Relative Performance Information in Asset Markets:

An Experimental Approach

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Eric J. Schoenberg[a] and Ernan Haruvy[b]

[a] Columbia Business School, Uris Hall 316E, 3022 Broadway, New York, NY 10027, 212-854-0330, ejs2011@columbia.edu

[b] University of Texas at Dallas, 800 W. Campbell Rd, Richardson, TX 75080, 972-883-4865, eharuvy@utdallas.edu

ABSTRACT

An important issue in the study of asset market bubbles is the extent to which traders are influenced by their perceived performance relative to other traders. Extant research on laboratory asset market bubbles has generally kept performance information private, effectively excluding such considerations from experimental control. We provide traders in an experimental market with a 15-period finitely lived asset with periodic performance information for one other trader—either the best performer or the worst performer—and find a significant effect on trading behavior, satisfaction and market prices in the session.

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1. Introduction

The recent dramatic economic downturn, widely believed to have resulted from a broad mispricing of financial assets (especially, but not exclusively, mortgage backed securities) has increased interest by economists and public policy makers in asset market bubbles[1]. Of particular note are the large losses suffered by experienced financial professionals, apparently due their sustaining rather than correcting these mispricings, leading Alan Greenspan to express a "shocked disbelief" at bankers' failure to protect their own institutions. Camerer and Fehr (2006) attribute this failure to "institutional constraints such as performance pressure," that is, to the perceived (and possibly real) pressure to match the perceived performance of peers. Research suggests that this is a particularly important motivation for professional investors since relative performance has a large impact on the ability to increase funds under management (Sirri and Tufano 1998).

Beyond institutional constraints, there is copious experimental and correlational evidence that individual utility functions are affected by relative comparisons (Fehr And Schmidt 1999, Dufwenberg and Gneezy 2000, Charness and Rabin 2002, Diener and Biswas-Diener 2002, Luttmer 2005, Firebaugh and Tach 2005). DeMarzo, Kaniel, and Kremer (2008) provide a theoretical link between relative wealth concerns and asset bubbles using a finite horizon, overlapping-generations general equilibrium asset pricing model in which rational agents are concerned about the affordability of future "scarce goods." Since the price of the scarce goods will be determined by the overall wealth of

[1] Bubbles are generally considered to occur when the market price of an asset exceeds the expected value of its discounted future cash flows, at high volumes and for extended periods.

their cohort, traders rationally choose to herd in order to avoid the risk of poor relative performance, which can drive the price of a risky asset well above expected value.

In this study, we examine the impact of providing relative performance information in an experimental asset market where traders trade a finite-lived asset over 15 periods (Smith, Suchanek, and Williams 1987). A large literature has demonstrated that inexperienced traders in these markets typically generate a bubble-like pattern in asset prices, which begin below expected value and then exceed expected value for much of the experiment before collapsing near the end. This pattern is robust to a wide variety of manipulations (Porter and Smith 2003)[2] but is mitigated by the presence of even a minority of thrice-experienced traders (Dufwenberg, Lindqvist, and Moore 2005).

James and Isaac (2000) demonstrate that bubbles persist even after repeated experience when traders are incentivized to achieve above-average performance. In their design, traders receive feedback after each market period on both their own cash earnings and the average cash earnings for all traders and receive an incentive payment only if their cash earnings are greater than the market average after the final market period. They argue that under this tournament payment structure, rational traders who are below average at the beginning of the final market period will be willing to pay above expected value for the asset in the hope that a high dividend payment will move them above average, fracturing the backward induction argument that equates fundamental value with expected value (Tirole 1982).

[2] The literature on such markets includes studies on the impact of different institutional features, e.g. futures and spot markets (Porter and Smith 1995, Noussair and Tucker 2006), margin buying and short selling (King, Smith, Williams and Van Boening 1993, Haruvy and Noussair 2006), call markets (Van Boening, Williams, and LaMaster 1993), liquidity (Caginalp, Porter and Smith 1998, Caginalp, Porter and Smith 2001), capital gains taxes (Lei, Noussair, and Plott 2002), opportunities for speculation (Lei, Noussair, and Plott 2001) and characteristics of the traded asset, such as the skewness of its dividends (Ackert, Charupat, Deaves, and Kluger 2006) and its terminal value (Hirota and Sunder 2007).

Our approach differs from James and Isaac (2000) in that they jointly manipulate *both* the incentive structure *and* the information provided to traders whereas we study the effect of relative payoff information by itself. In other words, we ask whether traders might have preferences with regards to relative payoffs which affect their behavior and thus the evolution of market prices even in the absence of any direct monetary incentives linked to relative performance.

Another methodological difference between the current work and James and Isaac (2000) pertains to the form of the information provided to traders. In their design, following each market period, traders receive information about relative cash earnings only. Thus, traders who buy the asset at a price below expected value might have negative cash earnings for that period (since the purchase creates negative cash flow) even though they have increased their expected payoff. In contrast, our design provides feedback on assets in addition to cash earnings so that traders can directly compare their current performance with that of another trader. After each period of the market, we provided traders with information about their current Account Total, defined as the sum of cash *plus* the market value of their shares. In addition, we informed them of either the current highest or lowest Account Total among all the traders in that market.

We demonstrate that the type of information provided has a significant effect on market prices: average trading prices are higher, the peak deviation of trading price from fundamental value is higher, and there are more periods when trading prices are higher than fundamental value in markets where all participants observe the highest Account Total as compared to those where all participants observe the lowest Account Total. We also show that the reference outcome affects traders' reported subjective utility for their monetary outcome from the experiment. Finally, we demonstrate that average and peak trading prices in mixed markets, where some traders see the highest Account Total and some see the lowest, are intermediate to unmixed markets. We conclude with two possible theoretical explanations for these effects.

2. The Effect of Relative Performance Information

2.1. Motivation and Hypothesis Development

While the majority of asset pricing models assume that investors are solely motivated to maximize absolute expected returns subject to some degree of risk aversion, a number of models have been proposed based on the assumption that economic actors also care about how their returns compare to other actors' returns (Abel 1990; Gali 1994; Bakshi and Chen 1996). Nevertheless, the literature on laboratory asset markets is silent on the role relative performance concerns might play in price evolution. While James and Isaac (2000) demonstrate that experienced traders who are induced to care about relative payoffs via a tournament reward structure will generate bubbles, they do not answer the question of whether such relative payoff concerns are *inherently* present and hence might help explain bubbles even in absolute payoff markets.

