

# **Social Interaction Effects and Choice Under Uncertainty: An Experimental Study**

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**Abstract:** Extensive field evidence shows individuals' decisions in settings involving choice under uncertainty (e.g. savings and investment choices) depend on the decisions of their peers. One hypothesized cause of peer group effects is social interaction effects: an individual's utility from an action is enhanced by others taking the same action. We employ a series of controlled laboratory experiments to study the causes of peer effects in choice under uncertainty. We find strong peer group effects in the laboratory. Allowing feedback about others' choices increases group polarization and reduces the likelihood that subjects will choose risky or ambiguous gambles. We observe spillover effects, as observing another's choice of one risky (safe) gamble makes *all* risky (safe) gambles more likely to be chosen. Our design allows us to eliminate social learning, social norms, group affiliation, and complementarities as possible causes for the observed peer group effects, leaving social interaction effects as the likely cause. We use a combination of theory and empirical analysis to show that preferences including "social regret" are more consistent with the data than preferences including a taste for conformity. (JEL C91, D8, D14, G11)

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## 1. Introduction

Anyone who has ever bought a pair of uncomfortable shoes because they were fashionable or ordered sushi even if they prefer cooked fish understands the concept of peer group effects. There is often a strong relationship between the decisions of an individual and the choices they observe their peers making. Peer group effects have been identified for many economically important behavioral outcomes including saving and investment decisions, criminal activity, drug use, divorce, out-of-wedlock fertility, educational attainment, and welfare use.<sup>1</sup> Choice under risk and (Knightian) uncertainty is an important element of many of these decisions, such as criminal activity and out-of-wedlock fertility, and is central in the case of savings and investment choices (Duflo and Saez 2002 and 2003, Madrian and Shea 2000, Kelly and Ó Gráda 2000, and Hong, Kubik, and Stein 2004).

Many possible causes have been identified for peer group effects. A particularly intriguing possibility is that sensitivity to peers' choices reflects social interaction effects: an individual's utility from an action is enhanced by others taking the same action. Unfortunately, identifying social interaction effects in observational data is notoriously difficult due to suspicions of omitted variable bias (Manski, 1993 and 1995). Peers whom we might expect to influence one another are likely similar in ways not fully captured by observable characteristics. If peers are similar in their unobserved taste for risk taking behavior (possibly due to self-selection into peer groups) or are subject to unobserved common shocks that affect their decisions, we would expect to observe positive behavioral correlations even in the absence of social interaction effects. Adding to the difficulties, social learning (e.g. Banerjee 1992 and Bikhchandani et al. 1992) and knowledge spillovers provide equally good explanations as social interaction effects for many examples of peer group effects.<sup>2</sup> Little work has been done on separating these informational stories from social interaction effects.

The high degree of control available in the laboratory makes experiments a valuable complement to field studies. The primary goal of this paper is to use laboratory experiments to cleanly identify the existence of social interaction effects. Rather than picking a specific behavior to study, we examine choices under risk and uncertainty to isolate the preferences underlying many examples of peer group effects.<sup>3</sup> We observe strong peer group effects in choices between pairs of gambles. Taking advantage of the controlled environment, the experiments are designed to eliminate any possibility of omitted variable bias, removing the need for an instrumental variables approach. Our design also eliminates the possibility of knowledge spillovers and makes it possible to rule out social learning as a cause of the observed peer group effects. By

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<sup>1</sup> Notable examples include Crane 1991, Sacerdote 2001, Hoxby 2000, Lalive and Cattaneo 2005, Case and Katz 1991, Glaeser, Sacerdote and Scheinkman 1996, Katz, Kling and Liebman 2001, Ludwig, Duncan and Hirschfield 2001, Kling, Ludwig and Katz 2005, Bertrand, Luttmer and Mullainathan 2000, Aizer and Currie 2004.

<sup>2</sup> Social learning and knowledge spillovers both involve gaining information from peers, but differ in whether information is acquired indirectly or directly. See Section 3 for detailed descriptions of social learning and knowledge spillovers.

<sup>3</sup> For other experimental papers on peer group effects, see Falk and Fischbacher (2002) on reciprocity and crime, Falk and Ichino (2006) on labor productivity, Falk, Fischbacher, and Gächter (2002) and Großer and Sausgruber (2007) on public goods provision, and Thöni and Gächter (2008) on gift exchange. Our paper differs in its focus on choice under uncertainty as well as our desire to precisely identify the mechanism underlying peer group effects.

process of elimination, we conclude that the likely cause of the observed peer group effects are social interaction effects.

Having found social interaction effects in the data, we use a combination of theory and empirical analysis to show that preferences including “social regret”, defined below, are more consistent with the data than preferences including a taste for conformity. We also show that there are spillovers between decisions. Observing peers making a risky (safe) choice makes subjects more likely to choose the risky (safe) option, not only for that decision but for other decisions as well. This result suggests the existence of broad cultures of risk taking or avoidance.

Going into the details, participants make a series of choices between pairs of gambles that vary in expected value, risk, and ambiguity. The pairs of gambles are repeated three times, non-consecutively, allowing us to see how subjects change their decisions with experience. Critically, subjects are randomly divided into groups of six. In the “no feedback” treatment, subjects receive information about the choice they made the last time they faced the same pair of gambles in a previous round. In the “full feedback” treatment, subjects receive this information as well as information about the other group members’ choices the last time they chose between the same two gambles. We refer to this information about others’ decisions as “feedback”.

To generate theoretical predictions about the effects of feedback, we develop a model of decision making in our experimental environment. The model’s critical feature is “social regret.” “Regret” refers to disutility experienced when an action not taken would have led to higher payoffs ex post (Loomes and Sugden, 1982; Bell, 1982 and 1983). Models incorporating anticipated regret have proved useful for explaining overbidding in first price auctions (Filiz-Ozbay and Ozbay, 2007; Engelbrecht-Wiggans and Katok, 2007). To add a social element, the disutility due to regret is weighted by the likelihood that another individual would have chosen differently. In other words regret is assumed to be less intense if others have chosen the same – misery loves company.

The model yields four useful results: (1) Even in the no feedback treatment, individuals are more likely to switch gambles when their choice does not agree with the choices made by the majority of their group. We use econometrics to separate this regression to the majority from social interaction effects in the full feedback treatment. (2) Feedback in the full feedback treatment makes individuals more likely to switch to the choice of the majority. In other words, the model predicts social interaction effects. (3) Choices across groups in the full feedback treatment shift over time toward less risky gambles. (4) Assuming preferences for conformity rather than social regret also yields social interaction effects, but does not bias choices across groups toward or away from risky gambles. Instead, the more popular gamble in a pair, averaging across all groups, becomes more popular over time with full feedback.<sup>4</sup> Comparing (3) and (4) allows us to separate preferences that include social regret from preferences for conformity as the cause for social interaction effects.

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<sup>4</sup> This is not the same thing as (2) because the effect of feedback varies across groups. To understand this point, consider a pair of gambles where individuals are initially indifferent (and hence equally likely to choose either gamble). In any single group, a positive response to feedback will shift play in favor of the majority choice. Across groups the feedback effect is equally likely to favor either gamble, so average play does not shift.

Data from the full feedback treatment exhibits strong peer group effects. Controlling for reversion to the mean, the likelihood of switching gambles increases by fifteen percentage points if the majority of the feedback disagrees with the lagged decision. This is a large effect since the overall frequency of switching is only twenty percentage points. The peer group effects lead to increased group polarization – with feedback there is greater homogeneity within groups and greater heterogeneity between groups than in the no feedback treatment.

Models of social learning predict variation in the response to feedback by the type of ambiguity and whether or not ambiguity is present. These predicted effects are absent in the data, indicating that the response to feedback primarily reflects social interaction effects.

Compared with the no feedback treatment, feedback causes subjects, across groups, to shift away from choosing riskier gambles. No tendency is observed for popular gambles, averaging across groups, to become more popular over time. Given our theoretical results, these observations suggest that that social regret, rather than preferences for conformity, underlies the social interaction effects. This establishes social regret as a new source of social interaction effects, one which seems particularly relevant (and powerful!) in situations that involve choice under uncertainty and risk.

As noted previously, elements of risk and uncertainty are present in many settings where peer group effects have been observed. An interesting question is how context specific these effects are. In other words, if I observe my peers taking a specific risky action does this make me more likely to take other risky actions as well? Our theory of social regret implies this and our data supports this prediction with evidence of spillovers between gambles – if subjects receive feedback that others are choosing the risky gamble in one pair of gambles, it makes them more likely to choose the risky gamble in all pairs of gambles.

As a secondary treatment, we examine what happens when two small groups of three subjects are merged for the third repetition of choices into a single group of six subjects. Prior to the merge, the data is consistent with a decreasing marginal impact of feedback. Although the total effect of feedback is reduced when there are only two rather than five pieces of information, the marginal effect of each piece of feedback increases by about 50%. After the merge, subjects' decisions are (weakly) more sensitive to previous choices by their subgroup than by the other subgroup. This may suggest that subgroups will tend to maintain a culture of risk or ambiguity aversion even after being merged into a larger group.

Understanding when and why peer group effects occur is important for predicting the impact of policy reforms and economic shocks. With peer group effects, these events not only affect the choices of directly impacted individuals, but also alter the behavior of peers of the directly impacted group.<sup>5</sup> Predictions based solely on the direct effect therefore underestimate the impact of such events. The conclusion of this paper discusses the broader ramifications of our results, given the implied social multiplier, for financial decision making in field settings.

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<sup>5</sup> Multiple equilibria are also possible. See e.g. Brock and Durlauf 2001 and Glaeser, Sacerdote and Scheinkman 2003.

The organization of the paper is as follows. Section 2 lays out the experimental design. Section 3 describes a number of possible causes of peer group effects, presents a model of decision making in our experimental environment, and lays out initial hypotheses for the data. Section 4 summarizes the experimental results, and Section 5 discusses the implications of these results.

## 2. Experimental Design

Subjects in all treatments had to choose one of two gambles in a series of rounds. After subjects had played all rounds, we randomly selected one round for each subject for payment. Each round was equally likely to be selected and selected rounds were independent across subjects. For the selected round, we played out the subject's chosen gamble and paid the subject based on this outcome.

The remainder of this section describes the gambles, explains how the presentation of gambles varied, and introduces the main treatment variable – subjects' feedback about prior choices of others facing the same pair of gambles. We conclude by summarizing the procedures we used.

**(Figure 1 here)**

*A. The Gambles:* In each round subjects saw a pair of gambles projected on a screen in the front of the room. The gambles were presented in 20 x 20 colored grids as illustrated in Figure 1. If a gamble was played out, one of the 400 cells was randomly selected with all cells equally likely. The payoff for the gamble depended on the color of the selected cell. The key below the pair of gambles gives the relationship between colors and payoffs. The same value key was used for all rounds, making it easier for subjects to see the relationship between the visual representation of the gambles and the potential payoffs.

To separate social interaction effects from social learning, we varied the potential for social learning from feedback by introducing ambiguity. Gambles were either presented in a “simple” or an “ambiguous” format. The gamble shown on the left in Figure 1 is an example of the “simple” format. Cells of the same color are grouped together in continuous blocks, making it easy for subjects to determine the likelihood that a particular color will be drawn. The goal was for subjects to not feel that the gamble's probabilities and payoffs are ambiguous.

Ambiguity was introduced in two different ways, either by presenting gambles in a “scrambled” format or a “blackout” format. An example of the “scrambled” format is shown on the right of Figure 1. The cells for the gamble have been randomly scrambled,<sup>6</sup> making it difficult to rapidly ascertain how many cells of any particular color are present. While subjects probably had a general idea of a gamble's characteristics (e.g. there aren't many blue squares), our intention was that they would be unable to make precise statements (e.g. there are exactly 20 blue squares).

The ambiguity of the scrambled format relied on subjects not being able to quickly count the number of cells of each color. We therefore hired several undergraduates and tested how fast they could count the cells. Drawing on the results of this testing, we gave subjects eight seconds

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<sup>6</sup> To scramble a simple gamble, we generated a random number for each of the 400 cells. The cells were then reordered from the highest random number (top left) to the lowest random number (bottom right).

to view each pair of gambles in sessions where the scrambled format was used to generate ambiguity. Based on our testing, a subject could at most count about 25% of the cells in a scrambled gamble in this time period (and they had to view two gambles, not one). We therefore expect that most subjects only had an impression of the likelihood of the various outcomes for a gamble presented in scrambled format.<sup>7</sup>

Even with the preceding precautions, one can reasonably object that the scrambled format does not create true ambiguity. We normally think of ambiguity as applying to cases where subjects don't know the distribution of outcomes (Knight, 1921). With the scrambled format, subjects had all the necessary information to know the distribution of outcomes, but were given the information in a way that was (exceedingly) difficult to process. Sampling (e.g. counting colors for 25% of the squares) allowed subjects to at least approximate the proportion of each color, although it is our impression that few subjects did so. Even without any direct attempt at approximation, a subject's subjective probabilities can be viewed as reflecting the true probabilities with errors. Unlike true Knightian uncertainty, this form of ambiguity allows for the possibility that social learning can account for any observed feedback effects.

**(Figure 2 here)**

We therefore used a “blackout” format as a second alternative for creating ambiguity. Figure 2 shows the same gambles as Figure 1, but uses the blackout format rather than the scrambled format to generate ambiguity. Note that the columns of both the simple and ambiguous gamble are randomly scrambled. This was done to eliminate any spatial correlation in the colors of the columns. We generated ambiguity by randomly blacking out 40% of the columns as illustrated on the right of Figure 2. The instructions explained that, “[T]he color (red, purple, yellow, or blue) for a blacked out square is hidden from you. Blacked out squares are just as likely to be chosen as any other square. If a blacked out square is chosen, your payoff will be based on the color that is hidden beneath.” Subjects had no way of knowing what color was hidden in the blacked out squares, creating true Knightian uncertainty (e.g. LeRoy and Singell 1987).

To maintain comparability to the simple format when columns were not scrambled, subjects were given twenty seconds to view each pair of gambles when ambiguity was created by blackout. This was more than enough time to count the number of columns of each color. Ex post, we ran regressions confirming that the use of scrambled columns in gambles with no ambiguity has no significant effect on behavior. In the following we refer to gambles presented with no ambiguity as simple, regardless of whether the columns were scrambled or not.

**(Table 1 here)**

Table 1 summarizes the pairs of gambles used in the experiment. Ten or twelve of these pairs were used for each session. Subjects first saw all ten or twelve pairs in a random order, saw the pairs a second time in a different random order, and then saw them a third time in yet another random order. We refer to these sets of ten or twelve pairs of gambles as “blocks”. Subjects saw each pair of gambles with the same presentation format (e.g. simple vs. scrambled) in all three

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<sup>7</sup> To make counting harder for the subjects, we did not allow them to have anything to write with (pen, pencil, etc.) during the experiment and monitored to make certain they weren't taking notes while the gambles were displayed.

blocks. In sessions using the blackout format to generate ambiguity, the scrambling of columns was identical and the same columns were blacked out in all three viewings. Thus, repeated viewings did not provide subjects with any additional information about what lay beneath the blacked out cells.

