

Relative Performance Information in Asset Markets:

An Experimental Approach

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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 significant effects on both aggregate market measures, such as market prices and boom duration, and individual subjective satisfaction.

JEL classifications: C91, D83, G11

Keywords: Asset markets, experiments, bubbles, relative performance

This is a preprint version of an article published in the Journal of Economic Psychology. The final version may be found at < <http://dx.doi.org/10.1016/j.joep.2012.08.008> >.

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 mispricing. Of particular note are the large losses suffered by experienced financial professionals, apparently due to their sustaining rather than correcting mispricing, a process which Camerer and Fehr (2006) propose might be the consequence of “institutional constraints such as performance pressure... [forcing] well-informed investors to follow a poorly informed crowd, rather than betting against it.” Relative performance is a particularly important motivation for professional investors since it has a large impact on the ability to increase funds under management (Sirri and Tufano 1998), but there is also copious experimental and correlational evidence that individual utility functions are affected by relative comparisons (Fehr And Schmidt 1999, Charness and Rabin 2002, Diener and Biswas-Diener 2002, Luttmer 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 concerned about the affordability of future “scarce goods” 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 1988). 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)¹ but is mitigated by the presence of even a minority of thrice-experienced traders (Dufwenberg, Lindqvist, and Moore 2005). James & Isaac (2000), however, offer empirical support for the model of DeMarzo, Kaniel, and Kremer (2008) by showing that markets with experienced traders do not stabilize at expected value when traders are paid for above average performance (thus creating a scarce good).

In the present study, we examine whether concerns with relative performance are present in experimental markets even in the absence of direct monetary tournament incentives by introducing two methodological innovations on top of the standard Smith, Suchanek, and Williams (1988) design. First, we provide traders with information about relative performance, utilizing a key distinction made in the literature on social comparison processes between upward and downward social comparisons (Buunk and Gibbons 2007). After each market period, traders receive information about either the highest or the lowest earnings in the market. Thus, whereas James and Isaac (2000) manipulate traders' payment structure while keeping the type of information provided about relative earnings the same across treatments, we hold the payment structure the same across treatments while varying the social information given to traders.

¹ The literature on such markets includes studies on the impact of different institutional features, e.g. futures and spot markets (Noussair and Tucker 2006, Palan 2010), 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 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 2009) and its terminal value (Hirota and Sunder 2007).

Second, following each period, traders are asked to report their subjective satisfaction with their performance. There is now a large literature of research based on directly asking individuals to report measures of “utility,” primarily as a global assessment of subjective well-being (see Dolan, Peasgood, and White 2008 for a review) but also as a moment-by-moment indicator of mood, or “experienced” utility (Kahneman et. al. 2004). Yet despite evidence supporting the reliability of such subjective responses (Krueger and Schkade 2008), this approach has not been widely used in economics experiments. While a few exceptions have focused on testing social preferences in dyadic interactions such as the dictator game (Charness and Grosskopf 2001) and the investment game (Becchetti and Degli Antoni 2010), we are aware of no research examining subjective experiences in market settings. Given that the assumption that traders derive utility solely from their absolute payoff is central to the theoretical prediction that prices in markets should track expected value, we consider it useful to test this assumption directly.

We demonstrate that the reference outcome affects traders’ reported subjective utility for their monetary outcome from the experiment in a manner consistent with the existence of tournament incentives. We also show that the type of relative performance 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 (where Account Total is defined as the sum of cash and market value of shares) as compared to those where all participants observe the lowest Account Total.

The rest of the paper is organized as follows. In section 2 we motivate the issue of relative comparisons in asset markets, discuss the relevant literature, provide background information, define key terms, and set up the key hypotheses for this study. Section 3 describes the experimental design. Section 4 provides the results and Section 5 concludes, contrasts the findings with previous results, and discusses alternative explanations.

2. 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 rank relative 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 paid in a tournament reward structure will generate bubbles, they do not address the question of whether relative payoff concerns affect traders' utilities in a way that would explain bubbles in the absence of direct monetary tournament rewards.