Our research methodology was inspired by a key distinction made in the literature on social comparison processes: the difference between the motivations for and consequences of upward vs. downward comparisons (Buunk and Gibbons 2007). According to Blanton, Buunk, Gibbons and Kuyper (1999), people who compare themselves to superior, as opposed to inferior, performers are more interested in improving performance and also more likely to succeed in doing so, both because upward comparisons may reveal useful information about how to improve and because they may increase the motivation to improve, in part because upward comparisons are also associated with decreased satisfaction with one's own performance.

We investigate experimental asset markets in which participants are exposed to either upward or downward reference point information. After each period, participants are informed of their own Account Total, defined as the sum of their cash and the current market value of their shares. Participants also receive either Upward Reference information -- the current Account Total of the trader with the highest Account Total in the market (the "Leader") – or Downward Reference information -- the current Account Total of the trader with the lowest Account Total in the market (the "Laggard"). In both cases, they are explicitly told that they are seeing the highest or lowest Account Total in the market.

We immediately confront a difficulty in applying the insight that upward comparisons increase the motivation to improve performance to predict behavior in experimental markets. From an economic perspective, the rational strategy for a risk neutral trader facing a population of other rational risk neutral traders is a "fundamental value strategy" which is a rule of buying the asset at prices lower than fundamental value and selling at prices higher than fundamental value, actions which will push prices towards fundamental value (however, see Abreu and Brunnermeier 2003 and Morris 1996 for models of bubbles with rational traders possessing heterogeneous beliefs). The reduction in bubbles seen in markets with twice-experienced traders might therefore be attributed to their having learned to use this type of strategy.

In contrast, Lei, Noussair and Plott (2001) and Caginalp, Porter and Smith (2001) argue that a critical factor in the formation of laboratory bubbles is that the observation of market prices far from fundamental values actually causes even participants who understand the logic of the fundamental value strategy to choose to use a speculative strategy. Caginalp et. al. model such speculation as being momentum-based, i.e. traders place orders with the expectation of a continued rise in prices, presumably because they believe (either rightly or wrongly) that they can extract profits from other, irrational traders by selling the shares at a higher price before the end of the experiment. The fact that bubbles recur in markets with once–experienced traders (Hussam, Porter and Smith 2008) likewise suggests that traders initially "learn" to use speculative rather than fundamental strategies.

In the absence of any explanation for when and why traders learn to use one type of strategy or another, it is difficult to predict the impact of a greater motivation to improve performance. We note, however, that in the early periods of a market, when prices typically begin below fundamental value and then trend upwards, traders learning *either* fundamental or momentum-based strategies will increase their demand for shares, driving the price upwards. Since Caginalp, Porter and Smith (2000) provide evidence that larger positive price movements cause an enhanced momentum effect, and hence bigger bubbles, we predict that giving traders Upward Reference information, by promoting faster learning, will result in bigger bubbles.

A number of measures have been suggested in the literature to test for the magnitude of bubbles. Since our hypothesized mechanism depends on factors which drive price upwards, we focus on three measures which test for the extent and duration of overvaluation. The first, *Maximum deviation* from fundamental value, is the measure identified as most relevant by Caginalp, Porter and Smith (2001) and is defined by $max_t(P_t\text{-}f_t)$, where P_t denotes price and f_t denotes fundamental value at time t. The second measure, *Boom duration*, utilized by King et al. (1993) and Haruvy and Noussair (2006). is the maximum number of consecutive market periods when the median market price is above expected value ($P_t > f_t$). The third measure, the overall *Average Price* paid for the asset across an entire market, is isomorphic to a measure used by Haruvy and Noussair (2006) called Average Bias, the two differing by a constant which is equal to the average fundamental value.

> Hypothesis 1: Markets where all traders receive Upward Reference information have higher Maximum Deviations from fundamental value (1a), longer Boom Durations (1b), and higher Average Price (1c) than markets where all traders receive Downward Reference information.

The above line of reasoning posits that social reference information affects prices by influencing the learning process which determines traders' choice of strategies. James and Isaac (2000) provide evidence that tournament incentives affect prices even after traders are sufficiently experienced that price equilibrates at fundamental value in a non-tournament market. The essential feature of tournament incentives is that outcomes are determined by rank order, making relative performance more important than absolute performance. It is natural, therefore, to ask whether the mere provision of relative performance information directly affects utility, creating the psychological equivalent of a tournament incentive by increasing the hedonic impact of traders' relative outcomes. The influence of relative outcomes on subjective well-being is well established in general (in addition to the references above, see also Clark and Oswald 1996 and Loewenstein, Thompson and Bazerman 1987), but we wished to specifically test for the presence of tournament-like utility effects in a market context.

In order to test this in our studies, at the end of each market period, immediately after participants are informed of their own Account Total and either the Leader or Laggard Account Total, they are asked to report how they feel about their current Account Total using a 7-point Likert scale whose values ranged from "very negatively" to "very positively" (the satisfaction ratings). If traders are risk-neutral and solely concerned with absolute outcomes, subjective satisfaction ratings ought to be monotonically increasing in the trader's own Account Total and unaffected by the reference outcome.

By contrast, traders who are also concerned with relative outcomes will be affected by the reference information that they observe, leading to a prediction that traders' satisfaction ratings will be affected by the difference between a trader's own outcome and the reference outcome. This will have the effect of raising the general level of satisfaction ratings for traders given Upward Reference information and lowering them for traders given Downward Reference information, with one important exception: Leaders given Upward Reference information ought to be happier with their outcome, knowing that it is the best, while Laggards given Downward Reference information ought to be less happy with their outcome, knowing that it is the worst. We note that this prediction of a specific incremental utility (disutility) for being the Leader (Laggard) makes these markets similar to those in a winner-take-all (or loser takes a booby prize) tournament (as in Hvide 2003).

Hypothesis 2: Market participants' satisfaction ratings increase when a trader is the Leader (2a), decrease when a trader is the Laggard (2b) and increase the greater the difference between one's own outcome and that of the Laggard or the smaller the difference between one's own outcome and that of the Leader (2c).

2.2. Experimental Design

We examined the influence of relative performance information on price evolution using an experimental asset market where participants trade a stock-like asset with a declining, public knowledge expected value (Smith, Suchanek, and Williams 1988). In the AllUp condition (7 sessions, N=66) all participants saw Upward Reference information, while in the AllDown condition (7 sessions, N=65) all participants saw Downward Reference information. The type of reference observed within a given market was public knowledge.

Participants were recruited from the student population at a large research university via a combination of posters and e-mails. Approximately half of participants were undergraduates and half were from an assortment of graduate and professional schools. Ages ranged from 18 to 59, with a median of 21. 48.0% of participants were male.