For each gamble Table 1 reports the expected value, standard deviation of payoffs (our primary measure of risk), and whether the presentation was simple or ambiguous. In the blackout treatment, the underlying expected value and standard deviation of payoffs of an ambiguous gamble might differ substantially from what subjects could observe from columns that were not blacked out. Since subjects' decisions necessarily cannot rely on information they don't observe, we report in parentheses the *observed* expected value and standard deviation for ambiguous gambles in the blackout treatment. The pairs of gambles in our experiment are constructed such that one gamble has a lower probability of earning the worst possible payoff, zero, and a lower standard deviation of payoffs. By any reasonable standard, this is the less risky gamble.

Note that a number of the gambles were repeated with different presentations. The second column of Table 1 gives the "class" of the gamble pair – two pairs in the same class contain identical gambles with different presentations. Both Class 1 and 2 compare a certain outcome with a risky gamble. All gambles in these classes are presented in the simple format.<sup>8</sup> The remaining twelve pairs of gambles represent four classes of gambles. All of these gambles involve risk. The structure of the gambles in Classes 3 – 6 was designed to distinguish interactions between feedback and expected value, feedback and risk, and feedback and ambiguity.<sup>9</sup> In Classes 3 and 6, subjects were asked to choose between a gamble with a higher expected value and a gamble with lower risk. For Classes 4 and 5, the gamble with higher expected value also has lower risk. Classes 3 and 5 form a pair with identical expected values, but with the risk ordering reversed. The same is true for Classes 4 and 6.

Within each class of gambles, there are three pairs differentiated by how the gambles were presented: simple versus simple, simple versus ambiguous, and ambiguous versus ambiguous. Considering only pairs that compare simple and ambiguous gambles; Classes 3 and 4 associate ambiguity with higher risk, while Classes 5 and 6 have lower risk for the ambiguous gambles.

*B. Treatments:* Treatments vary by the use of feedback and presentation format for ambiguous gambles. The feedback treatments are the heart of the paper. As noted above, subjects saw each pair of gambles three times. Subjects did not learn the realized outcome of any gamble until all decisions were completed. In all treatments, subjects in Blocks 2 and 3 (the second and third time they saw each pair of gambles) were shown their previous choice for the same pair of gambles. In the no feedback treatment, subjects received no further information about previous choices. This treatment serves as a control for the full feedback treatment.

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<sup>8</sup> The purpose of including these gambles was to compare choices using our visual representation of gambles with choices from earlier experiments (using the same gambles and subjects drawn from the same population) where subjects were explicitly told the probability of each outcome.

<sup>9</sup> Due to the random choice of which columns were blacked out, the observed differences between gambles is weakened for several pairs in the blackout format and reversed in two cases (expected value for Pairs 9 and 10). Thus, all cells of the design are not filled for ambiguity versus ambiguity pairs with the blackout format.

For the full feedback treatment, subjects were divided into groups of six. In Blocks 2 and 3, each subject was informed about the other five group members' choices in the previous block between the same pair of gambles. The instructions made clear to subjects that their payoffs depend solely on their own choices, not on the choices of other subjects. In other words, they received feedback for purely informational purposes. Subjects were still informed about their own previous decision. This eliminates any possibility of the feedback biasing subjects' memories of their own previous actions as well as giving subjects a stimulus to respond to beyond the feedback about others' previous decisions. Instructions for all treatments told subjects that they might be receiving feedback and subjects never knew what feedback they might be seeing in future blocks. Thus, behavior in the first block cannot depend on the type of feedback being used. Subjects knew that the feedback was coming from the same five other subjects for all twelve periods in a block.

We anticipated that giving feedback about others' previous choices would lead to group polarization, with groups developing cultures of risk/uncertainty taking or aversion. We were therefore interested in what happens when we merge two groups that may have developed opposing norms. The merge feedback treatment addressed this question by forming groups of six subjects, as in the full feedback session, and then splitting them into subgroups of three subjects. In Block 2, subjects only received feedback from the other two subjects in their subgroup. In Block 3 they received feedback from all five other subjects in the group. Once again, subjects had no way of knowing in Block 2 that the feedback will be changed for Block 3. The feedback for Block 3 made it obvious whether information was coming from a subject who was part of the subject's feedback for Block 2. As always, subjects in Blocks 2 and 3 knew their own choice in the previous block for the same pair of gambles.

**(Table 2 here)**

*C. Procedures:* Table 2 summarizes the sessions we ran. We ran three sessions of each treatment with one exception.<sup>10</sup> Given that subjects never interact with subjects outside of their group, there are at least ten independent observations per treatment. For the no feedback and full feedback treatments, sessions were run with both the scrambled and the blackout format for ambiguous gambles. The merge feedback treatment is a secondary treatment, and therefore was only run with the scrambled format.

Sessions 1 and 2 used Pairs 1 and 2 but did not include Pairs 11 – 14. After some reflection, we decided it was more important to get the simple versus simple comparisons these pairs provide for Classes 3 - 6 than to include Pairs 1 and 2. Time constraints made it impossible to include all fourteen pairs, so we used only Pairs 3 – 14 in the final session of the full feedback treatment, using the scrambled format for ambiguous gambles. To maintain parallelism, Pairs 1 – 10 were used in the first two sessions of the no feedback treatment using the scrambled format. All other sessions used Pairs 3 – 14. Given that the simple versus simple comparisons are basically identical in the scrambled and blackout format sessions, there are four sessions that allow us to compare behavior with full and no feedback for Pairs 11 – 14. This should be sufficient to identify effects of feedback in these pairs.

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<sup>10</sup> One session had only eight subjects due to a torrential rainstorm. An additional session was scheduled to make the number of subjects in this cell similar to other treatments.



The random ordering of pairs of gambles was determined prior to the beginning of each session. Different orderings were used for each session within a treatment. To ensure that effects of adding feedback were not confounded by experience effects, the same ordering was used for parallel sessions with no feedback and full feedback. Specifically, the three no feedback sessions with the scrambled format for ambiguous gambles have the same ordering over pairs as the three full feedback sessions with the scrambled format, and the three no feedback sessions with the blackout format for ambiguous gambles have the same ordering over pairs as the three full feedback sessions with the blackout format. The ordering of pairs in the merge feedback treatment does not mirror the ordering in any of the other treatments, and (since different pairs of gambles were being used) the ordering of pairs is not identical in sessions with the scrambled and blackout formats for ambiguous gambles.

Sessions were run in the fall and spring of 2006 at Case Western Reserve University. Subjects were recruited from the CWRU undergraduate population via emails. Subjects with blue-yellow color blindness were excluded. The average session took about an hour and average earnings were slightly more than fifteen dollars, including a five dollar show-up fee. These payments were sufficient to guarantee a steady supply of subjects.

All sessions were run in a computerized laboratory using z-Tree (Fischbacher, 2007). Instructions, as well as sample PowerPoint slides showing the gambles from a session, are available at [add url]. Following the instructions, subjects were asked to complete a short quiz testing their understanding of the experimental instructions.

When Pairs 11 – 14 were added, we were concerned that subjects might notice they were seeing the same gambles repeatedly. This could conceivably undo the intended ambiguity. For example, Pairs 3 and 13 are in the same class. If a subject noticed that Gamble A was identical in these two pairs, they might correctly guess that Gamble B was also the same and thereby face reduced ambiguity in Pair 3. In designing the pairs of gambles and the slides that displayed them, we therefore made a number of choices intended to limit spillovers of this sort between different pairs of gambles in the same class. The most basic and probably most powerful of these features was to have multiple classes of gambles displayed in random order. Since subjects were not allowed to take any notes and there was a large amount of information on each slide, it would take an exceptional memory to recall specific details about gambles shown in earlier rounds. As another basic precaution, subjects were given no information hinting at the relationship between different gambles in the same class. We also added some subtler features. For Pairs 11 – 14, we displayed Gamble A on the right rather than the left (as in all other pairs). In sessions using the blackout format to display ambiguous gambles, we worried that the relative simplicity of the displays, even with ambiguity, would make it relatively easy to recognize that different pairs in the same class were related. To combat this, the scrambling of columns was independent between pairs of gambles.

The two gambles were projected on an overhead screen at the beginning of each round for eight seconds in sessions using the scrambled format for ambiguous gambles and twenty seconds in sessions using the blackout format. The display then switched to a “waiting” slide. This was posted long enough to have a total of 48 (45) seconds for the round in sessions using the

scrambled (blackout) format. Subjects could make a decision at any time during the round. If no decision was made within the allotted time, the computer randomly choose a gamble for the subject. This was observed on rare occasions in the first few rounds, presumably due to some initial confusion by subjects. Subjects only received information about the outcome of their chosen gambles when all rounds were completed. At this time subjects saw a table reminding them which gamble they had chosen in each round and showing the realized outcome (color and payoff) for this gamble. A final screen told them which round had been randomly selected to be paid in cash as well as their earnings (including the five dollar show-up fee). Subjects were paid privately in cash at the conclusion of the experiment.

Due to a software error, data from the last period of session 3 was not saved. For one period in session 4 and one period in session 11, the gamble that was displayed on the overhead did not match the gamble that the computer used in calculating payoffs and generating feedback due to a programming error. All data from periods directly affected by these errors as well as all data from periods where the feedback was affected by these errors have been removed from the dataset (150 of 11,200 observations).

### **3. Causes of Peer Group Effects**

This section summarizes possible causes of peer group effects that have been identified in the field literature and discusses how our experimental design rules out these causes as possibilities or allows us to distinguish between them. We begin by discussing possible causes for peer group effects that would *not* be classified as social interaction effects. The next subsection describes mechanisms for social interaction effects that our design does not allow as possibilities. The third subsection presents a theoretical model of decision making in our experimental environment. We develop predictions for this model if preferences include either social regret or a taste for conformity as possible causes of social interaction effects. The section concludes with a summary of our hypotheses about the experimental results.

#### *A. Omitted Variable Bias, Social Learning, and Knowledge Spillovers*

*Omitted Variable Bias:* Omitted variable bias is not a cause of peer group effects per se, but instead is an empirical problem confounding the identification of peer group effects. Because individuals in a peer group are likely to have common unobserved characteristics, their behavior may be correlated even in the absence of any peer effects. For omitted variable bias to cause identification problems in our experiment there would have to be some *unobserved* common factor for subjects in the full feedback treatment that was not present in the no feedback treatment and caused subjects to change their behavior in the direction of the majority choice. Given that subjects are randomly assigned to treatments, it is difficult to imagine any such factor.

*Social Learning:* The best known version of social learning is provided by the economic literature on herding behavior and informational cascades (Banerjee 1992; Bikhchandani et al. 1992). In these models decision makers receive noisy signals about the values of their options. Because these signals are not perfectly correlated across individuals, decision makers can infer something about others' information by observing the previous choices of others. We refer to this as "rational" social learning.

All subjects in our experiments receive identical information about the gambles. However, the scrambled format for ambiguous gambles is designed so subjects don't have adequate time to assess the probability of each outcome. This is "as if" each subject receives noisy information about the probability distribution over outcomes. Rational social learning could then occur if subjects can gain better information about distribution over outcomes by observing the choices of others.

Rational social learning should be largely absent when the blackout format is used for ambiguous gambles. No subject has information about the contents of blacked out cells and more than adequate time to count the number of columns of each color. Comparing the response to feedback with the differing formats for ambiguous gambles therefore gives us a way of checking for rational social learning.

As an alternative to rational social learning, social learning could also arise from a subject's belief that he may have made a mistake. By a mistake, we mean a persistent error in judgment which causes a subject to misidentify the optimal choice given their preferences. Suppose an individual observes he chose a different gamble from everyone the previous time the same pair of gambles was available. Knowing that other subjects received the same information as him about the gambles, he might conclude he has made a mistake. Consequently he may switch and adopt the choice of others. This is not because he learned anything new about the gambles from observing others' choices, but because he has learned something about the quality of his own decision making. To distinguish this from rational social learning, we label it "boundedly rational" social learning. Boundedly rational social learning is consistent with imitation models in evolutionary game theory (see e.g. Weibull 1995, ch. 4.4.).

Because boundedly rational social learning is driven by attempts to correct mistakes, it should be least prevalent in settings when mistakes are unlikely. Pairs that present both gambles using the simple format are by far the most straight forward decisions our subjects make. If boundedly rational social learning is partially responsible for the observed peer group effects, we would expect these effects to be weakest in simple versus simple comparisons.

*Knowledge Spillovers:* Like social learning, knowledge spillovers are an explanation of peer group effects that rely on information transmission. The difference is that knowledge spillovers involve direct sharing of information rather than indirect inference of others' information based on their choices. Peers' decisions are likely to be correlated when there are knowledge spillovers; the shared information plays the role of an unobserved common shock. Field studies of peer group effects have noted the possibility of knowledge spillovers but have done little to separate these from social interaction effects (e.g. Bertrand, Luttmer, and Mullainathan 2000, Duflo and Saez 2003; Hong, Kubik, Stein, 2004). In our experiments there are no direct interactions between individuals, eliminating knowledge spillovers as a possible cause for the observed peer group effects.

#### *B. Social Interaction Effects Ruled Out by Design*

A social interaction effect occurs when an individual's utility from an action is enhanced by others taking the same action. In Section 3.C we present a model showing how social interaction effects can occur in our experiments because of social regret and tastes for conformity. In the context of risk taking behavior, however, there are at least two other channels through which social interaction effects could occur: social norms and payoff complementarities. To narrow down the potential causes of the observed peer group effects, our experimental design rules out these alternative channels. Below, we describe these channels and explain how our experimental design eliminates them.

*Social Norms:* There is ample evidence that people's choices are affected by social norms regarding correct behavior (Ostrom 2000). Coleman (1990) defines a social norm as a rule of behavior that is enforced by social sanctions, which can take the form of social approval or disapproval. Social interaction effects arise if social norms are conditional in nature, that is, when the disapproval associated with not adhering to a norm is felt more strongly when one's peers adhere to the norm. This source of social interaction effects doesn't seem terribly likely for settings involving choices under risk and uncertainty, but it isn't completely implausible. For example, in some groups people may experience social approval if they have their retirement savings invested in the stock market because this is considered a wise thing to do; in other groups such behavior may be perceived as a foolish risk. In our experiment subjects play anonymously and there is no way to express approval or disapproval. Social norms therefore play a minimal role in our experiment.

*Payoff Complementarities:* An activity can be more desirable if others are participating. For example, it is more fun to go to a nightclub if the crowd is lively. Payoff complementarities can also occur in savings and investment decisions. For example, investment clubs have a strong social aspect as well as their role in helping participants make investment decisions. This type of social interaction effect is ruled out by our experimental design. Complementarities are not built into the payoff functions, as the monetary payoff of a gamble does not depend on how many other individuals choose it, and there are no social interactions included in the experiment.

### *C. Social Regret and Tastes for Conformity as Mechanisms for Social Interaction*

This subsection develops a simple theory of how individuals choose between pairs of gambles like those used in our experiments. We use this theory to generate initial hypotheses about when subjects are likely to switch choices between blocks for a pair of gambles.

