Rationality spillovers

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Abstract

We design an experiment to test whether the rationality that is induced by market-like discipline spills over to nonmarket valuation settings—a rationality spillover. Our results confirm that this new phenomenon exists. The rationality stimulated by market-like discipline extends to the nonmarket setting, and these spillover effects are robust even when the nonmarket setting involves hypothetical choices and environmental lotteries. We observe that people stop reversing their preferences for lotteries by revising downward their stated values to buy and sell high-risk lotteries; they do not change their preference ordering.

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1. Introduction

The concept of rationality belongs to economics. Commentators within and outside the discipline treat the two as synonymous. Though a good deal of parsing is done, rationality simply amounts to doing the best one can with the resources one has or expects to have. The parsing revolves around what constitutes "the best" and how perceptions of it evolve, and the role of social relationships in the definition of "resources". Constraints on a person's cognition and on his computational skills limit his ability to achieve the best, e.g., [30,51]. These internal constraints imply that rationality is a scarce commodity, which he must somehow ration.

But the burden of this scarcity is reduced and even eliminated if he has access to the resources of a market to help him construct his rationality rather than having to rely only on his internal resources [1,28]. Barring new entrants, markets diffuse rationality through the population by means of accidental or purposive imitation of and selection for rational behaviors [7]. Rationality is then an institutional or social phenomenon rather than an individual phenomenon (see for example [2,23,55]).

Critics, nevertheless, persist in two complaints about rational choice. The first holds that a person's preferences change with the context, individual or social [6,13]. An observer therefore has difficulty telling whether a better alignment of preferences to choice is due to a change in preferences or to a relaxation of internal constraints (i.e., refining stated values for a good). A second complaint grants the consistency of preferences and actions in market settings but is dubious of extending rational choice to the decisions made by socially isolated people [31]. These nonmarket choices purportedly do not answer to the disciplined beat of the rationality drummer: they are autonomous.

Herein we subject the context-dependent preference and nonmarket autonomy complaints to empirical test. We report results of experiments designed to test whether the rationality that markets induce can spill over to nonmarket settings, and whether any spillovers that occur are due to changes in preference or to revisions in stated values. We use the classic preference reversal phenomenon as the case study. Our results suggest *rationality spillovers* exist. Market and nonmarket choices are not autonomous. Once a person's rationality is induced in a market-like setting, he or she can transfer that rationality to nonmarket settings. Further, results indicate that preferences are not context dependent. People gain rationality by revising their stated valuations of low-probability/high-payoff lotteries; they do not change their preferences for these lotteries.

These findings arise from our initial experimental treatment that purposely created conditions most favorable for rationality spillovers. Market-like and nonmarket settings were identical except for the arbitrage condition—people made real choices over monetary lotteries. If rationality spillovers did not emerge in these hospitable circumstances, we would question whether the phenomenon could ever exist in more hostile environs like contingent valuation surveys [45]. Such surveys presume rational choices over hypothetical changes in risks to nonmarket goods like the environment (see [21]). But this presumption is subject to challenge for two reasons. First, the market discipline that induces rationality is absent, and environmental risks like climate change and biodiversity loss are low-probability/high-severity events—both conditions create an inviting context for anomalous behavior [16,60].[1] Critics have therefore argued that new behavioral theories are needed for nonmarket valuation to account for systematic deviations from rationality [32,33].

But before asking nonmarket valuation research to abandon rationality completely, we extend our initial treatment to consider whether rationality spillovers remain robust when we weaken the link between the market-like and nonmarket choice. We test robustness by keeping the

[1] Numerous risk perception studies have revealed that people commonly overestimate the chance that they will suffer from a low-probability/high-severity event, e.g., a nuclear power accident or climate change catastrophe like a shift in the Gulf stream (see e.g. [38]). When the outcome is potentially very bad, people inflate the chance that the outcome will be realized. People often use a heuristic in which they deal with probabilities and severity additively rather than in combination as expected utility presumes (see [42]). In addition, experience tells people little about low-probability risks like climate change. They must rely on outside sources of information to help them make judgments about the likelihood that a bad event will actually come to pass. And if that outside information stresses severity without giving some notion of the odds, people systematically bias their risk perceptions upward. Policymakers are not immune to this human fallibility either.

market-like conditions constant, while making the nonmarket choices hypothetical with the options being uncertain wildlife experiences. We find that rationality spillovers continue to be robust in this contingent valuation context—people achieved rationality in the nonmarket setting despite the lack of arbitrage, the nonbinding choices, and the nonmonetary, wildlife lotteries. And we again find that people gain rationality through refining values, not changing preferences. This suggests that while people in the typical one-shot survey might overstate their values for low-probability/high-severity environmental risks, with arbitrage-like feedback they can make their stated values more consistent with their preferences.

2. Preference reversals and rationality spillovers

We explore this idea of rationality spillovers in the context of the classic irrationality labeled as preference reversals.[2] Preference reversals provide an excellent basis to explore rationality spillovers because the phenomenon has been shown to persist in isolation and to diminish with arbitrage [9,18]. When facing two lotteries with similar expected values but different levels of risk, people frequently reverse their preferences by choosing the *low-risk lottery* (high probability–low payoff) but placing a higher value on the least preferred, *high-risk lottery* (low probability–high payoff).[3] This inconsistent behavior may arise from misjudging preferences or valuations or both. And this behavior creates opportunities for others to extract gains through exchange. Inconsistencies are reduced or eliminated when the arbitrage provided within an exchange institution that makes the person learn to maximize the surplus he captures.

The two open questions we explore are: (a) whether rationality spillovers exist—an indirect effect from arbitrage that induces rational behavior in a secondary institution which lacks arbitrage; and (b) what underlying behavior adjustments are made that lead to more rational behavior—when arbitrage induces rationality spillovers, do people adjust preferences, valuations or both? We develop a series of testable hypotheses to address these questions.

To begin, we provide a benchmark test to calibrate our results with earlier work. The *direct rationality hypothesis* tests whether arbitrage in an institution directly induces rationality within the same institution. That highly arbitraged, market-like settings possess great power to induce rational behavior is not news. We know other people will often take advantage of a person if he engages in irrational behaviors in the arbitrage setting. Given that he values separately each of any series of bets he makes [40], this Dutch book scenario can turn the irrational person in an arbitrage setting into a money pump who will either go broke or come to see that it is not smart to be irrational in the sense of not trying to maximize expected utility [47]. Controlled experiments

[2] The preference reversal phenomenon contradicts the presumption that elicited preferences should be invariant to the elicitation method (see [37,56,58]). See for example the overviews on procedural invariance and preference reversals by Camerer [16] and Tammi [57]. Evidence shows a robust inconsistency in an isolated person's preference orderings and expressed valuations despite inducements like greater rewards, different presentations, training, record keeping, and a hypothetical setting (see [26,39,43,46]).

[3] The study of preference reversals is especially relevant for us because we are also interested in the management of environmental risks like climate change and biodiversity loss, which are low-probability/high-severity events (see [54]).

have often demonstrated the power of arbitrage.[4] If our results are consistent with previous experiments, we expect to support the:

Direct rationality hypothesis—a person's initial irrationality in a market institution is reduced in the presence of arbitrage in the same market institution.

