Rationality Crossovers

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Abstract

Herein we further explore whether the power of arbitrage to induce people to exhibit more rational behavior extends to diverse decision-making tasks and stated valuation over preferences for gambles. We examine how arbitrage in a preference reversal setting affects behavior for the valuation of low probability food safety risks, the Allais Paradox, and the Ellsberg paradox. We design a three-stage experiment that elicits choices and values over gambles, with and without the experience of arbitrage. Our results suggest that a rationality crossover can exist – arbitrage in one setting can cross-over to affect the choices in unrelated tasks. Stated values for safer food dropped by 20–50%, and the frequency of the Allais paradox is cut in half. People acted more rationally by reducing their stated value for a lottery, or if monetary adjustments are impossible they adjust their choice away from the lottery. Rationality crossovers have their predicted limits in that the frequency of the Ellsberg para-dox, the most distinct decision environment, remained the same. We also found that the form of arbitrage as captured by stricter real market-like experience or a weaker version of cheap-talk (i.e., hypothetical) arbitrage did not affect the results. This paper shows that arbitrage-induced rationality can transfer across contexts.

Todd L. Cherry & Jason F. Shogren (2007) "Rationality Crossovers" *Journal of Economic Psychology* Volume 28 Issue 2 [DOI: 10.1016/j.joep.2006.12.002] Version of Record Available From (www.researchgate.net)

1. Introduction

For 50 years now laboratory evidence has revealed that people acting outside the realm of marketlike discipline can exhibit behavior inconsistent with rational choice theory. Such anomalous behavior undercuts the classic and still popular expected utility model of decision-making and the non-market valuation methods used to evaluate private choice and public policies regarding environmental resources. Landmark examples of anomalistic behavior include the well-documented phenomena of the Allais paradox, the Ellsberg paradox, and preference reversals, among others.[1] A person seems unduly influenced by how a gamble is framed, which violates a basic principle of rational choice which says preferences should be invariant to how gambles are described.

Recent lab evidence suggests some of these individual lapses in rationality – defined here as consistent choices and preferences – can be corrected by the social discipline provided by an active exchange institution like the market. To illustrate, consider the case of preference reversals. A person reverses her preferences when she faces two lotteries of similar expected value but different risk, and she indicates a *choice preference* for the safer lottery but states a *value preference* for the riskier bet (Irwin, Slovic, & Lichtenstein, 1993; Grether & Plott, 1979; Lichtenstein & Slovic, 1971).[2] Results from lab experiments shows that once this isolated person makes her choices within the social context of others who could exploit the inconsistent decisions, she stops reversing her preferences (Berg, Dickhaut, & O'Brien, 1985; Chu & Chu, 1990). The research shows people quickly respond to the feedback and discipline of an active exchange institution by adjusting their behavior to more closely match rational choice theory.

Cherry, Crocker, and Shogren (2003) extends this result by showing the increased rational behavior from the discipline of an exchange institution is not limited to the decisions within that institution; rather the amplified rationality spills over to parallel decisions that lack any discipline or exchange. They found that arbitrage not only stopped preference reversals subject to the discipline of an exchange institution, but the discipline also stopped preference reversals in a separate non-market setting in which the inconsistency did not matter financially. This so-called *rationality spillovers* phenomenon suggests the ability of markets to induce rational behavior extends to behavior beyond the market, which has implications for the elicitation of individual preferences and values in isolated and undisciplined settings (e.g., contingent valuation).[3]

Herein we follow up on Cherry et al. (2003) by investigating the presence of *rationality crossovers* – when the rational behavior arising from an active exchange institution not only spills over to isolated settings, but also *crosses over* to different decisions. We examine whether arbitrage in a preference reversal setting crosses over to induce people to exhibit more rational behavior when indicating preferences over gambles and valuations of reduced risks. Results corroborate previous findings of rationality spillovers, and extend the literature by showing rationality crossovers exist in some, but not all cases. Arbitrage in a preference reversal setting did crossover to impact isolated choices in related but distinct tasks. Stated values for safer food dropped by 20–50% and the frequency of the Allais paradox is cut in half; whereas the frequency of the more distinct Ellsberg paradox remains

[1] For good overviews, see Thaler (1992), Baron (2000), and Kahneman and Tversky (2000).

[2] See Camerer's (1995) review of the history and developments related to the preference reversal phenomenon.

[3] Interestingly; Laury and Taylor (in press) find some evidence of *altruism spillovers* in public good experiments.

exactly the same. The reach of exchange institutions to yield more rational choices is shown to be longer than previously revealed, but it also has its limits.

In addition to exploring the reach of arbitrage, we also investigate whether a weaker and less onerous form of arbitrage can provide similar effects on behavior, including rationality spillovers and rationality crossovers. Identifying less costly forms of corrective arbitrage is crucial to harnessing the power of exchange institutions to yield more rational choices in the elicitation of preferences and values in isolated non-market settings. We find the form of arbitrage may not need to be as cogent as previously studied; a weaker form of arbitrage that only provides verbal feedback is shown to increase rational choices at rates similar to the standard form of arbitrage entailing real financial consequences.

2. Preferences over gambles

We now describe the decision-making tasks in more detail. We first discuss the preference reversal phenomenon in the context of arbitrage (Lichtenstein & Slovic, 1971). We then present the three crossover tasks we use to explore whether arbitrage over preference reversals creates enough rationality to crossover to affect behavior in other tasks. Each crossover task provides a unique deviation from the preference reversal decisions of choices and valuations: (I) the valuation of food safety, which involves the valuation of a low probability/high severity lottery – no choices between lotteries (Shogren, Shin, Hayes, & Kliebenstein, 1994); (II) the Allais paradox, which entails choices among two pairs of lotteries – no valuation of lotteries (Allais, 1953); and (III) the Ellsberg paradox, which involves a choice between a pair of two-outcome lotteries, one with ambiguous odds – no valuation of lotteries (Ellsberg, 1961).