Although the few experimental research studies directly examining the impact of relative economic outcomes on subjective well-being have generally found that subjects manifest pro-social utility functions, disliking even advantageous inequality (Loewenstein, Thompson and Bazerman 1989, Charness and Grosskopf 2001, Charness and Rabin 2002, Becchetti and Degli Antoni 2010), a larger body of research examining the connection in large, real-world data sets suggests the opposite: people are happier

when they have more than a relevant reference group (Clark and Oswald 1996, Diener and Biswas-Diener 2002, Luttmer 2005), consistent with psychological research on social comparison processes showing that people who compare themselves to superior, as opposed to inferior, performers are less satisfied with their own performance and also more motivated to improve (Buunk and Gibbons 2007, Blanton, Buunk, Gibbons and Kuyper 1999).

We note that experimental studies have focused on dyadic interactions where the outcome received by the target of social comparison is directly affected by the choices made by the subject, whereas the correlational and psychological studies examine preferences when individuals are not directly responsible for others having less (or more) and/or the comparisons are anonymous or implicit. Since market interactions are anonymous and the impact of ones' choices on others' outcomes is indirect, we predict that in our experimental setting, subjects will report greater (lesser) satisfaction when their outcome is superior (inferior) to a social comparison outcome.

To test this, we exposed participants in an asset market to either upward or downward reference point information. After each market period, they 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.

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 their own Account Total, 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 accounts will be affected by the reference information that they observe, leading to a prediction that traders’ Satisfaction Ratings will be affected by the comparison between a trader’s own outcome and the reference outcome. This will have the effect of lowering the general level of satisfaction ratings for traders given Upward Reference information and raising 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.

Hypothesis 1: Market participants’ satisfaction ratings (1a) increase when a trader is the Leader, (1b) decrease when a trader is the Laggard, (1c) are lower for non-Leaders given Upward Reference information than for non-Laggards given Downward Reference information.

Hvide (2002) offers a theoretical model showing that tournaments which provide a large reward for the winner create an incentive for rational agents to become more risk-seeking while Gilpatric (2009) extends this to show that tournaments with a penalty for the loser have the opposite effect. Since Hypotheses 1a and 1b posit that traders experience a utility reward for winning and a utility penalty for losing in addition to the utility they derive from their monetary reward, these models imply that rational traders exposed to Upward Reference information will be more risk-seeking (because they aspire to be the leader) than those exposed to Downward Reference information (who aspire to avoid being the loser) and hence will manifest a greater demand for the risky asset. If market prices are significantly influenced by rational traders, their greater demand for shares in Upward Reference markets should result in higher share prices than in Downward Reference markets. To test this, we calculate the Average Price in each market, the average across all fifteen periods of $medianP_t$, the median trading price in period t .

Hypothesis 2: The Average Price paid for the asset across an entire market will be higher in markets where all traders receive Upward Reference information than in markets where all traders receive Downward Reference information.

While the explanations proffered for experimental market bubbles have focused on the role of irrationality or confusion (e.g., Lei, Noussair and Plott 2001, Kirchler, Huber and Stöckl 2011), the model of DeMarzo, Kaniel, and Kremer (2008) and the findings of James & Isaac (2000) demonstrate that bubbles can be a rational response to a

market's incentive structure. Similarly, the logic of Hypothesis 2 is that differences between Average Price and expected value might not represent mispricings but rather rational differences in risk attitudes. That said, Average Price is isomorphic to Average Bias, a measure often used to compare the magnitude of experimental bubbles (e.g. Haruvy and Noussair 2006; Average Bias is equal to Average Price minus the (constant) average fundamental value across all periods). Moreover, since Caginalp, Porter and Smith (2001) provide evidence that upward price movements can be self-reinforcing, if the influence of rational traders posited by Hypothesis 2 is not instantaneous but occurs dynamically, then feedback mechanisms would amplify the upward trend created by rational traders' greater demand for shares in Upward Reference markets. Therefore, we predict that Upward Reference markets will manifest higher levels of other measures used in the literature to test the magnitude of bubbles. Since our hypothesized mechanism depends on factors which drive price upwards, we focus on two measures which test for the extent and duration of overvaluation².

Maximum Deviation from fundamental value, the measure identified as most relevant by Caginalp, Porter and Smith (2001) for measuring the magnitude of bubbles, is defined by $\max_t(\text{median}P_t - f_t)$, where the t subscript denotes period number in a given session, $\text{median}P_t$ denotes the median trading price in period t in a given session and f_t denotes fundamental value at time t . We computed this measure for each session and report the average over sessions for a given treatment. Our *Maximum Deviation* measure is technically not identical to Caginalp et. al., since they use the single price per period

² While the literature on bubbles frequently comments on an empirical association of higher prices with higher trading volumes, we know of no compelling theoretical explanation for this linkage. Our hypothesis, in particular, is silent on the question of trading volume and thus we focus primarily on measures of valuation rather than of market thickness.

generated from a call auction institution³ whereas we calculate the median of all transactions in a given period in our double auction market, but we believe ours is a valid approach to compare our treatments.

