

Contagion of Wishful Thinking in Markets

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Prior research provides only weak and controversial evidence that people overestimate the likelihood of desirable events (wishful thinking), but strong evidence that people bet more heavily on those events (wishful betting). Two experiments show that wishful betting contaminates beliefs in laboratory financial markets because wishful betters appear to possess more favorable information than they actually do. As a consequence, market interaction exacerbates rather than mitigates wishful thinking. This phenomenon, “contagion of wishful thinking,” could be problematic in many settings where people infer others’ beliefs from their behavior.

Key words: wishful thinking; betting; desirability bias; unrealistic optimism; motivated reasoning; contagion; markets; investors; investing; gambling; information aggregation

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1. Introduction

A staple of experimental economics research is to identify a robust individual decision-making bias and show how competitive pressures can keep the bias from having effects on prices in laboratory markets (e.g., Camerer 1987, Budescu and Maciejovsky 2005). This study provides a new twist on this familiar approach: we identify a bias (wishful thinking) that is weak, if not nonexistent, at the individual level, and show how interaction in financial markets can amplify the bias, causing strong effects on prices and beliefs.

People are said to succumb to wishful thinking (also known as the “desirability bias”) when there is a causal influence of preferences (wishes, desires, motivations) on expectations about the future (Krizan and Windschitl 2007).¹ Early studies suggested that wishful thinking could arise even in simple card games where people could base their predictions on objective probability information and errors would be quickly verifiable (Irwin 1953, Irwin and Snodgrass 1966). Such a bias would have important implications across a variety of managerial, social, and political contexts. However, Bar-Hillel and Budescu (1995) demonstrated that these early experiments provided evidence only of wishful *betting*, not wishful *thinking*: people might bet more aggressively on desirable

outcomes than undesirable outcomes, but their stated beliefs about outcomes do not seem to be biased.

We report two experiments that show how wishful betting by individual traders can lead to wishful thinking in financial markets. In our first experiment, participants traded securities with values determined by the sum of two random numbers, of which they knew only one. We manipulated desirability by altering whether all traders were initially endowed with long positions (in which they benefited when share values were high) or short positions (in which they benefited when share values were low). This is just the type of simple, objective, and verifiable prediction task for which wishful thinking biases are unlikely, but we found that closing market prices and post-trade beliefs were significantly higher for long positions than for short positions. We argue that this result arose because each trader drew inferences of security value by observing trading behavior that was contaminated by wishful betting. This wishful betting led participants to develop—reasonably, though falsely—favorable beliefs about other traders’ information, and therefore about the value of the traded asset.

It is difficult to clearly identify the link between wishful betting and wishful thinking in our market setting, in which beliefs and trades change and affect one another in real time. We therefore conducted a second experiment using the far simpler card game paradigm developed by Irwin (1953). For each deck of cards, participants learned the proportion of desirable and undesirable cards, indicated their beliefs about the probability of drawing a card at random, and

¹ Whereas Krizan and Windschitl (2007) delineate the “desirability bias” from other forms of “wishful thinking,” we prefer the broader term because it is more familiar to readers outside of psychology and allows us to easily distinguish between wishful thinking and wishful betting.

chose how much to bet on that outcome. Participants in this initial setting showed clear evidence of wishful betting (consistent with the Irwin studies), but expressed accurate probability beliefs (consistent with Bar-Hillel and Budescu 1995). We then provided three groups of participants with either the bets, beliefs, or both bets and beliefs of the initial participants. These latter groups did not learn the proportion of desirable and undesirable cards in each deck—they had to draw inferences from the bets and beliefs of the first group. The results conform to those of our experimental market: players who saw prior bets exhibited wishful betting as well as wishful thinking, whereas those who saw only beliefs exhibited only wishful betting. Participants who saw both bets and beliefs exhibited levels of wishful thinking and wishful betting in-between the levels exhibited by the other two groups, indicating that transmitting beliefs serves to weaken the biasing effects of wishful betting.

Our results illustrate how wishful betting and wishful thinking propagate through laboratory financial markets. How (and how strongly) wishful betting and wishful thinking affect real-world markets will depend on the rules governing trading and the information environment; however, a variety of phenomena in real-world markets are consistent with these biases, including the tendency to invest disproportionately in the stock of local firms (Coval and Moskowitz 2001, Hau 2001) and one's own employer (Benartzi 2001).² Our results also imply that contagion of wishful thinking could affect a variety of social and business activities where people must form beliefs using the behavior of others. For example, if people realize it is dangerous to talk on cell phones while driving but engage in the behavior as if they believe it is not so dangerous, other drivers' assessments of the danger may be reduced. Similarly, if companies engage in aggressive earnings manipulation as if they believe it is beneficial, other companies' assessments of the benefits of such manipulation may be inflated.

1.1. Wishful Thinking and Wishful Betting in Individual Decisions

In classic experiments by Irwin (1953) and Irwin and Snodgrass (1966), participants bet money on drawing either a marked or blank card from a deck

composed of both types. Participants also received an independent payment for drawing a marked card. As the independent payment increased, participants bet more frequently on drawing a marked card. This behavior suggests that participants became more confident in drawing a card when doing so was more desirable. Budescu and Bruderman (1995) expanded on these results by showing that wishful thinking is distinct from the illusion of control.

The Irwin experiments are often taken as evidence of wishful thinking (Slovic 1966, Kunda 1990, Budescu and Bruderman 1995). However, Bar-Hillel and Budescu (1995) noted the difficulty in inferring wishful thinking from behavior rather than from cognitive measures that directly elicit beliefs. Bar-Hillel and Budescu found no evidence of wishful thinking in a series of studies in which participants were provided with probability information and asked to estimate the probability of desirable and undesirable events. Thus, the Irwin results may provide evidence of wishful *betting* (taking positions that will be advantageous if the desirable outcome occurs), but not wishful *thinking*. Krizan and Windschitl (2007) conducted a meta-analysis of the wishful thinking literature and concluded that support for wishful thinking is present but weak, and called on researchers to provide evidence concerning the conditions necessary to produce wishful thinking.