*Basic Setup:* Consider an individual choosing between two gambles,  $G$  and  $G'$ . The support for both gambles consists of three possible payoffs:  $A$ ,  $B$ , and  $C$ . Assume  $A > B > C \geq 0$ . Let  $p_A$ ,  $p_B$ , and  $p_C$  be the probability of the three outcomes for  $G$ , and let  $p'_A$ ,  $p'_B$ , and  $p'_C$  be the probability of the three outcomes for  $G'$ . Without loss of generality assume that  $G$  is the less risky of the two gambles. For all pairs of gambles used in our experiments,  $C = 0$  and  $p_A = 0$ .

Individual  $i$ 's utility for choosing  $G$  in block  $t$  is given by Equation 1. The first term is an "inherent utility" from the gamble. Given our focus on switching and feedback, the source of the inherent utility is not particularly important. The second term is a "social regret" function,

described in more detail below. The utility for choosing  $G'$ , shown in Equation 2, is defined in an analogous manner.

$$\text{Equation 1} \quad u_G^i = U_G - \rho_{it}(G, G')$$

$$\text{Equation 2} \quad u_{G'}^i = U_{G'} - \rho_{it}(G', G)$$

An individual experiences non-negative disutility  $k$  from regret if he gets the worst outcome (C) and would have done better (A or B) if he had chosen the other gamble. As a simplifying assumption, the disutility of regret does not depend on how much better he would have done by choosing differently. To give regret a social aspect, disutility  $k$  is weighted by the probability that another individual would have chosen differently. Anticipated social regret is the likelihood of experiencing regret multiplied by the disutility due to social regret. Equation 3 gives the anticipated social regret for an individual choosing  $G$  and Equation 4 gives the anticipated social regret if  $G'$  is chosen. The first two terms of Equation 3,  $p_C(1-p'_C)$ , give the probability that individual  $i$  receives the worst outcome and would have done better by choosing  $G'$ . The next term,  $\pi'_{it}$ , gives individual  $i$ 's probability estimate that another individual will choose  $G'$  in block  $t$  – this is a belief not an exogenous probability. The final term,  $k$ , is the (unweighted) disutility from regret.

Intuitively, a decision maker feels regret if, *ex post*, he made the wrong decision. This isn't to say the decision maker made a mistake, as long as the decision was correct *ex ante*, but there is a natural tendency to think about how things would have been better if only a different choice had been made. How badly an individual feels about a negative outcome may depend on how defensible his decision is, not to others (after all, nobody but the experimenter can see a subject's choices) but to himself. One way to defend a decision is by appealing to the judgment of others. An individual might tell himself that his decision couldn't have been too wrong if virtually everyone did the same thing. This is the essence of social regret – regret is less intense when others made the same choice.

$$\text{Equation 3} \quad \rho_{it}(G, G') = p_C(1-p'_C)\pi'_{it}k$$

$$\text{Equation 4} \quad \rho_{it}(G', G) = p'_C(1-p_C)\pi_{it}k$$

To close the model we need to specify beliefs. Let  $\pi_{i0}$  and  $\pi'_{i0}$  be individual  $i$ 's initial beliefs. For now we make no assumption about these priors. Absent feedback, individual  $i$ 's beliefs remain fixed. Suppose individuals receive feedback about the share of people playing  $G$  in the previous block. Let  $\alpha_{i,t-1}$  denote the proportion of others individual  $i$  observes choosing  $G$  in the previous block. We assume individuals respond naively to information about  $\alpha$  in block  $t-1$ , believing that  $\alpha$  will be the same in block  $t$ . Thus,  $\pi_{it} = \alpha_{i,t-1}$  and  $\pi'_{it} = 1 - \alpha_{i,t-1}$ . This is a simplifying assumption, as the main results go through with any sort of reasonable updating process.

Individual  $i$  chooses between  $G$  and  $G'$  in block  $t$  by comparing the utility for the two gambles and choosing the gamble with the higher utility. Adding an element of stochastic choice, the model includes the possibility of randomly occurring mistakes (choices of the gamble with lower utility). These occur with decreasing probability as the difference between the gambles' utilities

increases. Specifically, Gamble G is chosen if  $\Delta U_{it} \geq 0$  as defined in Equation 5, where  $\tau_i$  is an individual error term uniformly distributed over the interval  $[-T, T]$  and  $\varepsilon_{it}$  is an idiosyncratic error term uniformly distributed over the interval  $[-E, E]$ . Use of uniformly distributed error terms greatly simplifies the analysis, but otherwise isn't important for our results.

$$\text{Equation 5} \quad \Delta U_{it} = u_G^{it} - u_{G'}^{it} + \tau_i + \varepsilon_{it} = (U_G - U_{G'}) - (\rho_{it}(G, G') - \rho_{it}(G', G)) + \tau_i + \varepsilon_{it}$$

We assume T and E fulfill the condition shown in Equation 6. This guarantees that both gambles are chosen with positive probability for all individuals.

$$\text{Equation 6} \quad T + E > \max \left[ \text{abs}((U_G - U_{G'}) + kp'_c(1 - p_c)), \text{abs}((U_G - U_{G'}) - kp_c(1 - p'_c)) \right]$$

*Reversion to the Majority:* A notable feature of our data is reversion to the majority in the no feedback treatment. Even though subjects in this treatment receive no feedback, the software still assigns them to a group of six subjects. Looking at the raw data, subjects in the no feedback treatment are more likely to switch gambles if their choice did not agree with the choice of the majority of others in their group. In other words, subjects' choices are correlated with feedback they aren't receiving! Given that choices can vary solely due to changes in the idiosyncratic error term,  $\varepsilon_{it}$ , our simple model predicts this effect. The intuition behind Theorem 1 is that a person is more likely to switch gambles if he made a mistake. Moreover, a person is more likely to have made a mistake if he played differently than the majority. The proofs of all theorems are included in the Appendix.

**Theorem 1:** With no feedback, subjects are more likely to switch gambles between Block  $t - 1$  and Block  $t$  if their choice from Block  $t - 1$  disagrees with the majority of choices in Block  $t - 1$  by others in their group.

*Response to Feedback:* Theorem 2 predicts that preferences including social regret lead to social interaction effects. This also implies that feedback leads to group polarization. Intuitively, the social regret associated with a gamble is an increasing function of the probability that others choose a different gamble. Observing *more* individuals who agree with his choice in Block  $t - 1$  makes an individual believe it is *less* likely that others will choose differently than him in Block  $t$ . This decreases the expected social regret for the gamble he chose in Block  $t - 1$  and increases the expected social regret for the gamble he did not choose. Hence, an individual's likelihood of switching gambles decreases when more feedback agrees with his choice in Block  $t - 1$ .

**Theorem 2:** Assume  $k > 0$ . The increase in the likelihood of an individual switching gambles between Block  $t - 1$  and Block  $t$  if their choice from Block  $t - 1$  disagrees (rather than agrees) with the majority of choices in Block  $t - 1$  by others in their group is larger with feedback than without feedback.

Without loss of generality assume that G is the less risky of the two gambles. To discuss the effect of allowing feedback on the probability of switching to or from risky gambles we must specify initial beliefs. Let  $\pi^*(k)$  be the probability that G is chosen given  $\pi_{it} = \pi^*(k)$ . This probability is a function of  $k$ , disutility due to regret. Using Equation 5 we can solve for the value of  $\pi^*(k)$ , as shown in Equation 7.

$$\text{Equation 7} \quad \pi^*(k) = \frac{(U_G - U_{G'}) + kp'_c(1 - p_c) + E}{k[p_c(1 - p'_c) + p'_c(1 - p_c)] + 2E}$$

If initial beliefs are correct,  $\pi_{i0} = \pi^*(k)$ , then beliefs (on average) don't change between Blocks 1 and 2. By extension, average behavior cannot change. What is more interesting is what happens when individuals don't have "rational" expectations. To be specific, suppose  $\pi_{i0} = \pi^*(0)$ . In other words, individuals have correct expectations except they fail to account for social regret. Given this assumption, Theorem 3 shows that, under weak conditions, risky gambles will be chosen less often in Block 2 for the full feedback treatment than in the no feedback treatment.

Intuitively, social regret biases choices against the risky gamble. Choosing the risky gamble makes the worst outcome more likely *and* makes it more likely that choosing the other gamble would have resulted in a better outcome. On both accounts, the risky gamble leads to greater anticipated social regret. If initial beliefs do not account for the effects of social regret, feedback shifts beliefs, on average, in favor of the less risky gamble making it more likely to be chosen in Block 2.

**Theorem 3:** Assume  $k > 0$  and  $\pi_{i0} = \pi^*(0)$ . With feedback the probability of choosing the less risky gamble,  $G$ , increases in Block 2 if the condition shown in Equation 8 holds.

$$\text{Equation 8} \quad \frac{\pi^*(0)}{1 - \pi^*(0)} > \frac{p_c(1 - p'_c)}{p'_c(1 - p_c)}$$

To interpret the condition shown in Equation 8, first note that it must hold if  $\pi^*(0) > 1/2$ . For the specific probabilities we used in our gambles, the worst case is Class 3 (see Table 1) which requires  $\pi^*(0) > .35$ . For all the other classes it is sufficient that  $\pi^*(0) > .18$ . Looking ahead to the data, for the relevant observations (Block 1 with full feedback) the less risky gamble is chosen for 60% of the observations. Even in the worst case, Class 3, the safe gamble is chosen for 44% of the observations. Theorem 3 is driven by the idea that a failure to account for social regret biases initial beliefs toward the risky outcome, but this bias would have to be enormous to reverse the prediction of fewer risky choices in Block 2.<sup>11</sup>

To ease exposition, Theorem 3 is stated in terms of specific initial beliefs,  $\pi_{i0} = \pi^*(0)$ . These are reasonable beliefs to propose, since anticipating the effect of social regret on others' choices requires a high degree of empathy by subjects. More generally, Theorem 3 holds if initial beliefs underweight the effect of social regret ( $\pi_{i0} = \pi^*(k')$  where  $0 < k' < k$ ). The result also holds for other reasonable specifications of priors. For example, if priors are drawn from a uniform distribution over the  $[0,1]$  interval and the value of  $(U_G - U_{G'})$  is drawn from a uniform distribution with mean zero, an analogous result to Theorem 3 holds. Of course, feedback does not make the less risky gamble more likely for *any* prior we might choose as already illustrated by the example of correct initial beliefs ( $\pi_{i0} = \pi^*(k)$ ).

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<sup>11</sup> If beliefs are too biased, social regret pushes individuals toward the risky gamble in Block 1. The risky gamble leads to very little anticipated social regret because the probability assigned to choice of  $G$  is so low.

It is important to note that Theorem 3 implicitly assumes that the probability of choosing the risky gamble is fixed in the absence of feedback. To the extent that this isn't true, we can interpret Theorem 3 as predicting a greater increase (or smaller decrease) in choices of the less risky gamble, G, in Block 2 with full feedback than with no feedback.

*Conformity:* Social psychology provides evidence that people feel better if they behave as others do. In particular, people maintain positive self-assessments by identifying with and conforming to the behavior of valued groups (Brewer and Roccas, 2001; Pool *et al*, 1998). Subjects in our experiments play anonymously in randomly matched groups, limiting the importance of this sort of group affiliation. Nevertheless, it is possible that social interaction effects arise simply because people like to behave as others. Our basic model can be modified to replace social regret with tastes for conformity by giving an individual additional utility,  $k/N$ , for each other group member who chooses the same as him where N is the number of other group members. Removing the social regret function and incorporating the appropriate beliefs, the following utility functions replace Equations 1 and 2 from above. The second term is the expected utility from conformity. The stochastic choice rule is constructed in the same way as above.

$$\text{Equation 9} \quad u_G^i = U_G + k\pi_{it}$$

$$\text{Equation 10} \quad u_{G'}^i = U_{G'} + k\pi'_{it}$$

It is trivial to prove that Theorems 1 and 2 continue to hold if social regret is replaced with tastes for conformity. This implies that tastes for conformity are an alternative cause for any observed social interaction effects. As above, define  $\pi^*(k)$  as the probability that G is chosen given  $\pi_{it} = \pi^*(k)$ . Theorem 3 can be modified into Theorem 4.

**Theorem 4:** Assume  $k > 0$  and  $\pi_{i0} = \pi^*(0)$ . Allowing feedback increases the probability of choosing G in Block 2 if and only if  $\pi^*(0) > 1/2$ .

Unlike Theorem 3, Theorem 4 does not imply that risky gambles are either more or less likely to be chosen over time. Instead, Theorem 4 implies a bias in favor of the majority choice *across all groups* (rather than within groups, as implied by Theorem 2). Intuitively, tastes for conformity bias individuals in favor of the more popular gamble. If this is not anticipated, more of the popular choice is observed (on average) than expected. This leads to even greater movement toward the popular choice.

*Spillovers:* With either social regret or tastes for conformity, we predict positive spillovers between pairs of gambles. Specifically, assume subjects believe (correctly) that use of risky gambles is correlated across pairs of gambles. Observing feedback in favor of the risky gamble, G', in one pair will then affect beliefs for other pairs of gambles, increasing the probability assigned to the risky gamble. The same argument used in proving Theorem 2 then applies. If a subject receives information that makes him believe the risky gamble, G', is more likely, this decreases  $\pi_{it}$ . It follows from Equations 3 and 4 that social regret for the risky gamble decreases and social regret for the safe gamble increases. By Equation 5, the probability of choosing the risky gamble increases.



Analogous arguments apply to pairs of gambles where one gamble is presented in the simple format while the other uses an ambiguous format. Observing feedback in favor of the ambiguous gamble in one pair affects beliefs for other pairs of gambles where there is a simple and an ambiguous gamble, increasing the probability assigned to the ambiguous gamble. Social regret is decreased for the ambiguous gambles in these other pairs and increased for the simple gambles, yielding an increased probability of choosing the ambiguous gamble.

#### *D: Summary of Hypotheses*

We conclude this section by summarizing our hypotheses about the experimental data.

- H1) Controlling for regression to the majority, subjects are more likely to switch gambles if the majority of the feedback disagrees with their previous choice. Models of social learning, social regret, and tastes for conformity all predict this effect.
- H2) Due to peer group effects, greater group polarization (homogeneity within groups, heterogeneity between groups) will be observed in the full feedback treatment.
- H3) If rational social learning plays a role in the observed peer group effects, the effect will be larger when the scrambled format is used for ambiguous gambles than with the blackout format.
- H4) If boundedly rational social learning plays a role in the observed peer group effects, the effect will be smallest for simple vs. simple comparisons.
- H5) If social regret plays a role in the observed peer group effects, choices in Block 2 of the full feedback treatment will be biased against the risky gamble as compared to the no feedback treatment.
- H6) If tastes for conformity play a role in the observed peer group effect, choices in Block 2 of the full feedback treatment will be biased in favor of the more popular gamble *across all groups* as compared to the no feedback treatment.
- H7) Observing feedback in favor of the risky (ambiguous) gamble in one pair will make choice of the risky (ambiguous) gamble more likely in other pairs.

The theory did not lead us to any initial hypotheses about the merge treatment. This was largely an exploratory treatment.

## **4. Results**

Table 3 summarizes choices for each pair of gambles, broken down by block and treatment. The numbers reported are proportions of choices of Gamble B, regardless of whether Gamble A was displayed on the left (Pairs 1 – 10) or right (Pairs 11 – 14).