Boom Duration (Haruvy and Noussair 2006) is the maximum number of consecutive market periods when the median market price is above expected value ($median P_t > f_t$), where $median P_t$ and f_t are as defined above⁴.

Hypothesis 3: Markets where all traders receive Upward Reference information have higher Maximum Deviations from fundamental value (3a) and longer Boom Durations (3b) than markets where all traders receive Downward Reference information.

3. Experimental Design

Investigations into the impact of changing the information structure on asset market prices and volumes are commonly done using a double auction design (e.g., Ang and Schwarz 1985; Copeland and Friedman 1987). We implement a double auction design using an asset with a declining, public knowledge expected value, as in the much replicated Smith, Suchanek, and Williams (1988) design. In the AllUp condition (7 sessions, 5 with 10 participants and 2 with 8, N=66) all participants saw Upward Reference information, while in the AllDown condition (7 sessions, 3 with 10 participants, 3 with 9, and 1 with 8, N=65) all participants saw Downward Reference

³ Specifically, at the end of each period, the program matched the orders in accordance with the sealed bid-offer double auction mechanism (also see Van Boening, Williams and LaMaster, 1993).

⁴ King et al (1993) use a slightly different definition of Boom Duration, which is the number of periods from low to high mean price deviation from the rational expectations dividend value of a share.

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. There was no interest paid on cash holdings. 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 double auction market process 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, defined in the instructions as the highest bid made for a share in the preceding period) 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.”

4. Results

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

Table 1: Summary of Hypothesis Tests

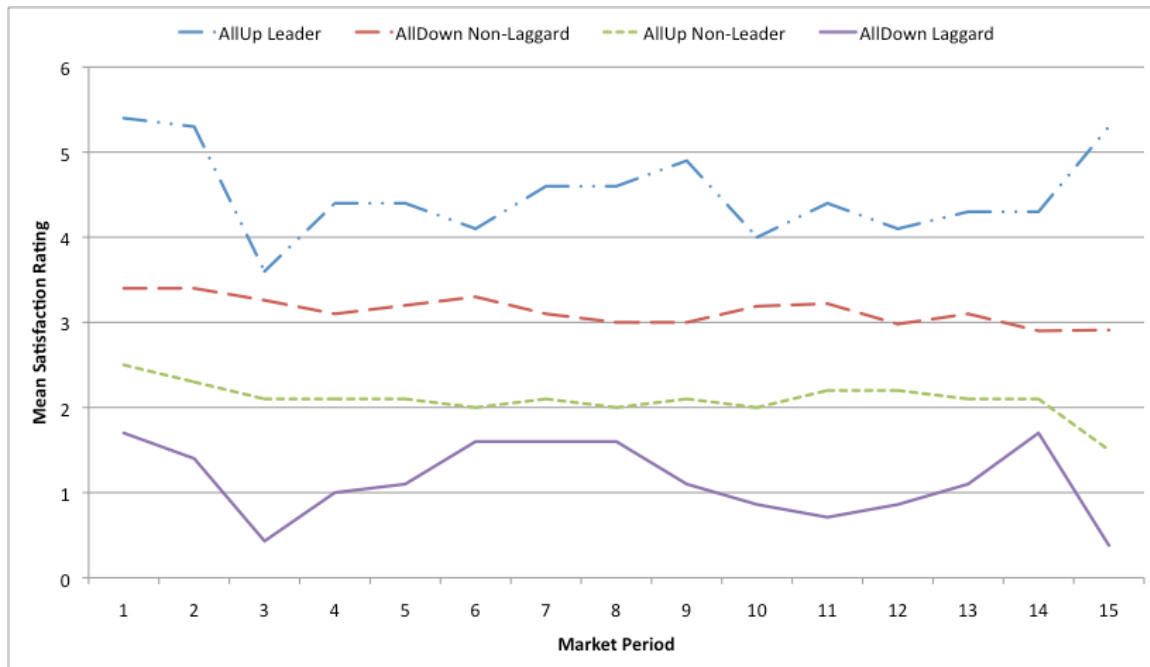
Each session is considered a single observation, for a total of 14 observations. The t-statistic is the first number, followed by degrees of freedom and the p-value. The first two tests concern differences between player roles within treatment and are therefore pairwise t-tests with 6 degrees of freedom. The remaining tests are two-sample t-tests with 12 degrees of freedom.