1.2. Wishful Thinking and Wishful Betting in Markets

How will wishful thinking and wishful betting affect financial markets? Camerer (1987) listed a variety of economic forces that might allow markets to eliminate decision-making biases observed at the individual level. These forces include the presence of incentives, the ability of unbiased traders to discipline price errors, and the ability of biased market participants to extract information from the activity of less-biased traders. Given that incentives are the driving force behind wishful thinking, incentives seem as likely to magnify these biases as to mitigate them. Thus, it seems reasonable that wishful betting was observed by Forsythe et al. (1992) in their examination of the Iowa Presidential Stock Market. In this market, traders traded one security that paid off \$1 if Dukakis won the 1988 presidential election, and another that paid off \$1 if Bush won. Traders who supported Dukakis were greater net purchasers of Dukakis stock, whereas Bush supporters were greater net purchasers of Bush stock. However, other market forces did appear to mitigate the effect of this biased individual behavior on market prices. In particular, the wishful betting of Dukakis and Bush supporters cancelled one another out, allowing the most neutral traders to also be the marginal (price-setting) traders. Wishful betting is likely to have a stronger effect in markets

² There are also alternative explanations for these phenomena. For example, it could be that investors possess a larger quantity of information concerning local firms that is more costly for investors in other areas to gather or process. A number of papers support this explanation, showing that the home bias is reduced for foreign firms that adopt accounting methods more similar to U.S. firms (Bradshaw et al. 2004) and that more wealthy, well-informed investors are likely to invest abroad (Bailey et al. 2007). However, Strong and Xu (2003) survey investment fund managers and find that they display significant relative optimism toward their home country equity market, suggesting that wishful thinking may well contribute to the home bias.

for financial assets than in election markets, because preferences tend to be more uniform—whereas elections usually hew to within a few percentage points of a 50-50 split, almost everyone wants the economy to grow. Thus, it is less likely that wishful sellers will cancel out wishful buyers. When traders must draw inferences from market prices, wishful betting by some traders may even lead to (unintentional) wishful thinking by others. This outcome, contagion of wishful thinking, could impede unbiased information aggregation.

A variety of research has examined information aggregation in the laboratory. In the experimental market settings of Plott and Sunder (1982, 1988), Forsythe and Lundholm (1990), and Lundholm (1991), for example, the liquidating dividend of a security depends on which of several states is selected at random. In a typical experiment, some traders are told “the state is not X,” whereas others are told “the state is not Y,” so that the market as a whole knows that the state must be Z. Each trader’s information is aggregated by the market price through trade, allowing the market price to convey more information than is known by any individual. Lundholm (1991) found that prices were likely to be efficient in this simple setting unless traders possessed diverse sets of information that did not reveal the true asset value in aggregate.

Because most laboratory markets do not allow any direct communication between traders, they presumably aggregate information through trading decisions. Bloomfield et al. (1996) confirmed this presumption. In one experiment, four traders received almanac-style questions with answers ranging from 0 to 100 (e.g., “What percent of France’s electricity comes from nuclear power?”). Each security had a true value equal to the answer to one of these questions. In estimate-based markets, participants responded by stating whether their estimate was above or below the current price, whereas in a trade-based market, participants responded by buying or selling up to 10 shares at the current price. The authors showed that prices were more accurate than even the most accurate individual in the trade-based markets, and were significantly more accurate than those generated by the estimate-based markets. A second experiment confirmed that the superiority of the trade-based market arose because participants would infer large trade sizes as indications of confidence, so that the market could weight the most informed participants’ beliefs most heavily.

The power of trades to communicate information can cause contagion of wishful thinking when investors have preferences over the values of the securities they trade. Assume that everyone prefers the asset to have a high value. Even if no one actually overestimates the value of the asset (there is no wishful thinking), wishful betting will cause people to

buy more shares (or sell fewer) at any given price. Unless other traders completely anticipate and undo this trading distortion, they are likely to infer that the information held by other traders indicates a higher asset value. Thus, wishful betting leads to wishful thinking—not because people engage in wishful thinking, but because the data they have available (trades) are biased.

Why wouldn’t traders account for wishful betting by others and draw unbiased inferences about others’ information? People may simply be unaware of the tendency for wishful betting. People might also engage in motivated reasoning (Kunda 1990) when interpreting market information, perhaps accepting high prices uncritically as good news (Ditto and Lopez 1992). Hales (2007) provided evidence that critical evaluation of unappealing information can exacerbate biases when the unappealing information is ambiguous. Market activity is likely to be sufficiently ambiguous to provide the room needed for motivated reasoning. As discussed by Bloomfield (1996a), interpreting trades requires filtering out the effects of risk preferences and public information to extract the trader’s private information. This inversion process becomes more difficult for traders as trade progresses, because the information content of the trade becomes smaller (relative to risk preferences and inferences concerning the meaning of other trades) and because the sequential nature of trade makes it necessary for traders to consider what other traders thought other traders thought other traders thought, etc. Eventually, it becomes too difficult for traders to invert the trade function, and further information aggregation becomes impossible. This process can also lead to information “mirages” (Camerer and Weigelt 1991, Schnitzlein 1996), in which the market appears to believe that aggregate information indicates a state of nature that has not, in fact, arisen. As a result, even when traders receive feedback about actual security values, it may be difficult for them to attribute errors to wishful betting.

The next two sections of this paper describe two experiments examining how wishful betting can contaminate beliefs. In the first experiment, we demonstrated contagion in an information-aggregation market, even though the prediction task was one in which wishful thinking is extremely unlikely *ex ante*. In the second experiment, we used a version of the Irwin card task to demonstrate more definitively that the desirability of outcomes leads to wishful betting, and that people observing those bets overestimate the probability of a desirable outcome.

2. Study 1

In our first study, we manipulated traders’ preferences by altering whether all traders were initially

endowed with long positions (in which they benefited when share values were high) or short positions (in which they benefited when share values were low). Each group of four traders traded eight securities with a long starting position, and traded another eight with a short position. We used a computerized specialist market in which market prices are a linear function of cumulative net demand, so that buying shares increases prices and selling shares decreases prices. The asset value was the sum of two random numbers, each known by half of the traders. Wishful betting would lead traders to buy more shares (or sell fewer) when they are endowed with long positions than when they are endowed with short positions, resulting in higher prices (which are determined entirely by the balance of trading). Wishful thinking would lead to higher estimated values (because traders fail to account for wishful betting). Thus, our first hypothesis predicts that starting positions will affect both closing prices (wishful betting) and closing estimates of value (wishful thinking):

HYPOTHESIS 1. Both closing prices and posttrade value estimates are higher when all traders are endowed with long positions than with short positions.

Whereas biased value estimates suggest contagion of wishful thinking through market behavior, we provide further evidence on contagion by comparing variations in this measure of wishful thinking within and across groups. To the extent that traders' wishful thinking is reinforced by other traders' wishful betting, we should see the magnitude of the bias correlated among traders within the same group. This expectation is formally reflected in the following hypothesis:

HYPOTHESIS 2. Traders' wishful thinking is positively associated with the wishful thinking of other traders in their group.

It is worth noting that these hypotheses contradict those that would be derived from a standard model of diversification by risk-averse traders. Economic theory predicts that traders will discount the value of securities whose returns correlate positively with those of their own assets (Lintner 1965, Sharpe 1964; see Gollier and Pratt 1996 and Kimball 1993 for analyses of "background risk"). In contrast, traders who exhibit wishful betting and wishful thinking will place a premium on those securities, because they will overestimate the likely returns.