**(Table 3 here)**

*A. Effect of Feedback:* The fundamental question is whether subjects respond to feedback about others' choices. The answer to this question can be seen in Figure 3. The red bars show data from the no feedback treatment and the blue bars show data from the full feedback treatment,

aggregated in both cases over sessions using the scrambled and blackout formats for ambiguous gambles. The top panel displays data from Block 2 and the bottom from Block 3. The x-axis sorts the data by how many of the other individuals in the group made the opposite choice from the decision maker in the previous block for the same pair of gambles. For the full feedback treatment, this indicates how much the feedback group disagrees with the decision maker's previous choice. In the no feedback treatment, the program puts the subjects into groups of six even though these groups are never used for anything. Thus, for the no feedback treatment the x-axis shows what the feedback would have been *if the decision maker had received feedback*.<sup>12</sup> The y-axis shows the proportion of decision makers who switched their decision from the same pair of gambles in the previous block (either from Gamble A to Gamble B or vice versa).

For both the no feedback and full feedback treatments in Blocks 2 and 3, there is a positive relationship between the number of others disagreeing with the previous choice and the probability of switching. For the no feedback treatment this cannot be due to any effect of feedback, but is consistent with reversion to the majority as predicted by Theorem 1. Critically, the association between the number of others disagreeing with the previous choice and the probability of switching is stronger in the full feedback treatment for both Blocks 2 and 3. This is consistent with feedback causing subjects to switch their choices as predicted by Theorem 2. The effect of feedback on switching is stronger in Block 2 than in Block 3.

**(Table 4 here)**

The regressions shown in Table 4 put the preceding points on firmer ground. These are linear probability models. An observation is a single choice in a pair of gambles. The dependent variable is a dummy for whether the subject switched their choice from the same pair of gambles in the previous block. The first two lines of the table tell which feedback treatment (full or no feedback) and block of periods the data is being drawn from. Data is combined from sessions with the scrambled and blackout formats for ambiguous gambles.

As Figure 3 shows, reversion to the majority induces correlation between switching and feedback even when no actual feedback is present. To control for this problem, all models on Table 4 include fixed effects for the pair of gambles interacted with dummies for sessions where the blackout format was used and which gamble was chosen for the same pair of gambles in the previous block. This gives us a total of 52 fixed effects (the regressions do not include a constant and the blackout treatment only used twelve of the fourteen pairs). Parameter estimates for the fixed effects are not displayed on Table 4 or subsequent tables, since they are of no direct interest. The fixed effects always achieve joint statistical significance at the 1% level.

The right hand variable of interest is “Others Disagreed.” This gives how many other individuals in the group made the opposite choice as the decision maker did in the previous block for the same pair of gambles. We expect the parameter estimate to be positive for this variable – increasing the amount of feedback that disagrees with my previous choice makes me less likely to switch.

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<sup>12</sup> For the no feedback treatment, we did not force participation in multiples of six. This leaves a few subjects who are not assigned to a group by the software. For the purposes of Figures 3, 4, and 6 as well as Model 3 on Table 4, their data is discarded.

Our dataset includes multiple observations from the same individuals. These are not independent observations. In the full feedback treatment we also cannot treat choices from the same group as being independent. The regression model controls for feedback received for the same pair of gambles, but not for feedback received for other pairs of gambles. If there are spillovers between pairs of gambles, observations from different individuals in the same group will not necessarily be independent. We therefore correct the standard errors for clustering at the individual level in the no feedback treatment and at the group level in the full feedback treatment.

Model 1 tests for the effect of feedback in Block 2. The parameter estimate for feedback is positive, as expected, and statistically significant at the 1% level. Model 2 replicates Model 1 using data from Block 3. The magnitude of the estimated feedback effect is roughly halved, with the difference between the feedback effects for Blocks 2 and 3 being statistically significant at the 10% level.<sup>13</sup> It isn't surprising that the responsiveness to feedback is lower in Block 3. Feedback for Blocks 2 and 3 is highly correlated, so Block 3 feedback generally is not news to the subjects. Their response will already be built into their choice from Block 2.

Model 3 is a specification check. We claim that the inclusion of fixed effects controls for reversion to the majority, allowing us to estimate the effect of feedback. As a test of this claim, Model 3 replicates Model 1 using data from the no feedback treatment. The parameter estimate for "Others Disagreeing" is small and does not approach statistical significance at any standard level. This indicates that the fixed effects effectively control for reversion to the majority.

Model 4 uses the same dataset as Model 1, but replaces "Others Disagreed" with dummies for the specific values of this variable (zero is the omitted category). This allows us to test whether the response to feedback is linear. The results indicate that it is not. There are large jumps in the probability of switching when a majority disagreed (Others Disagreed  $\geq 3$ ) and when disagreement is unanimous (Others Disagreed = 5).

Model 4 gives us a clean measure of the magnitude of feedback effects. Not only are these effects statistically significant, they are also large in magnitude. For Block 2, moving from unanimous agreement to unanimous disagreement increases the chance of a switch by 33 percentage points. Moving from two others disagreeing to three others disagreeing increases the chance of a switch by 11 percentage points and switching from one other agreeing to unanimous disagreement increases the chance of a switch by 17 percentage points. To fully appreciate the economic significance of these effects, note that the overall probability of a switch in Block 2 with full feedback is only 21%.

Model 4 establishes that the effect of feedback is non-linear, with an obvious (and statistically significant) breakpoint at majority disagreement. Consequently, from this point forward we investigate the effect of feedback through regressions where the primary independent variable is a dummy for whether the majority disagreed with the choice of the decision maker in the previous block for the same pair of gambles. The basic version of this regression is shown in

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<sup>13</sup> This result comes from a single regression combining data for blocks 2 and 3 with all the variables in Model 1 being interacted with a dummy for the block. The parameter estimate for the difference in feedback effects is .033, consistent with the results of Models 1 and 2, with a standard error of .019.

Model 5 – majority disagreement increases the likelihood of a switch by 15 percentage points compared to majority agreement. Use of this specification greatly simplifies the exposition without qualitatively affecting our results.

*Conclusion 1: Subjects respond to feedback, confirming H1. This feedback effect is stronger in Block 2 than in Block 3 and cannot be attributed to reversion to the majority. Majority disagreement and unanimous disagreement are significant breakpoints in the likelihood of a switch. The magnitude of feedback effects are large relative to the likelihood of a switch occurring.*

One implication of the feedback effects documented above is that there should be greater group polarization with feedback. In other words, in Blocks 2 and 3 there should be more heterogeneity between groups and more homogeneity within groups. Both statements are true for our data. As a simple method of analyzing heterogeneity between groups, we have calculated the number of subjects in each group choosing Gamble B. The standard deviation of this statistic is a good measure of the heterogeneity between groups. Aggregating across all fourteen pairs, the standard deviation of the number of subjects in a group choosing Gamble B is virtually identical for the no and full feedback treatments in Block 1: 1.69 versus 1.78. In Blocks 2 and 3, this standard deviation becomes substantially higher in the full feedback treatment: 1.54 versus 1.95 for Block 2 and 1.53 versus 1.97 for Block 3.

**(Figure 4 here)**

To establish that feedback increases the homogeneity within groups, we examine majority sizes (e.g. the number of subjects choosing the more popular option within a group). Figure 4 shows the distribution of majority sizes broken down by block and feedback (no feedback versus full feedback). Since there are six members in each group, the majority size must range between 3 and 6. Data is aggregated over all fourteen pairs of gambles. In Block 1, the distribution of majority sizes is similar for the no feedback and full feedback treatments. In Block 2, large majorities are more frequent with full feedback and unanimity is more than 2½ times more likely. The difference between the treatments is even more obvious in Block 3.

*Conclusion 2: Feedback increases group polarization, confirming H2: there is greater heterogeneity between groups and greater homogeneity within groups.*

B. *Social Learning:* As described in Section 3, the observed feedback effects could reflect social learning rather than social interaction effects. The regressions shown in Table 5 are designed to separate these causes. The dataset and basic specification for the regressions on Table 5 are identical to those used for Model 5 on Table 4. Model 5 from Table 4 is repeated as Model 1 on Table 5 as a point of comparison.

**(Table 5 here)**

Model 2 compares the effect of feedback in pairs of gambles with no ambiguity to pairs of gambles with ambiguity. We do this by constructing a “No ambiguity” dummy for pairs of gambles with no ambiguity (i.e. both gambles are in the simple format) and an “Ambiguity”

dummy for pairs of gambles where at least one of the gambles was in an ambiguous format. The resulting two dummies are interacted with “Majority Disagree”. Model 2 shows that the effect of majority disagreement is slightly larger in pairs of gambles with no ambiguity, but this difference is not statistically significant. Recall that we predicted the effect of feedback to be smaller in pairs of gambles with no ambiguity if boundedly rational social learning played a role in the feedback effects. We therefore reject H4.

Model 3 breaks down the feedback effect further, distinguishing between pairs of gambles using the blackout format and pairs of gambles using the scrambled format for the presentation of ambiguous gambles. We do this by constructing a “scrambled” and a “blackout” dummy and interacting these with the “Ambiguity” and the “Majority Disagree” dummies. Model 3 finds that the estimated effect of majority disagreement is somewhat larger in pairs of gambles using the blackout format compared to pairs of gambles using the scrambled format, but the difference isn’t statistically significant. We predicted the effect of feedback to be larger in gambles using the scrambled format if rational social learning played a role in the feedback effect, but based on Model 3, H3 is rejected.

*Conclusion 3: Feedback effects in our data are not consistent with either variety of social learning. We reject H3 and H4.*

*C. Feedback and Choice of Risky and Popular Gambles:* This section addresses whether allowing feedback moves subjects away from riskier gambles and toward more popular gambles, averaging across groups in both cases. As shown in Theorems 3 and 4, the existence or absence of these effects helps us understand the roles of social regret and tastes for conformity in generating the observed peer group effects.

**(Figure 5 here)**

The top panel of Figure 5 shows how the proportion of subjects choosing the risky gamble changes over time in the no feedback and full feedback treatments. The middle panel shows the proportion of subjects in the no feedback and full feedback treatments choosing the more popular gamble, defined as the gamble chosen by the majority of subjects *across all groups*.<sup>14</sup> All panels use the same scale on the vertical axis to make comparisons easier.

The top panel shows that more subjects choose the risky gamble over time in the no feedback treatment, but this proportion holds steady in the full feedback treatment. This causes a growing difference between the two treatments in the proportion of subjects choosing the risky gamble. The observed differences in choice of the riskier gamble for Blocks 2 and 3 are consistent with H5, and hence support the role of social regret in generating the observed peer group effects.

The middle panel shows that the proportion of subjects choosing the more popular gamble, averaging across groups, is basically flat in both the no feedback and full feedback treatments. While popular gambles do become slightly more popular over time in the full feedback treatment, the effect is tiny and doesn’t appear until Block 3. As such the data gives little

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<sup>14</sup> To be specific, the data was divided into cells by pair of gambles, format used for ambiguity (scrambled or blackout), and block. The popular gamble was then determined for each cell.

support for H6. This suggests that tastes for conformity do not play an important role in generating the observed peer group effect.

**(Table 6 here)**

The regressions shown on Table 6 confirm our impressions from Figure 5. Once again, these are linear probability models. In Models 1 and 2 the dependent variable is a dummy for what type of gamble was chosen in Block 2, specifically choice of the risky gamble in Model 1 and choice of the popular gamble (as defined above) in Model 2. The dataset for all three models is all data from Block 2 for the no feedback and full feedback treatments, pooling data from both formats for ambiguous gambles.

All models include fixed effects for the pair of gambles interacted with dummies for sessions where the blackout format was used and which gamble was chosen for the same pair of gambles in the previous block. The dependent variable of interest in Models 1 and 2 is a dummy for the full feedback treatment. A positive (negative) estimate for this parameter indicates that subjects are more (less) likely to choose the risky/popular gamble in Block 2 of the full feedback treatment, controlling for the choice made in Block 1. The standard errors are corrected for clustering at group level in the full feedback treatment and at the individual level in the no feedback treatment.

In Model 1 the parameter estimate for “Full Feedback” is negative and statistically significant. Running analogous regressions for Block 3 we find no significant effects – virtually all of the effect of full feedback plays out in Block 2. This non-result follows from the logic underlying Theorem 3. Theorem 3 predicts (relative) movement away from the risky gamble with full feedback because individuals who don’t take social regret into account when forming beliefs will, on average, be surprised by observing fewer risky choices in the feedback than expected. Given that feedback is highly correlated across blocks, there is little reason for them to be surprised a second time in Block 3.

In Model 2 the parameter estimate for “Full Feedback” is positive but tiny and nowhere close to statistical significance. The standard error is smaller than the standard error for “Full Feedback” in Model 1, so the failure to find a significant effect cannot be attributed to a lack of precision in the estimate.

If full feedback shifts the choice between simple and ambiguous gambles, there is a possible confound in Models 1 and 2. By construction, risk and ambiguity are orthogonal in our dataset. This implies the result reported in Model 1 will not change if we control for ambiguity, but we’d prefer to have direct evidence rather than relying on a theoretical argument. If full feedback shifts subjects toward ambiguous (simple) gambles and ambiguous (simple) gambles are relatively unpopular, a failure to control for ambiguity could lead to a false negative in Model 2.

Model 3 addresses these concerns by measuring the effect of full feedback on shifting play away from the risky gamble while controlling for any shifts vis-à-vis popular and ambiguous gambles. The dependent variable for Model 3 is a dummy for whether the subject switched their choice from the same pair of gambles in the previous block. The independent variables need to be

transformed to reflect this change in the dependent variable from Models 1 and 2. The first independent variable (other than the usual fixed effects) is a dummy for full feedback multiplied by an indicator variable that takes on a value of 1 if the less risky gamble was chosen for the same pair of gambles in the previous block and a value of -1 if the riskier gamble was chosen. The parameter estimate for this new variable, labeled “Full Feedback Shift to Risky,” captures the effect of full feedback in shifting play toward the riskier gamble controlling for the choice in the previous gamble. If this is the only independent variable other than the fixed effects, the parameter estimate and standard error must be identical to those shown in Model 1 for “Full Feedback.” The second independent variable (“Full Feedback Shift to Majority”) is constructed in an analogous manner for popular gambles. A dummy for full feedback is interacted with an indicator variable that takes on a value of 1 (-1) if the less (more) popular gamble was chosen for in the previous. This measures the effect of full feedback in shifting play toward the more popular gamble. If it is the sole independent variable, we get identical results to Model 2. A final independent variable (“Full Feedback Shift to Ambiguity”) equals a dummy for full feedback multiplied by a dummy for pairs of gambles comparing simple and ambiguous gambles multiplied by an indicator variable that takes on a value of 1 (-1) if the simple (ambiguous) gamble was chosen for the same pair of gambles in the previous block. This estimates the effect of full feedback in shifting play toward the ambiguous gamble when both a simple and an ambiguous gamble are available.