Previous demonstrations of arbitrage-induced rationality have, however, been limited to stand-alone settings. The possibility that the rationality induced in one setting can spill over to another setting has escaped scrutiny.[5] The key idea in our story is that an isolated person makes his choices conditional on the degree of rationality he earlier achieved when his choices were susceptible to arbitrage. We ask whether the weight of prior arbitrage can help him ease the burden of his isolation. Can he build habits of mind to envision and to act upon opportunities to extract surpluses in nonmarket settings? Just as an individual's rationality need not have a role in achieving collective rationality, rationality spillovers to nonmarket from arbitraged settings can be undesigned—a lengthened invisible hand.[6] Nevertheless, rationality spillovers from arbitrage must *ex post* cause a person's cognition and computational skill to have a role in what he can achieve in nonmarket settings. Because no one is around to press him, the isolated person must draw upon his internal resources if he is to use any lessons in rationality gained from his arbitrage experiences. Therefore, we test the:

Rationality spillover hypothesis—a person's irrationality in a nonmarket institution is reduced by the presence of arbitrage in a parallel market institution.

If rationality spillovers are found to exist, the question then is whether people adjust their preference ordering or their valuation statements to reverse their preference reversal. Further, we ask whether the type of lottery a person confronts—low-risk or high-risk lotteries—influences these adjustments. Three nonmutually exclusive hypotheses are tested:

Preference adjustment hypothesis—irrationality is corrected by adjusting preference orderings.

Valuation adjustment hypothesis for low-risk lotteries—irrationality is corrected by adjusting stated valuations of the high-probability/low-severity lottery.

Valuation adjustment hypothesis for high-risk lotteries—irrationality is corrected by adjusting stated valuations of the low-probability/high-severity lottery.

3. Experimental design

One hundred and sixty-six subjects were recruited from the undergraduate student body at the University of Wyoming during the 1998 spring semester. The experiment consisted of 12 sessions of four treatments; each treatment had 41 or 43 participants with 8–13 subjects in each session. After entering the lab, participants signed a consent form acknowledging their voluntary

[4] For representative empirical demonstrations of the power of arbitrage to induce rational behavior in single, not multiple, settings, see [3,10,15,19,23,27,53,59].

[5] An analytical framework for the rationality spillover hypothesis, and the formal statements of the associated experimental hypotheses are available from the authors on request.

[6] An alternative perspective would permit differences in arbitrage settings between instantaneous preference satisfaction and choices, thus allowing these choices to be premised on what they would do for the person's rationality in a subsequent nonmarket setting. This potentially leads to an infinite regress since the question of how the person determines these joint choices must be answered.

Table 1
Description of treatments

	Market	Nonmarket
Treatment 1	Real choice No arbitrage Money lottery	Real choice No arbitrage Money lottery
Treatment 2	Real choice Arbitrage Money lottery	Real choice No arbitrage Money lottery
Treatment 3	Real choice Arbitrage Money lottery	Hypothetical choice No arbitrage Money lottery
Treatment 4	Real choice Arbitrage Money lottery	Hypothetical choice No arbitrage Environmental lottery

participation while agreeing to abide by the instructions. Written protocols ensured uniformity across sessions, and all subjects were inexperienced with preference reversal experiments.[7] As a guide, Table 1 summarizes the experimental design.

3.1. *Treatment 1—the no-arbitrage baseline*

Treatment 1 is our baseline in which subjects make binding choices over money lotteries and preference reversals are uncontested by arbitrage. The design used a computer program to simulate two simultaneous but independently operated institutions—a *market-like choice* with arbitrage, and a *nonmarket choice* without arbitrage. Each round followed a nine-step process.

Step 1: Each subject was presented with two pairs of lotteries—one pair in the market-like setting, the other pair in the nonmarket setting. In each pair, the two lotteries had similar expected values, with a type A-lottery being low risk and type B-lottery being high risk. Subjects were endowed with $10.00 as a beginning balance in each setting. Fig. 1 shows the computer screen presented to the subjects.

Step 2: We elicited preferences over the pair of lotteries in the market-like setting by asking each person ‘‘which [lottery] option do you prefer, A or B?’’

Step 3: We then elicited preferences for the second lottery pair in the nonmarket setting with the same question.

Step 4: We elicited values for each lottery in the market-like setting by asking the subjects ‘‘what is your fair value for [lottery] option A (B)?’’

[7] The protocol included: randomly seating the subjects as they entered the room, disallowing any communication whatsoever among subjects, reading the experimental instructions aloud as the subjects followed along, administering a test of comprehension, addressing any questions or concerns raised by the subjects, and conducting the market sessions. Experimental instructions are available from the authors on request.

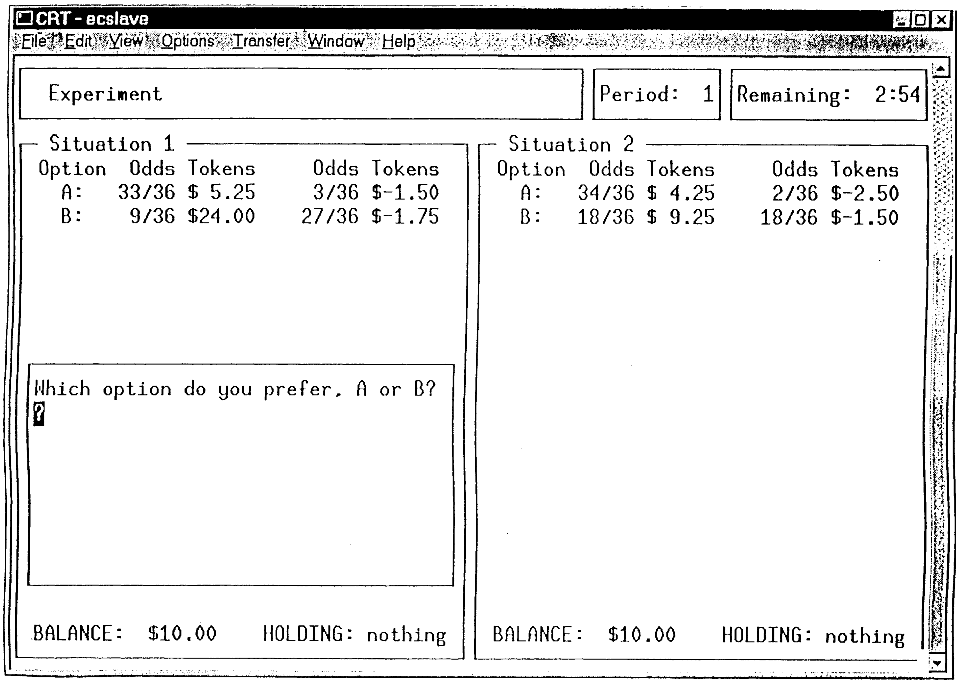

Fig. 1. Example of experimental computer screen.

Step 5: We then elicited values for each lottery in the nonmarket setting by asking the same valuation question. Note that stated values could not exceed the $10.00 endowment, thereby creating a real budget constraint. The experimental instructions indicated that preferences and valuations were binding contracts that subjects could be asked to fulfill. Thus, subjects might have had to trade their least preferred lottery for their most preferred, and to buy or sell a lottery for their stated value. Subjects had 3 minutes to complete Steps 2–5. A clock was provided for them on the computer screen.

Step 6: After establishing preferences and valuations, an offer price was randomly drawn for each lottery in the market institution. A subject was sold a lottery if his or her valuation of that lottery was not less than the offer price. If a lottery was purchased, the purchase price was subtracted from the subject's money balance, and he or she owned the lottery. Outcomes of the lotteries were then determined by a random draw with money balances adjusted according to any winnings or losses.[8]

Step 7: We repeated this procedure in the nonmarket setting.

