal	3.859	2.664	4.145	2.722
Cumulative Earnings	6.992	3.341	7.224	4.331
Participants	38		38	
Male Participants	19		16	
Monday Experiments	2		0	
Dec/Jan Difference of Means (t)	3.16***			

Table II

Regression Results for the Common Value Auction Experiment

Table II presents the regression results from our 2003-2004 Common Value Auction experiment. *Bid* is a given subject's bid for the common value "prize," *January* is a dummy variable that takes on the value one 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. Coefficient estimates are rounded to the nearest thousandth, test statistics to the nearest hundredth. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

	Robust OLS		**GLS, Random Effects**		**OLS, Cluster**	
Bid	**Coefficient**	**T**	**Coefficient**	**z**	**Coefficient**	**t**
Signal	0.411	37.20***	0.401	43.71***	0.411	19.24***
Cumulative Earnings	-0.068	-8.01***	-0.058	-7.73***	-0.068	-4.96***
Monday	0.116	1.67*	0.124	0.91	0.116	0.89
January	0.201	3.61***	0.207	1.76*	0.201	1.66*
Round	-0.043	-13.44***	-0.044	-15.30***	-0.043	-7.96***
Male	-0.31	-6.18***	-0.318	-3.31***	-0.31	-3.09***
Constant	2.765	24.14***	2.748	20.04***	2.765	13.04***
R^2	0.588		0.588		0.588	
F	464.34***				140.91***	
Wald Chi-Square			3049.20***			
Number of Observations	2130		2130		2130	
Number of Groups			76		76	

Table III
Descriptive Statistics for the Double Auction Experiment

Table III presents descriptive statistics for our 2003-2004 Double Auction Experiment. Pooled results are based on 514 observations in December and 562 observations in January. First round results are based on 52 observations in December and 56 observations in January. *Market Price* is the price at which a given subject agreed to buy or sell a unit and *Cumulative Earnings* her cumulative earnings up to the point of the transaction in question. Means and standard deviations are rounded to the nearest thousandth, test statistics to the nearest hundredth. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

	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	5.906	0.533	5.98	0.547	5.973	0.56	6.208	0.555
Cumulative Earnings	5.07	4.004	5.201	4.072				
Participants	58		60		58		60	
Male Participants	32		31		32		31	
Monday Experiments	3		3		3		3	
Dec/Jan Difference of Means (t)	2.24**				2.19**			

Table IV
Regression Results for the Double Auction Experiment

Table IV presents the regression results from our 2004-2005 Double Auction Experiment. *Market Price* is the price at which a given subject agreed to buy or sell a unit, *January* is a dummy variable that takes on the value one 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. *Eight Participants* is a dummy variable to control for the effect of fewer market participants in one session of our experiment. We use ordinary least squares with heteroskedasticity-consistent standard errors to estimate the model. Coefficient estimates are rounded to the nearest thousandth, test statistics to the nearest hundredth. Statistical significance at the 10, 5, and 1 percent levels is indicated by *, **, and ***, respectively.

	Pooled Data		**First Round Only**	
Market Price	**Coefficient**	**t**	**Coefficient**	**t**
Cumulative Earnings	0	-0.11		
January	0.225	8.76***	0.37	3.48***
Monday	0.592	21.48***	0.195	1.81*
Male	-0.01	-0.37	0.051	0.5
Eight Participants	1.068	45.64***	0.867	8.46***
Round	-0.016	-2.59***		
Constant	5.557	137.8***	5.713	49.61***
R^2	0.398		0.186	
F	418.30***		22.87***	
Number of Observations	1076		108	

APPENDIX A : INSTRUCTIONS FOR COMMON VALUE AUCTION

(reprinted from the Veconlab web site: http://veconlab.econ.virginia.edu/admin.htm)

- **Rounds:** The experiment consists of a sequence of "rounds".

- **Matchings:** In each round, you will be matched with another person selected at random from the other participants. Each of you will submit a bid for a prize being sold in an auction.