The results of Model 3 indicate that our conclusions from Models 1 and 2 are robust. The parameter estimates for shifts toward riskier and popular gambles with full feedback are virtually unchanged with the addition of controls for ambiguity. The regression also shows a significant shift away from ambiguous gambles with full feedback, which can also be seen in the bottom panel of Figure 5. This result does not help us sort out the causes of peer group effects, but is an interesting feature of the data that merits note.

*Conclusion 4: Full feedback leads to less choice of the riskier gamble with experience. This result is consistent with H5, suggesting that social regret plays a role in generating the observed peer group effects. Full feedback does not cause popular gambles to become more popular over time, contrary to H6. This is inconsistent with tastes for conformity playing an important role in generating the observed peer group effects.*

*D. Spillovers:* Thus far we have documented direct feedback effects, where information about play for the *same* pair of gamble in previous blocks affects current decisions. Indirect effects may exist as well, where information about play for *other* pairs of gambles affects current decisions. We use measures of group risk and ambiguity “cultures” based on Block 1 choices to examine this possibility. For each individual and each pair of gambles in Block 1, the measure of risk culture calculates the average number of other individuals in the same group choosing the risky gamble for *other* pairs of gambles in Block 1.<sup>15</sup> Likewise, the measure of ambiguity culture calculates the average number of other individuals in the same group choosing the ambiguous gamble for *other* pairs of gambles in Block 1 that offer a simple and ambiguous gamble (i.e. Pairs 3, 4, 7, and 8).

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<sup>15</sup> For ambiguous gambles displayed in the blackout format, the designation of the riskier gamble is based on the observed standard deviation of payoffs rather than the true (unobserved) value of this statistic.

We study the effects of risk and ambiguity culture on choices in Block 3 rather than Block 2 because subjects have received feedback about all pairs of gambles in Block 3 while in Block 2 they have only received feedback about some pairs. This lets us avoid controlling for exactly which pairs of gambles have been seen previously and lessens concerns about cultural effects being cumulative – it is difficult to imagine any cultural effects in the first few rounds on Block 2 when subjects have little experience with feedback from other gambles.

**(Figure 6 here)**

Figure 6 illustrates the relationship between risk and ambiguity culture in Block 1 and switches between Blocks 1 and 3. We split data from the full feedback treatment by whether the observation has below or above average risk/ambiguity culture. The bars on the left show the (net) proportion of observations switching to the risky gamble between Blocks 1 and 3. There is a positive relationship between the risk culture in Block 1 and the probability of switching to the risky gamble between Blocks 1 and 3. In other words, subjects are more likely to switch to a risky gamble between Blocks 1 and 3 if more risky choices by other subjects have been observed for *other* pairs of gambles in Block 1. The bars on the right show the (net) proportion switching to the ambiguous gamble between Blocks 1 and 3. This is based on observations for pairs where subjects choose between gambles in the simple and ambiguous formats. Once again there is a positive, albeit weaker, relationship between culture in Block 1 and the likelihood of switching between Blocks 1 and 3. Subjects are more likely to switch to an ambiguous gamble between Blocks 1 and 3 if more ambiguous choices by other subjects have been observed for *other* pairs of gambles in Block 1 where simple and ambiguous gambles were available.

Two problems in measuring the impact of risk and ambiguity culture necessitate the use of regression analysis. First, regression to the majority is an issue again. Because pairs of gambles that are more likely than average to have the risky (ambiguous) gamble chosen must have also less than average measures of risk (ambiguity) culture,<sup>16</sup> regression to the majority partially counteracts the effects of risk (or ambiguity) culture. It follows that Figure 5 systematically understates the effects of risk and ambiguity culture. In addition, if there are individual effects in the choice of risky or ambiguous gambles, the measures of risk and ambiguity culture are correlated with the feedback on Block 1 choices for the *same* pair of gambles. Thus, the apparent effect of risk or ambiguity culture could be due to omitted variable bias rather than any true causal relationship.

**(Table 7 here)**

The regressions shown in Table 7 address these issues. Once again these are linear probability models. The dataset for Model 1 is all choices in Block 3 from the full feedback treatment, while Model 2 uses the subset of choices that compare a simple and ambiguous gamble. The dependent variable in Model 1 is a dummy for whether the risky gamble was chosen. In Model 2 it is a dummy for choice of the ambiguous gamble. As in Table 4, both regressions on Table 5 include fixed effects for the pair of gambles interacted with dummies for sessions where the blackout format was used and which gamble was chosen for the same pair of gambles in the

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<sup>16</sup> Risk culture is based on choices in the other pairs. If the current pair leads to above average usage of the risky gamble, the remaining pairs must have below average usage of the risky gamble.



previous block. This controls for regression to the majority. Both regressions control for the feedback about Block 1 choices for the same pair of gambles, eliminating omitted variable bias as a cause of the estimated spillover effects. To match the dependent variable, the independent variable is whether the majority of choices in the Block 1 feedback were for the risky (ambiguous) gamble, not whether the majority disagreed with the subject's previous choice for the same pair of gambles. Standard errors are corrected for clustering at the group level.

The parameter estimate in Model 1 for Block 1 risk culture (defined as the average number of *other* individuals in the group choosing the risky gamble for *other* pairs of gambles in Block 1) is positive and statistically significant at the 5% level. The size of the effect is large given that only 23% of subjects change choices between Blocks 1 and 3. In Model 2, the parameter estimate for Block 1 ambiguity culture is positive, but small and not statistically significant. This lack of statistical significance stems in part from having only a third as many observations for Model 2 as in Model 1. Nonetheless Model 2 deepens our impression from Figure 6 that spillover effects in our experiment are weaker for choices between simple and ambiguous gambles than for choices between safe and risky gambles.<sup>17</sup>

*Conclusion 3: Choice of risky gambles responds to feedback about others' choice of risky gambles for other pairs of gambles in previous blocks. In other words, there are positive spillover effects, confirming H7 for risky gambles. There are also positive spillover effects for ambiguous gambles, but weaker than for risky gambles and not statistically significant. The data is consistent with H7 for ambiguous gambles but far from a confirmation.*

**(Table 6 here)**

*E: Feedback and Group Mergers:* Data from the merge treatment allows us to address two questions: (1) how does the effect of feedback vary when the quantity of feedback is changed and (2) following the merge, is the effect of feedback from a subject's own subgroup more or less than the effect of feedback from the other subgroup?

**(Table 8 here)**

The regressions in Models 1 and 2 on Table 8 use data from Block 2 of the merge treatment to address the first of these questions. Both models include fixed effects for the pair of gambles interacted with a dummy for the choice in the same pair of gambles in the previous block. The dependent variable is a dummy for whether the subject switched choices from the same pair of gambles in the previous block. Standard errors are corrected for clustering at the subgroup level.

In Model 1, the independent variable of interest is how many others in my subgroup disagreed with the subject's choice for the same pair of gambles in the previous block. There are only three subjects in each subgroup, so this variable ranges between 0 and 2. The parameter estimate for "My Subgroup Disagreed" is positive and statistically significant at the 1% level. Model 2

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<sup>17</sup> This relative weakness may be a function of how the experiment is constructed. Pairs where subjects choose between simple and ambiguous gambles are interspersed with pairs that force the subjects to choose a simple gamble and pairs that force choice of an ambiguous gamble. As such, it is relatively difficult for subjects to notice that one sort of gamble is generally being chosen when both are available.

allows for non-linearity in the response to feedback, including dummies for “My Subgroup Disagreed” equal to 1 and 2 (0 is the excluded category). The response to feedback is non-linear, with a large break between unanimous agreement and one other subject disagreeing. Model 2 gives us a direct measure of the size of the feedback effect – the likelihood of switching gambles is reduced by 18.4% with unanimous agreement as opposed to unanimous disagreement.

Comparing the results of Models 1 and 2 with the analogous regressions for the full feedback treatment (Models 1 and 4 on Table 4), the response to an individual piece of feedback is about 50% larger in the merge treatment but the total response to feedback is 40% smaller since there are fewer pieces of feedback. These results are consistent with a decreasing marginal effect of feedback. If feedback affects choices through its effect on beliefs, any reasonable model of updating predicts decreasing marginal effects of information.

Models 3 and 4 use data from Block 3 of the merge treatment to examine whether feedback from the two subgroups is treated differently. In Model 3, the dependent variable is a dummy for whether the subject switched choices from the same pair of gambles in Block 2. This model includes fixed effects for the pair of gambles interacted with dummies for the choice in the same pair of gambles in the previous block. There are two independent variables beyond the fixed effects in Model 3: the number of other individuals in my subgroup whose Block 2 choices disagreed with mine and the number of individuals in the *other* subgroup whose Block 2 choices disagreed with mine. Standard errors are corrected for clustering at the group level.

The estimate for “My Subgroup Disagreed” is relatively small in Model 3 and not statistically significant. As with full feedback, feedback from previously observed subjects has less than half the effect in Block 3 as in Block 2. The parameter estimate on “Other Subgroup Disagreed” is somewhat larger and statistically significant at the 1% level. The difference between the two parameter estimates is not statistically significant. It is unsurprising that new feedback has a larger effect on changes in behavior between Blocks 2 and 3; the feedback from an individual’s own subgroup is highly correlated between Blocks 2 and 3, so most of the impact from this feedback has already been incorporated into Block 2 choices. Note that the marginal effect of new feedback from the other subgroup in Block 3 is smaller than the marginal effect of feedback from my own subgroup in Block 2, consistent with a decreasing marginal impact of feedback.

Model 4 compares the overall effects of feedback from an individual’s own subgroup and the other subgroup. The dependent variable is a dummy for whether the subject switched choices from the same pair of gambles in *Block 1*. The fixed effects are now for the pair of gambles interacted with dummies for the *Block 1* choice. Independent variables (beyond the fixed effects) are the average over Blocks 1 and 2 of how many other individuals in the decision maker’s subgroup disagreed with his Block 1 choice and the number of individuals in the other subgroup whose Block 2 decisions disagreed with the decision maker’s Block 1 choice.

The results of Model 4 contrast with those of Model 3. The estimate for “Average My Subgroup Disagreed” is larger than the estimate for “Other Subgroup Disagreed”. This suggests that subjects respond more strongly to feedback from their own subgroup, but we don’t want to oversell the results from Model 4 since the difference between the parameters isn’t statistically significant. In the future we hope to expand on the exploratory work presented here to

definitively establish whether subjects respond more strongly to feedback from their own subgroup and to determine the cause of any such effect.<sup>18</sup> If we can confirm that subjects respond more strongly to feedback from their own group, it would follow that overcoming existing norms will be relatively difficult in merged groups.

*Conclusion 7: The total effect of feedback is reduced in smaller groups, but the marginal impact of each piece of feedback is larger. Average feedback from one's own subgroup has a larger marginal impact on changes between Blocks 1 and 3 than feedback from the other subgroup, although this difference is not statistically significant.*

## **5. Discussion**

This paper presents an experimental investigation of social interaction effects in settings that involve choice under risk and (Knightian) uncertainty. We document large and significant peer group effects. Controlling for reversion to the mean, the likelihood of switching gambles increased by 15 percentage points if the majority of the feedback disagreed with one's choice in the previous period. This is a large effect since the overall frequency of switching is only 20 percentage points.

A central goal of our paper is to use the tightly controlled environment available in the laboratory to cleanly identify the observed peer group effects as being due to social interaction effects. Alternative causes such as omitted variable bias, knowledge spillovers, and social learning are eliminated through the experimental design. By process of elimination, social interaction effects are the likely cause of the observed peer group effects. This is direct evidence for the existence of social interaction effects and shows that, independent of other causes for peer group effects, social interaction effects can generate strong peer group effects for choices under risk and uncertainty.

Comparing theoretical predictions with the experimental data, the observed social interaction effects are consistent with preferences that include social regret but not with tastes for conformity. This result must be interpreted with care. Some important causes of social interaction effects, such as social norms, group affiliation, and payoff complementarities, have been eliminated through the experimental design. It is also likely that the environment being studied plays a role in determining the sources of any social interaction effects. For example, Falk and Ichino (2006) provide clean evidence of social interaction effects in work effort. In their experiment subjects stuff envelopes either working alone or in the presence of another subject. Subject pay is independent of work effort, yet partnered workers respond to the other subject's effort. Given the lack of consequences for the subjects, social norms or tastes for conformity seem like more plausible mechanisms for the observed social interaction effects than social regret. Our work identifies social regret as a potentially powerful source of social interaction effects, particularly in environments where risk and uncertainty play an important role, but further work is needed to determine the relationship between environments and the causes of social interaction effects.

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<sup>18</sup> Possible causes include group identity, better estimates of own subgroup's likely decisions, and overweighting of evidence from small groups.

An obvious doubt about our results, as for many results from lab experiments, is that the observed peer group effects could reflect a demand induced effect. In other words, subjects feel obligated to respond to the information that the experimenters have given them. Several factors argue against this. First, subjects observe their own past choices in addition to information about others' past choices. The argument for demand induced effects therefore requires subjects to feel an obligation to use *all* the information they are given, a much tougher standard. Second, if demand induced effects exist in our experiment and explain the observed peer group effects, they must make the subjects want to do what others have done. Even though this occurs because the subjects want to please the experimenter rather than having a taste for acting like others, the predicted effect is identical to the effect of having tastes for conformity. As has been established above, the data isn't consistent with this story. Finally, we observe strong spillover effects for the choice of risky gambles. This is a subtle effect that doesn't square well with demand induced effects as the driving force.

The results of our paper suggest a number of interesting avenues for further investigation. The strong peer group effects in our data lead to increased group polarization – with feedback there is greater homogeneity within groups and greater heterogeneity between groups. This becomes especially interesting when considered in conjunction with the spillovers observed between pairs of gambles. Together these observations imply that local cultures of risk taking or avoidance will emerge which encompass a broad range of activities as well as very specific behaviors of the type usually examined in field studies. For example, there are substantial geographic differences in people's saving behavior in Norway. In some municipalities people take substantial risk having more than 90 percent of their private investments in relatively risky mutual funds, while in other municipalities people have less than 40 percent in mutual funds.<sup>19</sup> The greater part of this geographic variance cannot be explained by variation in income, wealth or education. Mutual fund managers in Norway have started to talk about *cultures of saving behavior*. Beyond establishing that these local cultures are due to social interaction effects, we think it would be especially interesting to see if these cultural differences spill over into other risk taking behavior, either in the financial sphere (e.g. decisions to purchase insurance) or even into more general everyday decision making (e.g. getting preventive checks like mammograms or colonoscopies).

The important role social regret plays in our data has interesting implications for trading in financial markets. Apparent behavioral abnormalities in financial markets such as stock market crashes may be partially due to social regret. Consider the snowballing that occurs in a stock market crash (Shiller 1989, Kindleberger 2000). Initially there is a piece of bad news that causes many people holding stocks to sell. This is a *direct* response to the change in fundamentals. However, if preferences include anticipated social regret, then there will also be an *indirect* effect of the change in fundamentals. As investors observe their peers selling stocks, their anticipated social regret rises. For investors already on the margin of selling, this triggers additional sales that are not caused by changes in the relevant fundamentals for their portfolio. Iterating this process, the reaction to negative news can far outstrip the expected effect based on fundamentals. If financial bubbles are driven by the theory of the greater fool, we argue that financial crashes may be driven by a fear of being the greater fool. Unlike models that rely on social learning (e.g. Avery and Zemsky, 1998), in our model a crash could not have been avoided

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<sup>19</sup> The numbers refer to the top and bottom vingtiles. Thank you to the Norwegian Mutual Fund Association for providing us with these numbers ([www.vff.no](http://www.vff.no)).

if people were rational and had perfect information about fundamentals. We admit that this line of reasoning is speculative (pun intended), but feel there is much to be gained by exploring such possibilities.