[8] These procedures follow Grether and Plott [26] and Chu and Chu [18] in which preferences over lotteries are obtained and valuations were elicited using a variation of the BDM [8] pricing procedure—in which rational expected-utility maximizing people should truthfully reveal their values and people do not reverse preferences. Thus the procedure assumes buying and selling prices to be equal, which imposes constant absolute risk aversion on the subjects. Berg et al. [9] observed that behavior was statistically equivalent regardless of whether one imposed the restriction of constant absolute risk aversion or not.

Step 8: We determined a subject's ending money balance for the round in each setting. The money balance equaled his beginning balance ($10.00) less the amount he spent on purchasing a lottery, plus or minus any of his winnings or losses from playing the owned lottery. In the event that no lotteries were purchased (indicated values were less than the offer price), the round ended with no change in his or her money balance.

Step 9: We repeated Steps 1–8 until 15 rounds were completed. The end round was unknown to subjects. Subjects faced a different pair of lotteries across rounds and between settings. For each round, one of 15 lottery pairs was randomly drawn for each setting given two restrictions: (i) each lottery pair was used only once per setting; and (ii) the same lottery pair could not be used in both settings in the same round.[9] Ending balances for each round were added to total payoffs. We then reset each subject's initial endowment to $10 for the next round.

This baseline treatment provides a behavioral benchmark in which stated preferences and values are *real* (i.e., they influence take-home pay); lotteries are in terms of *money*; and preference reversals are *not arbitraged*. We now vary these features to construct three additional treatments to test our hypotheses.

3.2. Treatment 2—introducing arbitrage and rationality spillovers

Treatment 2 is constructed to test the rationality spillover hypothesis. As in the baseline, subjects made *real* choices over *money* lotteries, but unlike the baseline, preference reversals are *arbitraged* in the market-like setting after round 5. The first five rounds of treatment 2 were identical to the baseline for comparison purposes. Beginning in round 6, preference reversals were arbitraged in the market-like setting, but not in the nonmarket setting. As such, treatment 2 only differed from the baseline in rounds 6–15 by contesting irrational behavior in the market-like setting. This design highlights the direct rationality effects in the market setting, which could spill over to the nonmarket setting.

In each round, subjects first indicated preferences and valuations in the market-like and nonmarket settings as in the baseline treatment. Lotteries were exchanged and played in both settings according to the procedures laid out in the baseline with one exception—subjects were arbitraged if preferences were reversed in the market-like setting after round 5. For example, suppose a subject preferred lottery A over B but stated a value of $3.00 for A and $5.50 for B. The simulated market sold lottery B to the subject by presenting him the following message, "The market sells you B for your fair value of $5.50." The subject's money balance in that market decreased accordingly to $4.50 (= $10–$5.50) and the subject owned lottery B. The market then acted again with the following message, "The market trades you A for B." At this point, the subject owned lottery A and had a money balance of $4.50. The market then concluded the arbitrage procedure with the message, "The market buys lottery A for your fair value of $3.00." Now the subject owned no lotteries and had an ending money balance of $7.50; the beginning balance minus $2.50.

The simulated market thus engaged each subject, and left him with a hole in his pocket as the price of his irrational behavior. In subsequent rounds, reversal rates in the market setting indicated whether the person adjusted behavior due to this market discipline—the direct

[9] The lottery pairs used in the treatments are available on request.

rationality hypothesis. If so, reversal rates in the nonmarket setting would then show whether this induced rationality spilled over to the nonarbitraged setting—the rationality spillover hypothesis.

3.3. Treatments 3 and 4—testing the robustness of rationality spillovers

Treatments 3 and 4 were designed to test the robustness of the rationality spillover hypothesis by weakening the symmetry between the market-like and the nonmarket settings. In both treatments, the market setting was identical to treatment 2—subjects made *real* choices over *monetary* lotteries and preference reversals were *arbitraged* after round 5.

In treatment 3, the nonmarket setting involved *hypothetical* choices over *monetary* lotteries. Each subject had no monetary endowment and they did not exchange or play lotteries. Accordingly, they did not face a binding budget constraint and their stated preferences and values were uncontested.[10] Reversal rates in the nonmarket setting indicate whether rationality spillovers persist even when the decisions are hypothetical rather than real.

In treatment 4, we further relaxed the link between the market and nonmarket settings by having subjects make *hypothetical* choices over *environmental* lotteries. The environmental lotteries depict nonmonetary states of nature rather than monetary wins and losses. As with monetary lotteries, a subject chooses between two environmental lotteries, and then states his fair price for each lottery. For example, a person could be asked to choose between lottery A—seeing a grizzly bear with 30 percent likelihood, and lottery B—catching a cutthroat trout with 67 percent probability. In all, we constructed 15 environmental lotteries on five events in Yellowstone National Park: seeing a grizzly, seeing a bird of prey (e.g., eagle or osprey), catching a lake trout in Yellowstone Lake, catching a cutthroat trout, and visiting the core attractions (e.g., old faithful). The hypothetical–environmental lottery reversal rates further tests the boundary of the rationality spillover phenomenon in light of evidence that preference reversals exist in the nonmarket valuation of many low-probability/high-severity environmental risks (see [14,32,41]).

4. Results and discussion

This section first reports empirical findings regarding the *direct rationality hypothesis*—the effect of arbitrage within the market-like setting. We then discuss results on the rationality spillover hypothesis—the impact of arbitrage on nonmarket choice. The section ends by reporting findings on the behavior adjustment hypotheses—the increased rational behavior arise from a change in preferences or a change in stated values.

4.1. Direct rationality effects from arbitrage

Result 1. Consistent with the direct rationality hypothesis, preference reversal rates are significantly reduced when arbitrage is introduced in a real market setting.

[10] Past experimental evidence has revealed a *hypothetical bias*, in which behavior in hypothetical settings often diverges from what people actually do when confronting real economic commitments. See for example [24], and the citations therein.

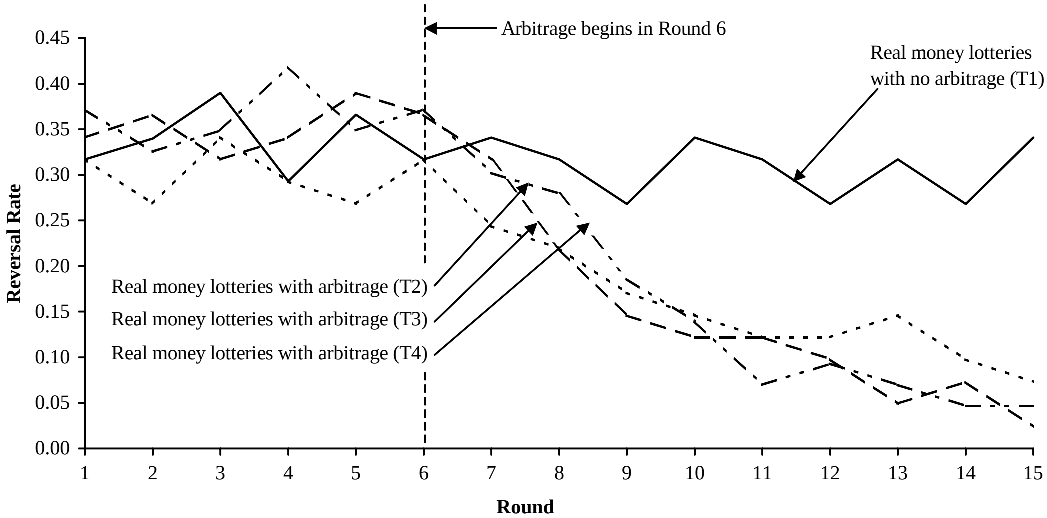

Fig. 2. Preference reversal rates in the market setting.