<i>Hypothesis</i>	<i>Description</i>	<i>T[d.f.], p-value</i>
<i>H1a</i>	<i>Satisfaction ratings are higher for Leaders in AllUp</i>	<i>3.26[6], p=0.02</i>
<i>H1b</i>	<i>Satisfaction ratings are lower for Laggards in AllDown</i>	<i>3.68[6], p=0.01</i>
<i>H1c</i>	<i>Satisfaction ratings are lower for non-Leaders given Upward Reference information than for non-Laggards given Downward Reference information</i>	<i>3.02 [12], p=0.01</i>
<i>H2</i>	<i>Average Price higher in AllUp condition</i>	<i>3.34[12], p<0.01</i>
<i>H3a</i>	<i>Max Dev higher in AllUp condition</i>	<i>2.19[12], p=0.05</i>
<i>H3b</i>	<i>Boom Duration higher in AllUp condition</i>	<i>2.88[12], p=0.01</i>

Figure 1 shows average satisfaction ratings by market period for the four trader types specified in Hypothesis H1. We conduct simple pairwise t-tests for differences between player roles and two-sample t-tests for differences between treatments, using each session average as a single observation. This gives us 14 observations for the 2-sample tests and 7 observations for the within-treatment pairwise tests. As predicted by H1a, Leaders report significantly greater satisfaction than non-Leaders in AllUp markets (mean Satisfaction rating of 4.5 vs. 2.1 across all periods, $t[6]=3.26$, $p=0.02$). As predicted by H1b, Laggards report significantly lower satisfaction than non-Laggards in AllDown markets (1.2 vs. 3.2, $t[6]=3.68$, $p=0.01$). For H1c, non-Leaders in AllUp

markets report significantly lower satisfaction than non-Laggards in AllDown markets (2.1 vs. 3.2, $t[12]=3.02$, $p=0.01$).

Figure 1: Mean Satisfaction Ratings (vertical axis) by Market Period (horizontal axis)



These simple t-tests, however, cannot distinguish the impact of the social comparison information from the impact of the trader's portfolios, which are highly correlated with these categories (the average expected value of trader's portfolios across all periods is E\$3333, 1565, 1077, and 221 for Leaders, non-Laggards, non-Leaders, and Laggards, respectively). We therefore constructed a variety of regression models utilizing a number of controls to determine which factors are most salient in predicting Satisfaction Ratings.

First, we created a four-factor linear regression model predicting final Satisfaction Ratings (that is, traders' Satisfaction Ratings for their payments at the end of the

experiment) using (1) the traders' own final Account Total (2) a dummy variable to denote whether a trader is aware they are the Leader (3) a dummy variable to denote whether a trader is aware they are the Laggard and (4) a continuous variable calculated as own final Account Total minus final reference Account Total (the "Relative Account"). For inference, we cluster standard errors at the session level, implying 14 independent observations (13 degrees of freedom in the regression t-statistics).

$$\text{Model 1: Final Satisfaction Ratings} = \alpha_1(\text{Account Total}) + \alpha_2(\text{Leader dummy}) + \alpha_3(\text{Laggard dummy}) + \alpha_4(\text{Relative account})$$

We also considered a second model allowing utility from the Relative Account to be asymmetric, depending on whether one's Relative Account is positive ("Non-Laggard") or negative ("Non-Leader").

$$\text{Model 2: Final Satisfaction Ratings} = \alpha_1(\text{Account Total}) + \alpha_2(\text{Leader dummy}) + \alpha_3(\text{Laggard dummy}) + \alpha_5(\text{Relative Account} \times \text{Non-Laggard dummy}) + \alpha_6(\text{Relative Account} \times \text{Non-Leader dummy})$$

Column 1 of Table 2 indicates that in Model 1 all three of the tournament-like factors are significant ($t=3.93[13]$, $p<0.01$ for Relative Account, $t=3.70[13]$, $p<0.01$ for being a Laggard, and $t=2.36[13]$, $p=0.03$ for being a Leader). While the impact of Account Total is larger than that of Relative account (raising satisfaction ratings by 0.038 for every incremental absolute dollar vs. 0.024 for every incremental relative dollar),

Relative Account explains a higher proportion of variability (a partial correlation of 0.28 vs. 0.19 for Account Total). 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 Account Total (vs. \$65.29 for being the Laggard), though it should be noted that there are only seven data points in each case.