- **Prize Values:** Before bidding, you will not know precisely the value of the item being auctioned. Instead you will receive a "signal" that tells you something about the value. The person who you are matched with will typically receive a different signal. The money value of the item is the average of these two signals. This money value is how much you earn, after we deduct your bid, if you have the high bid in the auction. The signals, which will be determined randomly, will generally differ from person to person as explained below.

- **Bid:** After finding out the value of your signal (but not that of the other person), you will choose a number or "bid". The other person will also choose a bid at the same time. You cannot see their signal or their bid while making your bid, and vice versa.

- **Earnings:** A bid is an amount of money offered for the item, and the person with the high bid will purchase the item being sold. A random "coin flip" will select the winner in the event of a tie bid. The winner will earn an amount that equals the average of the two signals, minus their own bid. The other bidder will earn nothing for that round. To see some examples, press:

- **Example 1:** If each of the two signals is 2, then the prize value is 2 cents. In this case, if you make a bid of 1 cent, you would earn 2 - 1 = 1 cent if you have the high bid. You would earn 0 otherwise.

- **Example 2:** Suppose that both you and the other person tie with bids of 2 cents. Then we would use a computer-generated random number to select the winner, who would earn the difference between the prize value (average of signals) and the bid of 2 cents. The loser would earn 0 cents. Note: The random device is like a fair coin flip, it ensures that each person has an equal chance of winning in the event of a tie, regardless of their signal, their bid, or of whether or not they have won in previous rounds.

- **Note:** The numbers used in the actual experiment to follow will be much larger than these numbers, which are for illustrative purposes only. Now let's look at the actual numbers to be used.

- **Possible Signal Values:** At the beginning of each round, the computer will select a randomly determined signal for you, which may be any penny amount between **$0.00** and **$1.00**, with each amount in this interval being equally likely to be chosen. Imagine a

- roulette wheel with the stops labeled as 0.00, 0.01, ... 0.99, 1.00. Then a hard spin of the wheel would make each of these signals equally likely. Your signal will be independent of the other person's signal, so it's as if we spin the wheel once for you and a different time for the other person. Your signal in one period is independent of that in the next, so it's as if we spin the wheel for each bidder again at the start of each period.

- **Bids:** The round begins when you and the other person find out your own signal values, but neither of you will know the other's signal at that time. Then each of you will select a bid. In addition, your bid must be greater than $0.00.

- **Earnings:** If you are the high bidder or win by random draw in the event of a tie, your earnings will equal the value if the item (average of the signals) minus your bid amount. Otherwise, your earnings will be zero for the round. Notice that you will not know the value of the item when you make your bid, and you will only find out this value if you happen to have the high bid and end up purchasing the item. Since you do not know the prize value for sure when you make a bid, it is possible that you may end up bidding more than its value, in which case your earnings if you "win" will be negative, and will be subtracted from your earnings in previous rounds of bidding. Positive earnings will be added to your total.

In the following examples, please use the mouse button to select the best answer.

Question 1: Suppose your signal is $X.

a) Then the value of the item is the average of both signals (including $X).

b) Then the value of the item is the sum of $X and the other's signal.

Question 2: Suppose you have a signal of $X and your bid of $B is equal to the other's bid.

a) If you win the random draw, you will earn $X - $B and the other bidder will earn 0.

b) The person who wins the random draw will earn an amount that is greater than or equal to $X/2 - $B.

- At the beginning of each round, there will be a new random pairing of all participants, so the person who you are matched with in one round may not be the same person you are matched with in the subsequent round. Matchings are random, and you are no more likely to be matched with one person than with another.

- There is a new random draw for each person at the start of a round to determine that person's signal. Signal values are equally likely to be any penny amount between **$0.00** and **$1.00.** The value of the item being auctioned is the average of the 2 signals. After seeing their own signal, but not the other's signal, each person will choose a bid. The high bidder earns the difference between the average of the signals and their bid, and the low bidder earns 0, with ties decided by the flip of a coin. If the value of the item turns out to be less than the amount of the winning bid, that person will have negative earnings, which will be subtracted from that person's total earnings.