Participants in groups of 8 to 10 traders sat at computer terminals in separate individual cubicles and were given instructions (Appendix A) on the structure of the market, which had 15 trading periods during which participants could buy and sell shares of the stock. They were informed that at the end of each period, each share would pay a dividend in cash, determined by a computerized random draw from four equally probable values (0, 8, 28 or 60 experimental units, with a payoff conversion rate of 1EU:$0.01), with an expected value of 24 units. Participants were initially endowed with one, two or three shares of the asset plus a cash account of 945, 585 or 225 units, respectively, so that all participants began with the same expected value payoff of 1305 units. The experimental currency held by a trader at the end of the experiment was the sum of the starting cash account, the total value of the dividends received, and the net amount received from purchases and sales of shares. At the end of the experiment, participants were paid their show-up fee plus an amount of money equal to the payoff conversion rate multiplied by the experimental currency. Final payments (including a $5 show-up fee) ranged from a high of $73.55 to a low of $5.00. The average payoff was $18.54, and the median payoff was $14.20.

Participants were instructed in the use of a multiple unit double auction market process (Plott and Gray 1990) programmed and conducted with the software Ztree (Fischbacher 2007). Participants were also given a reference sheet that showed the expected value of each share at the beginning of each trading period, along with an explanation of how it was calculated (Appendix B). Participants were given unlimited time to ask questions and then played a two-round practice game to experience the trading process (see Appendix C for an example of the trading screen). Finally, their accounts were reset to their initial values and the actual market commenced.

After each period of the session, participants viewed an "Account Status" screen (Appendix D) which included the following information about the current state of their account: (1) Total Cash (including accumulated dividends), (2) Total Shares, (3) Share Price (i.e., the current market value of a share), and (4) Account Total (defined in the instructions and on screen as cash plus market value of shares).

On their "Account Status" screen, participants also received one of two types of reference information; they either saw the largest Account Total of any trader in the session (Upward Reference information) or they saw the smallest Account Total of any trader in the session (Downward Reference information). After reviewing their Account Status screen, participants were asked to report how they felt about their current Account Total using a 7-point Likert scale ranging from "very negatively" to "very positively."

2.3. Results

Table 1 summarizes the results of the hypothesis tests, to be discussed below.

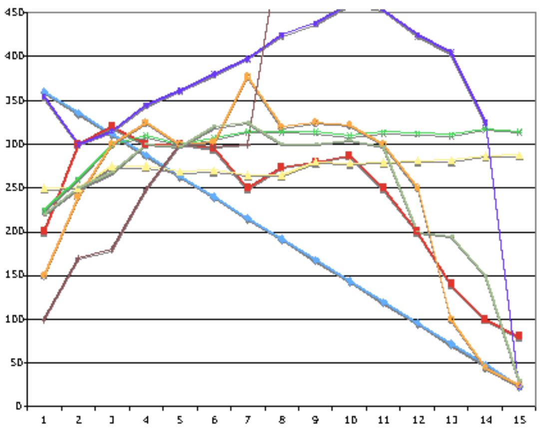
Table 1: Summary of Hypothesis Tests

Hypothesis	Description	T[d.f.], p-value
H1a	Max Dev higher in AllUp condition	2.19[12], p=0.05
H1b	Boom Duration higher in AllUp condition	2.91[12], p=0.01
H1c	Average Price higher in AllUp condition	3.45[12], p<0.01
H2a	Satisfaction ratings increase by being the Leader	2.1[126], p=0.03
H2b	Satisfaction ratings decrease by being the Laggard	2.3[126], p=0.02
H2c	Satisfaction ratings increase in distance from reference outcome	3.2[126], p<0.01

Figure 1 shows the median prices for each period of each session in the AllUp and AllDown conditions. A visual examination of the graphs suggests that, as predicted, the AllUp sessions manifest larger bubbles than the AllDown sessions. This is confirmed by statistically significant differences in the three measures of the magnitude of bubbles defined in section 2 and enumerated in Table 2 (which also includes comparative data from a second study described and discussed in Section 3 below): (H1a) Maximum Deviation: 299.4 vs. 129.2 (t = 2.19[12], p=0.05), (H1b) Boom Duration: 11.4 vs. 7.3 (t = 2.91[12], p=0.01), and (H1c) Average Price: 304.2 vs. 190.8 (t = 3.45[12], p<0.01).

Figure 1: Median Prices by Market Period

AllUp Markets (N=7)



AllDown Markets (N=7)

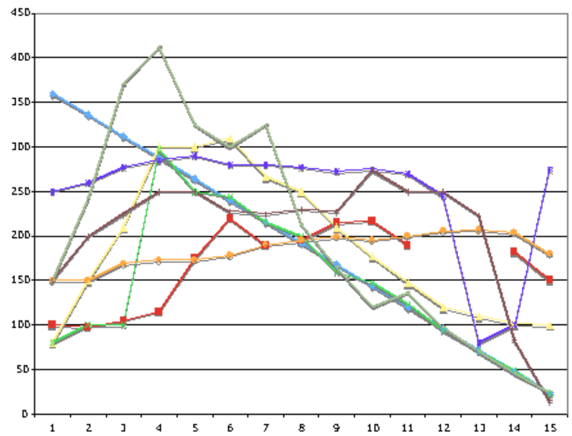


Table 2: Average Bubble Measures by Condition

Condition [N]	Maximum Deviation (Experimental $)	Boom Duration (Periods)	Average Price (Experimental $)
AllDown [7]	129.2	7.3	190.8
AllUp [7]	299.4	11.4	304.2
0U [5]	107.4	7.2	172.4
3U [6]	173.7	8.5	220.5
6U [7]	124.9	7.1	202.1
9U [5]	182.8	9.8	222.8

To test Hypotheses 2a, 2b, and 2c, we created a four-factor linear regression model predicting final satisfaction ratings using (1) the traders' own final Account Total (the "Absolute Outcome") (2) a dummy variable to denote whether a trader is a Leader (3) a dummy variable to denote whether a trader is a Laggard and (4) a continuous variable calculated as own final Account Total minus final reference Account Total (the "Relative Outcome").

Model 1: Final Satisfaction Ratings = α_1(Absolute Outcome) + α_2(Leader dummy) + α_3(Laggard dummy) + α_4(Relative Outcome)

Table 3 shows that all three of the tournament-like factors are significant (t=3.2[126], p<0.01 for Relative Outcome, t=2.3[126], p=0.02 for being a Laggard, and t=2.1[126], p=0.03 for being a Leader). The second column of the table reports the partial correlation of each variable, which measures the marginal contribution to prediction of one explanatory variable when all other explanatory variables are already included in the model. While the impact of Absolute Outcome is larger than that of Relative Outcome (raising satisfaction ratings by 0.038 for every incremental absolute dollar vs. 0.024 for every incremental relative dollar), Relative Outcome explains a higher proportion of variability (a partial correlation of 0.28 vs. 0.19 for Absolute Outcome). Moreover, the impact of the Leader and Laggard dummies is strikingly large; the marginal reported satisfaction of being the Leader is equivalent to an incremental $46.84 in Absolute Outcome (vs. $65.29 for being the Laggard), though it should be noted that there are only seven data points in each case.