Fig. 2 shows that arbitrage directly induces rational behavior. The figure presents the rate of preference reversals in the real market setting over the 15 rounds for all four treatments. The baseline treatment 1 reveals the nonarbitraged reversal rate to be about 33 percent. This rate persisted over the 15 rounds. Treatments 2–4 introduce arbitrage after round 5. Prior to arbitrage, the reversal rates coincide with the baseline, approximately 33 percent. The incidence of reversals decreases dramatically once arbitrage is introduced—rates fall below 20 percent after three rounds of arbitrage, below 12 percent after six rounds, and approach 5 percent in the final round.

Tests of proportions across treatments provide unconditional support for the existence of direct rationality effects from arbitrage. Table 2 presents the reversal rates in the market setting across rounds and treatments. Prior to arbitrage, reversal rates across the treatments were not significantly different at any standard level with p-statistics ranging from –1.20 to 0.95. After four rounds of arbitrage, reversal rates were significantly lower in the arbitrage treatments relative to the no-arbitrage baseline (p-values <0.020). In the last rounds, the direct rationality effects from arbitrage are highly significant across all treatments (p-values <0.001).

We complement the "between" analysis by performing a conditional "within" treatment analysis. We estimate each treatment separately with Chamberlain's [17] logit model for panel data:[11]

$$\text{Prob}(y_{it} = 1) = \frac{\exp(z_{it})}{[1 + \exp(z_{it})]}, \quad i = 1, 2, \ldots, N; \;\; t = 6, 7, \ldots, T, \tag{1}$$

[11] While determining the appropriate effects specification (fixed or random) in standard models is straightforward, how to do so with limited dependent variable models is less clear. We opt to report results for Chamberlain's [17] logit fixed effects model. The alternative is a probit random effects specification. Our results remain unchanged across the two procedures.

Table 2
Reversal rates for the market and nonmarket settings relative to the baseline

	Round														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Market															
Treatment 1	0.317	0.0340	0.390	0.293	0.366	0.317	0.341	0.317	0.268	0.341	0.317	0.268	0.317	0.268	0.341
Treatment 2	0.341	0.366	0.317	0.341	0.390	0.366	0.317	0.219	0.146*	0.122***	0.122**	0.098**	0.049***	0.073***	0.024***
Treatment 3	0.317	0.268	0.342	0.293	0.268	0.317	0.244	0.220	0.171	0.146**	0.122**	0.122**	0.146**	0.098**	0.073***
Treatment 4	0.372	0.326	0.349	0.419	0.349	0.372	0.302	0.279	0.186	0.140**	0.070***	0.093**	0.070***	0.047***	0.047***
Nonmarket															
Treatment 1	0.317	0.366	0.341	0.390	0.293	0.317	0.390	0.341	0.415	0.341	0.366	0.317	0.390	0.366	0.340
Treatment 2	0.317	0.317	0.293	0.366	0.317	0.341	0.310	0.244	0.244	0.220	0.170**	0.122**	0.090***	0.090***	0.049***
Treatment 3	0.366	0.341	0.390	0.341	0.390	0.317	0.366	0.317	0.244**	0.190*	0.190**	0.170*	0.122***	0.090***	0.120***
Treatment 4	0.256	0.302	0.279	0.326	0.256	0.279	0.326	0.279	0.302	0.279**	0.233*	0.140**	0.163***	0.116***	0.116***

*, ** and *** indicate significance at the 10, 5 and 1 percent levels with the null being reversal rates in the arbitrage treatment (2–4) is equal to the rates in the non-arbitrage baseline (treatment 1).

where $z_{it} = \phi_t + \omega_i$. With this specification, y_{it} is an indicator variable that takes on a value of 1 if preferences are reversed and 0 if preferences are consistent. Important unobserved subject attributes such as risk preferences and propensity to reverse preferences are captured by individual effects, ω_i. Of special interest are the estimated time effects, ϕ_t, a set of $T-1$ dummy variables referring to rounds which indicate whether the likelihood of irrational behavior changes over time. This specification allows for flexibility in behavior adjustments, i.e., linear, quadratic, cubic or any other shape the data dictate.[12]

Table 3 presents the estimated coefficients across treatments and market settings for the model of preference reversals.[13] These results illustrate how the presence of arbitrage directly affects irrational behavior. In the nonarbitrage baseline (treatment 1), we cannot reject the null hypothesis that the incidence of inconsistent choices and valuations is unaffected by time effects in the market ($\chi^2(9) = 1.68$). Without arbitrage, individuals do not significantly deviate from initial levels of reversals—irrationality persists. In the arbitrage treatments (2–4), we do reject the null that time effects, or the feedback from arbitrage, does not influence the likelihood of reversing preferences with Chi-square statistics of 37.97, 16.69 and 42.80. Though estimated coefficients are not marginal effects, the negative signs within the arbitrage treatments correspond to the thickening of rationality in the presence of arbitrage, which is depicted in Fig. 2. While facing arbitrage, people adjusted behavior to eliminate inconsistent choices and valuations—rationality overtook irrationality. And estimates of individual time effects reveal that induced rationality began to surface at significantly higher levels (relative to initial round) after only three exchanges in the contested markets. Subsequent rounds show preference reversals continued to decline as the subject received more feedback through arbitrage. Conditional results from the real market setting thus provide strong evidence that arbitrage induces more rational behavior within an institution—rejecting the null and supporting the direct rationality hypothesis.

As expected, our results confirm Chu and Chu's [18] money pump treatments in which subjects stopped reversing preferences within two or three rounds after being arbitraged. Experience with costly mistakes teaches people that rationality pays.[14] The open issue is, as stated by Camerer [16, p. 661], "there is no evidence of whether subjects who are disciplined this way then learn to express preferences more consistently in the future." We turn to this evidence next.

4.2. *Rationality spillovers*

Result 2. Reversal rates in the nonmarket setting are significantly reduced when arbitrage is present in the market setting; therefore, we reject the null and support the rationality spillover hypothesis.

[12] Specific lottery effects are not included because it is the relative, not absolute, probabilities and payoffs within the framework that lead to preference reversals. Chi-square tests confirm this reasoning with lottery effects being insignificant.

[13] Models were estimated using data from the rounds that had variations across treatments—rounds 6–15. Note that it is impossible to calculate the marginal effects because the fixed effects are unrecoverable after being conditioned out during estimation.

[14] In general, the results support the view that economic rationality is a social construct in which decisions should be understood not in isolation, but within the context of the relevant exchange institution (e.g., [11,12,34]). Preference reversals are less likely with repetition. Experience with arbitrage mechanisms serves as a disinterested psychological "consistency check" (see e.g. [5]).