2.1. Method

2.1.1. Participants. Thirty-two graduate and undergraduate students from a U.S. university participated in our market experiment. They volunteered in response to an e-mail announcing two scheduled

laboratory sessions of 16 traders each. They spanned a variety of academic disciplines, and all had participated in at least one prior experimental market session. These prior sessions involved software, trading rules, and market structure similar to the present study. Participation in a prior market experiment guaranteed that participants had traded securities using one of a variety of interfaces and received payment for doing so. An analysis of early and late trading periods shows no significant differences in price error, suggesting no significant experience effects, at least throughout the course of our experiment.³

Although we cannot trace participants to specific prior experiments, it is unlikely that more than a handful had participated in any studies that manipulated starting positions in a manner likely to lead to wishful thinking or other biases. The laboratory had run many experiments investigating unrelated aspects of market behavior, but only one pilot study manipulating starting positions. If anything, participation in that study would have biased against finding wishful thinking.

2.1.2. Instructions. Each session began with an experimenter handing out written instructions detailing all aspects of the market and verbally describing the market structure, as well as answering questions that participants might have had. Because all participants had previously participated in similar experimental markets, the task was relatively straightforward for them, and we included no practice securities.⁴

2.1.3. Securities and Information. Participants in our study traded a sequence of securities in a laboratory market setting. Security values, denominated in laboratory dollars, were determined by the sum of \$50 and two randomly generated signals, each with a range of [−\$25, \$25]. Securities could thus assume values ranging from \$0 to \$100. In each group, two traders received one component of value (signal 1) and two traders received the other component (signal 2). Traders benefited by correctly inferring the signal that they did not receive and thereby driving security price closer to true value.

Traders were told that they would receive an outcome bonus or penalty for each security, which depended only on the security's true value. When traders started with a long position, they received an "outcome bonus" equal to $(\text{value} - \$50) \times 10$ if value

³ Although the plots in Figure 2 suggest learning, this represents a within-security effect where traders slowly learn other traders' information rather than a general experience effect across securities.

⁴ Instructions for participants and a screenshot of the trading interface for Study 1, as well as instructions and the paper instrument for Study 2, are presented in the electronic companion as part of the online version available at <http://mansci.journal.informs.org/>.

was greater than \$50, and incurred an “outcome penalty” equal to $(\$50 - \text{value}) \times 10$ if value was less than \$50. For short positions, these payments were reversed. We explicitly reminded participants before each trading round that they had no control over their outcome payments, and that outcome payments were based on true underlying value rather than security price.

We minimized physical proximity of traders within a group in order to reduce the possibility of viewing others’ screens, or communicating within a group by other means (such as listening for other traders’ mouse clicks to infer trading source).

2.1.4. Experimental Design. Participants were randomly assigned to groups of four traders, and traded only within their own group. This resulted in a total of eight groups. Each group traded 28 securities, of which 16 represented a fully balanced design described below. The remaining 12 securities were included to allow the entire set of securities to be representative of the probabilities we told participants to expect.

Within the 16 securities we analyzed, each group traded 8 securities in which all four traders in the group were effectively endowed with a long position (because they received an outcome bonus when value was high and an outcome penalty when value was low). All four traders in the group were effectively endowed with a short position in the remaining 8 securities (because they received an outcome bonus when value was low and an outcome penalty when value was high). Within a group, starting position was balanced with the extremity of value (close to \$50, far from \$50), sign of individual signal (+/-), and discrepancy between the two signals (+/-, +/+, -/-, -/+). We also balanced starting position and order of securities between groups such that half of the groups began with a long position whereas the other half began with a short position in any given security, and half of the groups traded securities in reverse order. We preselected randomly generated signals to allow for identical securities between groups and to achieve balancing. Thus, the design manipulated position (all four traders long or short), value extremity, sign of signal, and signal discrepancy within group; and the order of position (first security long, first security short) and security between groups.

2.1.5. Market Structure, Trading, and Estimates. Our experiment utilized a “robot specialist market,” where traders engaged in transactions with a robot trader that bought and sold shares. Each security began with a color-coded screen (green for long positions and red for short positions) and large, bold text describing the trader’s independent outcome payment, indicating that the outcome payment was

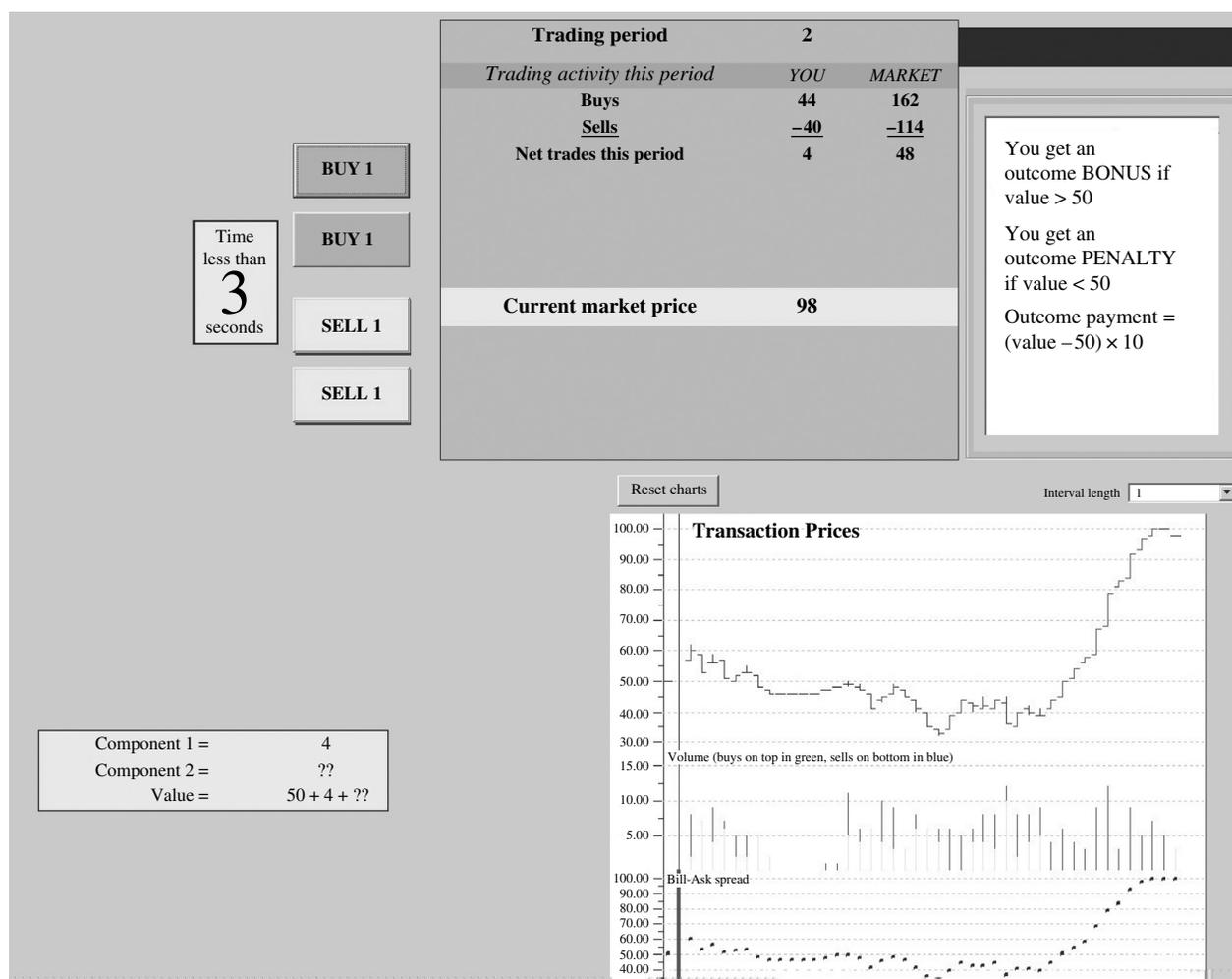
similar to a long or short position, and providing a numerical example of a gain and loss situation for the specific position. Traders then proceeded to the main trading screen, received their signal, and began trading when ready. Each security began trading at a price of \$50. Traders could click a “Buy 1” button to purchase a share of the security or a “Sell 1” button to sell a share. Shares were purchased at \$1 above market price and sold at \$1 below market price. Thus, each trade moved the market price by \$1. Starting position, individual and market volume (buys and sells), market price, and traders’ signals were displayed in information windows on the main trading screen (see Figure 1⁵ for a screen capture of the trading interface). Traders accumulated trading gains by buying (selling) shares at prices below (above) true value. Each security’s trading period lasted 60 seconds, after which participants learned the true value of the security, their total trading gains/losses, and their independent outcome payments.