In the second column of Table 2, we offer Model 2 where Relative Outcome appears only as an interaction term with condition. In this set of estimates, the difference between one's own outcome and the Leader's outcome remains significant but the difference between one's own outcome and the Laggard's outcome loses its significance, while the Laggard dummy remains significant but the Leader dummy does not. Thus, we see that the impact of being a laggard appears to be more status-driven whereas the impact from being a leader might be driven more by relative account considerations.

The R^2 for Model 1 is 0.35 and for Model 2 is 0.37, both higher than that for a regression model using absolute outcomes and intercept alone ($R^2=0.28$). Converting these R^2 to an F-test⁵, we reject the model that does not include Relative Account in favor of the either of the models specified in Table 2 ($p<0.01$), supporting our argument that providing relative performance information creates tournament-like incentives. We can further reject Model 1 in favor of Model 2 with $p=0.05$. This suggests that the impact of Relative Account is not symmetric and critically depends on the treatment.

⁵ $F = (0.35-0.28)/3 / ((1-0.35)/(131-5))=4.65$; $(0.37-0.35)/1 / ((1-0.37)/(131-6))=3.97$

Table 2. Linear regression model -- Dependent variable: Final satisfaction ratings (N=131; df=13)

Standard errors are clustered at the session level and shown in parentheses.

<i>Factors</i>	<i>Model 1</i>	<i>Model 2</i>
Intercept	1.90 (0.25)**	2.13 (0.25)**
Account Total (E\$)	0.038 (0.01)*	0.058 (0.015)**
Leader Dummy	1.78 (0.75)*	0.71 (0.82)
Laggard Dummy	-1.57 (0.42)**	-1.80 (0.46)**
Relative Account (E\$)	0.024 (0.006)**	----
Relative Account x AllUp	----	0.035 (0.009)**
Relative Account x AllDown	----	-0.014 (0.023)
R^2	0.35	0.37

*p<0.05 **p<0.01

Our data also allow us to examine how Satisfaction Ratings vary over the course of the experiment. It is important to note that prior to the final period, Account Totals are based on market prices of shares (specifically, the highest bid for a share in the preceding period) rather than expected values. We consider this appropriate since real-world portfolios are likewise evaluated using market prices rather than expected values (which are, of course, unknown), but it creates the possibility that our methodology might cause traders to infer that the market price is “correct” even when it is significantly different from expected value. If this were occurring, we would expect interim Satisfaction Ratings to be more highly correlated with nominal Account Totals than with expected value of holdings (which, though not explicitly provided to subjects, could be calculated by using the sheet provided (Appendix B)).

In fact, however, Table 3 shows that in a modified version of Model 1 predicting interim period Account Totals which includes controls for subject fixed effects (since there are 14 observations for each subject), expected value is a marginally better predictor

of Satisfaction Ratings in a head-to-head comparison of single-factor models (Models 3 and 4, with adjusted r-squared of 0.75 vs. 0.74, respectively) while in a joint model (Model 5), Satisfaction Rating goes up by 0.07 for every E\$ increase in expected value ($p < 0.01$) but actually decreases by 0.008 for every E\$ increase in Account Total ($p < 0.05$). Relative Account, which is the difference between one's Account Total and the reference and is based on nominal Account Totals rather than expected value, has a non-significant effect in all three Models.

Furthermore, consistent with a substantial body of research indicating that individual utility is based on changes in wealth in addition to absolute levels of wealth (Kahneman and Tversky 1979), Model 6 shows that interim Satisfaction Ratings reliably increase when expected value increased in the prior period, whether due to trading gains or dividend distributions ($p < 0.01$), but not when Account Total increased. Thus, it seems reasonable to conclude that participants understand that market-based Account Totals are not the same as expected values and that they correctly anticipate the hedonic impact of the four factors that will affect their utility for experimental outcomes, as listed in Table 2.

Table 3. Linear regression model -- Dependent variable: Interim satisfaction ratings (N=1834, df for significance based on robust clustered standard errors = 13)

Standard errors are clustered at the session level and shown in parentheses. Each column represents a regression model with different set of explanatory variables.