Table 3
Logit fixed effects results: preference reversal

	Treatment 1		Treatment 2		Treatment 3		Treatment 4	
	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket
Round 7	0.1148	0.3219	−0.2220	−0.1236	−0.4362	0.2397	−0.3542	0.2716
	(0.4793)	(0.4646)	(0.4720)	(0.4975)	(0.5422)	(0.4903)	(0.4876)	(0.5221)
Round 8	0.0001	0.1110	−0.7396	−0.5375	−0.6011	0.0001	−0.4818	0.0001
	(0.4831)	(0.4713)	(0.5070)	(0.5226)	(0.5539)	(0.4995)	(0.4940)	(0.5337)
Round 9	−0.2433	0.4232	−1.2684**	−0.5375	−0.9746*	−0.4044	−1.0696**	0.1389
	(0.4942)	(0.4622)	(0.5674)	(0.5226)	(0.5860)	(0.5217)	(0.5350)	(0.5274)
Round 10	0.1148	0.1110	−1.5011**	−0.6942	−1.1923**	−0.7209	−1.4460**	0.1389
	(0.4793)	(0.4713)	(0.6029)	(0.5342)	(0.6090)	(0.5455)	(0.5741)	(0.5274)
Round 11	0.0001	0.2180	−1.5011**	−1.0492*	−1.4407**	−0.7209	−2.2708***	−0.3018
	(0.4831)	(0.4676)	(0.6029)	(0.5656)	(0.6397)	(0.5455)	(0.7078)	(0.5506)
Round 12	−0.2433	0.0001	−1.7845***	−1.4905**	−1.4407**	−0.9009	−1.9396***	−1.0619*
	(0.4942)	(0.4759)	(0.6537)	(0.6169)	(0.6397)	(0.5620)	(0.6449)	(0.6143)
Round 13	0.0001	0.3219	−2.6509***	−1.7660***	−1.1923**	−1.3292**	−2.2708***	−0.8440
	(0.4831)	(0.4646)	(0.8593)	(0.6577)	(0.6090	(0.6118)	(0.7078)	(0.5926)
Round 14	−0.2433	0.2180	−2.1484***	−1.7660***	−1.7331**	−1.5976**	−2.7192***	−1.3085**
	(0.4942)	(0.4676)	(0.7311)	(0.6577)	(0.6824)	(0.6519)	(0.8183)	(0.6428)
Round 15	0.1148	0.1110	−3.4578***	−2.5621***	−2.0934***	−1.3292**	−2.7192***	−1.3085**
	(0.4793)	(0.4713)	(1.1317)	(0.8285)	(0.7464)	(0.6118)	(0.8183)	(0.6428)
$\chi^2(\phi_t = 0)$	1.68	1.71	37.97	27.22	16.69	20.90	42.80	19.51
(p-value)	(0.9956)	(0.9953)	(0.0001)	(0.0013)	(0.0539)	(0.0131)	(0.0001)	(0.0212)
N^a	380	410	330	350	300	360	330	350

Standard error in parentheses unless otherwise noted.
*, **, and *** indicates significance at the 10, 5 and 1 percent level.
[a] Numbers of observations were 410 for treatments 1–3 (41 subjects over 10 rounds) and 430 for treatment 4 (43 subjects over 10 rounds)—the reported number accounts for those dropped during the estimation process because of invariance in the dependent variable.

Rationality spillovers exist and are robust across various settings, which lack institutional discipline. Fig. 3 presents the reversal rates in the nonmarket setting across the four treatments. In each case, the incidences of reversals are statistically equivalent in the initial five rounds with rates hovering around 33 percent.[15] Once arbitrage is introduced in the market-like setting of treatments 2–4, the incidence of preference reversals in nonmarket choices diminished significantly in each case. Reversal rates decreased to approximately 20 percent after five rounds of arbitrage

[15] While not a problem with the analysis, we note that reversal rates for the environmental lotteries are consistently lower than those observed with the money lotteries. This likely arose due to the challenge of constructing a pair of environmental lotteries that fit the preference reversal paradigm—two lotteries possessing similar expected values with one being characterized as high probability–low payoff and the other being low probability–high payoff. Pilot surveys assisted in the selection of appropriate environmental lottery pairs, but nevertheless, the reversal rates appear different than those observed with money lotteries.

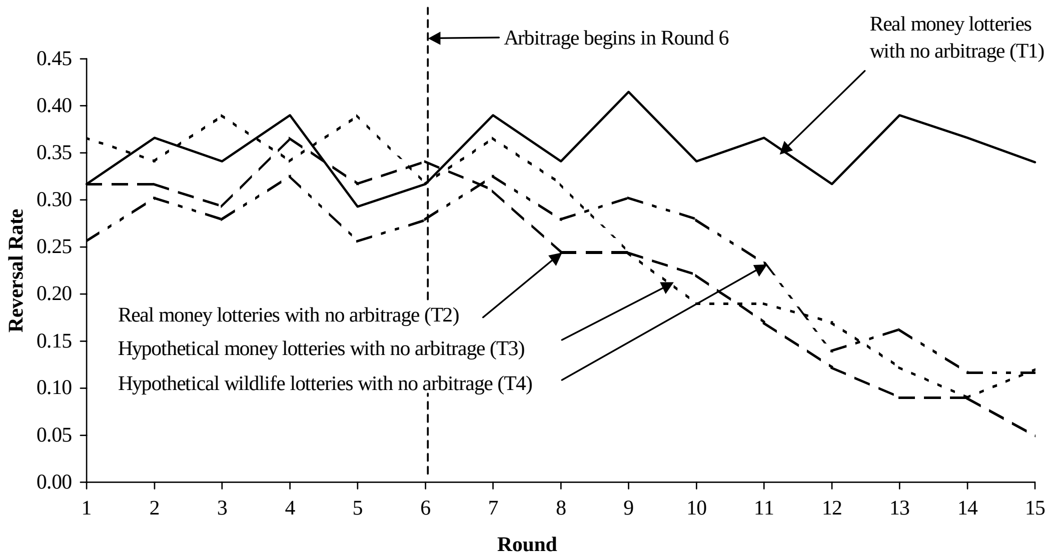

Fig. 3. Preference reversal rates in the nonmarket setting.

and 10 percent after nine rounds. Rationality spillovers remained robust in the hypothetical and the environmental lottery treatments.

Tests of proportions across treatments provide evidence regarding the significance of rationality spillovers. Table 2 presents the reversal rates across rounds by treatment for the nonmarket setting. Prior to the introduction of arbitrage in the market setting, reversal rates in the nonmarket settings for treatments 2–4 are statistically equivalent to the baseline treatment with *p*-test statistics ranging from –0.926 to 0.621. After five rounds of arbitrage in the market, reversal rates for nonmarket choice began to differ significantly from those in the nonarbitrage baseline. In the final rounds, the reversal rates in treatments 2–4 are significantly different from the rates observed in the baseline (*p*-values of 0.0004, 0.009 and 0.007).

While the unconditional tests across treatments indicate the existence of rationality spillovers, we now consider a within treatment conditional test to control for unobserved heterogeneous subject attributes such as ability and risk aversion that may play an important role in the results. Again we use Chamberlain's [17] fixed effects logit model (Eq. (1)) to examine how arbitrage in one market increases the likelihood of rational behavior in a nonmarket setting that lacks such market discipline. Recall the results for market settings of the preference reversal models in Table 3. Here we examine the estimates for the nonmarket settings to determine the presence of any rationality spillovers from market settings. In the nonarbitrage baseline, the rates of reversal in the nonmarket settings correspond closely to those observed in the market setting—we cannot reject the null that the level of rationality differs across the repeated undisciplined, nonmarket settings ($\chi^2(9) = 1.71$). In the absence of arbitrage, irrational behavior persists over time.