After trading was complete for each security, but before true value was revealed, we asked each trader to estimate the true security value. This posttrade estimate serves as our measure of wishful thinking, which is distinct from market price (our measure of wishful betting). We did not elicit pretrade estimates because our oral instructions explicitly stated that “your best estimate of security value is 50 plus the number you know.” Prior studies in similar settings (e.g., Bloomfield 1996a, b) lead us to believe it is highly unlikely that pretrade estimates would differ from this explicit recommendation, as does the research indicating little evidence of wishful thinking in similar tasks (e.g., Bar-Hillel and Budescu 1995). Our second experiment corroborated the expectation that there is no wishful thinking in initial beliefs.

The robot specialist market utilized here has been shown to be quite effective in aggregating information. For example, in robot specialist markets with very similar characteristics (two groups of traders informed about different uniformly distributed random numbers), security values explained over 95% of the variation in closing market prices (Bloomfield 1996b). Prices in that study were even more informative than prices in computerized double-auction markets with a similar information structure (Bloomfield et al. 2009). On the other hand, the information structure used in this experiment is far more challenging than that used in early experiments on aggregation (e.g., Plott and Sunder 1982), which included only a few possible values. For example, in one state of a typical market created by Plott and Sunder, a security could pay a dividend of \$400, whereas in the other state

⁵For a full-color trading interface screen, see the electronic companion.

Figure 1 Trading Interface Screen from Study 1



the security would pay a dividend of \$700. Once the market price falls sufficiently below \$700, the next reasonable price becomes \$400, effectively eliminating a wide range of potential pricing error. In contrast, any price from \$0 to \$100 is reasonable in our setting. This nearly continuous distribution of values and reasonable prices increases the chances that modest degrees of wishful betting will create detectable effects in prices and beliefs. As a result, we might expect wishful betting and wishful thinking to be stronger in the robot specialist or double-auction markets previously discussed than in those utilized by Plott and Sunder (1982) and similar papers. A large literature on market microstructure indicates that the details of trading rules and information environments can alter market behavior significantly both in theory (O'Hara 2004) and in the laboratory (Sunder 1995). We discuss how our results are likely to generalize to other market settings in §4.

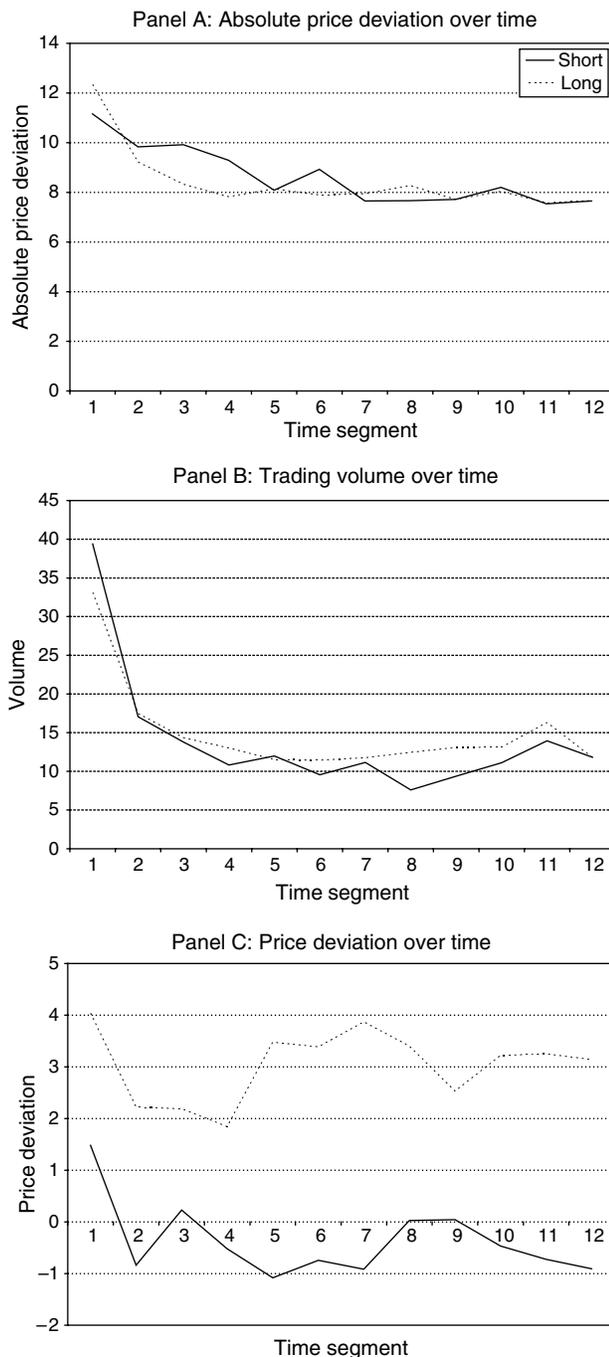
2.1.6. Incentives. Gain or loss from trading each round was calculated by multiplying the number

of shares a participant purchased by the difference between the price at which securities were purchased and the true security value. Total gain or loss for each security was the sum of the trading gain or loss and the outcome payment. At the end of the experiment, a constant was added to the total number of laboratory dollars gained or lost, and the sum was multiplied by a conversion rate. Participants were guaranteed a minimum payment of \$5. Although subjects did not know the constant or conversion rate, we informed them that average winnings would be approximately \$25 per session.