The R^2 for the full model (with four explanatory variables and an intercept) in table 3 is 0.35. This is higher than that for a regression model that used absolute outcomes and intercept alone (R^2=0.28). Converting these R^2 to an F-test[3], we get $F(3,126)= 4.65$, $p < 0.01$. Thus, we reject the model that does not include relative outcomes in favor of the full model specified in table 3, providing support for our argument that providing relative performance information creates tournament-like incentives.

Table 3. Linear regression model -- Dependent variable: Final satisfaction ratings (N=131)
Standard errors in parentheses.

Factors	*Coefficients*	*Partial Correlation*
Intercept	1.9 (0.31)***	
Absolute Outcome (E$)	0.038 (0.02)**	0.19
Leader Dummy	1.78 (0.8)**	0.19
Laggard Dummy	-1.57 (0.7)**	0.20
Relative Outcome (E$)	0.024 (0.007)***	0.28
R^2	*0.35*	

**p<0.05 ***p<0.01

2.4. Discussion

Relative performance information is commonplace in real-world markets but is almost never explicitly provided in laboratory asset market experiments. James and Isaac (2000) offer a notable exception, but their focus is the impact of tournament monetary incentives on equilibrium outcomes. In this study, we examined the impact of the information itself on the process that precedes equilibrium, and offered evidence that providing this information to inexperienced traders in an experimental asset market can

[3] $F = \frac{(R^2_{unrestricted} - R^2_{restricted}) / \#restrictions}{(1 - R^2_{unrestricted}) / (n - \#coefficients)}$ = (0.35-0.28)/3 / ((1-0.35)/(131-5))=4.65

have a significant impact on price evolution even when that information has no direct impact on trader payoffs.

We also demonstrated that relative performance information affects trader utility in a manner consistent with a tournament reward structure: traders get particular utility [disutility] from being the Leader [Laggard] and report decreasing [increasing] utility the farther they are from the Leader [Laggard].

3. Moderators of the Effect of Relative Performance Information

3.1. Motivation and Hypothesis Development

We are interested in identifying moderators of the reference effect. Isaac and James (2003) extended their results to show that the impact of tournament contracts was mitigated if half of the traders in a market did *not* have tournament incentives, but persisted if only one trader did not. Dufwenberg, Lindqvist and Moore (2005) demonstrated that markets where either one-third or two-thirds of the traders were inexperienced and the remainder thrice-experienced were indistinguishable from ones with entirely twice-experienced traders (and also indistinguishable from each other).

It is therefore useful to examine mixed markets where some traders are given Upward Reference information and others are given Downward Reference information. Mixing trader types in a single market requires that we keep each trader’s reference outcome private. This is in contrast to the first study where the type of reference information was common knowledge. Changing the experimental design to allow for such mixed markets enables us to both replicate and extend our initial results by testing additional hypotheses.

Our first hypothesis relates to replicating the previous results with the private reference information we have in the present design. A possible critique of the results in the first study is that the effects of our manipulation are due not to differences in response to feedback but to differences in trader expectations regarding the effect of feedback on other traders. That is, if traders expect *other* traders to be affected by the information, they might change their own strategies. Hypothesis 3 is intended to examine whether the effect of reference price in the first study was due to reference information or to expectations about the effect of others' reference information.

Hypothesis 3: Markets where all traders *privately* receive Upward Reference information have higher Maximum Deviations (3a), longer Boom Durations (3b), and higher Average Price (3c) than markets where all traders *privately* receive Downward Reference information.

With regards to the question of the impact of different ratios of traders receiving Upward vs. Downward Reference information, we note that while Isaac and James (2003) and Dufwenberg, Lindqvist and Moore (2005) demonstrate empirically that markets with a even a small percentage of experienced, non-tournament incentivized traders look no different from ones with only such traders, it is unclear whether such patterns apply here. While we demonstrate in the first study that Upward Reference information results in larger bubbles than Downward Reference information, we cannot say how much of this difference is attributable to one condition or the other. Thus, with no clear theory to guide us, we considered the most parsimonious hypothesis to be that the effect will be continuous in the percentage of traders in each condition.

> Hypothesis 4: Maximum Deviation (4a), Boom Duration (4b), and Average Price (4c) are higher the larger the percentage of traders in the market receiving Upward Reference information

A final benefit of manipulating the number of traders in each condition in a given market is the ability to test for within-market differences between traders in the two conditions. Since traders base demand not only on exogenous factors such as the asset's dividend structure and their own risk preferences, but also on predictions of future prices which are formed by observing trading patterns within the market itself (Haruvy, Lahav and Noussair 2007), the fact that our condition has an effect on market prices makes it difficult to separate the market-level effect of the condition on behavior from the individual-level effect. By exposing traders in both conditions to the *same* price evolution, we can potentially disentangle these two factors by testing for differences between trader types within the same market. In particular, we wish to test for the mechanism which underlay our original Hypothesis 1, that Upward Reference information induced quicker learning and hence greater demand for the asset at prices below expected value.

Hypothesis 5: Traders receiving Upward Reference information will demand more of the asset at prices below expected value than traders in the same session who receive Downward Reference information

3.2. Experimental Design

We conducted a second study using the same methodology as detailed in Section 2.2 with two principal changes: (1) there were four conditions, each with exactly nine traders but varying in the number of traders receiving Upward Reference information: either 0, 3, 6, or 9 traders, with the remainder of the traders seeing Downward Reference information and (2) participants knew only the type of reference information that they themselves were receiving (to distinguish the two protocols in the Discussion below, we will refer to these as "private markets" and the sessions in the first study as "public markets"). There were 5, 6, 7, and 5 sessions in the 0U, 3U, 6U and 9U conditions, respectively (see Table 4 for summary data). A number of other minor differences in this second study are enumerated in Appendix E.

The second study was conducted using subjects at a research university with a similar demographic profile as the first study: ages ranged from 18 to 46, with a median age of 23, and 68.1% of participants were male. Final payments (including $5 show-up fee) ranged from a high of $58 to a low of $5. The median payoff was $21.