Preference reversals without and with arbitrage. The preference reversal phenomenon arises when a person faces a pair of lotteries that have similar expected values but different levels of risk. For example, consider the following pair of lotteries: P-bet: p chance of \$$X$, and $1-p$ chance of \$$x$ and \$-bet: q chance of \$$Y$, and $1-q$ chance of \$$y$, where $X>x$, $Y>y$, $p>q$, and $Y>X$. Expected utility theory presumes a person exhibits consistent choices and valuations – a person values his most preferred lottery more than his least preferred lottery. Evidence, however, reveals this is not always the case. People reverse preferences by preferring one lottery to the other while valuing the most preferred lottery less than the other lottery. The typical instance of preference reversal entails a person preferring the \$-bet while placing a greater value on the p-bet. Many researchers have challenged the persistence of the reversal phenomenon, only to show the depth of people's irrationality (e.g., Mowen & Gentry, 1980; Reilly, 1982).

One explanation of preference reversals is that people do not think about odds and consequences simultaneously as expected utility requires. Rather people separate the two elements and make their decision based on the most attractive element – either more certain odds or a very large prize, or the most like-minded element – \$'s for \$'s or %'s for %'s (see for example March & Shapira, 1987; Tversky, Slovic, & Kahneman, 1990). People simplify their choices by detaching probability from consequences and thinking about one element at a time to make their decision. When a person does not frame the choice-valuation problem comprehensively as expected utility presumes and no mechanism exists to force a person to reconsider the consequences of his irrational choice, his view on the relative importance of probability and consequences affects his decision.

Preference reversals are a powerful reminder that people sometimes behave in ways rational choice theory does not predict. In economics, however, rationality is a social construct not an individual phenomenon. Markets create rationality in the population by putting a cost on irrational behavior (Becker, 1962). People in marketlike settings have a reason to be rational because rationality pays; people outside markets can afford to be irrational because others do not exploit their choices (e.g., Smith, 1991). As Machina (1997, p. 227) notes, "we should expect most of the testable implications of (the reasons for preference reversals) to appear as *cross-institutional predictions*" (e.g., choices outside and within marketlike settings). Results from several lab experiments shows that the rates of preference reversals differ across institutions. Reversals can be stopped once a person makes his choices within the context of others who could exploit his inconsistent decisions, he stops reversing his preferences (see Berg et al., 1985; Bohm, 1994; Bohm & Lind, 1993; Chu & Chu, 1990; Cox & Grether, 1996). Preference reversals disappear once a person decides he prefers more money to less.

Cherry et al. (2003) extended the notion of socialized rationality in the case of preference reversals by showing that *rationality spillovers* exist – the rationality induced in a marketlike setting can transfer to decisions outside the marketlike environs. These spillovers were robust even when people faced choices over hypothetical and environmental gambles. In addition, and most importantly for our new experiments, the results show that people did not change their preference ordering; rather they revised their stated values downward for low-probability, high-severity lotteries. The observation that preferences remain stable and stated values drop with marketlike experience suggests that rational choice theory is robust.[4] This result suggests that arbitrage caused people to curtail inflated expectations about the value of risky lotteries, a result that has implications on whether rationality spillovers might crossover from preference reversals to the three other decision tasks we consider next. Tasks that involve risky gambles – those that have low probabilities and large outcomes – might be more likely to cause lapses in rationality, and therefore might be more open to rationality crossover effects.

Crossover task I: Food safety risks. For the first crossover test, we consider the task of valuing a low probability-high severity lottery – food borne pathogen illness. Evidence suggests this is a relevant task since the preference reversal phenomenon has been documented in non-monetary lotteries such as environmental and health risks (Irwin et al., 1993). Crossover task I had people evaluate an objective food safety lottery, and then state their willingness to pay for a reduction in the risk to life and limb. Following Shogren et al. (1994), after receiving information on the food safety lottery as defined by the objective level of risk and illness outcome, people answered an open-ended willingness to pay question:

> *Salmonella* is a food-borne illness that entails 1–3 weeks of acute abdominal pains, vomiting, diarrhea, and usually requires hospitalization. And 1 in 1000 people who get *Salmonella* die. The risk of contracting *Salmonella* from a typical meal is 1 in 137,000. This per meal risk translates into an annual risk of 1 in 125 people. There is food technology available that virtually eliminates this risk. In addition to the price

[4] Gunnarsson, Shogren, and Cherry (2003) used panel regression methods to test explicitly whether average preferences for skewness in risk were fixed or fungible in the presence of arbitrage; they could not reject the hypothesis of stable preferences.

of a typical meal, what is your maximum willingness to pay to virtually eliminate the risk of *Salmonella* in a typical meal?

Previous laboratory evidence reports that people will actually pay a premium per meal for such a risk reduction technology in the form of irradiation.[5] Using a second-price auction, Shogren et al. (1999) found that consumers were willing to pay approximately $0.80 per chicken breast for safer food. Further, they found that nearly 80% of the laboratory consumers preferred the irradiated chicken to the non-irradiated chicken; 30% of the consumers were willing to pay a 10% premium; and 20% were willing to pay a 20% premium. Note these results came from real exchanges in a repeated second-price auction with 10–15 bidders; our subjects here are answering a hypothetical food safety valuation question without marketlike feedback. We anticipate these stated values will be inflated relative to the earlier lab values due to hypothetical bias in bidding behavior (see Fox, Shogren, Hayes, & Kliebenstein, 1998).

Food safety differs from the preference reversal setting in two respects: there are no choices over lotteries, just the valuation of a lottery; and a person values a compound lottery – the odds of catching salmonella and surviving (1 in 125 times 999 in 1000) versus the odds of death (1 in 125 times 1 in 1000). In the psychological structure of Goldstein and Weber (1995), the food safety crossover task is similar to the preference reversal design. Since arbitrage over preference reversals caused people to adjust their stated values for gambles and not their choices between gambles, the valuation task should provide the best chance for a rationality crossover, even if the gamble is a compound lottery.