2.2. Results

2.2.1. Descriptive Statistics. Figure 2 illustrates the evolution of absolute price errors, signed price errors, and trading volume over the course of trading, averaged over all securities with long and short starting positions. Each panel divides trading into 12 equally long intervals for each security. Panel A shows that absolute price errors dropped from approximately

Figure 2 Price and Volume Plots



\$12.50 (long) or \$11 (short) in the opening seconds of trading to \$7.50 (both long and short) by the end of trade, resulting in a 33% reduction in initial pricing errors. This degree of informational efficiency is comparable to that observed in double-auction markets with similar informational structures, such as Bloomfield et al. (2009), which examined the tendency for more- versus less-informed traders to provide liquidity in markets and assume the role of market makers. Panel B shows that trading volume

decreased over the course of the markets, dropping from about 36 shares per group in the opening seconds to about 12 shares in the closing seconds. However, absolute price changes were quite small in the closing seconds (averaging well under \$1 over the last 10 seconds), suggesting that closing prices did indeed reflect convergence to an equilibrium level. Finally, panel C shows that signed price errors started at approximately \$4 when traders began with long positions, and closed at approximately \$3.10; in contrast, signed price errors started at approximately \$1.50 when traders began with short positions, and closed at approximately -\$1. These results are consistent with wishful betting.

2.2.2. Wishful Betting and Wishful Thinking. To test whether starting positions affected wishful betting (Hypothesis 1), we computed two average signed closing price errors for each group: one for the eight securities in which traders began with a long position, and another for the eight securities in which traders began with a short position. We then calculated the mean difference in these two measures for each group. These means are reported in Table 1 for each of the eight groups. Although economic theory predicts that risk aversion will yield lower prices when traders begin with a long position than when they begin with a short position, closing prices averaged \$3.38 above value when traders began with a long position, and averaged -\$0.83 below value when traders began with a short position. A repeated-measures ANOVA that took our between-group balancing factor into account showed that the closing price difference of \$4.20 was statistically significant ($F(1, 24) = 3.76, p = 0.05$), despite the conservative analysis that assumed we had only eight independent observations (one for each group). Results were similar using nonparametric analyses. Thus, the results supported the prediction that traders engage in wishful betting.

Table 1 Wishful Betting

Group	Price deviation for long positions	Price deviation for short positions	Wishful betting (long - short)
1	0.63	1.75	-1.13
2	3.00	-1.13	4.13
3	0	3.25	-3.25
4	2.13	-2.50	4.63
5	-0.13	1.38	-1.50
6	2.88	-0.63	3.50
7	7.88	-5.25	13.13
8	10.63	-3.50	14.13
Mean	3.38	-0.83	4.20
Standard deviation	2.87	3.89	6.49

Notes. This table reports the average signed deviation of closing price from value by group (1–8) and starting position (long, short) from Study 1. Wishful betting is the difference between price deviation for long and short positions.

Table 2 Wishful Thinking

Group	Estimate error for long positions ^a	Estimate error for short positions ^a	Wishful thinking (long – short)
1	0.09	2.06	-1.97
2	3.90	0.86	3.04
3	2.34	-0.34	2.67
4	1.94	-0.16	2.09
5	-0.44	2.34	-2.78
6	3.28	-0.16	3.44
7	6.41	-2.25	8.66
8	8.57	-5.33	13.91
Mean	3.26	-0.37	3.63
Between-group standard deviation			5.44
Average within-group standard deviation			2.77

Notes. This table reports the signed error in each individual trader's post-trade estimate of value from Study 1. We report the error for long and short positions, as well as means and standard deviations for the difference between the long and short estimate. The difference between estimates for long and short positions provides a measure of the degree of wishful thinking in traders' beliefs.

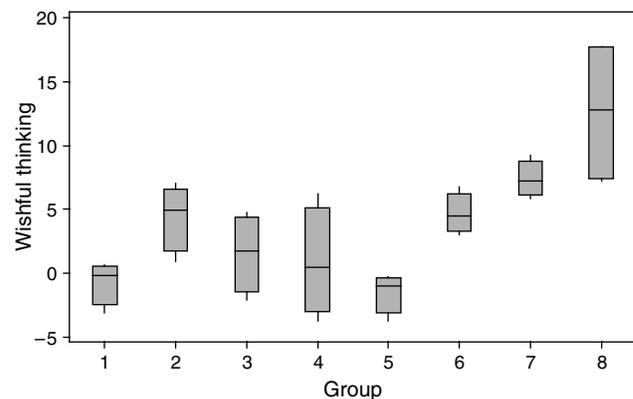
^aEstimate error is defined as (value estimate – actual value), averaged over all securities for each position. We winsorize this measure to reduce the influence of outliers. Our results are similar when we do not winsorize the data.

We measured individual degrees of wishful thinking by examining posttrade estimates of value, which were recorded after traders had an opportunity to draw inferences about other traders' signals from biased trading behavior (the mechanism we propose drives contagion). To test whether starting positions affected beliefs, we calculated for each of the 32 traders their estimate errors averaged over all 16 securities (calculated as estimate of value – actual value). We then computed the average of this measure separately for long and short positions, and report in Table 2 the average difference between these measures for each of the eight groups and the standard deviations both within and between groups.

To test for the presence of wishful thinking, we conducted an ANOVA with factors for group membership, position, and the interaction between group and position. The ANOVA showed a very strong effect for starting position ($F(1, 24) = 20.83, p = 0.0001$), indicating that long positions led to higher value estimates than short positions. Averaging this wishful thinking effect across the four participants in each group, six of the eight groups exhibited the bias. Thus, the individual estimates provide support for the wishful thinking component of Hypothesis 1.