- There will be a number of rounds, with random rematchings at the beginning of each. You will begin with an initial payment of **$7.00**, to which earnings will be added. In the event that you lose money in a period, we will subtract the loss from this amount. The computer program will keep track of your cumulative earnings from round to round. If you have a question now, please raise your hand and someone will come to your desk to answer it.

APPENDIX B: INSTRUCTIONS FOR DOUBLE AUCTION MARKET

(reprinted from the Veconlab web site: http://veconlab.econ.virginia.edu/admin.htm)

- **Rounds:** The experiment consists of a sequence of market trading periods or "rounds".

- **Roles:** Each person will either be a **buyer** or a **seller** in all rounds of this part of the experiment. Buyers submit prices to buy units of a commodity to be described below, and sellers submit price offers to sell such units. We will refer to buyers' submissions as "*bid prices*" and to sellers' offers as "*ask prices*". Your role in this part is that of a (seller or buyer).

- **Earnings:** In a given period, each seller will have up to 1 units of a commodity to sell, and they will be told the **monetary cost** of producing each of these units. Sellers may earn money by selling at a price that is above the cost of a unit. Similarly, each buyer will have up to **1** units of a commodity to buy, and they will be told the **monetary value** of each of these units. Buyers may earn money by buying at a price that is below the value of the unit.

- **Your Role:** Seller. For each unit that you sell, you will earn the difference between the selling price of the unit and your cost for that unit. So high prices and low costs are good for sellers like you. The costs for your 1 units will be given to you, and the determination of price is explained next.

- **Or: your Role:** Buyer. For each unit that you purchase, you will earn the difference between your monetary value of the unit and the purchase price. So high money values and low prices are good for buyers like you. The money values for your 1 units will be given to you, and the determination of price is explained next.

- **Bid and Ask Prices:** When the market opens, any buyer may submit a bid price at which he or she is willing to purchase a unit. Similarly, any seller may submit an offer (or "ask") price at which he or she is willing to sell a unit.

- **Transactions:** A transaction is finalized when a buyer accepts a seller's offer, or when a seller accepts a buyer's bid.

- **Bid-Ask Spread:** At all times the program will display the highest outstanding bid to buy and the lowest outstanding offer to sell. The lowest offer will be above the highest bid, and the difference will be called the "bid-ask spread".

- **Bid and Ask Revisions:** A new bid from a buyer need not be above the buyer's own outstanding bid (if any) for that unit. Similarly, a new ask from a seller need not be below the seller's own outstanding offer (if any) for a particular unit. The option to adjust one's own bid or ask in either direction permits one to correct an error or essentially withdraw a bid (by lowering it) or withdraw an ask (by raising it).

- **Making a Purchase or Sale:** A buyer can make a purchase by offering to pay a seller's price, i.e. by entering a bid that is at or above a seller's ask price. Similarly, a seller can make a sale by entering an offer that is at or below a buyer's bid price.

- **Resubmission of Bids or Offers:** A transaction automatically cancels all prior bids and offers made by the buyer and seller involved, although they are free to enter new bids and offers if they have additional units to buy or sell. Bids and offers made by those who are not involved in the transaction do not have to be reentered; they remain in the queue, although only the lowest available offer and the highest available bid will show at any moment.

- **Example 1:** Suppose buyer 1 makes a bid of 1 and seller 3 makes an offer of 3. The next message could be **either** a new bid (from buyer 1 or from another buyer) **or** a new offer (from seller 3 or from another seller). Suppose that buyer 2 first bids 1.5 and then raises that to 2, and seller 4 accepts by making an offer at 2. Then both of buyer 2's bids would be removed, and the highest available bid would be buyer 1's original bid of 1. Seller 3's offer of 3 would still stand and would represent the lowest offer at that point.

- **Example 1 (continued):** When seller 4 sells to buyer 2 at a price of 2, each of them will earn money. If buyer 2's value for the unit were 6, then the buyer would earn 6 - 2 = 4. Similarly, if seller 4's cost were 1, then the seller would earn 2 - 1 = 1 on the sale. The buyer would not have been permitted to pay more than 6 for the unit with a value of 6, and the seller would not have been permitted to sell at a price below 1 for the unit with a cost of 1.