Crossover task II: Allais paradox. Allais' (1953) classic experiment revealed people routinely violate the independence axiom used to construct expected-utility theory. According to the independence axiom, if a person prefers lottery A to lottery B, that person prefers the probability mixture $\alpha A+(1-\alpha)Z$ to $\alpha B+(1-\alpha)Z$ for all $\alpha>0$ and Z. Intuitively, the independence axiom says that a person's choice between two options depends only on the states of nature in which those options yield different results. Allais' counterexample used the following two pairs of choices:

1. *Choose between* (A) and (B):
   - (A) 100% chance of $1 million
   - (B) 10% chance of $5 million, 89% chance of $1 million, and a 1% chance of zero dollars
2. *Choose between* (C) and (D):
   - (C) 10% chance of $5 million, and a 90% chance of zero dollars
   - (D) 11% chance of $1 million, and a 89% chance of zero dollars

Allais first asked people to compare and choose their preferred lottery between A or B, and next he had them indicate a preference between C or D. If the independence axiom holds, a person who selected A should then pick D (or, if his first choice was B, his second choice should be C). To be consistent, a person who prefers a certain $1 million (A) to a 10% chance to win $5 million (B), should also prefer the 11% chance of $1 million (D) to the 10% chance of $5 million (C). But among his subjects, Allais observed many chose A

[5] Shogren, Fox, Hayes, and Roosen (1999) observed the willingness to pay for irradiation is statistically equivalent whether the technology is identified; we therefore did not identify the risk reduction technology in the statement and question.

and C – the certain outcome and the risky lottery. People like certainty but they are willing to give up 1% point to go for the $5 million prize, even if it implies irrational choice in the sense of inconsistent risk preferences. Many other studies have since replicated the Allais findings (see Camerer, 1995).

Again the Allais setting has a similar essential structure, but differs from the preference reversal setting in one respect – there are no valuation decisions, just choices over lotteries. If people adjust stated values not choices between gambles, this difference may inhibit the ability of preference-reversal-arbitrage to induce rational behavior in the Allais setting. Arbitrage that causes people to stop reversing preferences by adjusting valuation decisions might not affect choices between lotteries, if valuation is not considered. But if the rationality crossover affects a person at a more fundamental level, these low probability-high outcome lotteries might well look less attractive. Our *a priori* expectation is that rationality crossovers will reduce the frequency of the Allais choice task, but the impact will be less robust relative to the food safety valuation task.

Crossover task III: Ellsberg paradox. Ellsberg (1961) demonstrated that people are averse to ambiguous probabilities, another violation of expected utility theory. Ellsberg obtained his results by asking people to choose between two probability distributions in a lottery; one of which was known and the other was ambiguous. People usually chose the known distribution even though the ambiguous distribution had a higher expected utility. The Ellsberg paradox suggests most people will usually avoid the ambiguous risk.

Consider the following example. Two urns contain a large number of red and black balls. Urn A is known to contain 50 red balls and 50 black balls. Urn B contains red and black balls in unknown proportions. Suppose a person wins $100 if we draw the color ball of her choice from of the urns. Which urn do most people select to draw from? Most people prefer Urn A with known odds to Urn B with ambiguous odds, even though they do not care whether they go for a red ball or a black ball in Urn B. This indifference suggests that people's subjective odds are 50:50 in the ambiguous Urn B, the same as the objective odds in Urn A. This violates expected utility theory in that the decision weights assigned to different states of the world should be independent of the origin of the uncertainty.

The Ellsberg setting deviates from the preference reversal framework in two respects: there are no valuation decisions and the choice decisions involve ambiguous risk rather than risky lotteries. The question again is whether preference-reversal-arbitrage in which people adjust values for risky lotteries, not preferences, will impact how people choose in the Ellsberg setting. Given the substantial deviation from the preference reversal framework and the lack of risky lottery comparisons, we expect fewer instances of rationality crossovers for the Ellsberg paradox relative to the Allais paradox and food safety valuation.

3. Experimental design

We designed a three-stage experiment involving the presented decision tasks with and without forms of arbitrage. We recruited 166 subjects from introductory and intermediate economics classes at the University of Wyoming. Each subject was inexperienced with specific decision tasks they were given, and each participated in only one treatment. All treatments were pencil and paper sessions, and took between 30 and 60 min to complete. Consider now the specifics of our three treatments – real arbitrage ($n=54$), cheap talk arbitrage ($n=61$), and for control purposes we also conduct a no-arbitrage baseline ($n=51$).

Real arbitrage. For the real arbitrage treatment, the monitor recruited subjects to participate in individual one-on-one sessions over a 4-day period. In stage one, subjects were asked to answer a set of four decision-making tasks: preference reversal, food safety risk, Allais paradox, and Ellsberg paradox. Stage two provided subjects experience with arbitrage in the preference reversal framework. In stage three, subjects faced the same four decision-making tasks in the same order as they faced in stage one, but with different parameter values for the lotteries to avoid perfunctory learning of each task.

The arbitrage mechanism follows earlier work on preference reversals (e.g., Chu & Chu, 1990). After completing the first set of tasks in stage one, subjects participated in real exchanges with a monitor. After being endowed with \$7, each subject individually faced a pair of lotteries that fit the preference reversal framework and had to indicate their preferred lottery and their fair value for each lottery. If the subject indicated consistent preferences and values, he purchased a lottery if his indicated value exceeded a randomly determined offer price.[6]

If the subject reversed his preferences, the monitor conducted the three exchanges to arbitrage the subject's inconsistent responses. The subjects were told that the monitor would buy, sell and/or trade with him only if it is beneficial to the monitor. Each subject knows the monitor will only do the following three exchanges: (1) buy when the monitor's 'reservation value' for an option is greater than or equal to the subject's indicated 'fair value' for that option; (2) sell when the monitor's 'reservation value' for an option is lower than or equal to the subject's indicated 'fair value' for that option; and (3) trade when the monitor's most preferred option is the subject's least preferred option. The monitor's 'reservation value' and 'preferences' are determined randomly. Three rolls of a single die determine the monitor's reservation value – one for each digit of the value. The value ranged between \$1.11 and \$6.66.