2.2.3. Contagion of Wishful Thinking. If wishful thinking is contagious, as predicted by Hypothesis 2, we should see that the extent of wishful thinking exhibited by a trader was more similar to that exhibited by other traders in the same group than to that

Figure 3 Boxplot of Wishful Thinking by Group Membership



Notes. Wishful thinking: This box plot provides evidence concerning the contagious nature of wishful thinking in Study 1. Groups tend to cluster, suggesting that wishful thinking is transmitted to other traders through wishful betting behavior. If traders fully adjusted for wishful betting, within-group wishful thinking would be uncorrelated.

exhibited by traders in other groups. The boxplot in Figure 3 displays, for a single group, the range from the 25th and 75th percentiles (boxes), means (line inside each box), and maximum and minimum observations (line outside each box) of wishful thinking across all securities for each trader in that group. The plot reveals that traders tended to exhibit relatively small dispersion of wishful thinking magnitudes within each group, compared to the relatively large dispersion between groups. We examined this contagion effect more rigorously with several statistical analyses. First, the ANOVA showed a strong group by position interaction ($F(7, 24) = 6.72, p < 0.001$). This interaction implies that traders exhibited greater evidence of wishful thinking when other traders in their group did so. As shown in Table 2, the standard deviation of the eight group means was 5.44, compared to only 2.77 for the average standard deviation of the four traders within each group. The ANOVA provides one method for comparing these standard deviations, confirming the existence of a contagious element to wishful thinking in markets, as predicted in Hypothesis 2. Similarly, we performed a discriminant analysis with cross validation to determine whether traders could be properly classified into their respective groups based on the wishful thinking evident in their beliefs. The trader belief measure alone provided a significant discriminant model (Wilks' $\lambda = 0.22, F(7, 24) = 11.95, p < 0.0001$). This analysis correctly classified 41% of traders into their respective groups, which is significantly greater than the expected chance performance of 12.5% ($Z = 4.81, p < 0.0001$).

Finally, a more conservative but less frequently used method of analyzing within- and between-group differences is the WABA (within- and between-units analysis) test. This method involves comparing a

measure of within- and between-groups variance to determine whether the similarity of individuals within groups attains *practical* significance (see Dansereu et al. 1984 for a detailed discussion). The appropriate statistical test for the WABA analysis is the *E*-test of practical significance. The $p < 0.05$ level of significance for sample sizes greater than 30 holds when the *E*-statistic is greater than 1.73. For our data, $E = 1.87$, more than satisfying the standards for practical significance of our group-level effects. It thus appears that wishful thinking was a contagious effect in our markets. Note that support for contagion also guarantees that our posttrade evidence of wishful thinking is not simply a reflection of pretrade wishful thinking (which we do not test for directly), because the lack of pretrade communication would make pretrade beliefs uncorrelated across traders within each market.

Models of financial markets suggest that traders who are less susceptible to wishful thinking should trade actively to exploit others' errors, potentially causing market prices to be less influenced by wishful thinking than the average individual. Simple inspection of Tables 1 and 2 suggests that our markets imposed no such discipline: the average wishful thinking effect was \$3.63 at the individual level, whereas the average market-level wishful betting effect was \$4.20. Supplemental analyses showed that traders who exhibited greater wishful thinking traded more actively and lost more money. Additionally, traders and groups who engaged in greater wishful betting took on more risk as evidenced by greater variation in winnings across securities.

2.3. Discussion

The results of our first study are consistent with contagion of wishful thinking: wishful betting led traders to conclude that other traders possessed preference-consistent information, which in turn led to wishful thinking. However, the continuous nature of trade, as well as the absence of pretrade belief data, makes it difficult to confirm this explanation conclusively. To provide further evidence on how wishful betting leads to wishful thinking, we conducted a second experiment within the traditional card game paradigm used by Irwin (1953). The experiment used a very simple mechanism for conveying information about one group's betting and beliefs to a second group, which makes it far easier to assess how that information can influence individual behavior and market outcomes.

3. Study 2

In our second study, participants played a card game in which they bet on random card draws and recorded their beliefs about the probability of drawing

certain cards. Each of 12 decks of cards was composed of some proportion of red and black cards, and one card was drawn from each deck. We manipulated desirability within participants by providing bonus payments when a red or black card was drawn (the bonus color varied by deck). We also manipulated one variable between participants: the information about the proportion of red cards. Participants in the control condition learned that decks could be in one of three states. Prior to playing each deck, they were informed that it contained a range of either 20%–40% red cards, 40%–60% red cards, or 60%–80% red cards, following a uniform distribution. In three subsequent conditions, participants played the same game but did not learn which of the three states each deck fell into. Instead, they learned either the betting behavior (behavior condition), probability beliefs (belief condition), or betting behavior and beliefs (behavior-belief condition) of participants in the control condition.

The design of the card game mirrored our experimental market in several ways. First, the initial information about outcome probabilities was objective (a uniform distribution over a range of values). Second, participants who received only the betting behavior of prior participants faced a task similar to traders in our market (infer an accurate expected outcome based on the behavior of others). A card game also has two advantages over an experimental market. First, we can partition out initial wishful thinking in beliefs and behavior using the control group responses. Second, we can control whether subsequent participants have access to the control group's behaviors, beliefs, or both. To provide corroborating evidence for our theory of contagion, we expect that initial beliefs will be accurate whereas initial betting behavior will be biased. The following two hypotheses reflect these expectations (Hypothesis 4 is stated in null form):

HYPOTHESIS 3. *Participants in the control condition will exhibit wishful betting.*

HYPOTHESIS 4. *Participants in the control condition will not exhibit wishful thinking.*

If subsequent participants fail to adjust for wishful betting, then participants in the behavior condition should exhibit greater wishful betting and wishful thinking than the other groups. Participants in the belief and behavior-belief conditions should be able to avoid the contagion effect caused by the wishful betting, because they have access to the unbiased beliefs of prior participants. This expectation is formally stated in the following hypothesis:

HYPOTHESIS 5. *Participants provided only with betting behavior will exhibit stronger wishful betting and wishful thinking than participants provided with beliefs or both beliefs and betting behavior.*

3.1. Method

3.1.1. Participants. Seventy-four graduate and undergraduate students from a U.S. university participated in our experiment. They signed up to participate through a computerized administration system in return for performance-based monetary compensation. They spanned a variety of academic disciplines, but were all currently enrolled in one of several business courses.

3.1.2. Experimental Design. Participants were randomly assigned to one of four conditions created by manipulating the type of probability information provided between participants. The control, behavior, belief, and behavior-belief conditions received the information described in the following section. In all conditions, the participants' task was to estimate the probability of drawing a red card from each of 12 decks and to place bets on red or black cards to make money.

3.1.3. Card Decks and Information. The same 12 decks of cards were used for all participants. Four decks contained 30% red cards, 4 contained 50% red cards, and 4 contained 70% red cards (with the remainder of each deck composed of black cards). To allow for some ambiguity similar to our experimental market (and to Bar-Hillel and Budescu 1995), control condition participants were told for each deck whether it fell into a probability range of 20%–40%, 40%–60%, or 60%–80% red cards, following a uniform distribution and with the expected value being the midpoint of that range. For half of the decks, participants received a bonus payment of 50 cents if a red card was drawn, and for the other half they received a bonus if a black card was drawn (the within-participants desirability manipulation). Each bonus color occurred twice at each probability level, and the color order was reversed for half of the participants, creating a fully balanced design.⁶ As a result, an unbiased player would bet equal amounts of money on desirable and undesirable cards. Participants in three subsequent conditions faced the exact same decks and bonus payments, but were not told the probability range for each deck. Instead, they learned the betting behavior and/or beliefs of the control group. Participants in the behavior condition learned how many of the prior participants bet on black and how many bet on red. Participants in the belief condition learned the average probability of a red card assessed by prior participants. Participants in the behavior-belief condition learned both pieces of information.