Table 4: Summary information for private market experimental conditions

Condition	Number of Sessions	# Upward Reference traders per Session (N)	# Downward Reference traders per Session (N)	Upward Reference average earnings	Downward Reference average earnings
9U	5	9 (45)	0 (-)	1095.4	-
6U	7	6 (42)	3 (21)	1142.5	1287.1
3U	6	3 (18)	6 (36)	1211.7	1345.7
0U	5	0 (-)	9 (45)	-	1266.6

3.3. Results

Table 5 summarizes the results of the hypothesis tests proposed in section 3.1. We discuss each result below.

Table 5: Summary of Hypothesis Tests

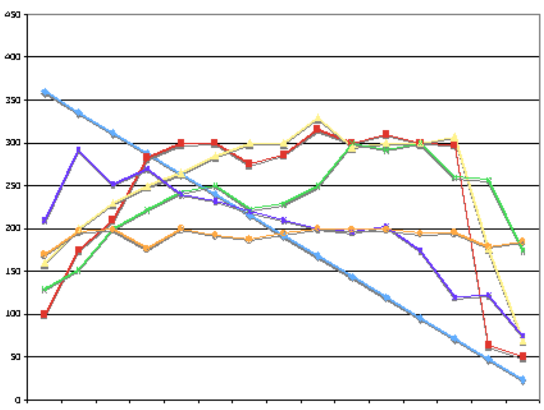
Hypothesis	Description	T[d.f.], p-value
H3a	Max Dev higher in 9U vs. 0U condition	2.25[8], p=0.05
H3b	Boom Duration higher in 9U vs. 0U condition	3.15[8], p=0.01
H3c	Average Price higher in 9U vs. 0U condition	3.66[8], p=0.01
H4a	Max Dev increasing in % traders seeing Upward Reference information	0.98[21], p=0.34
H4b	Boom Duration increasing in % traders seeing Upward Reference information	t=1.1[21], p=0.28
H4c	Average price increasing in % traders seeing Upward Reference information	t=1.1[21], p=0.30
H5a	Within session, average purchase prices higher for Upward Reference traders at prices below expected value	1.54[12], p=0.07
H5b	Within session, average purchase volume higher for Upward Reference traders at prices below expected value	1.50[12], p=0.08

Figure 2 shows the median prices for each period of each session in the 9U, 0U, 6U and 3U conditions, with summary details provided in Table 2 above. Hypothesis 3 is supported; there are significant differences between the 9U and 0U markets for all three bubble metrics (182.8 vs. 107.4, t = 2.25[8], p=0.05 for Maximum Deviation (H3a) ; 9.8

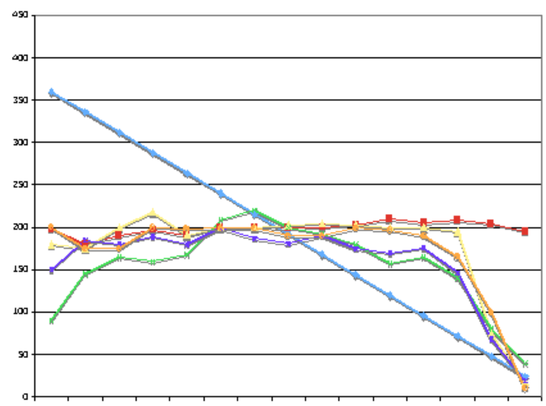
vs. 7.2, t = 3.15[8], p=0.01 for Boom Duration (H3b); and 222.8 vs. 172.4, t = 3.66[8], p=0.01 for Average Price (H3c). This clearly demonstrates that making the type of relative performance information public is not necessary for the information to affect prices. That said, the average values on all three measures in the AllUp condition in the public markets are noticeably higher than those for the 9U condition in the private markets, though only the differences for Average Price (t=2.16[10], p=0.06) and for Boom Duration (t=2.31[10], p=0.04) are significant. This suggests that awareness of the type of relative performance information received by others might exacerbate its impact.

Figure 2: Median Prices by Market Period

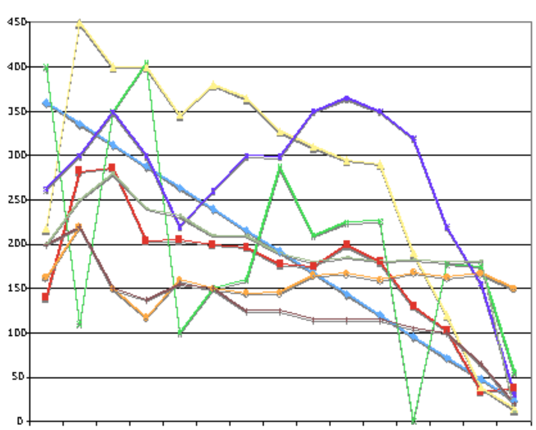
All 9U Markets



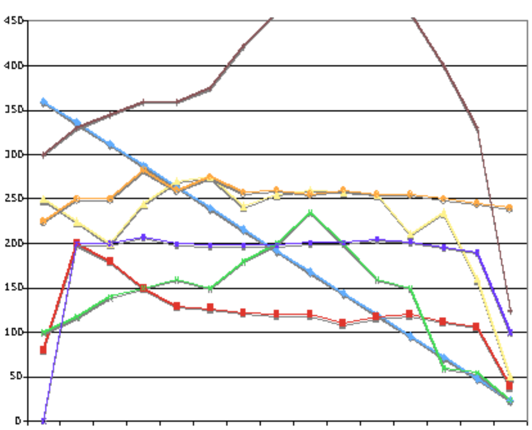
All 0U Markets



All 6U Markets



All 3U Markets



With regards to the impact of having different proportions of traders observing each type of reference information, Table 2 shows that markets with mixed trader types (6U and 3U) have average values intermediate to those of 9U and 0U (except in the case of Boom Duration, where 6U markets are indistinguishable from 0U markets), but not in a strictly increasing order. None of the mixed reference condition values are significantly different from each other or from the pure reference conditions. Furthermore, in a linear regression model treating each market session as a single data point, the number of Upward Reference traders is not a significant predictor of any of Maximum Deviation (t=0.98[21], p=0.34), Boom Duration (t=1.1[21], p=0.28), or Average Price (t=1.1[21], p=0.30). Thus, Hypothesis 4, that we will observe a continuous effect on prices as the percentage of traders in a given condition rises, must be rejected.

In order to test Hypothesis 5, we generated data on two key indicators of demand for all traders in a given condition within the same mixed market: the average price paid for the asset and the average volume of purchases (Table 6). Upward traders manifest a higher average purchase price in 7 out of 13 sessions and a higher volume of purchases in 6 out of 13 sessions (in 2 sessions, trading volume for the two groups is identical). The average difference across all sessions is higher in both cases, and these differences are marginally significant in one-sided paired t-tests (t=1.54[12], p=0.07 for Average Purchase Price and t=1.50[12], p=0.08 for Average Purchase Volume).