<i>Factors</i>	<i>Model 3</i>	<i>Model 4</i>	<i>Model 5</i>	<i>Model 6</i>
Intercept	3.96** (0.16)	2.96 (0.21)	3.02 (0.21)	3.03** (0.22)
Account Total (E\$)	-0.001 (0.011)		-0.008* (0.007)	-0.005 (0.008)
Expected Value (E\$)		0.061** (0.013)	0.066** (0.014)	0.061** (0.015)
Leader Dummy	1.21 ⁺ (0.64)	1.24* (0.49)	1.27* (0.49)	1.24* (0.46)
Laggard Dummy	-1.04** (0.29)	-0.91** (0.28)	-0.92** (0.28)	-0.90** (0.28)
Relative Account (E\$)	0.001 (0.002)	0.0001 (0.001)	0.0001 (0.001)	
Prior Period Change in Account Total				-0.0006 (0.003)
Prior Period Change in Expected Value				0.04** (0.012)
R^2	0.74	0.75	0.76	0.76

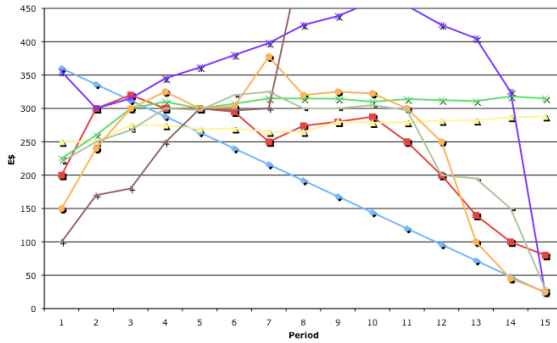
⁺p<0.10 *p<0.05 **p<0.01

Turning to our hypotheses about market level effects, Figure 2 shows the median prices for each period of each session in the AllUp and AllDown conditions. In support of Hypothesis 2, the average of the Average Price for AllUp markets, 304.4, was significantly higher than that for AllDown markets, 194.0 ($t = 3.34[12]$, $p < 0.01$). There were also statistically significant differences in the two measures of the magnitude of bubbles defined in section 2 and enumerated in Table 4: (H3a) Maximum Deviation: 299.4 vs. 129.2 ($t = 2.19[12]$, $p = 0.05$) and (H3b) Boom Duration: 11.4 vs. 7.6 ($t = 2.88[12]$, $p = 0.01$).

Figure 2: Median Prices by Market Period

Each curve represents one session over 15 periods. The vertical axis is the median price and the horizontal axis is the period. The straight diagonal line represents the fundamental value of the asset in each period.

AllUp Markets (N=7)



AllDown Markets (N=7)

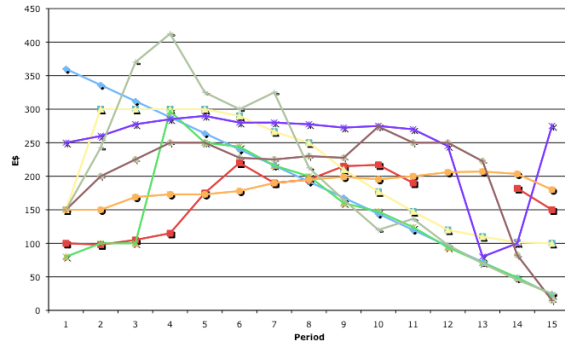


Table 4 also exhibits the corresponding measures for these three metrics for markets where no relative performance information is provided, taken from Haruvy and Noussair (2006), an appropriate benchmark for the markets reported here since both the parameters and software were the same. We also provide data for another common bubble measure from the literature, Turnover, a measure of the volume of trading which is defined as $(\sum_t q_t)/(TSU)$ where q_t is the quantity of units of the asset exchanged in period t and TSU is the total stock of units agents hold. Though none of the no-reference benchmark metrics are significantly different from the corresponding metrics for either AllDown or AllUp, we see that for two of them (Maximum Deviation and Average Price), the value for the benchmark falls in between the AllUp and AllDown reference points, suggesting that the two types of reference point provided are each having an independent effect on market prices, with Downward Reference information lowering

prices and Upward Reference information raising prices. The lack of significance between our treatments and the benchmark treatments of Haruvy and Noussair (2006) may not be surprising given that there are only two observations in the benchmark treatment, but the clearly significant differences between the AllUp and AllDown treatments suggest a significant impact of the reference point.

Table 4: Average Bubble Measures by Condition

Maximum Deviation is $\max_t(\text{median}P_t - f_t)$, where t denotes period number, $\text{median}P_t$ denotes the median trading price in period t in a given session and f_t denotes fundamental value at t .