- **Subsequent Units:** If seller 4 were to have a second unit with a cost of 2, the seller's next offer would have to be at least 2. In contrast, seller 1 who did not yet sell the unit with a cost of 1 would be able to submit any offer that is greater than 1. So you see that some sellers may be trying to sell their first units at the same time that others are trying to sell their second units. Similarly, a buyer who makes a purchase and who has an additional unit value will be required to bid below the value of that second unit.

Page 5 - On this page I will first read the instructions for buyers, and sellers can listen, and then I will read the instructions for sellers, and buyers can listen.

- **(Buyer) Values:** In this market, you are a buyer with 1 units that can be bought. The values for these units are shown in the table below (on your screen if you are a buyer). For example, the first unit has a value of **$*.****.

- **(Buyer) Earnings:**. Thus the first unit that you buy will yield money earnings of **$*.** - price paid**. Of course, if a unit is not purchased, no price is paid but no value is obtained.

- **(Seller) Costs:** In this market, you are a seller with 1 units that can be sold. The costs for these units are shown in the table below (on your screen if you are a seller). For example, the first unit has a cost of **$*.****.

- **(Seller) Earnings:**. Thus first unit that you sell will yield money earnings of **sale price - $*.****. Of course, if a unit is not sold, no price is received, but no cost is incurred.

- **Earnings:** Suppose that you made a price (bid or ask) of $*.** on the first unit, and that this price (bid or ask) was accepted. Then the number in the price column would be the same as the number in the bid/ask column, and the associated earnings would be calculated as shown in the right-hand earnings column below. Earnings are listed as $0.00 on all unpurchased units.

- **(Bid or Ask) Prices:**. You enter your (bid or ask) price in the bid/ask column (try typing in a number, with a decimal to distinguish dollars and cents).

	value	**bid/ask**	**price**	**earnings**
unit 1	$	**$*.****	$*.**	$*.**
unit 2	$			$

In the following examples, please use the mouse button to select the best answer. You may change your answers as many times as you wish by clicking on the other dot.

Question 1: Suppose that the market period begins with buyer 1, who bids $1.00 on the buyer's first unit. Which is correct?

a) The next submission **must** be a bid above $1.00.

b) The next submission **may** come from any buyer (including buyer 1) or from any seller, or it may consist of a seller accepting the buyer's bid.

Question 2: Suppose that the first buyer bids $7.00 and a seller decides to accept by entering an ask of $7.00. Before this seller can enter the ask, however, a second buyer bids $7.50. The seller, not knowing this, enters the ask of $7.00, *after* the bid of $7.50 is confirmed. The seller, of course, would be willing to sell at this higher bid price. What will the unit sell for?

a) $7.50

b) $7.00

- **Values and Costs:** Unit values may differ from buyer to buyer, and unit costs may differ from seller to seller. There may be several trading "periods," and the values and costs will not change from period to period.

- **Bids and Asks**: Any buyer may submit a bid at any time, or may accept the lowest available ask price. Any seller may submit an ask at any time or may accept the highest available bid. A bid must be no higher than the buyer's value for the unit. An ask must be no lower than the seller's cost for the unit.

- **Resubmissions**: A trade between a buyer and a seller results in the cancellation of all prior bids for that buyer and of all prior asks for that seller. Others' bids and asks still stand and do not need to be resubmitted.

- **Earnings:** Buyers earn money on one or more units by buying at prices that are below unit values, and sellers earn money by selling units at prices that are above costs. Final earnings for a period are the sum of earnings for all units transacted; earnings are zero for untraded units.

- **Periods:** Each trading period will last 5 minutes, at which time total earnings for that period are calculated and a new period begins. The experimenter will keep track of the time and make announcements (or send messages) about remaining time.
  In particular, I will keep time and close the period from the PC I am working on. Are there any questions?