The arbitrage process was repeated with different lottery pairs until the subject indicated consistent preferences and values for the presented lotteries, when he was allowed to purchase a lottery depending on the indicated fair value and the randomly determined offer price. Subjects indicated consistent preferences and values within 3 rounds, with two exceptions requiring 4 and 6 rounds. Subject earnings consisted of a \$15 participation fee and any payoffs received from the arbitrage session. This concluded the arbitrage session. Each subject then answered the second set of decision-making tasks in stage three.

Cheap-talk arbitrage. The cheap-talk arbitrage treatment is identical to the real arbitrage treatment, except that arbitrage is now just *cheap talk* – the "non-binding communication of actions by two or more players in an experiment prior to their hypothetical commitment" (Cummings & Taylor, 1999, p. 650).[7] Our motivation comes from Cummings and Taylor (1999), who argue integrating a cheap talk script directly into a valuation survey can improve the ex ante validity of stated preference results.[8] They show that

[6] While recognizing its limits in demand revelation (e.g., Bohm, Johan, & Joakim, 1997), we used a variation of the Becker, DeGroot, and Marschak (1964) (BDM) mechanism to improve the correspondence between the subject's indicated values and true values. We hold the BDM mechanism constant across treatments.

[7] The use of "cheap talk" in non-market valuation differs from the usual game theory use, in which the "talk" is between the players in the game. Here the "talk" is between the monitor and the subject. Both uses refer to the costless, non-binding, non-verifiable form of communication sent prior to actual play (e.g., Farrell, 1987).

[8] Peter Bohm raised the open question of whether arbitrage was a necessary condition for more rational choices or whether explaining the arbitrage process to subjects would be a sufficient condition to generate consistent behavior.

hypothetical willingness to pay values for public goods drop significantly toward real values once people are explicitly told via a cheap talk script that most people have a tendency to overstate hypothetical values.

Here we examine whether *cheap talk arbitrage* can have a similar impact on irrational behavior. By considering cheap talk, we create a weaker form of arbitrage than the strict no preference-reversal-left-unpunished real arbitrage treatment. This allows us to create a range of arbitrage – strict to weak – that brackets more intermediary forms of arbitrage that might be found in the wilds.

In the cheap talk treatment, we followed the first stage with an explanation of how irrational behavior could be costly. Subjects did not purchase lotteries, the monitor did not use actual arbitrage to money pump irrational subjects, and they did not exchange money. We used a hypothetical illustration on how someone who reverses his preference could be impacted by arbitrage.[9] After the cheap talk session, subjects answered the second set of decision-making tasks in stage three. Given the hypothetical nature, subject earnings consisted of a flat $15 participation fee. If the cheap talk arbitrage is successful in reducing anomalous behavior relative to actual arbitrage, it supports the idea that a reasonable low cost method exists to increase the consistency of choices and stated values of survey respondents asked to think about gambles.

No-arbitrage baseline. The baseline treatment was conducted to disentangle potential confounding effects that may lead to behavioral changes between stage one and stage three, e.g., learning. This treatment eliminates any arbitrage in stage two; thereby providing a clean illustration of the impact of arbitrage. As in the other treatments, each subject was asked to answer the same sets of four decision-making tasks in stages one and three. Stage two left off the arbitrage experience; there was only a pause to indicate two distinct periods. As in the cheap talk treatment, subjects were paid $15 for participating in the session.

4. Hypotheses

We test three sets of hypotheses – the *real arbitrage hypotheses* to examine whether arbitrage in the preference reversal framework can crossover to other tasks; the *cheap talk hypotheses* to explore whether the weak cheap-talk arbitrage is a viable substitute for the strict real arbitrage; and the *no-arbitrage baseline hypotheses* to isolate the impact of arbitrage by testing for changing behavior in the absence of arbitrage. Table 1 summarizes our 3×4 experimental design and sets of hypotheses.

Denoting the mean value and rate of irrational behavior as $\Phi(\cdot)$, we test the null of equality between subjects' behavior before and after arbitrage – H_O: $\Phi(\text{pre}) = \Phi(\text{post})$. If the experience of arbitrage over preference reversals has a significant impact on the behavior of the four other tasks, we should reject the null in favor of less irrational behavior after arbitrage – H_A: $\Phi(\text{pre}) > \Phi(\text{post})$. Testing this hypothesis for each of the four tasks yields four tests per treatment.

[9] The monitor provided a pair of lotteries and asked each subject to consider which lottery he or she preferred and his or her reservation value for each lottery. The monitor stated the cheap-talk script: "people sometimes indicate inconsistent preferences and values, which may cause them to lose money in a real setting. The loss of money could occur by a person buying the lower-valued, but most-preferred, lottery for your reservation price, then trading this most-preferred lottery for your least-preferred lottery, and then sell your least-preferred, but highest-valued, lottery to you for your reservation price". Subjects were then asked to reconsider their preferences and values.

Table 1
Summary of experimental design and hypotheses

Task	Treatment		
	Real arbitrage	Cheap-talk arbitrage	Baseline – no arbitrage
Preference reversal	H_1: real direct	H_5: cheap-talk direct	H_{B1}: direct
Allais paradox	H_2: real crossover I	H_6: cheap-talk crossover I	H_{B2}: crossover I
Ellsberg paradox	H_3: real crossover II	H_7: cheap-talk crossover II	H_{B3}: crossover II
Food safety valuation	H_4: real crossover III	H_8: cheap-talk crossover III	H_{B4}: crossover III

Real arbitrage hypotheses. We initially re-examine previous reports that arbitrage over preference reversals will cause people to rethink their behavior and eliminate reversals (H_1: *real direct hypothesis*). The first test of rationality crossovers is undertaken by examining whether arbitrage over preference reversals impacts the valuation behavior of people assessing a reduction in the risk of food borne pathogens (H_2: *real crossover* I *hypothesis*). Subsequent tests examine whether rationality crossovers impact the choice-based behavior over certain lotteries in the Allais paradox (H_3: *real crossover* II *hypothesis*) and ambiguous lotteries in the Ellsberg paradox (H_4: *real crossover* III *hypothesis*).