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**Table 1**  
**Description of Gambles**

| Pair Number | Class | Gamble A    |             |           | Gamble B    |             |           |
|-------------|-------|-------------|-------------|-----------|-------------|-------------|-----------|
|             |       | EV          | StDev       | Ambiguous | EV          | StDev       | Ambiguous |
| 1           | 1     | 10.0        | 0.0         |           | 12.0        | 12.0        |           |
| 2           | 2     | 10.0        | 0.0         |           | 12.0        | 36.0        |           |
| 3           | 3     | 12.0        | 12.0        |           | 13.2 (18.0) | 26.8 (32.7) | X         |
| 4           | 4     | 8.4         | 11.4        |           | 7.2 (12.0)  | 26.4 (33.2) | X         |
| 5           | 3     | 12.0 (12.0) | 12.0 (12.0) | X         | 13.2 (18.0) | 26.8 (32.7) | X         |
| 6           | 4     | 8.4 (6.0)   | 11.4 (10.4) | X         | 7.2 (12.0)  | 26.4 (33.2) | X         |
| 7           | 5     | 12.0        | 31.5        |           | 13.2 (12.0) | 11.9 (12.0) | X         |
| 8           | 6     | 8.4         | 26.6        |           | 7.2 (8.0)   | 11 (11.3)   | X         |
| 9           | 5     | 12.0 (16.0) | 31.5 (39.6) | X         | 13.2 (12.0) | 11.9 (12.0) | X         |
| 10          | 6     | 8.4 (2.0)   | 26.6 (6.6)  | X         | 7.2 (8.0)   | 11 (11.3)   | X         |
| 11          | 5     | 12.0        | 31.5        |           | 13.2        | 11.9        |           |
| 12          | 6     | 8.4         | 26.6        |           | 7.2         | 11.0        |           |
| 13          | 3     | 12.0        | 12.0        |           | 13.2        | 26.8        |           |
| 14          | 4     | 8.4         | 11.4        |           | 7.2         | 26.4        |           |

Notes: Numbers in parentheses give expected values and standard deviations observed by subjects for ambiguous gambles in the blackout treatment. Pairs 11 - 14 were displayed with Gamble B on the left and Gamble A on the right, but are displayed here in the same order as other pairs in the same class to ease the exposition.

**Table 2**  
**Summary of Treatments**

| Session | Feedback |      |       | Form of Ambiguity |          | # Subjects | Gambles |
|---------|----------|------|-------|-------------------|----------|------------|---------|
|         | None     | Full | Merge | Scrambled         | Blackout |            |         |
| 1       |          | X    |       | X                 |          | 24         | 1 - 10  |
| 2       |          | X    |       | X                 |          | 18         | 1 - 10  |
| 3       |          | X    |       | X                 |          | 24         | 3-12    |
| 4       |          |      | X     | X                 |          | 24         | 3-12    |
| 5       |          |      | X     | X                 |          | 24         | 3-12    |
| 6       |          |      | X     | X                 |          | 24         | 3-12    |
| 7       | X        |      |       | X                 |          | 20         | 1 - 10  |
| 8       | X        |      |       | X                 |          | 22         | 1 - 10  |
| 9       | X        |      |       | X                 |          | 25         | 3-12    |
| 10      |          | X    |       |                   | X        | 24         | 3-12    |
| 11      |          | X    |       |                   | X        | 18         | 3-12    |
| 12      |          | X    |       |                   | X        | 18         | 3-12    |
| 13      | X        |      |       |                   | X        | 16         | 3-12    |
| 14      | X        |      |       |                   | X        | 21         | 3-12    |
| 15      | X        |      |       |                   | X        | 8          | 3-12    |
| 16      | X        |      |       |                   | X        | 16         | 3-12    |

**Table 3**  
**Summary of Raw Choices (Proportion of Gamble B Choices)**

Block 1

| Gamble Pair | Feedback/Form of Ambiguity |                |               |               |                 |
|-------------|----------------------------|----------------|---------------|---------------|-----------------|
|             | None/Scrambled             | Full/Scrambled | None/Blackout | Full/Blackout | Merge/Scrambled |
| 1           | 0.262                      | 0.429          | ---           | ---           | ---             |
| 2           | 0.238                      | 0.238          | ---           | ---           | ---             |
| 3           | 0.597                      | 0.561          | 0.623         | 0.600         | 0.597           |
| 4           | 0.224                      | 0.333          | 0.754         | 0.583         | 0.403           |
| 5           | 0.552                      | 0.561          | 0.689         | 0.700         | 0.556           |
| 6           | 0.418                      | 0.409          | 0.672         | 0.700         | 0.479           |
| 7           | 0.716                      | 0.636          | 0.836         | 0.900         | 0.611           |
| 8           | 0.493                      | 0.424          | 0.918         | 0.867         | 0.417           |
| 9           | 0.821                      | 0.803          | 0.475         | 0.583         | 0.806           |
| 10          | 0.433                      | 0.470          | 0.738         | 0.817         | 0.569           |
| 11          | 0.760                      | 0.875          | 0.869         | 0.883         | 0.708           |
| 12          | 0.680                      | 0.667          | 0.721         | 0.817         | 0.444           |
| 13          | 0.480                      | 0.500          | 0.590         | 0.417         | 0.486           |
| 14          | 0.320                      | 0.083          | 0.180         | 0.067         | 0.264           |

Block 2

| Gamble Pair | Feedback/Form of Ambiguity |                |               |               |                 |
|-------------|----------------------------|----------------|---------------|---------------|-----------------|
|             | None/Scrambled             | Full/Scrambled | None/Blackout | Full/Blackout | Merge/Scrambled |
| 1           | 0.452                      | 0.524          | ---           | ---           | ---             |
| 2           | 0.262                      | 0.214          | ---           | ---           | ---             |
| 3           | 0.552                      | 0.485          | 0.639         | 0.483         | 0.542           |
| 4           | 0.299                      | 0.136          | 0.836         | 0.567         | 0.250           |
| 5           | 0.657                      | 0.561          | 0.770         | 0.817         | 0.639           |
| 6           | 0.358                      | 0.379          | 0.656         | 0.762         | 0.458           |
| 7           | 0.597                      | 0.561          | 0.787         | 0.786         | 0.556           |
| 8           | 0.448                      | 0.470          | 0.869         | 0.917         | 0.313           |
| 9           | 0.791                      | 0.879          | 0.508         | 0.517         | 0.819           |
| 10          | 0.463                      | 0.576          | 0.852         | 0.850         | 0.569           |
| 11          | 0.800                      | 0.875          | 0.918         | 0.867         | 0.708           |
| 12          | 0.520                      | 0.667          | 0.656         | 0.750         | 0.528           |
| 13          | 0.680                      | 0.625          | 0.557         | 0.450         | 0.667           |
| 14          | 0.320                      | 0.167          | 0.148         | 0.117         | 0.208           |

Block 3

| Gamble Pair | Feedback/Form of Ambiguity |                |               |               |                 |
|-------------|----------------------------|----------------|---------------|---------------|-----------------|
|             | None/Scrambled             | Full/Scrambled | None/Blackout | Full/Blackout | Merge/Scrambled |
| 1           | 0.429                      | 0.500          | ---           | ---           | ---             |
| 2           | 0.333                      | 0.190          | ---           | ---           | ---             |
| 3           | 0.552                      | 0.530          | 0.604         | 0.500         | 0.625           |
| 4           | 0.224                      | 0.106          | 0.721         | 0.567         | 0.250           |
| 5           | 0.642                      | 0.515          | 0.820         | 0.850         | 0.653           |
| 6           | 0.388                      | 0.364          | 0.820         | 0.786         | 0.458           |
| 7           | 0.642                      | 0.576          | 0.852         | 0.900         | 0.528           |
| 8           | 0.522                      | 0.439          | 0.869         | 0.883         | 0.306           |
| 9           | 0.761                      | 0.864          | 0.492         | 0.617         | 0.792           |
| 10          | 0.537                      | 0.548          | 0.852         | 0.883         | 0.500           |
| 11          | 0.600                      | 0.792          | 0.902         | 0.883         | 0.694           |
| 12          | 0.520                      | 0.583          | 0.705         | 0.817         | 0.486           |
| 13          | 0.560                      | 0.667          | 0.590         | 0.500         | 0.681           |
| 14          | 0.320                      | 0.083          | 0.066         | 0.067         | 0.264           |

**Table 4**  
**Linear Probability Models: Switching Responds to Feedback**

|                    | Model 1                       | Model 2                      | Model 3        | Model 4                       | Model 5                       |
|--------------------|-------------------------------|------------------------------|----------------|-------------------------------|-------------------------------|
| Treatment          | Full Feedback                 | Full Feedback                | No Feedback    | Full Feedback                 | Full Feedback                 |
| Block              | 2                             | 3                            | 2              | 2                             | 2                             |
| Subjects/Obs       | 126/1392                      | 126/1386                     | 108/1452       | 126/1392                      | 126/1392                      |
| Others Disagreed   | .058 <sup>***</sup><br>(.015) | .025 <sup>**</sup><br>(.012) | .007<br>(.015) |                               |                               |
| 1 Disagreed        |                               |                              |                | -.002<br>(.028)               |                               |
| 2 Disagreed        |                               |                              |                | .049<br>(.038)                |                               |
| 3 Disagreed        |                               |                              |                | .154 <sup>***</sup><br>(.038) |                               |
| 4 Disagreed        |                               |                              |                | .160 <sup>**</sup><br>(.065)  |                               |
| 5 Disagreed        |                               |                              |                | .330 <sup>***</sup><br>(.096) |                               |
| Majority disagreed |                               |                              |                |                               | .153 <sup>***</sup><br>(.033) |

Notes: Standard errors are corrected for clustering at the individual level in the no feedback treatment and at the group level in the full feedback treatment.. Three (\*\*\*) , two (\*\*), and one (\*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. All regressions include fixed effects for the pairs of gambles interacted with dummies for the blackout format and the lagged choice for the same pair of gambles (52 fixed effects total). No feedback data only includes groups with six members.

**Table 5**  
**Linear Probability Models: Social Learning**

|   | Model 1                       | Model 2                       | Model 3                       |
|---|-------------------------------|-------------------------------|-------------------------------|
| Majority disagreed                            | .153 <sup>***</sup><br>(.033) |                               |                               |
| Majority disagreed<br>* No ambiguity          |                               | .198 <sup>***</sup><br>(.061) | .198 <sup>***</sup><br>(.061) |
| Majority disagreed<br>* Ambiguity             |                               | .141 <sup>***</sup><br>(.035) |                               |
| Majority disagreed<br>* Ambiguity * Scrambled |                               |                               | .130 <sup>**</sup><br>(.056)  |
| Majority disagreed<br>* Ambiguity * Blackout  |                               |                               | .148 <sup>***</sup><br>(.044) |

Notes: All regressions include 1392 observations from 126 subjects. Standard errors are corrected for clustering at the group level. Three (\*\*\*), two (\*\*), and one (\*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. All regressions include fixed effects for the pairs of gambles interacted with dummies for the blackout format and the lagged choice for the same pair of gambles (52 fixed effects total).

**Table 6**  
**Linear Probability Models: Feedback and Risk/Ambiguity Aversion**

|                                  | Model 1                       | Model 2                   | Model 3                      |
|----------------------------------|-------------------------------|---------------------------|------------------------------|
| Dependent Variable               | Choice of Risky Gamble        | Choice of Majority Gamble | Switch                       |
| Full Feedback                    | -.047 <sup>**</sup><br>(.023) | .002<br>(.014)            |                              |
| Full Feedback Shift to Risky     |                               |                           | -.046 <sup>*</sup><br>(.024) |
| Full Feedback Shift to Majority  |                               |                           | .002<br>(.015)               |
| Full Feedback Shift to Ambiguity |                               |                           | -.059 <sup>*</sup><br>(.035) |

Notes: Models 1 and 3 include 2844 observations from 254 subjects. Model 2 includes 998 observations from 254 subjects. Standard errors are corrected for clustering at the individual level in the no feedback treatment and at the group level in the full feedback treatment.. Three (\*\*\*) , two (\*\*), and one (\*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. All regressions include fixed effects for the pairs of gambles interacted with dummies for the blackout format and the lagged choice for the same pair of gambles (52 fixed effects total).

**Table 7**  
**Linear Probability Models: Cultural Effects**

|  | Model 1          | Model 2          |
|--|------------------|------------------|
| Dataset                                | All Data         | Simple/Ambiguous |
| Block 1<br>Majority of Group Risky     | .085**<br>(.030) |                  |
| Block 1<br>Risk Culture                | .077**<br>(.031) |                  |
| Block 1<br>Majority of Group Ambiguous |                  | .074*<br>(.037)  |
| Block 1<br>Ambiguity Culture           |                  | .032<br>(.026)   |

Notes: Model 1 includes 1368 observations from 126 subjects. Model 2 includes 486 observations from 126 subjects. Standard errors are corrected for clustering at the group level. Three (\*\*\*), two (\*\*), and one (\*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. All regressions include fixed effects for the pairs of gambles interacted with dummies for the blackout format and the lagged choice for the same pair of gambles (52 fixed effects total).

**Table 8**  
**Linear Probability Models: Feedback Effects in the Merge Treatment**

|  | Model 1                       | Model 2                       | Model 3                       | Model 4                       |
|--|-------------------------------|-------------------------------|-------------------------------|-------------------------------|
| Block  | 2                             | 2                             | 3                             | 3                             |
| Subjects/Obs   | 72/816                        | 72/816                        | 72/816                        | 72/816                        |
| My Subgroup Disagreed                                | .096 <sup>***</sup><br>(.032) |                               | .040<br>(.024)                |                               |
| My Subgroup 1 Disagreed                              |                               | .140 <sup>**</sup><br>(.033)  |                               |                               |
| My Subgroup 2 Disagreed                              |                               | .184 <sup>***</sup><br>(.065) |                               |                               |
| Other Subgroup Disagreed                             |                               |                               | .055 <sup>***</sup><br>(.016) |                               |
| Average My Subgroup Disagreed w/ My Block 1 Decision |                               |                               |                               | .073 <sup>*</sup><br>(.036)   |
| Other Subgroup Disagreed w/ My Block 1 Decision      |                               |                               |                               | .057 <sup>***</sup><br>(.016) |

Notes: Standard errors are corrected for clustering at the subgroup level in Models 1 and 2 and at the group level in Models 3 and 4. Three (\*\*\*), two (\*\*), and one (\*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. All regressions include fixed effects for the pairs of gambles interacted with a dummy for the lagged choice for the same pair of gambles (24 fixed effects total).