Boom Duration is the maximum number of consecutive market periods when the median market price is above expected value.

Turnover is $(\sum q_t)/(TSU)$ where q_t is the quantity of units of the asset exchanged in period t and TSU is the total stock of units.

Condition [N]	Average Price (E\$)	Maximum Deviation (E\$)	Boom Duration (Periods)	Turnover
AllDown [7]	194.0	129.2	7.6	9.09
AllUp [7]	304.4	299.4	11.4	9.86
NoReference*	234.8	161.0	11.5	12.2
T-tests comparing:				
AllDown vs. AllUp	3.34 [12] p<0.01	2.19 [12] p=0.05	2.88 [12] p=0.01	0.41 [12] p=0.69
AllDown vs. NoRef	1.58 [7] p=0.16	0.50 [7] p=0.63	1.57 [7] p=0.16	0.94 [7] p=0.38
AllUp vs. NoRef	1.15 [7] p=0.29	0.95 [7] p=0.37	0.08 [7] p=0.94	0.82 [7] p=0.44

*The data for the no reference point treatment is from Haruvy and Noussair (2006)

5. 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 different types of social information on the process that precedes equilibrium, and offered evidence that providing this information to inexperienced traders in an experimental asset market can 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].

We think it useful to contrast our design and results to those of James and Isaac (2000). In their design, traders gain experience in repeated markets which alternate between the “standard” payoff structure and a tournament where incentive payments are provided only if the trader is above the average level of payoff at the end of the market. They find that experience does not eliminate bubbles in markets with tournament rewards, which they argue is due to this incentive structure inducing rational risk-seeking behavior among traders, fracturing the backwards induction argument that underlies the prediction that prices will stabilize at fundamental value (Tirole 1982)⁶. Cheung and Coleman (2011) extend this to demonstrate that mispricing also increases when experienced traders are only partially incentivized for relative performance through periodic tournament-based “bonuses” based on interim performance.

Our approach differs in that we provide information about relative position but do not provide monetary tournament incentives, but we find that this information by itself is

⁶ The logic of James and Isaac (2000), like our own, does not require that all traders be risk-seeking, but only that the behavior of those who are risk-seeking affects demand and hence prices.

sufficient to raise prices in markets with inexperienced traders. We propose that the underlying process is the same: since traders experience a utility benefit (penalty) from being the market Leader (Laggard), relative performance information creates an implicit tournament reward structure which increases risk-seeking among rational traders when a reward goes to the Leader and reduces risk-seeking when a penalty goes to the Laggard.

We also observe that Prospect Theory (Kahnemann and Tversky 1979) offers an alternative mechanism whereby Upward Reference information might induce increased risk-taking since it posits that decision makers become risk-seeking when they perceive themselves to be in a situation of potential loss relative to some reference point. The social comparison information we provide offers a highly salient reference point which might cause non-Leading traders given Upward Reference information to perceive their current circumstances as a “loss” and consequently become risk-seeking relative to the non-Lagging traders given Downward Reference information. In this regard, we consider it noteworthy that whereas the average non-Laggard reports a Satisfaction Rating of 3.2, just slightly above neutral, the average non-Leader reports a Satisfaction Rating of 2.1, corresponding to a slightly negative affective response (see Figure 1).

We note, however, that we do not offer a model of how *individual* traders might alter their demand for shares in response to information about their status. Despite the significant impact of treatment on market prices, we were unable to find statistically significant differences in individual demand using two-sample t-tests based on changes in individual share holdings between any of the following groups: Leaders and non-Leaders in Upward treatments; Laggards and non-Laggards in Downward treatments; Leaders in Upward treatments and (unaware) leaders in Downward treatments; and Laggards in

Downward treatments and (unaware) laggards in Upward treatments. Changes in individual share holdings will depend on one's initial endowment, individual history, prices in the market, and the actual and predicted demand and supply of others. We did not present a model accounting for all these factors, but it is clearly an important direction for future research.

Our focus on rational behavior magnifying experimental bubbles can also usefully be contrasted to research which has suggested that reducing “confusion” mitigates them. Dufwenberg, Lindqvist, and Moore (2005) find that bubbles can be eliminated by giving traders repeated experience with the market environment while Kirchler, Huber and Stöckl (2011) show that describing the experimental asset as representing shares of a depletable gold mine reduces overvaluation, an effect they ascribe to altering traders' expectation in the standard setting that the fundamental value of a dividend-paying asset usually stays constant. One question this raises is whether our results arise from greater confusion among traders due to our use of market prices in calculating Account Totals; might traders be misunderstanding that information, presumably by inferring that market prices are correct even when they are not?