3.1.4. Incentives and Betting. Participants began the study with a base payment of \$6 and were

informed that they could win additional bonus payments if the correct color card was drawn from a deck. The bonus payment was always 50 cents. In addition, they could bet up to 50 cents on either a red or black card, but betting was not required. Bets were recorded on a fill-in-the-blank betting sheet. If the bet was correct, the amount wagered would be added to the participant's final payment. If it was incorrect, they would lose the amount wagered. Total winnings could thus range from \$0 to \$18 (\$6 in possible bonuses, \$6 in possible betting winnings, and \$6 base payment).

3.1.5. Procedure. Participants entered the study and received written instructions detailing their incentives, betting procedure, bonus payments, and the potential probability ranges of the card decks. They also received a betting sheet on which they could circle a card color and write the number of cents they wished to bet on that color. Below the betting information for each deck, participants provided a point estimate of the probability of drawing a red card. The administrator answered any questions and then explained that the 12 decks of cards would be shuffled and drawn from sequentially, and the outcomes would be revealed at the end of the game. In the control condition, the administrator began with the first deck, stated the probability range of red cards, the card color that would result in a bonus payment, and then shuffled while participants wrote down their bet and belief about the probability of drawing a red card. The administrator then drew a card, placed it at the bottom of the deck, and returned the deck to the table. This procedure was repeated for all 12 decks. After the twelfth deck, the administrator collected participants' betting sheets and calculated their winnings based on the drawn cards. The procedure was identical for participants in the behavior, belief, and behavior-belief conditions, except that instead of reading out the probability range for each deck, the administrator read out either the number of prior participants who bet on each card color, prior participants' average belief about the probability of a red card, or both.

3.2. Results

3.2.1. Wishful Betting and Wishful Thinking. Table 3, panel A reports mean measures of wishful thinking and wishful betting, standard deviations, and cell sizes for each experimental condition. Hypothesis 3 stated that participants in the control condition would exhibit wishful betting, which we measured as the percent of money bet on desirable cards. This measure captured each participant's relative preference for desirable cards but controlled for overall participant differences in general risk preferences and willingness to bet. Because normative

⁶ Neither the order nor the individual bonus card color significantly affected participant behavior.

Table 3 Wishful Betting and Wishful Thinking

Panel A: Wishful betting and wishful thinking by condition					
Condition		Wishful betting		Wishful thinking	
Control		69%		−0.7%	
		(2.6)		(2.6)	
		[22]		[22]	
Belief		56.4%		−0.5%	
		(2.4)		(1.2)	
		[18]		[18]	
Behavior-belief		67.8%		0.2%	
		(2.2)		(2.2)	
		[16]		[16]	
Behavior		83.5%		5.5%	
		(1.2)		(4.7)	
		[18]		[18]	

Panel B: Detailed breakdown of betting behavior					
Condition	Card type	No. of participants betting more money on card type	Total no. of bets	No. of bets per participant	Average total bet (\$)
Control	Desirable	19	161	7.3	2.30
	Undesirable	3	75	3.4	1.12
Belief	Desirable	12	108	6.0	1.99
	Undesirable	6	94	5.2	1.76
Behavior-belief	Desirable	13	118	7.4	2.36
	Undesirable	3	51	3.2	1.00
Behavior	Desirable	18	139	7.7	2.85
	Undesirable	0	36	2.0	0.60

Notes. Panel A reports means, (standard deviations), and [cell sizes] of the wishful thinking and wishful betting by experimental condition in Study 2. Wishful betting is the percent of total money bet on desirable cards, and is in evidence when the measure is greater than 50%. Wishful thinking is the average probability estimate error in favor of drawing desirable cards, and is in evidence when the measure is greater than 0%. Panel B reports the number of participants who bet more money on each type of card (desirable and undesirable) total number of bets on each type of card, average number of bets per participant, and the average participant's total monetary bet on each type of card.

behavior would comprise an even split between bets on desirable and undesirable cards, wishful thinking is in evidence when more than 50% of a participant's wagered money was directed at desirable cards. On average, participants in the control condition bet 69% of their money on desirable cards, a strong wishful betting effect that confirms Hypothesis 3 ($t(21) = 4.58$, $p < 0.0001$).

Hypothesis 4 stated that participants in the control condition would not exhibit wishful thinking in their beliefs. Wishful thinking is measured by taking the average error in probability estimates in the direction of the desirable card. In other words, if a participant estimated a 75% chance of a red card when red was desirable and the true probability was 70%, the wishful thinking measure was +5%. If that estimate was instead 65%, the measure was −5%. Wishful thinking is in evidence if a participant's average belief error

was greater than zero. The average error in beliefs was −0.7%, which is not significantly different from zero ($t(21) = -1.21$, $p = 0.240$). If anything, participant beliefs were slightly pessimistic rather than consistent with wishful thinking, resulting in failure to reject Hypothesis 4. This pattern of results is consistent with prior research, demonstrating that wishful betting can occur even though beliefs may be accurate.

3.2.2. Contagion of Wishful Thinking. To test for contagion of wishful thinking, we examined the three additional conditions. Hypothesis 5 stated that participants provided only with betting behavior would exhibit stronger wishful betting and wishful thinking than participants provided with beliefs or both beliefs and betting behavior. To test this hypothesis, we first performed a one-way ANOVA, which demonstrated a highly significant effect of treatment on betting behavior ($F(1, 51) = 7.41$, $p = 0.001$) and beliefs ($F(1, 51) = 20.62$, $p < 0.0001$). An examination of means revealed that participants in the behavior condition bet an average of 83.5% of their money on desirable cards, whereas those in the behavior-belief condition bet 67.8% on desirable cards and those in the belief condition bet 56.4% on desirable cards. Planned contrasts revealed that wishful betting is higher in the behavior condition than the behavior-belief ($t(32) = 2.36$, $p = 0.013$) and belief ($t(34) = 4.22$, $p < 0.0001$) conditions. Additionally, wishful betting was higher in the behavior condition than in the control condition ($t(36) = 2.74$, $p = 0.005$), and neither the behavior-belief nor belief condition significantly differed from the control condition. With respect to beliefs, participants in the behavior condition exhibited an average probability estimate error of 5.5%, whereas those in the behavior-belief condition exhibited an error of 0.2% and those in the belief condition exhibited an error of −0.5%. Planned contrasts revealed that wishful thinking was higher in the behavior condition than the behavior-belief ($t(32) = 4.15$, $p < 0.001$) and belief ($t(34) = 5.40$, $p < 0.0001$) conditions. Additionally, wishful thinking was higher in the behavior condition than in the control condition ($t(36) = 5.25$, $p < 0.0001$), and neither the behavior-belief nor belief condition significantly differed from the control condition. This pattern of results strongly supports Hypothesis 5.