Cheap talk hypotheses. Exploring the weaker cheap talk arbitrage, we first examine whether such arbitrage has an impact on behavior in the same preference reversal context (H_5: *cheap talk direct hypothesis*). We then test whether cheap talk arbitrage in the preference reversal setting impacts valuation behavior of food safety (H_6: *cheap talk crossover* I *hypothesis*) and choice-base behavior over certain lotteries in the Allais paradox (H_7: *cheap talk crossover* II *hypothesis*) and ambiguous lotteries in the Ellsberg paradox (H_8: *cheap talk crossover* III *hypothesis*).

Baseline hypotheses. Given our within-treatment analysis, we replicated the tests on data from a non-arbitrage baseline treatment to ensure any behavioral changes resulted from the arbitrage experience rather than other confounding factors such as learning. If we reject the null baseline hypotheses (H_{Bi}, $i = 1,2,3,4$), increased rationality may be attributable to these confounding factors. If we fail to reject the baseline hypotheses, we will be more confident that real and cheap-talk arbitrage caused any observed decline in irrational behavior.

5. Results

Our key result shows that arbitrage-induced rationality can transfer across tasks. Table 2 provides a summary of pre- and post-arbitrage behavior by task and treatment. We first consider the real arbitrage treatment.

Real arbitrage. The results illustrate how real arbitrage can affect behavior, both stated values and preference ordering. After experiencing arbitrage in the preference reversal framework, the rate of preference reversals decreased significantly – rejecting the real direct hypothesis ($p = 0.0006$; Fisher's exact test, FET). Table 2 shows that prior to experiencing arbitrage the rate of preference reversals was 37% and afterwards the rate drop to 9%. This aggregate decline is consistent with earlier findings in Chu and Chu (1990) and Cherry et al. (2003), in which reversals fell to between 3% and 8% from around 30% to 40%.

Table 3 presents more detailed results by grouping people into one of four categories: *rational change* (*yes–no*), *rational no change* (*no–no*), *irrational no change* (*yes–yes*), and *irrational change* (*no–yes*). The categories indicate how subject behavior changed across the pre- and post-arbitrage stage. For instance, for the rational change cell, a "(yes–no)" means that "yes", a person reversed his preferences in the pre-arbitrage stage; but then "no", he did not reverse his preference in the post-arbitrage stage. Table 3 reports that 30% (16 of 54) of all subjects fell into the "yes–no" rational change category. Considering only those that reversed preferences in the pre-arbitrage stage, 80% (16 of 20) corrected preferences after having experienced real arbitrage. Further investigation reveals that in 69% (11 of 16)

Table 2
Incidents and values of irrational behavior by task pre- and post-arbitrage treatment [a,b]

Task	Real arbitrage ($n = 54$)		Cheap talk arbitrage ($n = 61$)		No arbitrage baseline ($n = 51$)	
	Pre	Post	Pre	Post	Pre	Post
Preference reversal (%)	37.0 (0.487)	9.3‡ (0.293)	23.0 (0.424)	4.9‡ (0.218)	43.1 (0.500)	39.2 (0.493)
Food safety valuation ($)	$6.89 Mean $4.00 Median (8.02)	$5.62† Mean $3.00 Median (6.41)	$5.97 Mean $3.00 Median (7.97)	$4.77† Mean $2.00 Median (6.69)	$6.83 Mean $3.75 Median (8.62)	$6.66 Mean $4.00 Median (8.15)
Allais paradox (%)	74.1 (0.442)	51.9‡ (0.504)	67.2 (0.473)	45.9‡ (0.502)	70.6 (0.460)	64.7 (0.483)
Ellsberg paradox (%)	79.6 (0.407)	72.2 (0.452)	77.1 (0.424)	77.1 (0.424)	78.4 (0.415)	82.4 (0.385)

[a] Standard deviations reported in parentheses.
[b] † and ‡ indicate the rejection of the null (H_O: $\Phi(\text{pre}) = \Phi(\text{post})$) at the 5% and 1% levels for one-tailed tests.

Table 3
Individual behavior types by task and treatment

Task	Treatment		
	Real arbitrage	Cheap talk arbitrage	No arbitrage baseline
Preference reversal			
% (#) Rational change (yes–no)	29.6 (16)	18.0 (11)	7.8 (4)
% (#) Rational no change (no–no)	61.1 (33)	77.1 (47)	52.9 (27)
% (#) Irrational no change (yes–yes)	7.4 (4)	4.9 (3)	35.3 (18)
% (#) Irrational change (no–yes)	1.9 (1)	0.0 (0)	3.9 (2)
Total	100.0 (54)	100.0 (61)	100.0 (51)
Allais paradox			
% (#) Rational change (yes–no)	25.9 (14)	23.0 (14)	9.8 (5)
% (#) Rational no change (no–no)	22.2 (12)	31.2 (19)	25.5 (13)
% (#) Irrational no change (yes–yes)	48.2 (26)	44.3 (27)	60.8 (31)
% (#) Irrational change (no–yes)	3.9 (2)	1.6 (1)	3.9 (2)
Total	100.0 (54)	100.0 (61)	100.0 (51)
Ellsberg paradox			
% (#) Rational change (yes–no)	13.0 (7)	3.3 (2)	2.0 (1)
% (#) Rational no change (no–no)	14.8 (8)	19.7 (12)	15.7 (8)
% (#) Irrational no change (yes–yes)	66.6 (36)	73.8 (45)	76.5 (39)
% (#) Irrational change (no–yes)	5.6 (3)	3.3 (2)	5.9 (3)
Total	100.0 (54)	100.0 (61)	100.0 (51)

of the rational change cases, subjects corrected reversals by revising downward their valuation of the high-risk lottery.