Table 6: Measures of Rational Demand in Mixed Markets
(Demand at Prices below Expected Value in conditions 6U and 3U)

		Average Purchase Price				Average Purchase Volume		
Condition	Session	Downward Traders	Upward Traders	Difference		Downward Traders	Upward Traders	Difference
3U	2	81.8	127.5	45.8		3.0	2.8	-0.2
	1	141.0	155.2	14.2		7.3	6.3	-1.0
	5	145.0	156.8	11.8		6.0	6.0	0.0
	4	199.0	203.5	4.5		4.7	4.7	0.0
	6	140.6	136.9	-3.7		11.7	18.2	6.5
	7	201.2	192.5	-8.7		7.3	5.7	-1.7
	3	126.1	111.5	-14.6		3.0	2.3	-0.7
6U	5	208.1	251.1	42.9		3.7	5.3	1.7
	6	271.4	300.0	28.6		1.7	0.7	-1.0
	1	127.8	140.6	12.9		19.5	25.3	5.8
	4	196.3	195.8	-0.5		1.7	4.7	3.0
	3	118.6	117.6	-1.0		5.2	6.0	0.8
	2	220.2	201.2	-19.1		6.0	6.7	0.7
	Average	**167.5**	**176.2**	**8.7**		**6.2**	**7.3**	**1.1**

3.4. Discussion

The second study replicated the key finding of the first, that relative performance information can affect market prices, and went further to demonstrate that this is not solely due to changes in trader's expectations about its impact on other traders, but rather to some pathway directly changing trader strategies. We also provided evidence supporting our assertion that this pathway might be via increased motivation to learn, and hence faster learning.

The study failed to demonstrate, however, that the effect on market prices was increasing in the number of Upward Reference traders. In fact, the only markets which were statistically distinguishable from the rest were the ones with all Upward Reference or all Downward Reference traders. Given the noise in the data, this does not preclude our hypothesized effect, but it prevents us from drawing any specific conclusion as to how the effect on individual traders aggregates at the market level.

In fact, we acknowledge that it is difficult to even draw a firm conclusion about the exact nature of that effect on individual behavior. The distinction we find between Upward Reference and Downward Reference traders in mixed reference sessions is minimal, even when we focus exclusively on the early periods of a market, suggesting that the market-level effect involves feedback mechanisms which amplify a subtle difference in individual strategies into a large difference in prices.

The one clear effect that relative performance information had at an individual level is on subjective well-being. We replicated Hypotheses 2a, 2b, and 2c using both the data from the second study and data pooled across both studies, demonstrating that final satisfaction ratings are significantly affected by Relative Outcome (t=3.1[202], $p<0.01$ for private markets and t=5.0[333], $p<0.01$ for the pooled data), being the Leader (t=1.8[202], p=0.07 and t=3.0[333], $p<0.01$, respectively) and being the Laggard (t=2.1[202], p=0.03 and t=3.4[333], $p<0.01$, respectively) when controlling for Absolute Outcome (Table 7). In addition, the impact of the Leader and Laggard dummies, while smaller in the private markets than in the public markets, remained quite large: the marginal reported satisfaction of being the Leader is equivalent to an incremental $17.16 in Absolute Outcome (vs. $17.71 for being the Laggard; the equivalent values for the pooled data are $33.67 for being the Leader and $33.04 for being the Laggard).

In both cases, the full models using our measures of relative rewards have greater predictive power than the one-factor models incorporating absolute outcomes only (R^2 of 0.30 vs. 0.26 for private markets and 0.31 vs. 0.25 for the pooled data). Converting these

R^2 to an F-test, we get $F(3,202)= 4.00$, $p = 0.01$ and $F(3,333)= 10.84$, $p < 0.01$, respectively. Thus, we reject the model that does not include relative outcomes in favor of the full model specified in table 7.

Table 7. Linear regression models -- Dependent variable: Final satisfaction ratings
Standard errors in parentheses.

Factors	*Private Markets [N=207]*	*Pooled [N=338]*
Intercept	1.8 (0.3)***	2.0 (0.2)***
Absolute Outcome (E$)	0.07 (0.02)***	0.05 (0.01)***
Leader Dummy	1.2 (0.7)*	1.5 (0.5)***
Laggard Dummy	-1.2 (0.6)**	-1.5 (0.4)***
Relative Outcome (E$)	0.03 (0.01)***	0.03 (0.005)***
R^2	*0.30*	*0.31*

* p<0.10 **p<0.05 ***p<0.01

4. Conclusions

The causes and consequence of asset market bubbles are central to the current debate about many important issues of monetary and regulatory policy. A number of researchers have focused on the role that relative performance concerns might play; the theoretical model of DeMarzo, Kaniel, and Kremer (2008) and the empirical findings of James and Isaac (2000) together demonstrate that tournament rewards (i.e. the presence of scarce goods) can generate bubbles even among fully rational/experienced traders. Our contribution here is to show that the mere presence of relative performance information can also affect prices: we provided traders with periodic updates of their accumulated Account Total as compared to the accumulated total of either the "best" or "worst" trader in the market, and find that markets where all traders get Upward

Reference information have higher average trading prices, larger peak deviations from fundamental value, and more periods when prices are higher than fundamental value.

We also demonstrate that relative performance information affects subjective utility; even controlling for Absolute Outcome, utility is significantly lower for Laggards, significantly higher for Leaders, and in general is affected by the distance between one's own outcome and the reference outcome. We argue that this demonstrates that the mere presence of relative performance information implicitly creates tournament-like incentives.

The presence of tournament incentives offers one possible explanation for the observed effect on prices. An important consequence of tournament incentives is to induce apparently risk-seeking behavior among rational risk-neutral (or even risk-averse) agents (Hvide 2002), a point emphasized by both DeMarzo, Kaniel, and Kremer (2008) and James and Isaac (2000) in explaining their results. Of particular note in the context of our studies, Gilpatric (2004) offers a theoretical model demonstrating the opposite effect: in the presence of a single booby prize for the worst performer in a tournament contest, rational traders will take on less risk.

Rational risk-seeking in response to incentives might explain why Upward Reference information result in larger bubbles than Downward Reference information. This explanation would be consistent both with our finding of higher Average Prices in markets where all traders see Upward Reference information and with our observation that in the mixed conditions (i.e., the 3U and 6U sessions), where both Upward Reference and Downward Reference traders experience the same dividend stream, Upward Reference traders (the risk seekers according to this argument) received lower payoffs than Downward Reference traders (1142.5 vs. 1287.1 for 6U and 1211.7 vs.1345.7 for 3U, both statistically insignificant differences). Thus, it may be the case that traders who rationally aspire to be the market leader engage in more risk-seeking behavior, driving prices away from fundamental values while lowering expected earnings for themselves.