But for treatments 2–4, the likelihood of irrational behavior in the nonmarket setting is significantly reduced by the presence of repeated arbitrate in the market ($\chi^2(9) = 27.22$, 20.90 and 19.51). With arbitrage providing discipline in the market setting, people adopted rational behavior in the nonmarket setting—rationality spills over to the nonmarket setting from that induced in the market setting. Significant behavior adjustments begin to appear after five exchanges in the contested market. While adjustments occur slower than this in the nonmarket setting, the estimates suggest that rationality continued to increase in subsequent rounds of the nonmarket setting. Results from the nonmarket choices provide conditional evidence that rationality induced by market discipline is not limited to the market setting; rather rationality can spill over to nonmarket setting—the rationality spillover hypothesis is supported.[16]

In fact, our results show that rationality spillovers are surprisingly robust in our context—subjects transfer their learned rationality irrespective of whether the nonmarket choice was real or hypothetical, or whether the choice involved money or hypothetical–environmental lotteries. Across all treatments, our results support the rationality spillover hypothesis. One might expect the spillovers to happen with similar settings, it was not so obvious that they would persist at a similar level when the choices are *hypothetical*, and when the choices involve low-probability/high-severity hypothetical–environmental goods. But the spillover phenomenon did persist. We observe people being more rational even though they face no direct discipline and expect no reward for their actions.

4.3. Behavior adjustment

Given the presence of direct and indirect effects from arbitrage, we now examine how people corrected their preference reversals—did they change their preferences, did they adjust their valuations of low- or high-risk lotteries, or both? Result 3 summarizes our findings, which we explain in detail below.

Result 3. People adjust valuations of high-risk lotteries to achieve rationality given arbitrage; they do not alter their preferences. In addition, people do not adjust their valuations of low-risk lotteries with or without arbitrage.

4.3.1. Preferences

People do not change their preference ordering. To see this we again turn to the fixed effects logit model specified in Eq. (1), in which the indicator variable takes on the value of 1 if the subject preferred the low-risk lottery and 0 if he or she preferred the high-risk lottery. Again time effects, ϕ_t, capture any mutation of preferences over time, and subject effects, ω_i, control for unobserved individual characteristics such as perceptions and risk aversion.

[16] This finding is similar in spirit to a number of psychological studies which suggest that people trained in one situation can apply what they learned to a new situation, i.e., statistical training has helped some people solve everyday problems involving statistical reasoning (see [4,35,44]). In psychological terms, this behavior is called ‘‘generalization’’ or ‘‘transfer’’. These studies suggest that people transfer learned behavior to new settings where they avoid the earlier behavior, which was punished. For instance, Baron et al. [5] observed that people reduced their inconsistent judgments about health after answering a set of questions, which clearly revealed their inconsistencies. People with this consistency training then transferred what they learned in this context to judgments framed in another context. Interestingly, although many psychological studies conclude that transfer of learning is inversely related to the level of extrinsic rewards, i.e., prizes or recognition or money for doing well (see e.g. [22]), we find the opposite. Our extrinsic money pump did not reduce the transfer of rationality to the nonmarket situations in which the reward was unavailable.

Table 4
Logit fixed effects results: preference for low-risk lottery

	Treatment 1		Treatment 2		Treatment 3		Treatment 4	
	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket
Round 7	0.1209	−0.4203	0.0001	−0.8513*	−0.2941	0.4320	−1.2417**	−0.2837
	(0.4919)	(0.4602)	(0.4814)	(0.5004)	(0.5431)	(0.5383)	(0.5372)	(0.5336)
Round 8	−0.2394	0.0001	−0.2285	−0.7353	−0.4392	0.5808	−0.7148	0.3128
	(0.4898)	(0.4683)	(0.4785)	(0.5006)	(0.5428)	(0.5419)	(0.5391)	(0.5608)
Round 9	0.0001	0.0001	0.1170	−0.8513*	−0.4392	0.1424	−1.2417**	0.1519
	(0.4905)	(0.4683)	(0.4838)	(0.5004)	(0.5428)	(0.5339)	(0.5372)	(0.5514)
Round 10	0.0001	−0.5219	0.1170	0.0001	0.0001	0.4320	−1.1119**	−0.4181
	(0.4905)	(0.4597)	(0.4838)	(0.5164)	(0.5462)	(0.5383)	(0.5370)	(0.5300)
Round 11	0.1209	−0.3178	0.0001	−0.7353	−0.1480	0.0001	−1.2417**	−0.2837
	(0.4919)	(0.4614)	(0.4814)	(0.5006)	(0.5442)	(0.5330)	(0.5372)	(0.5336)
Round 12	0.6298	−0.4203	−0.1150	−0.2571	0.0001	0.0001	−0.8489	−0.1447
	(0.5062)	(0.4602)	(0.4797)	(0.5079)	(0.5462)	(0.5330)	(0.5380)	(0.5383)
Round 13	0.0001	−0.2139	−0.4530	−0.7353	0.3052	0.5808	−0.7148	0.1519
	(0.4905)	(0.4630)	(0.4777)	(0.5006)	(0.5533)	(0.5419)	(0.5381)	(0.5514)
Round 14	0.1209	−0.3178	0.2365	−0.6184	−0.1480	0.1424	−1.2417**	−0.4181
	(0.4919)	(0.4614)	(0.4868)	(0.5014)	(0.5442)	(0.5339)	(0.5372)	(0.5300)
Round 15	0.2435	−0.1082	0.4862	−0.7353	0.0001	0.5808	−0.7148	0.1519
	(0.4941)	(0.4654)	(0.4956)	(0.5006)	(0.5462)	(0.5419)	(0.8183)	(0.5514)
$\chi^2(\phi_t = 0)$	3.64	3.30	4.99	8.21	3.18	4.04	10.21	4.38
(p-value)	(0.9337)	(0.9512)	(0.8355)	(0.5136)	(0.9566)	(0.9085)	(0.3336)	(0.8849)
N^{a}	370	410	400	380	330	320	380	360

Standard error in parentheses unless otherwise noted.
*, **, and *** indicates significance at the 10, 5 and 1 percent level.
[a] Numbers of observations were 410 for treatments 1–3 (41 subjects over 10 rounds) and 430 for treatment 4 (43 subjects over 10 rounds)—the reported number accounts for those dropped during the estimation process because of invariance in the dependent variable.

Table 4 presents the estimated coefficients for the preference models across treatments and markets. In each case, Chi-square tests fail to reject the null that preferences between the low-risk and the high-risk lotteries are equivalent over time. Therefore, we find no evidence of any systematic adjustment of preferences between the low-risk lotteries and the high-risk lotteries. The conclusion that repeated interaction does not impact the individual choice of lottery type arises whether the choice is in the market or nonmarket setting, and whether the treatment uses arbitrage or not. Treatment and round effects do not play a significant role in determining preferences—failing to reject the null, thereby not supporting the *preference adjustment hypothesis*. Preference mutation is not the reason for more rationality.

4.3.2. Valuations

People revise their stated values for high-risk lotteries; they do not revise their values for low-risk lotteries. For a given institution, examination of adjustments in valuation is undertaken with a fixed effects panel model to account for subject and lottery heterogeneity:

$$WTP_{it} = \phi_t + \omega_i + \psi_{it} + \varepsilon_{it},$$

where the dependent variable, WTP_{it}, denotes the ith subject's willingness to pay for lottery ℓ, in round t; ϕ_t captures any systematic valuation adjustments over time, including linear, quadratic, cubic or any other form the data dictate; ω_i captures individual subject effects; ψ_{it} captures any lottery attributes which affect valuations, including probabilities and payoffs, and ε_{it} is the contemporaneous additive error term.