This finding corresponds with the previous lab research that shows arbitrage induces people to act more rationally, and does so by inducing them to adjust values for the high-risk option downward rather than by changing preference ordering. People did not change their preference ordering for real, hypothetical, or environmental lotteries once they were arbitraged (see Cherry et al., 2003). Preference mutation was not the reason for acting more rationally nor was the upward revision of stated values for low-risk lotteries; rather they revised their stated values for high-risk lotteries downward. Our results confirm this pattern – people exposed to arbitrage adjust their statements of value to better reflect their preferences, rather than changing their preferences to the context.

Tversky et al. (1990) note the overpricing of the high-risk options was the reason for about two-thirds of the preference reversals observed in their experiments. Rather than a fundamental intransitivity in preferences, many people simply overpriced the high-risk option. Tversky et al. (1990) explained the overpricing result ex post by coining the "compatibility effect" or "contingent weighting", in which the weighting of stimulus (inputs) is enhanced by the compatibility with the response (output). For example, a person asked to state a $-value for a gamble will focus more on the compatible element of the stimulus – the $-value of the win or loss, than he will on the incompatible element – the %-probability of winning. This suggests he overprices a high-risk option because he concentrates on the dollar outcomes of the gamble, not the odds of winning the gamble. While the compatibility effect might explain the initial behavior, our results show the effect does not seem particularly robust once arbitrage is introduced into the decision frame. Our results suggest any compatibility effect fades away once individual behavior is subject to social exchange such as arbitrage.

We now address the question of whether the rationality induced by arbitrage in the preference reversal setting spills over to the other three tasks – valuing safer food, Allais gambles, and Ellsberg ambiguous urns. First consider the valuation task. Recall, since subjects generally respond to arbitrage in the preference reversal setting by adjusting stated values, the task of valuing safer food may be the crossover task most susceptible to any rationality crossover effect. Table 2 reports the mean stated value for food safety risk reduction decreased 18.4% after subjects experienced real arbitrage. Mean stated values dropped from a pre-arbitrage value of $6.89 to a post-arbitrage value of $5.62. The median value also declined – from $4 to $3. A paired test finds the mean pre- and post-arbitrage stated values are significantly different – rejecting the real crossover I null hypothesis ($t = 2.52$; $p = 0.0075$) and indicating a significant rationality crossover effect on valuations for safer food.[10]

We now consider the Allais paradox. Since people's preferences seem relatively stable, the prospects for rationality crossovers to affect the incidence of the Allais paradox may be lower. Table 2 however indicates that rationality crossovers do exist to some degree in the

[10] We note the post-arbitrage hypothetical values are high relative to the Hayes, Shogren, Shin, and Kliebenstein (1995) lab valuation experiments that elicited real economic commitments to reduce the risks of *Salmonella*, in which the average lab participant paid approximately $0.70 per meal using a Vickrey second-price auction. Even so, given these are open-ended hypothetical statements of value, we put more weight on the relative values than the absolute values. We find 54% of subjects reduced their stated values after arbitrage; compared to 19% that increased their stated values after arbitrage.

Table 4
Rate of preferred lotteries pre and post arbitrage in Allais framework[a]

	Pre-arbitrage			Post-arbitrage		
	C	D	Total	C	D	Total
Real arbitrage						
A	61.1	20.4	81.5	42.6	40.7	83.3
B	5.6	13.0	18.6	7.4	9.3	16.7
Total	66.7	33.4	100.0	50.0	50.0	100.0
Cheap talk arbitrage						
A	55.7	26.2	81.9	37.7	44.3	82.0
B	6.6	11.5	18.1	9.8	8.2	18.0
Total	62.3	37.7	100.0	47.5	52.5	100.0
No arbitrage baseline						
A	64.7	19.6	84.3	60.8	23.5	84.3
B	9.8	5.9	15.7	11.8	3.9	15.7
Total	74.5	25.5	100.0	72.6	27.4	100.0

[a] Rational choices in the Allais paradox are either (A and D) or (B and C).

Allais framework. The proportion of people who fell prey to the Allais paradox significantly decreased after facing real arbitrage in the preference reversal setting – rejecting the real crossover II hypothesis ($p = 0.0139$, FET). The incident rate of the Allais paradox was 74.1% prior to arbitrage, but dropped to 51.9% after subjects experienced real arbitrage with preference reversals. We acknowledge that the rationality crossovers phenomenon did not eliminate all occurrences of the Allais paradox,[11] but it did significantly reduce the rate of the inconsistent choices beyond any evidence that currently exists in the literature.

Again, Table 3 provides more details by reporting the type of behavioral changes observed across the pre- and post-arbitrage stages. About 26% (14 of 54) of all subjects corrected initial inconsistencies in the Allais framework after experiencing real arbitrage with preference reversals. Considering only those with initial inconsistent choices prior to arbitrage, 35% (14 of 40) of subjects corrected their initial inconsistencies after arbitrage while the remaining 65% (26 of 40) did not.

To better understand this result, Table 4 reports the specific choice adjustments related to the decreased incidence of the Allais paradox. The table provides the rates of behavior for the four possible choice categories in the Allais framework before and after the arbitrage session. With our parameters, rational choices are A and D or B and C; irrational choices are A and C or B and D. Of the 74.1% of subjects indicating irrational choices prior to arbitrage, 61.1% exhibited the typical paradoxical behavior of choosing lotteries A and C. After arbitrage, this number declined to 42.6%. With the rate of choosing A and D doubling after arbitrage, subjects seem to have adjusted preferences by switching from an A and C choice to A and D. The observed behavior suggests people became more risk averse from their experience with arbitrage because the correcting behavior appears to arise from

[11] While tests between pre- and post-arbitrage behavior reveal significant treatment effects, tests between post-arbitrage and predicted behavior suggest the remaining irrational behavior is non-trivial.

subjects adjusting the more risky C–D choice rather than the more certain A–B choice. Subjects stayed with the certain choice A, and then adjusted their preferences away from the high risk C choice to the lower risk D choice.