—



Figure 1  
Sample Pair of Gambles, Scrambled Format for Ambiguity

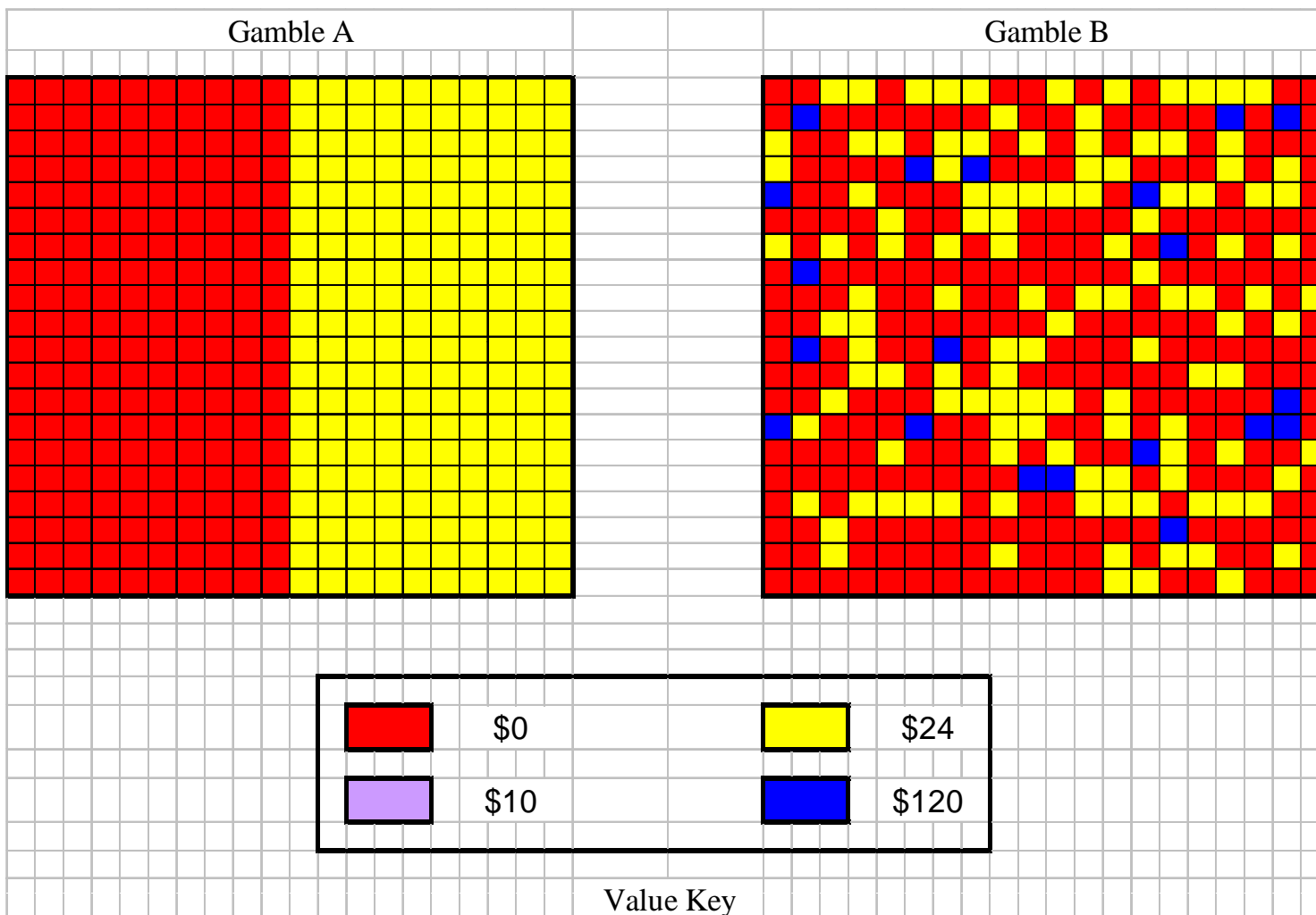
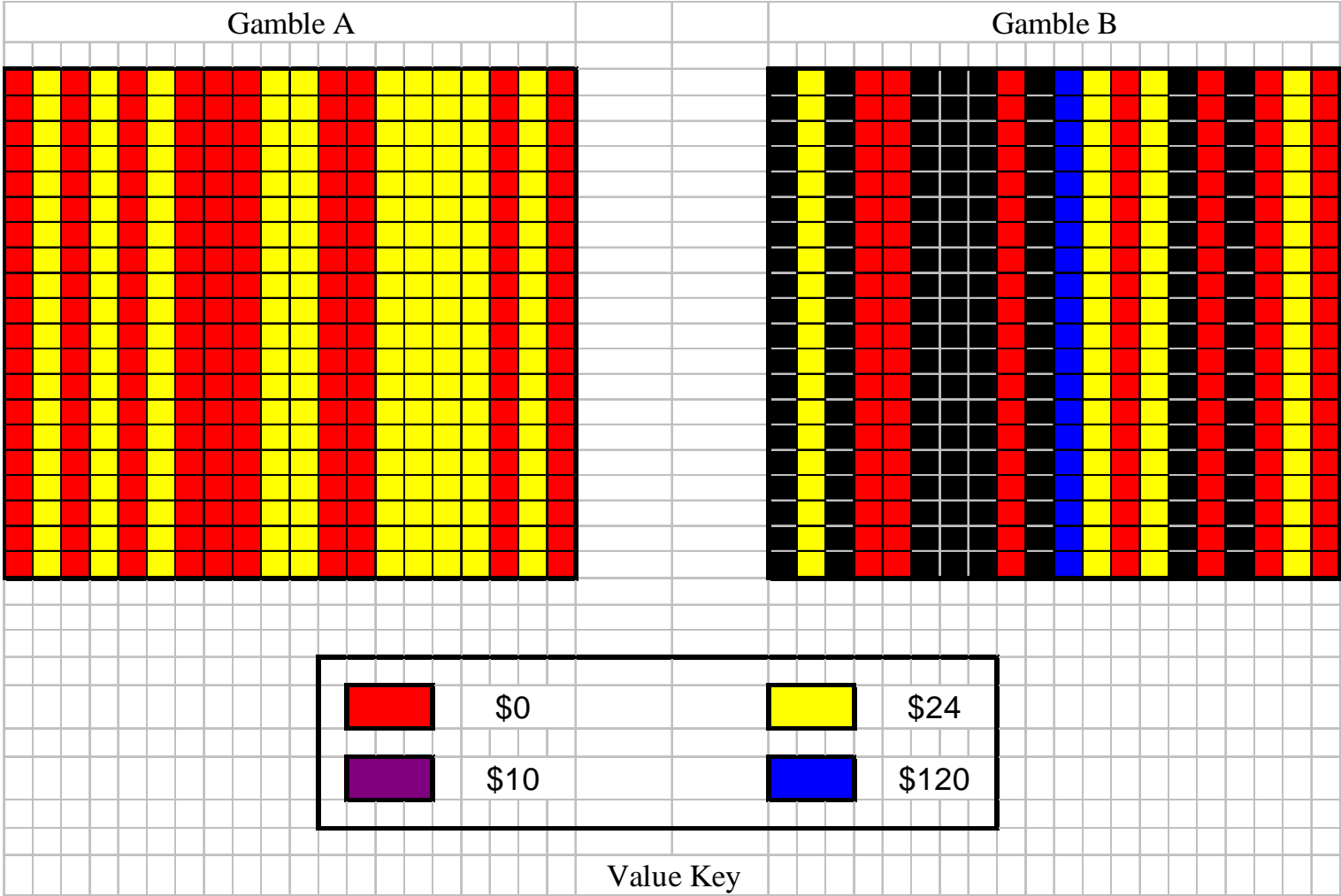
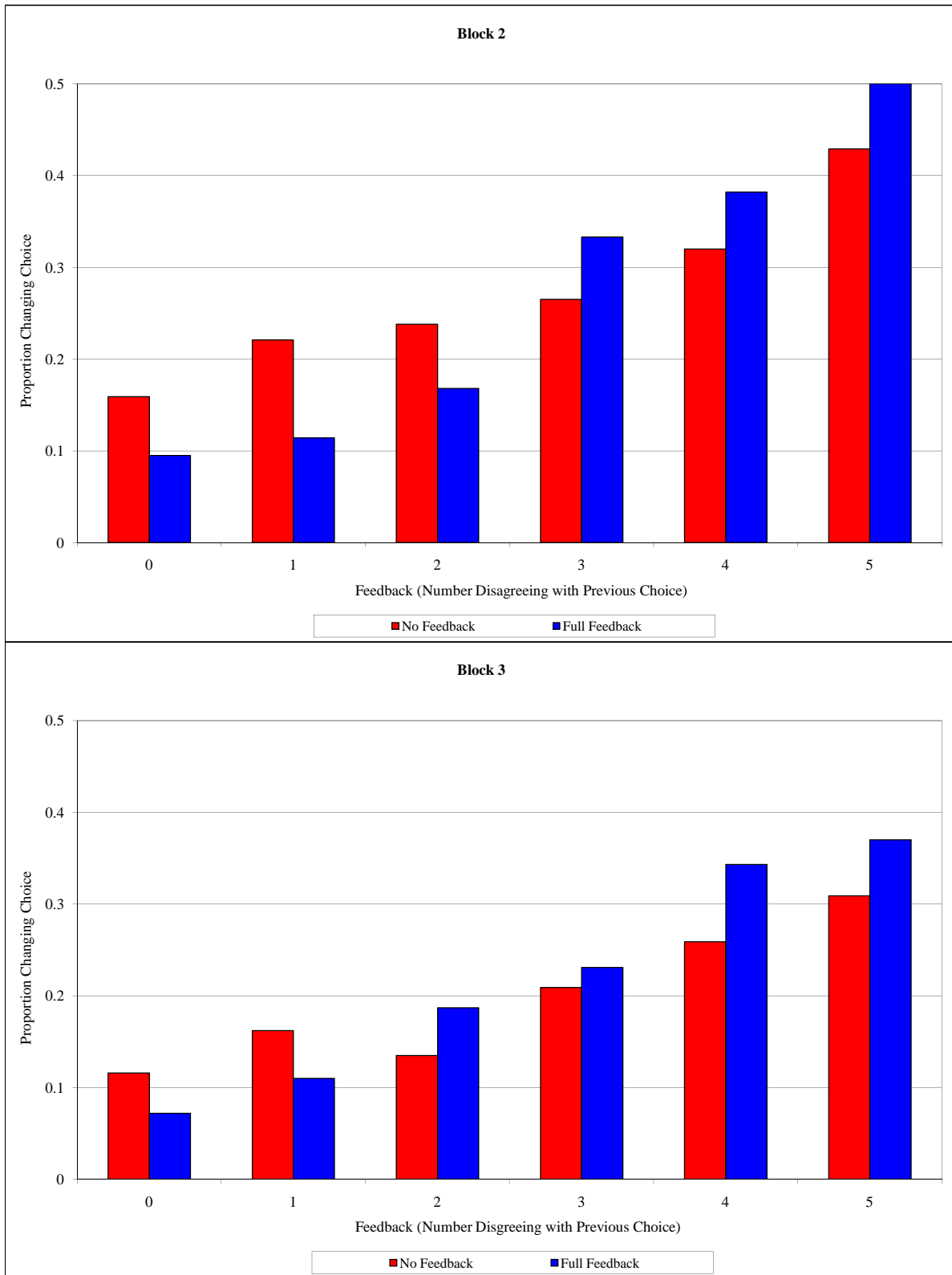