A second class of theoretical explanation for our results pertains to imitation. An extensive economics literature on herding and information cascades demonstrates that imitation is potentially a rational or boundedly rational strategy when a trader is faced with uncertainty or ambiguity which the trader believes might be resolved by information or insight gained from other traders (Hirshleifer and Teoh 2003). In the model of DeMarzo, Kaniel, and Kremer (2008), the pursuit of scarce goods causes agents to rationally choose to herd in order to avoid the risk of poor relative performance, just as Clark and Oswald (1998) show that rational agents who care about relative outcomes will imitate the strategies of other agents. In the experimental asset market setting, a preference for imitation might explain momentum strategies, which research has suggested play an important role in bubble formation (Caginalp, Porter and Smith 2001, Haruvy and Noussair 2006). Traders using momentum strategies imitate other traders because they predict the persistence of a price trend, so that the observation that other traders are buying will cause them to buy as well.

As is often the case, our research raises more questions than it answers, particularly with regards to the specific pathway by which relative performance information affects individual behavior. We therefore agree with Hussam, Porter and Smith (2008) that experimental market research, including ours, clearly shows what "traders *do not do*… they do not think about the problem the way we do as economists."

We suggest that understanding how they *do* think will require a closer examination of their basic motivations and the ways in which they utilize information in learning how to best pursue their goals.

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Appendix A: Public Market Instructions

This is an experiment in the economics of market decision making. The experiment will consist of a sequence of 15 trading periods in which you will have the opportunity to buy and sell in a market. Your payment at the end of the experiment will be equal to $5 PLUS whatever you earn during the course of the experiment. The average payment is $18.05, but your actual payment could be higher or lower.

You will each begin with a combination of cash and shares. Cash in the experiment is shown in cents. Shares are assets which pay a dividend at the end of each of 15 periods. The amount of the dividend is one of the four following values, each of which is equally likely: 0 cents, 8 cents, 28 cents, or 60 cents. The average dividend in each period is 24 cents. At the end of each of the 15 trading periods, the computer will randomly select one of those four values, and you will receive dividends for each share in your inventory. The dividend is added to your cash balance automatically. Your cash balance and your inventory of shares carries over from one trading period to the next. After the dividend is paid at the end of period 15, there will be no further earnings possible from shares. **In other words, at the end of the experiment, the shares are worth nothing.**

We will now explain how you will use your computer to buy and sell shares, and to keep track of your account through the course of the experiment. There will be 15 trading periods, each of which last for 3 minutes, or 180 seconds. The time remaining in the period is shown in the upper right corner of your screen.

At the beginning of the experiment, everyone receives a combination of shares of an asset plus cash. On the left-most column of your computer screen, in the top left corner, you can see the Money you have available to buy Shares and in the middle of the column, you see the number of Shares you currently have.

The shares can be bought and sold in a computerized market. If you would like to offer to SELL a share, use the text area entitled “Enter ask price” in the second column. In that text area you can enter the price at which you are offering to sell a share, and then select “Submit Ask Price”. Please do so now.

You will notice that nine numbers, one submitted by each participant, now appear in the third column from the left, entitled “Ask Price”. The lowest ask price will always be on the bottom of that list and will be highlighted. If you press “Buy”, the button at the bottom of this column, you will buy one share for the lowest current ask price. You can also highlight one of the other prices if you wish to buy at a price other than the lowest.

Please purchase a share now by highlighting a price and selecting “Buy”. Since each of you had put a share for sale and attempted to buy a share, if all were successful, you all have the same number of shares you started out with. This is because you bought one share and sold one share.

When you buy a share, your Money decreases by the price of the purchase. When you sell a share your Money increases by the price of the sale.
You may also make an offer to BUY a share by entering a number in the text area entitled "Enter bid price." Then press the red button labeled "Submit Bid Price". You can sell to the person who submitted an offer if you highlight the offer, and select "Sell". Please do so now for one of the offers.

Please note that you if you attempt to "Buy" a share listed in the "Ask" table, you must have enough money to buy the share at the offered price, and if you attempt to "Sell" for an amount listed in the "Bid" table, you must have a share to sell. If you do not have enough money, or enough shares, you will get an error message. You will also get an error message if you attempt to buy or sell a share from yourself. *Please be aware that once you post a bid or an ask, you CANNOT change it, so make sure you do not enter the wrong price in error.*

At the end of each trading period, you will have an opportunity to buy a single ticket for a lottery. The ticket costs 6 cents and offers a 5% chance (i.e. 1 in 20) of winning 120 cents. If you choose to buy a ticket, 6 cents will be subtracted from your account, and, if the winning number is drawn, everyone who has a bought a ticket will win 120 cents. Please choose "yes" for this practice round.

After everyone has decided whether or not to buy a lottery ticket, you will receive a status report for the period just ended. The status report includes the following information:

- Your cash account at the end of the prior period
- The dividend payment for this period
- The number of shares you currently own
- The total amount of dividends you receive (that is, the number of shares you own multiplied by the dividend payment for the period).

- The cost of the lottery ticket, if you bought one.
- The payoff of the lottery ticket.

- And finally, you will see your cash account at the end of this period
- The number of shares that you currently own
- The current price of the shares (defined as the best bid in the preceding period, since this represents what you could have sold the share for)
- Your Account Total (that is, your cash account plus the value of your shares)

In addition, you will also see how much the person with largest[smallest] account has.

Finally, you will be asked a question about how you feel about your current account level. Once everyone has answered that question, the next round of the game will begin. Please wait until I finish the instructions before entering a value.

The *only* earnings you will receive for the experiment will be $5 you receive for participating plus the amount of *cash* that you have at the end of period 15, after the last dividend has been paid. The amount of cash you will have is equal to:

The cash (called "money" on your screen) you have at the beginning of the experiment

+ dividends you receive (when you have more than zero shares)

+ money received from sales of shares

- money spent on purchases of shares

We have provided a sheet of paper to help you make decisions. First, it includes a basic reminder that if you want to sell a share for a particular amount, you enter an ask price, and if you want to buy a share for a particular amount, you enter a bid price.