This behavioral response corresponds to that observed in the preference reversal case. In both cases, arbitraged subjects who made a rational change did so by altering how they react to the high-risk choices – lowering their stated values for high-risk lotteries in the preference reversal setting and switching their preference ordering away from high-risk lotteries in the Allais paradox setting. Risk aversion and high-risk lotteries may be the catalyst for rationality crossovers. Many people experiencing arbitrage of preference reversals reacted by reducing their stated value for low probability food safety threats and switching preferences away from low probability-high outcome gambles that originally attracted them.

We now turn to the last crossover task, the Ellsberg paradox. While the Ellsberg framework remains a choice-based setting, it introduces ambiguity to the decision frame. The distinction between risk and ambiguity causes the prospects for rationality crossovers to be even lower. Indeed, results reported in Table 2 reveal no significant rationality crossover effect with regard to the Ellsberg paradox. Rates of inconsistent choices in the Ellsberg setting do not significantly differ between pre- and post-arbitrage stages – failing to reject the real crossover II hypothesis ($p = 0.250$, FET). Turning to individual data, Table 3 reports that only 13% (7 of 54) of subjects corrected their initial inconsistent Ellsberg choice after experiencing arbitrage with preference reversals. Among those subjects making inconsistent choices prior to arbitrage, only 16% corrected their inconsistent choices after arbitrage while the remaining 84% repeated their inconsistent choices. Results indicate the rationality crossover phenomenon has its limits and rationality learning from arbitraged preference reversals may not crossover to isolated settings involving ambiguous lotteries. The gap between high risk and ambiguous risk seems to impede learning across tasks.

Cheap talk arbitrage. Given the direct and crossover effects, we now test whether cheap-talk arbitrage is a viable substitute to real arbitrage. Observed behavior from the preference reversal framework suggests the weaker cheap-talk arbitrage induces people to act more rationally to a similar degree as the stricter real arbitrage. Table 2 shows that cheap-talk arbitrage significantly reduced the rate of preference reversals from 23.0% to 4.9% – rejecting the cheap-talk direct null ($p = 0.0037$, FET). This 79% decrease in reversal rates corresponds closely to the 75% drop observed with real arbitrage. Table 3 shows that among those subjects that initially reversed preferences, the proportion of subjects that corrected their reversals after experiencing arbitrage was similar across the cheap-talk and real arbitrage treatments – 78.6% (11 of 14) versus 80% (16 of 20). As before, people who switched to rational decisions did so by lowering stated values rather than changing preference orderings (7 of 11 subjects).

The potential for hypothetical arbitrage to replace real arbitrage as a motivating force for people acting more rationally is noteworthy. This suggests that monitors of isolated surveys can induce people to exhibit more rational choices by explaining arbitrage to respondents individually or as a group, without having to undertake a more costly individual-level money pump process of actually taking lotteries and money.

Now consider whether cheap-talk arbitrage yields any rationality crossovers to the three other tasks. Table 2 shows the rationality crossover effects observed with cheap talk arbitrage are similar to those with real arbitrage. Cheap talk arbitrage crosses over to impact food safety valuation with people significantly lowered their stated willingness to pay – rejecting the cheap talk crossover I hypothesis ($t = 2.44$; $p = 0.0088$). Mean stated values with cheap-talk arbitrage declined significantly from \$5.97 to \$4.77, while median fell from \$3.00 to \$2.00. The declines in both measures correspond closely to those observed in the real arbitrage treatment.

Cheap talk arbitrage also reduced the incidence of the Allais paradox from 67% to 46%, rejecting the cheap-talk crossover II null ($p = 0.0140$, FET). The 31.3% drop in incidence rates with cheap-talk arbitrage nearly matches the 30% decline observed with real arbitrage. Table 3 reveals 23.0% of subjects made a rational change in the cheap-talk arbitrage treatment, which compares well to the 25.9% of subjects that did so in the real arbitrage treatment. Table 4 provides the behavioral matrices for the cheap talk arbitrage treatment. As with the real arbitrage treatment, the majority of subjects exhibiting irrational choices prior to arbitrage chose A and C, 55.7%. But after cheap talk arbitrage, the percent of subjects choosing A and C fell to 37.7%, while the number of subjects choosing A and D nearly doubled. Just as in the real arbitrage treatment, subjects tended to correct their paradoxical choices by switching away from C to D while staying with A. Again we did not eliminate irrational behavior, but we do observe more rational behavior at both poles, A and D and B and C, after exposure to either the strict real or weak cheap talk arbitrage.

As with real arbitrage, the crossover ability of cheap-talk arbitrage failed to extend to the choice task involving ambiguous lotteries. Observed behavior within the Ellsberg paradox framework before and after the cheap-talk arbitrage did not differ at the aggregate level – failing to reject the cheap-talk crossover II hypothesis ($p = 0.5851$, FET). Table 3 indicates that only 3.3% of subjects made a rational change in the cheap-talk arbitrage treatment.