Figure 2  
 Sample Pair of Gambles, Blackout Format for Ambiguity



**Figure 3**  
**Effect of Feedback on Switching**



**Figure 4**  
**Effect of Feedback on Group Polarization**

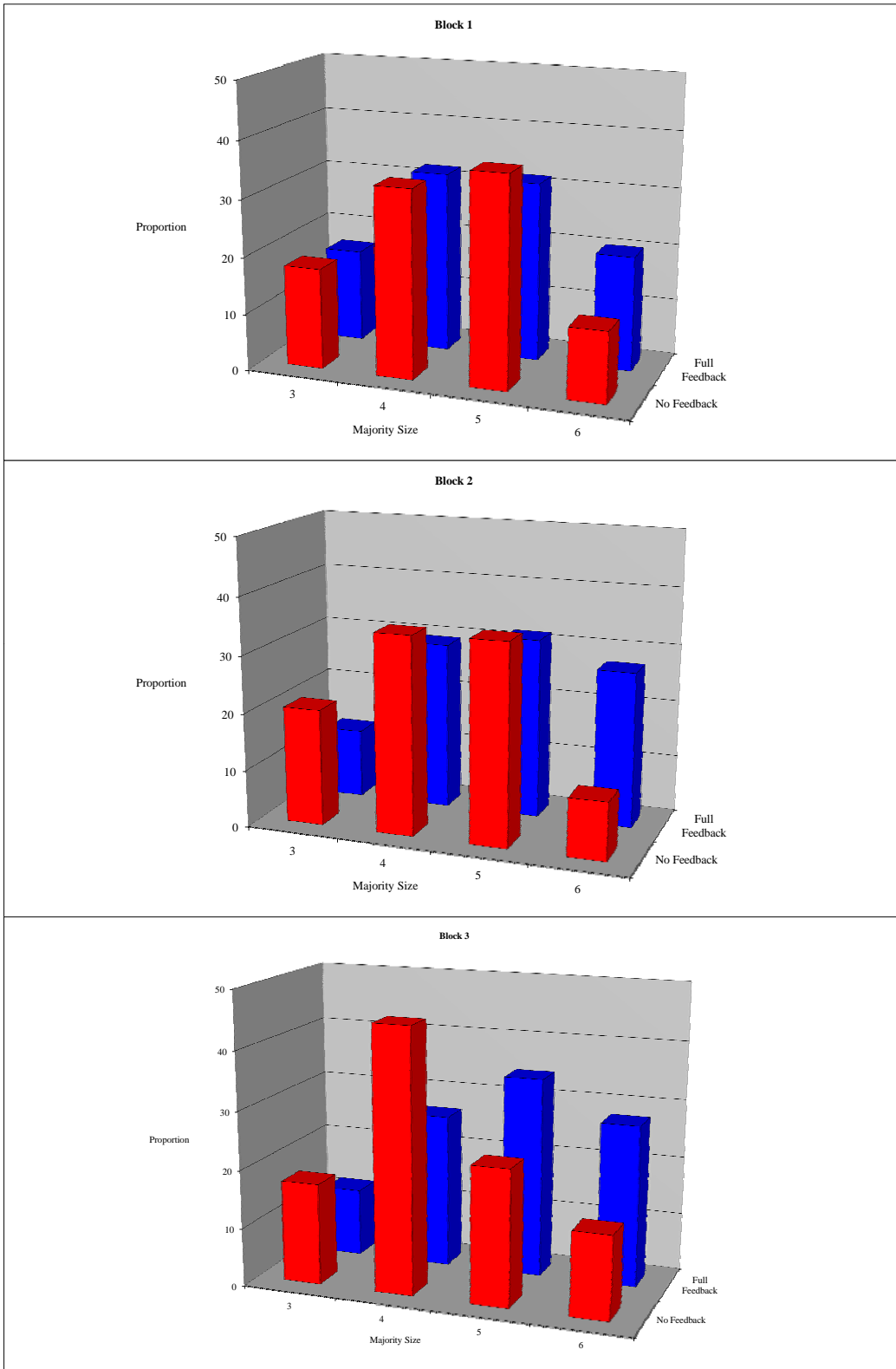
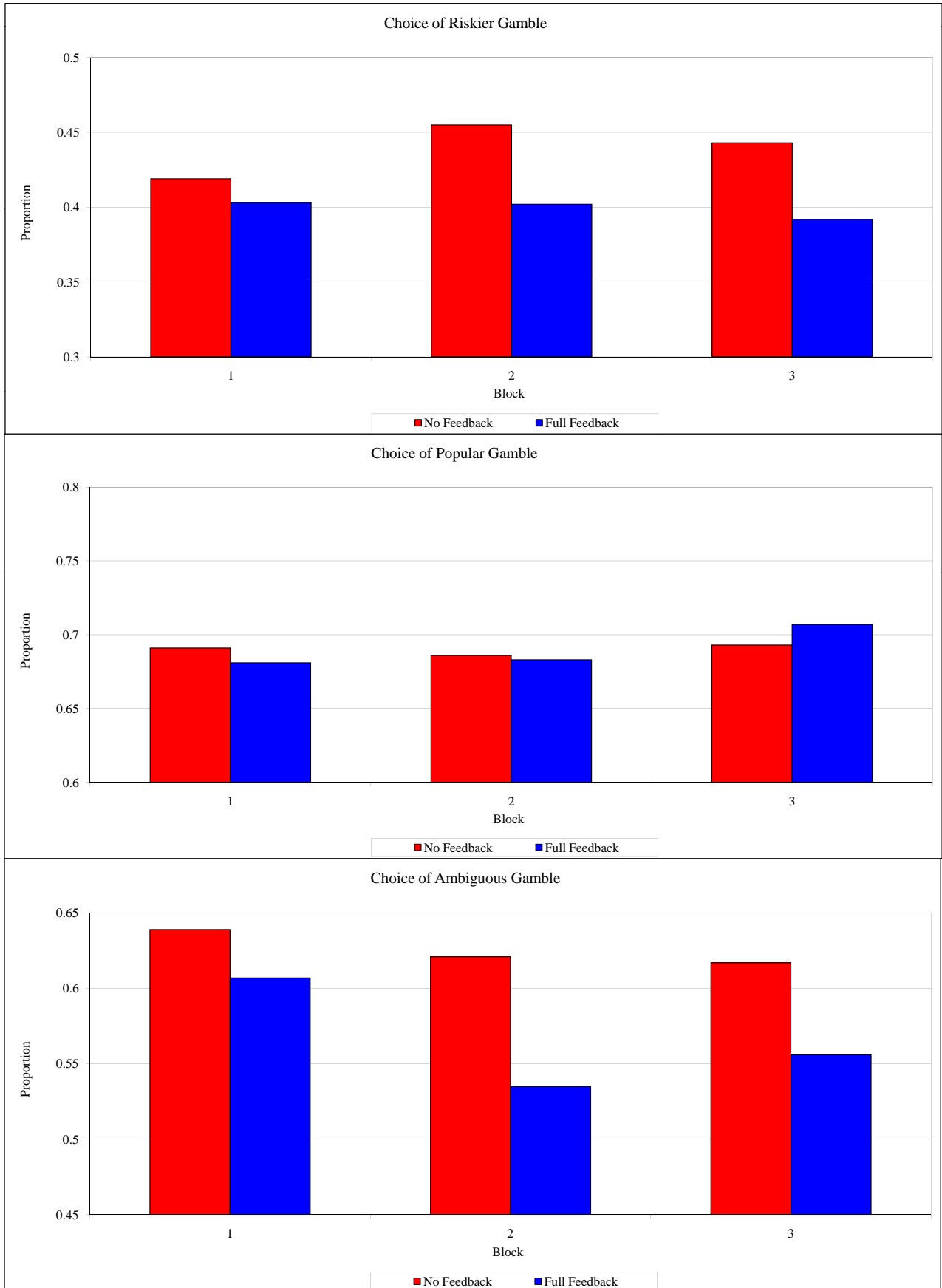
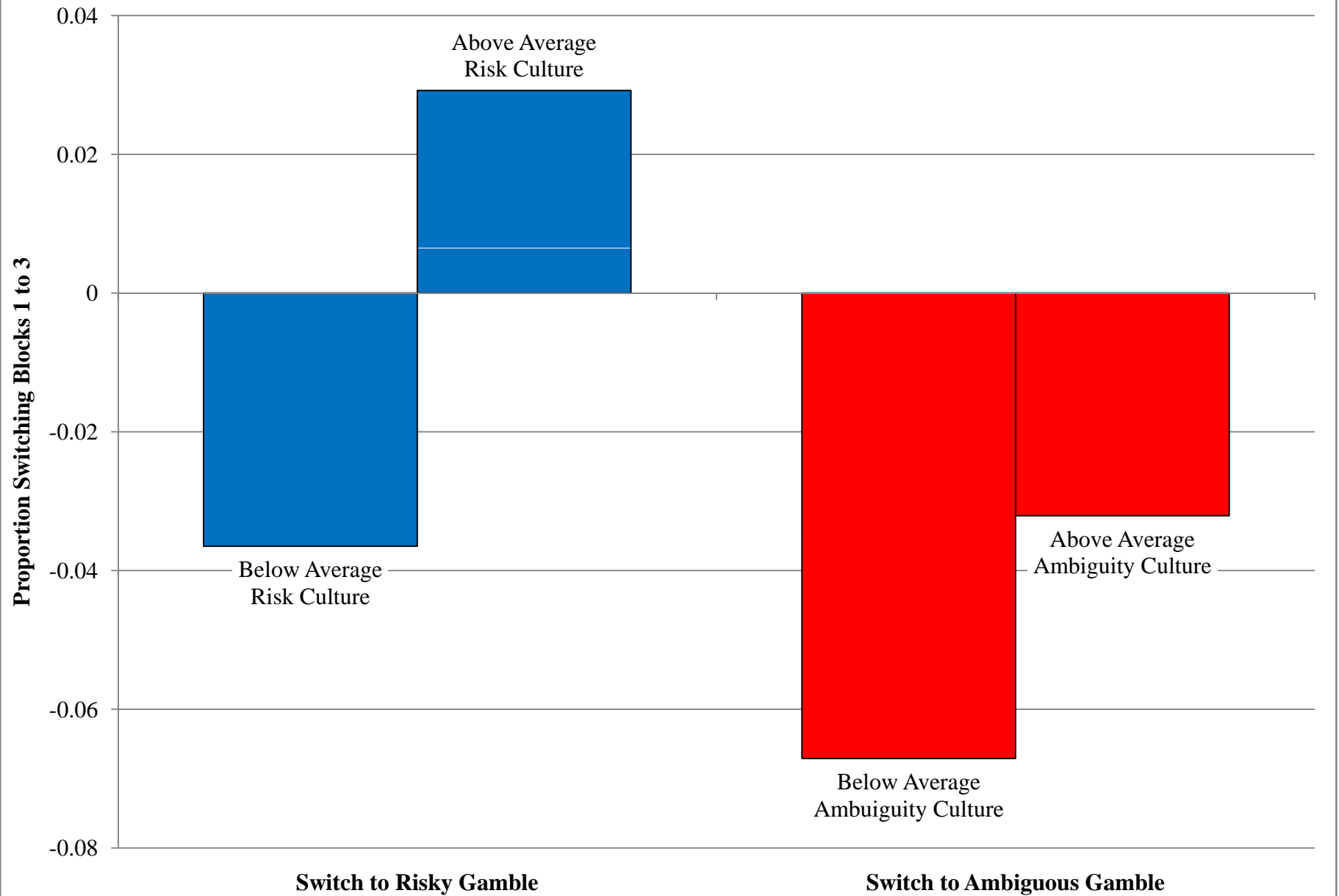


Figure 5  
Effect of Feedback on Choice of Risky, Ambiguous, and Majority Gambles



**Figure 6: Effect of Risk and Ambiguity Culture on Switching**



## Appendix

### A.1) Proofs of Theorems 1 – 4

The following proofs are based on the model laid out in Section 3.C of the paper. Assume there is an even number of players in each group, so the majority of the feedback is clearly defined.

*Theorem 1:* With no feedback, subjects are more likely to switch gambles between Block  $t - 1$  and Block  $t$  if the majority of choices in Block  $t - 1$  by others in their group disagree with their choice from Block  $t - 1$ .

*Proof:* Agent  $i$  chooses gamble  $G$  in period  $t$  if  $\Delta U_{it} \geq 0$ . Absent feedback, individual  $i$ 's beliefs remain fixed. We can therefore decompose  $\Delta U_{it}$  into a time invariant component  $\Delta \bar{U}_i = (U_G - U_{G'}) - (\rho(G, G') - \rho(G', G)) + \tau_i$  and the time varying component  $\varepsilon_{it}$ . Since  $\tau_i$  is uniformly distributed, the distribution of  $\Delta \bar{U}_i$  is also uniform. Without loss of generality assume the expected value of  $\Delta \bar{U}_i$  is strictly positive. This implies that the probability of a randomly selected individual choosing  $G$  in a randomly selected block is greater than the equivalent probability of choosing  $G'$ . It follows that probability of the majority of the feedback choosing  $G$  is also greater than the probability that the majority of the feedback chooses  $G'$ .

For the theorem to hold, the inequality shown in Equation A.1 must hold. To fix notation, there are four possible sequences of choices in Blocks  $t - 1$  and  $t$ :  $GG$ ,  $GG'$ ,  $G'G$ , and  $G'G'$ . Let  $P(g_{t-1}g_t)$  be the probability of the sequence  $g_{t-1}g_t$  for a randomly selected individual from the population. Abusing notation, let  $P(g_{t-1})$  be the probability of an agent choosing  $g_{t-1}$  in Block  $t - 1$ . Note that  $P(G) = P(GG') + P(GG)$  and  $P(G') = P(G'G) + P(G'G')$ .  $P^M(g_{t-1})$  is the probability that majority of the feedback in Block  $t - 1$  has chosen  $g_{t-1}$ . As noted above,  $P^M(G) > P^M(G')$ .

#### Equation A.1

$$\left( \frac{P^M(G)P(GG') + P^M(G')P(G'G)}{P^M(G)P(G) + P^M(G')P(G')} \right) < \left( \frac{P^M(G)P(G'G) + P^M(G')P(GG')}{P^M(G)P(G') + P^M(G')P(G)} \right)$$

Let  $p(g_{t-1}g_t, x)$  be the probability of the sequence  $g_{t-1}g_t$  subject to  $\Delta \bar{U}_i = x$ . Since the realizations of  $\varepsilon_{it}$  are independent across blocks, the probability of choosing  $G$  (or  $G'$ ) subject to  $\Delta \bar{U}_i$  is identical in all rounds. It follows that  $p(G'G, x) = p(GG', x)$  for any  $x$ . Therefore,  $P(G'G) = P(GG')$ . Substituting, the two numerators in Equation A.1 are equal.

The denominator on the left side of Equation A.1 is greater than the denominator on the right side if  $P^M(G)(P(G) - P(G')) > P^M(G')(P(G) - P(G'))$ . Since  $P^M(G) > P^M(G')$ , this inequality must hold. Q.E.D.

Theorem 2: Assume  $k > 0$ . Ceteris paribus, the difference between an individual's likelihood of switching gambles between Block  $t - 1$  and Block  $t$  if the majority of choices in Block  $t - 1$  by others in their group disagrees with their choice from Block  $t - 1$  and the likelihood of switching if the majority of choices in Block  $t - 1$  by others in their group agrees with their choice from Block  $t - 1$  is larger with feedback than without feedback.

Proof: Equation A.2 gives the relationship between utility in Block  $t$  and Block  $t - 1$ .

### Equation A.2

$$\Delta U_{it} - \Delta U_{i,t-1} = \left( (\rho_{i,t-1}(G, G') - \rho_{i,t-1}(G', G)) - (\rho_{it}(G, G') - \rho_{it}(G', G)) \right) + (\varepsilon_{it} - \varepsilon_{i,t-1})$$

Recall  $\pi_{it} = \alpha_{i,t-1}$  and  $\pi'_{it} = 1 - \alpha_{i,t-1}$ . The first term,  $\rho_{i,t-1}(G, G') - \rho_{i,t-1}(G', G)$ , is independent of  $\alpha_{i,t-1}$ . It follows from Equations 3 and 4 that  $\rho_{it}(G, G') - \rho_{it}(G', G)$  is a decreasing function of  $\alpha_{i,t-1}$ . Therefore,  $\Delta U_{it} - \Delta U_{i,t-1}$  is an increasing function of  $\alpha_{i,t-1}$  and the result follows directly. Q.E.D.

Theorem 3: Assume  $k > 0$  and  $\pi_{i0} = \pi^*(0)$ . With feedback the probability of choosing the less risky gamble,  $G$ , increases in Block 2 if the condition shown in Equation 8 holds.

$$\text{Equation 8} \quad \frac{\pi^*(0)}{1 - \pi^*(0)} > \frac{p_c(1 - p'_c)}{p'_c(1 - p_c)}$$

Proof: Let  $\Delta \bar{U}_{it} = (U_G - U_{G'}) - k(\rho_{it}(G, G') - \rho_{it}(G', G)) + \tau_i$ . Because the distribution of  $\varepsilon_{it}$  is uniform and both choices (given Equation 6) are always chosen with positive probability, the probability of choosing  $G$  is a linear function of  $\Delta \bar{U}_{it}$ . By Equations 3 and 4,  $\rho_{it}(G, G')$  and  $\rho_{it}(G', G)$  are linear functions of  $\pi_{it}$ . It therefore follows from Equations 3, 4, and A.2 that the theorem holds if the expected change in beliefs,  $E(\alpha_{i1}) - \pi_{i0} > 0$ . Given that  $\pi_{i0} = \pi^*(0)$ , this holds if the probability of choosing  $G$  in Block 1 is an increasing function of  $k$ . Using equations 3, 4, and 5, this is equivalent to setting the following condition:  $\pi^*(0)p'_c(1 - p_c) > (1 - \pi^*(0))p_c(1 - p'_c)$ . The result follows directly. Q.E.D.

Theorem 4: Assume  $k > 0$  and  $\pi_{i0} = \pi^*(0)$ . Allowing feedback increases the probability of choosing  $G$  in Block 2 if and only if  $\pi^*(0) > 1/2$ .



Proof: Because the distribution of  $\varepsilon_{it}$  is uniform and both choices (given Equation 6) are always chosen with positive probability, the probability of choosing G is a linear function of  $\Delta \bar{U}_{it}$ . By Equations 8 and 9,  $u^G_{it}$  and  $u^G_{it}$  are linear functions of  $\pi_{it}$ . It therefore follows from Equations 5, 8, and 9 that the theorem holds if the expected change in beliefs,  $E(\alpha_{i1}) - \pi_{i0} > 0$ . Given that  $\pi_{i0} = \pi^*(0)$ , this holds if the probability of choosing G in Block 1 is an increasing function of k. The result follows directly. Q.E.D.