3.2.3. Frequency and Magnitude of Betting. Table 3, panel B reports the number of participants who bet more money on desirable and undesirable cards, the total number of bets on each type of card, the average number of bets per participant, and the average total amount that each participant bet on each type of card. Our results do not appear to be driven by a few participants who bet frequently on desirable cards. The leftmost column of panel B shows that all

participants in each condition placed bets, and that the majority of participants in each condition bet more money on desirable cards. Consistent with our main results, the behavior condition is the only setting in which *every* participant bet more money on desirable than undesirable cards. Other conditions also generally mirror our main results. Participants could place a total of 12 bets—one on each deck. In each condition, participants bet more than 75% of the time on average (at least 9 out of 12 times in all conditions). The lowest frequency of betting occurred in the behavior condition, but closer inspection reveals that this was primarily because of participants betting on undesirable cards less frequently. Participants could wager a total of \$6.00 across all 12 decks, and the average participant wagered more than half of this amount in each condition. As can be seen from the rightmost column of panel B, the amount of money wagered on desirable (undesirable) cards increased (decreased) across conditions in a manner consistent with the percentage scores in panel A. This pattern of betting behavior suggests that participants bet frequently and placed significant wagers, substantiating the practical significance of our central results.

3.3. Discussion

Study 2 provided additional evidence that wishful thinking can be contagious if participants fail to account for the bias inherent in others' betting behavior. Although we argued in our first experiment that initial beliefs would be accurate, we did not measure those beliefs. Study 2 showed that initial beliefs in a card game were accurate, but participants engaged in wishful betting. By transmitting betting behavior, beliefs, or both to subsequent participants, we found that wishful betting contaminates subsequent behavior and beliefs. Participants were able to adjust for this bias when they were provided with the beliefs of prior participants.

4. General Discussion

Although laboratory markets often mitigate individual biases, we provide evidence that wishful thinking can arise through market interaction even when it is not present in initial beliefs. Our first experiment suggested that these wishful thinking effects were "contagious," with traders in each group showing similar levels of wishful thinking because they could not parse out wishful betting from legitimate beliefs behind market behavior. Our second experiment provided step-by-step evidence of this phenomenon through a card game. Initial beliefs were accurate, but participants engaged in wishful betting (betting as though they possessed more favorable information). Transmitting this betting behavior to subsequent participants led to greater wishful betting and wishful

thinking. It is notable that of the four groups in our second experiment, the group receiving prior traders' behavior was the only one to exhibit significant wishful thinking. Providing subsequent participants with both behavior and beliefs or just beliefs mitigated the contagion problem, but this solution is not viable in many real-world contexts.

Although we observed wishful betting in our studies, our experimental designs did not directly assess the psychological processes underlying this behavior. People may be unaware of their own tendency to bet on desirable outcomes, so that wishful betting is driven by the same factors that drive general motivated reasoning phenomena. Alternatively, people may superstitiously believe that they should not bet against their own potential favorable outcomes. In this case, markets in which hedging behavior is the norm may not exhibit wishful thinking. Wishful betting may also be driven by factors we have not yet identified. Because neither this paper nor prior work conclusively solves this puzzle, future research could contribute to the wishful thinking and wishful betting literature by including additional process measures in their experimental designs.

Our laboratory results suggest that wishful thinking could be a problem in real-world financial markets. Wishful betting could (though does not necessarily) contribute to investors' apparent tendency to overvalue stock of local companies (Coval and Moskowitz 2001, Hau 2001) because they already have many reasons to hope that firms in their vicinity perform well. Wishful betting could also contribute to employers' tendency to invest heavily in their employer's stock, despite the obvious lack of diversification (Benartzi 2001). Finally, the contagious nature of wishful thinking in markets may lead to speculative bubbles or information mirages (Camerer and Weigelt 1991, Schnitzlein 1996). Future research could investigate the contribution of wishful thinking to previously documented stock market anomalies by examining factors related to the magnitude of wishful thinking, such as investor mood (e.g., Edmans et al. 2007).

The ways in which wishful thinking and wishful betting propagate through markets, and the magnitude of those effects, will depend on the information environment. Ledyard (2006) notes that markets provide more accurate predictions when they are populated with many active traders who possess small bits of information. Because our markets included only a handful of traders, and each trader held half of the available information (which is quite a lot), Ledyard's perspective would suggest that wishful betting and wishful thinking could be stronger in our experiments than in "thicker" markets. However, the reverse outcome also seems possible: In markets with many traders who each hold a small bit of information,

each trader must rely heavily on their inferences from observed behavior in order to estimate asset values. As a result, wishful betting could have an even stronger effect on estimates than in our setting, simply because traders have more to learn from others.

Trading rules can also influence the propagation of wishful betting and wishful thinking. Future research might compare results in specialist markets such as ours to those in double-auction markets. Equilibrium prices in specialist markets evolve as traders buy from and sell to the specialist, resulting in opportunities for wishful betting. However, equilibrium prices in double auctions evolve as traders post buy and sell offers that convey information but are only rarely accepted. To the extent that double-auction markets reduce trading, they also reduce wishful betting, which may also mitigate wishful thinking.⁷

Contagion of wishful thinking could also be a problem in a variety of managerial and social contexts. For example, companies hoping to achieve profits by entering new industries may be unduly influenced by observing investment decisions of their competitors. Aspiring musicians, actors, and athletes could collectively overestimate their chances of success by observing others' investments of time, energy, and money into such careers. Wishful thinking might also be problematic in the "prediction markets" that have recently been used for business and political predictions (Forsythe et al. 1992, Oliven and Rietz 2004, Wolfers and Zitzewitz 2004, Hahn and Tetlock 2006, Russ 2006), especially when all traders are likely to have similar preferences for outcomes (as would be likely in Hewlett-Packard's predictions of future sales, for example). Again, because of differences between these environments and the market mechanism used in this paper, future research will be required to establish the robustness of our results across other settings.

5. Electronic Companion

An electronic companion to this paper is available as part of the online version that can be found at <http://mansci.journal.informs.org/>.

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⁷In theory, no trade would ever occur in a double-auction market with the information distribution used in our experiment. However, experiments consistently show significant levels of trading volume and speculative investment in double auctions (Hales 2009), although likely less trade than would be observed in a specialist market.

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