Comparison to the observed baseline. Finally, we verify that the observed changes in behavior arose from arbitrage and not from other confounding factors by providing a no-arbitrage baseline comparison for the arbitrage treatments. Table 2 shows that the baseline choices were similar across the two sets of tasks in the absence of arbitrage. Tests fail to reject the null of equality for the food safety valuation task ($t = 0.395$; $p = 0.35$) and the four choice tasks: preference reversal ($p = 0.42$), Allais paradox ($p = 0.34$), and Ellsberg paradox ($p = 0.40$). As Table 3 reports, only 7.8% (4 of 51) of baseline subjects made a rational change in the preference reversal setting, which contrasts with the 29.6% and 18.0% of subjects that did so in the real and cheap talk treatments. For the Allais paradox, 9.8% (5 of 51) made a rational change in the absence of arbitrage, which differs from the 25.9% and 23.0% of subjects making a rational change with real and cheap talk arbitrage.

Turning to the behavioral matrices in Table 4, we see the systematic behavior adjustments observed for Allais with arbitrage did not occur in the no-arbitrage baseline. The initial rates of choices over the lottery pairs are similar to those observed in the first stage of the arbitrage treatments, but unlike those treatments the behavior pattern does not significantly change in the post-arbitrage stage. For example, the rate of the typical Allais behavior (choosing A and C) is 64.7% in the initial stage and 60.8% in the last stage. Subjects stayed with A, and rarely adjusted their choice between C and D. These comparisons suggest that confounding factors did not play a significant role in our rationality crossover findings. Arbitrage, real or hypothetical, seems to be the driving force behind the significant increase in rational choices observed in the lab.

Two caveats are worth addressing at this point. First, the real and cheap talk arbitrage money pumps bracket the range of potential arbitrage in the wilds. As in Chu and Chu (1990), with the real money pump, no preference reversal escaped its reach, which represents an aggressive marketlike environment. In contrast, our cheap talk money pump simply discusses the general idea of arbitrage and its consequences, which represents a weak marketlike environment. Our results show similar behavior at both poles – people acted more rationally once arbitrage was introduced, either for real or in theory. Given similar behavior at the extremes, one can speculate that these results might remain robust to more intermediary forms of arbitrage found in the wilds, but this is an open question worthy of future research.

Second, the arbitrage mechanism may or may not have made people become more rational. We did not prove people *think* more rationally with arbitrage; rather we observed people *acting more* rationally. While more rational thinking might have been induced, people could have acted more rationally for countless behavioral reasons – arbitrage might have caused them to be more conservative, less capricious, and so on. Our goal is to explore the power that an institution posses to induce people to *act* more rationally or to act *as if* they were more rational, regardless of whether their thinking was more rational in the traditional expected utility sense.[12]

6. Conclusions

Tversky and Kahneman (2000, p. 223) contend "[t]he claim that the market can be trusted to correct the effect of individual irrationalities cannot be made without supporting evidence, and the burden of specifying a plausible corrective mechanism should rest on those who make the claim". We agree. Our results provide evidence to show that arbitrage-induced rationality can transfer across contexts. We find evidence that such *rationality crossovers* can exist for preferences over gambles: arbitrage in one setting can induce people to exhibit more rational behavior in another distinct setting. The idea of a rationality crossovers relates to a point made in the psychology literature about "generalization" or "transfer" – people with training can transfer what they learned in one context to judgments framed in another context because they can avoid the behavior that was punished in the first setting (see for example Nisbett, Fong, Lehman, & Cheng, 1987). Our use of gambles as stimuli follows earlier work that says, "generalizability is enhanced by studying decision making in its 'essential' form" (Goldstein & Weber, 1995, p. 84). And while some psychologists might perceive simplified marketlike arbitrage as a "harsh penalty" solution that reveals the economist's "defensive posture" about the reversal phenomena (Slovic, 2000, p. 493, 494), our findings support the idea that "generalization" exists in economic contexts too. If the experimenter can show a person that his instinctual behavior is costly, she can induce him to behave in a less instinctive manner. Some evidence exists in the psychology literature that when people are trained to think harder about the normative principle underlying a decision problem, some people, but not all, are more likely to behave in accordance with the principle, causing behavioral anomalies to disappear (see Slovic & Tversky, 1974, discussion of their understanding/acceptance principle). This suggests that

[12] The data, while limiting, indicate the observed increase in rational choices is not randomly distributed among subjects, suggesting that arbitrage affects choices across settings for some subjects, but not others. For instance, in the real arbitrage treatment, 72% of rational changes in all settings were made by 41% of subjects while 26% of subjects made no rational change in any setting. More specifically, of the 16 subjects that made a rational change in the preference reversal setting, seven made a rational change in at least two of three other settings, and only one failed to make a rational change in at least one other setting.

more understanding about the cause and effect of a choice pushes behavior toward more consistent responses, a finding supported by the decision experiments in Stanovich and West (1998). As our experiment shows, people act more rationally and this behavior transferred to other related tasks. More generally, the evidence supports the notion that economic rationality should be considered a social not an isolated construct (Smith, 1991).

Even more notably, the idea of a rationality crossover advances our understanding about rationality training – arbitrage in one task can crossover and influence behavior in other tasks. This rationality crossover effect was the strongest for valuation of low probability-high severity food safety and the Allais paradox. But as the experiment also showed, the ability to induce people to act more rationally has its limits. The Ellsberg paradox, with its complex context, was unaffected by the rationality training. Finally, the evidence that cheap-talk arbitrage could serve a viable substitute for real arbitrage to induce people to exhibit more rational behavior, both for rationality spillovers and crossovers is noteworthy. People acted more rationally after being exposed to a simple explanation of arbitrage and its consequences. Strict real arbitrage that punished each and every inconsistency was unnecessary.

While arbitrage – real or cheap-talk – did not eliminate irrationality in all choices and all people, our results show that rationality from arbitrage can spillover settings and crossover tasks and lead to stated values and preferences that are significantly more consistent. Future efforts should continue to explore and identify more robust methods of arbitrage in the lab and in field non-valuation work.

Acknowledgements

The authors thank the USDA/ERS for financial support, and B. Cherry and C. Berry for the motivation. Thanks to Peter Bohm, Peter Frykblom, Kate Johnson, Dale Menkhaus, Laura Taylor, and the reviewers for their helpful comments.

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