Table 5 presents estimates of the low-risk lottery valuation in the market and nonmarket settings of each treatment. In each case, people do not systematically change their stated values for low-risk lotteries. In the no-arbitrage baseline, we cannot reject the null that valuations of low-risk lotteries do not change over the repeated, uncontested trials. This finding is not surprising since the incidence of reversals with this baseline persisted over time in the no-arbitrage treatment. In the presence of arbitrage, rates of reversals did decrease significantly; but Table 5 does not suggest that adjustments in low-risk lottery valuation played a substantial role in increased rationality. While the set of time effects are significant in four of the six arbitrage treatments, results for individual periods reflect no systematic adjustment with significant coefficients being sparse and inconsistent. Consequently, findings do not support the valuation adjustment hypothesis for the low-risk lottery.

Table 6 tells a different story. Without arbitrage, we fail to reject the null hypothesis that valuations for high-risk lotteries are equal across trials—indicating that people did not significantly change their stated values for high-risk lotteries over the repeated, uncontested trials. With arbitrage, however, tests reject the null and indicate that people decreased their stated values over time. Again we see that this pattern of valuation adjustment occurred whether the lotteries were real or hypothetical. In fact, as Fig. 4 illustrates, the valuation adjustment is stronger in the hypothetical case. This result arises because initial hypothetical valuations are substantially higher than initial real valuations. But the decline in this hypothetical bias over time suggests that rationality spillovers might help mitigate this fundamental problem in contingent valuation.[17] Note that the one exception to our valuation results is found in column 8 of Table 4, in which valuation adjustments for high-risk lotteries are not statistically evident for the hypothetical–environmental lotteries.

Fig. 4, however, illustrates that aggregate valuation adjustments for environmental lotteries are similar to those for both money lotteries. We suspect the statistical insignificance in column 8 of Table 4 arises from the high variance exhibited in the values induced for the environmental lotteries. Given this single exception, our results support the valuation adjustment hypothesis for the high-risk lottery. In sum, our early round results do not contradict prior findings which

[17] See for example [21] for a discussion on hypothetical bias in the nonmarket valuation of public goods, and a verbal protocol to reduce the bias. Our result corresponds to Cummings and Taylor's [21] finding that stated hypothetical values are reduced with a verbal protocol explaining such values may be biased upward. While it is unclear whether the verbal protocol produces more rational responses or just lower numbers, we are able to unambiguously determine that lower valuations correspond with more rational behavior. The ease of the verbal protocol is attractive, but the subjective use of framing effects does not clarify issues of irrational nonmarket behavior. In contrast, implementing simulated market discipline in the field is relatively more demanding but the rationality of behavior adjustments is observable, and more importantly, arise from objective market discipline.

Table 5
Fixed effects results: valuation of low-risk lottery

	Treatment 1		Treatment 2		Treatment 3		Treatment 4	
	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket
Round 7	0.1578	0.3025	0.4068**	0.2210	−0.1364	−0.4456**	0.0542	−0.6992**
	(0.1909)	(0.2382)	(0.1720)	(0.2028)	(0.1561)	(0.2060)	(0.2091)	(0.2896)
Round 8	0.3639*	0.2147	0.2875	0.2517	0.1573	0.2135	0.2095	−0.2167
	(0.1931)	(0.2411)	(0.1749)	(0.2063)	(0.1559)	(0.2057)	(0.2063)	(0.2857)
Round 9	0.4673**	0.4874	0.1812	0.5001**	−0.1182	−0.2364	−0.1822	−0.4916*
	(0.1922)	(0.2399)	(0.1743)	(0.2055)	(0.1571)	(0.2074)	(0.2094)	(0.2900)
Round 10	0.0726	0.2644	0.4266**	0.2024	0.4456***	0.4017*	0.2068	−0.0722
	(0.1901)	(0.2373)	(0.1729)	(0.2039)	(0.1559)	(0.2058)	(0.2101)	(0.2910)
Round 11	0.4587**	0.3669	0.1227	0.1842	−0.1360	−0.2639	0.2219	−0.2702
	(0.1934)	(0.2414)	(0.1731)	(0.2041)	(0.1564)	(0.2064)	(0.2147)	(0.2973)
Round 12	0.3962**	0.0450	0.0278	0.3633*	−0.0048	−0.0307	−0.4023*	−0.6266**
	(0.1912)	(0.2386)	(0.1731)	(0.2041)	(0.1561)	(0.2060)	(0.2109)	(0.2921)
Round 13	0.1564	0.3975	0.3046*	0.0044	0.1698	0.0600	0.1874	−0.2444
	(0.1915)	(0.2390)	(0.1749)	(0.2062)	(0.1566)	(0.2067)	(0.2126)	(0.2945)
Round 14	0.0697	0.1702	0.4559***	0.0903	−0.0036	−0.2348	−0.0545	−0.4932*
	(0.1926)	(0.2403)	(0.1735)	(0.2046)	(0.1567)	(0.2068)	(0.2074)	(0.2872)
Round 15	0.3681*	0.2247	0.0183	0.2048	−0.1670	−0.1000	0.1234	−0.3839
	(0.1921)	(0.2397)	(0.1745)	(0.2057)	(0.1573)	(0.2076)	(0.2066)	(0.2862)
$\chi^2(\phi_t = 0)$	1.65	0.80	2.07	1.13	2.92	2.92	1.88	1.29
(p-value)	(0.1009)	(0.6144)	(0.0313)	(0.3402)	(0.0024)	(0.0024)	(0.0538)	(0.2388)
N	410	410	410	410	410	410	430	430

Standard error in parentheses unless otherwise noted.
*, **, and *** indicates significance at the 10, 5 and 1 percent level.
Individual lottery effects were significant and controlled for in the regression.

suggest (i) reversals seem to be driven more by mispricing of high-risk lotteries than by intransitivity [16]; and (ii) people overstate their hypothetical values relative to real values. Our later-round results show; however, that market experience induces people to revise downward their values to be consistent with their preferences, and that the hypothetical–real gap disappears [52].

Our results raise an interesting question about future directions in contingent valuation research: should we turn to psychology to explain biases by broadening the behavioral underpinnings of choices or do we turn to more powerful institutions that can induce rationality spillovers which temper these biases? Some argue for the first route given the thin markets that exist for environmental assets. Valuation work would benefit from channeling resources to develop a "behavioral theory of nonmarket valuation" which balances theory with pattern recognition from empirical data (e.g., [32,33]). A maintained belief in such a behavioral theory suggests that valuation work needs an overhaul with new behavioral restrictions that close the gap between theory and observation. Such an overhaul would require either the development of a new theory of value, which fits these behaviors, or a detailed grasp of how deviations from rational behaviors vary with the degree and the structure of arbitrage across institutional settings [20].