Second, we provide an AVERAGE HOLDING VALUE TABLE. There are 4 columns in the table. The first column, labeled Current Period, indicates the period during which the average holding value is being calculated. The second column gives the number of holding periods from the period in the first column until the end of the experiment. The third column, labeled Average Dividend per Period, shows the average amount of the dividend. Since the dividend on a Share has a 25% chance of being 0, a 25% chance of being 8, a 25% chance of being 28 and a 25% chance of being 60 in any period, the dividend is on average 24 per period for each Share. The fourth column, labeled Average Holding Value Per Share, gives the average value for each share from the current period until the end of the experiment.

Suppose for example that there are 7 periods remaining. If you hold a Share for 7 periods, the total dividend for the Share over the 7 periods is on average 7*24 = 168. Therefore, the total value of holding a Share over the 7 periods is on average 168.

You will now have a practice period. Your actions in the practice period do not count toward your earnings and do not influence your position later in the experiment. The goal of the practice period is only to master the use of the interface. Please be sure that you have successfully submitted bid prices and ask prices. Also be sure that you have accepted both bid and ask prices. You are free to ask questions, by raising your hand, during the practice period. Once everyone has entered a rating, the practice period will begin.

Appendix B: Public Markets Experiment Help Sheet

"Enter Ask Price" =
I want to sell a share for $X

"Enter Bid Price" =
I want to buy a share for $Y

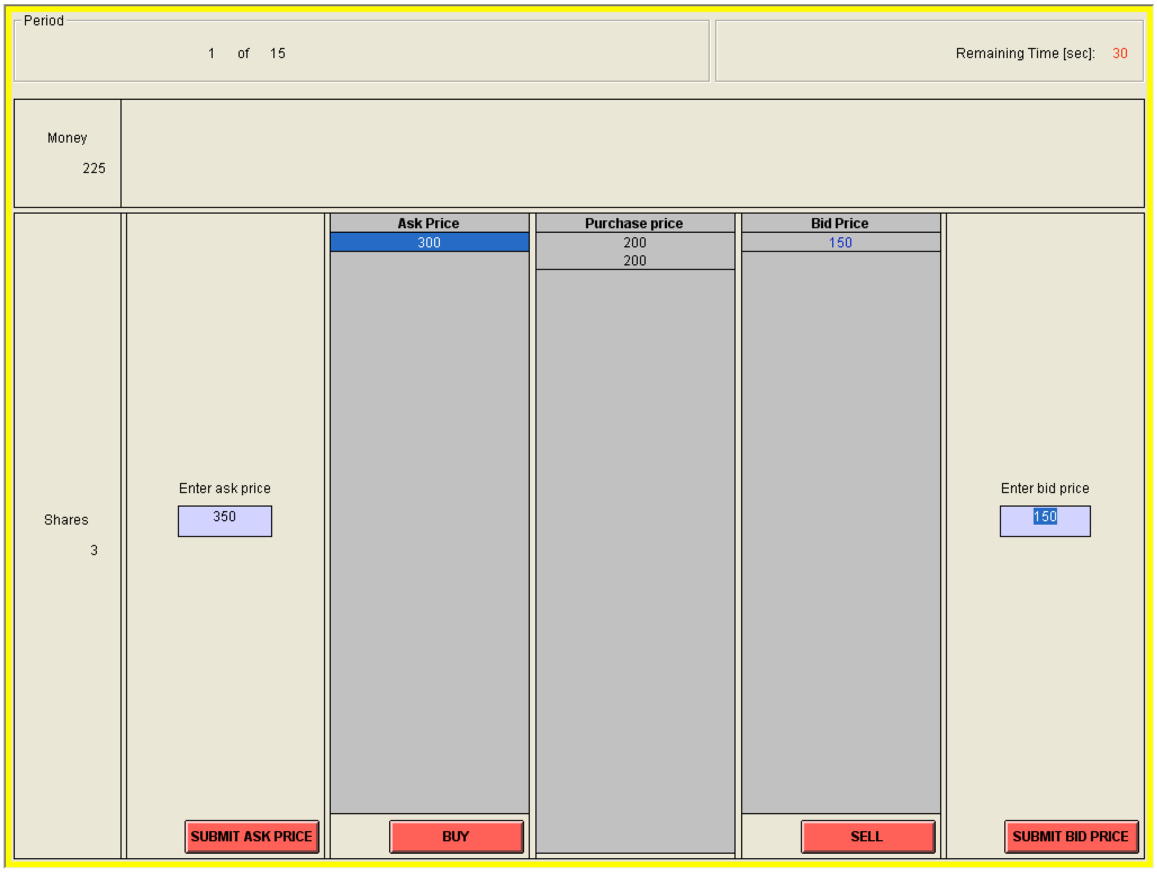
BUY = I will buy a share
for the price highlighted above

SELL = I will sell a share
for the price highlighted above

AVERAGE HOLDING VALUE TABLE

Current Period	Number of Periods Remaining	x	Average Dividend Per Period	=	Average Holding Value Per Share
1	15		24		360
2	14		24		336
3	13		24		312
4	12		24		288
5	11		24		264
6	10		24		240
7	9		24		216
8	8		24		192
9	7		24		168
10	6		24		144
11	5		24		120
12	4		24		96
13	3		24		72
14	2		24		48
15	1		24		24

Appendix C: All Markets Trading Screen



Appendix D: Public Markets Account Status Screen

Appendix E
Additional Notes on Methods

The public and private markets differed in the following ways:

- Traders in public markets only received oral instructions, while traders in private markets were given both written and oral instructions.
- In public markets, the "Stock Price" displayed on the Account Status screen was defined in the instructions as the highest "bid" made for a share in the preceding period. In private markets, it was defined as the average price of all trades in the preceding period (or the average price in the last period when there were trades).
- Trading periods lasted either 3 or 2 minutes in public markets vs. 2 minutes in all private markets.
- The conversion payout rate in public markets was 100 E$:1 US$ while it was 80 E$:1 US$ in private markets
- Traders in public markets were offered the chance to buy a lottery ticket at the end of each trading Period. The ticket cost E$6 and offered a 1 in 20 chance of winning E$120.
- Traders in public markets were able to post multiple bids and offers while traders in private markets could only have one bid or offer outstanding at a time (older ones were cancelled after a new one was posted).
- Traders in private markets were presented with specific labels for each possible satisfaction rating, while those in public markets only saw labels for the extreme and middle values.

The fourteen public markets reported in this research were conducted over a roughly thirty month period as part of a larger set of studies. A small number of participants participated in more than one session, but in only one session was there more than one experienced participant. There were two minor changes in market procedure within public markets.

- After the first three markets, the instructions were modified by adding an additional reminder, verbally stressed by the experimenter, that the shares were worthless at the end of the experiment
- After the first 10 markets, the length of each period of the game was shortened from 3 to 2 minutes.

Neither of these changes within the public market conditions had a statistically significant effect on any of the measures reported.