We cannot exclude the possibility of confusion, but we offer three pieces of evidence that confusion is not arising from the use of market prices rather than expected value in calculating Account Totals. First, the benchmark results for the key pricing metrics from markets without reference point information fall in between the results for AllUp and AllDown markets. Since we utilize market prices in both treatments, that manipulation does not appear by itself to be a source of greater confusion. Second, trading volume, which is typically considered a measure of irrationality when

fundamental values are public knowledge (since the only reason to trade would be radically different risk preferences) is actually lower in both our treatments compared to the benchmark. Third, if traders were confused about the “true” value of the shares, we would expect their Satisfaction Ratings through the course of the experiment to be more affected by their Account Totals than by the expected value of their holdings. In fact, we find the opposite. In sum, the evidence suggests that traders recognize the distinction between expected value and market price.

It is also worth noting that using market prices to calculate Account Totals for Leaders and Laggards does not in general create a misleading impression of interim performance since Account Total rankings correlated very closely with actual performance. Out of 105 observed market value Leaders in the AllUp treatment (7 sessions x 15 periods each), 85 were also leading in terms of fundamental value of their holdings, while 96 of 105 observed market value Laggards in the AllDown treatment were true laggards (an overall correspondence of 0.81). Moreover, while Account Totals were clearly inflated in periods of bubbles, over the entire experiment, the correlation between market values and fundamental values was 0.75 in the AllUp treatment, and 0.97 in the allDown treatment, as computed over all traders in all periods.

Finally, our demonstrated effect depends on making salient one particular type of social comparison information to all traders, which raises the question of the relevance of our findings for both standard experimental markets, where such information is not provided, as well as for real-world markets, where traders have information about both upward and downward comparisons. In either case, in order for our demonstrated effect to be relevant, traders must somehow be systematically or situationally biased to focus on

one social comparison or the other. There is very little research that directly examines how and when people choose to engage in upward vs. downward social comparison, but several studies have demonstrated a tendency for individuals to most want to compare themselves with others who are doing slightly better than themselves (Blanton, Buunk, Gibbons and Kuyper, 1999, and references contained therein). This tendency might explain the prevalence of bubbles in markets with inexperienced traders who do not receive any social comparison information, since they might reasonably infer from market prices alone that there are traders who are succeeding by buying an asset that is increasing in price and hence desire to emulate them.

In conclusion, we believe that our study opens up several fruitful avenues for future research about the role relative performance concerns play in asset pricing as well as offering two useful methodological innovations to assist in that research.

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Appendix A: 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 $7 \times 24 = 168$. 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: 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	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: Trading Screen

Period		1 of 15		Remaining Time [sec]: 30	
Money	225				
Shares	3				
	Enter ask price	Ask Price	Purchase price	Bid Price	Enter bid price
	<input type="text" value="350"/>	<input type="text" value="300"/>	<input type="text" value="200"/> <input type="text" value="200"/>	<input type="text" value="150"/>	<input type="text" value="150"/>
	<input type="button" value="SUBMIT ASK PRICE"/>	<input type="button" value="BUY"/>		<input type="button" value="SELL"/>	<input type="button" value="SUBMIT BID PRICE"/>

Appendix D: Account Status Screen

Period	
1 of 15	
Your cash before dividend distribution	225
Dividends per share	28
Your shares	3
Total Dividends	84
Purchase of lottery ticket	-6
Lottery payoff	0
Total cash	303
Total shares	3
Stock Price	200
Account total (cash plus market value of shares)	903
The person with the smallest account has	903
How do you feel about your current account total? Very negatively <input type="radio"/> <input type="radio"/> <input type="radio"/> <input type="radio"/> <input type="radio"/> <input type="radio"/> Very positively	
CONTINUE	

Appendix E

Additional Notes on Methods

The fourteen 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 several minor changes in procedure across markets.

- In all markets, traders only received oral instructions, but 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 market was shortened from 3 to 2 minutes.

Neither of these changes had a statistically significant effect on any of the measures reported.

One experimental feature visible on the Account Status Screen (Appendix D) but not noted elsewhere is that traders 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.