Table 6
Fixed effects results: valuation of high-risk lottery

	Treatment 1		Treatment 2		Treatment 3		Treatment 4	
	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket	Market	Nonmarket
Round 7	−0.2946	0.2355	−0.4989	−0.0333	−0.2944	−0.4435	−0.0185	0.3190
	(0.3534)	(0.4465)	(0.3077)	(0.3208)	(0.3282)	(0.5021)	(0.3088)	(0.7503)
Round 8	−0.2780	−0.0497	−0.1896	0.2670	−0.1439	−0.8150	0.3563	−0.3090
	(0.3576)	(0.4519)	(0.3129)	(0.3262)	(0.3276)	(0.5013)	(0.3047)	(0.7403)
Round 9	−0.0782	0.2999	−0.2713	−0.0592	−0.2136	−0.5013	−0.3853	0.5890
	(0.3559)	(0.4497)	(0.3117)	(0.3250)	(0.3303)	(0.5054)	(0.3092)	(0.7513)
Round 10	−0.2649	0.0194	−0.8706***	−0.6009*	−0.6840**	−0.9677**	−0.1141	0.3123
	(0.3520)	(0.4449)	(0.3092)	(0.3224)	(0.3278)	(0.5015)	(0.3103)	(0.7538)
Round 11	−0.1089	0.0088	−0.7361**	−0.2738	−0.9135***	−0.6590	−0.4118	−0.1022
	(0.3581)	(0.4525)	(0.3096)	(0.3228)	(0.3287)	(0.5030)	(0.3170)	(0.7703)
Round 12	−0.3755	−0.0402	−1.0219***	−0.3378	−0.5681*	−1.6764***	−0.3692	−0.4942
	(0.3539)	(0.4472)	(0.3095)	(0.3227)	(0.3282)	(0.5022)	(0.3115)	(0.7568)
Round 13	−0.3675	0.2970	−0.8267***	−0.7607**	−0.1328***	−1.5633***	−0.8785***	−1.1129
	(0.3546)	(0.4481)	(0.3128)	(0.3261)	(0.3293)	(0.5038)	(0.3140)	(0.7629)
Round 14	−0.1569	−0.0468	−1.0039***	−0.5027	−1.1522***	−1.8174***	−0.5475*	−0.7284
	(0.3565)	(0.4505)	(0.3103)	(0.3236)	(0.3295)	(0.5041)	(0.3063)	(0.7441)
Round 15	−0.3706	0.1450	−0.9461***	−0.6380**	−0.8684***	−1.5616***	−0.5930**	−1.0293
	(0.3556)	(0.4494)	(0.3120)	(0.3254)	(0.3307)	(0.5060)	(0.3052)	(0.7415)
$\chi^2(\phi_t = 0)$	0.29	0.20	2.88	2.13	3.25	3.02	2.60	1.20
(p-value)	(0.9775)	(0.9937)	(0.0027)	(0.0267)	(0.0008)	(0.0018)	(0.0064)	(0.2958)
N	410	410	410	410	410	410	430	430

Standard error in parentheses unless otherwise noted.
*, **, and *** indicates significance at the 10, 5 and 1 percent level.
Individual lottery effects were significant and controlled for in the regression.

Herein our results suggest a second route—design surveys to harness the rationality spillover phenomenon. Effort might be well spent to develop interactive, parallel market and nonmarket exchange institutions that generate the rationality spillovers, which keep *homo economicus* intact. Now researchers have an additional choice, and they can decide which approach is more useful for the question at hand—to reformulate the model to include behavioral extras in the value function (e.g., hypothetical, intransitivity, yea-saying, scoping), or to create a survey environment that provides incentives sufficient to reward rational choice over low-probability/high-severity events.

5. Summary and conclusions

Can people achieve rationality strictly on their own? Not necessarily—choices or values revealed in isolation need not always match preferences [25,29].[18] But people who cannot match preferences and choices open themselves to manipulation by others. An initially irrational person

[18] Sen [48,49] and Levi [36] forcefully argue this case.

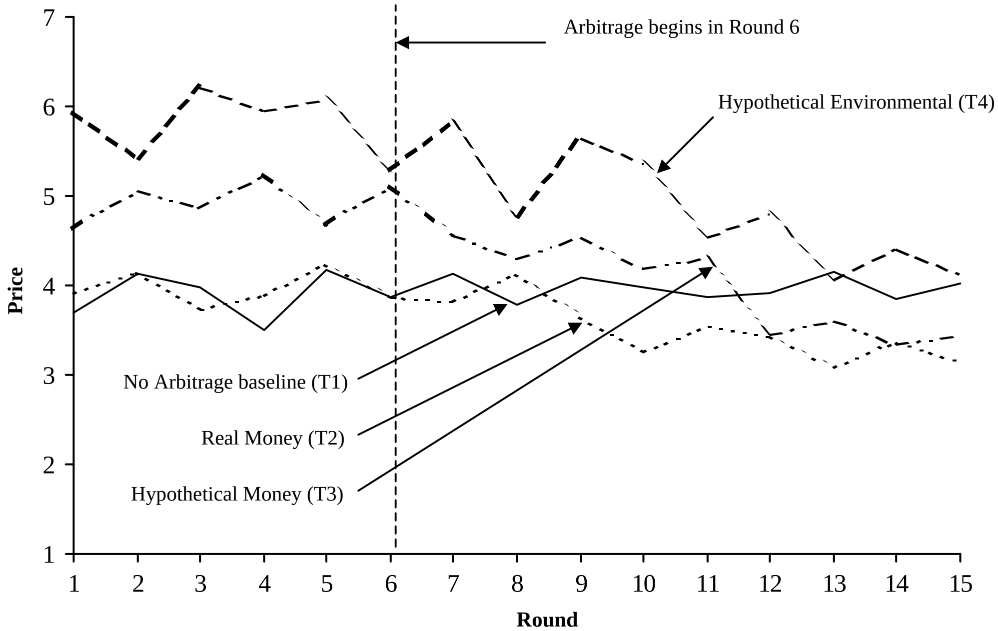

Fig. 4. Mean values for high-risk lotteries: real versus hypothetical.

will ultimately get fed up at being used and will learn how to match choices to preferences. Rationality sooner or later triumphs because rationality pays. Our experimental results confirm this conjecture, as do the results of numerous other controlled experiments. When the people in our experiments observed that their irrational choices lead to unprofitable outcomes, they stopped making irrational choices.

But what has been unknown, because it has not been asked, is whether there exists a continuity of rationality across institutional settings. Does the rationality induced from market discipline spill over to nonmarket settings in which a person is isolated from arbitrage pressures? Our results suggest that rationality spillovers can exist. This implies that market and nonmarket choice need not be autonomous spheres of activity but can instead be symbiotic. And the spillover phenomenon was robust: rationality spillovers occurred whether the nonmarket setting involved real, hypothetical, or environmental lotteries. Moreover, as people were exposed to arbitrage they adjusted their statements of value to better reflect their preferences, rather than changing their preferences.

Our findings do not contradict economists who wish to extend the collective concept of rationality to thin market or isolated settings, but our results also suggest a dynamic environment may be a necessary condition. Repeated exposure to competition and discipline was needed to achieve rationality. And in becoming rational, people refined their statements of value to better match their preferences. This suggests that efforts to harness the rationality spillover phenomenon may be worth more attention in nonmarket valuation research, especially when trying to value risk reductions to the low-probability/high-severity events that define many environmental assets (e.g., climate change, biodiversity loss). Dynamic survey designs, such as interactive web-based surveys, may be superior to the typical one-shot questionnaire. Settle, Cherry and Shogren [50], for example, use the approach developed herein to examine public preferences for the management of an exotic species in Yellowstone National Park. They apply the design of treatment 4 to the field using both the Internet and laptop computers in the park. Their results suggest that this method has the potential to provide market discipline to isolated respondents answering a hypothetical valuation survey. Rationality spillovers exist. Questions about the particular factors that encourage and discourage the transfer of rationality needed to keep *homo economicus* intact await answers.

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