The Role of Context and Team Play in Cross-Game Learning*

David J. Cooper
Department of Economics
Weatherhead School of Management
Case Western Reserve University
10900 Euclid Avenue
Cleveland, OH 44106-7206
david.cooper@case.edu

John H. Kagel
Department of Economics
Ohio State University
410 Arps Hall
1945 North High Street
Columbus, OH 43210-1172
kagel@economics.sbs.ohio-state.edu

2/5/2007

Abstract

We study the impact of meaningful versus abstract context on cross-game learning in a signaling game experiment. With individual decision makers (referred to as 1x1 games) meaningful context promotes positive cross-game learning in moving from a game with a pooling equilibrium to one where the only pure strategy equilibrium is separating. Abstract context (commonly employed in economic experiments) yields negative cross-game learning in this case. In 1x1 games a *change* in the (meaningful) context which accompanies "superficial" changes in the game stalls the learning process compared to an abstract context that does not change. This stall does not occur in games with two-person teams as decision makers. These also show substantially higher levels of strategic play throughout. We relate the effect of meaningful context on cross-game learning to the psychology literature on deductive reasoning processes.

Key words: learning transfer, meaningful context, teams, signaling games, experiment.

JEL Classification: C72, C92, D82, L12.

*Research support from the National Science Foundation (SES – 0451981) is gratefully acknowledged. Helpful research assistance has been provided by Kirill Chernomaz, John Lightle, and Susan Rose. We have received helpful comments from seminar participants at the ASSA meetings, the ESA meetings in Tucson, the IMEBE meetings in Valencia, the London School of Economics, Michigan State University, the Ohio State University, and the University of Copenhagen. Any opinions, findings and conclusions or recommendations in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation.

Cross-game learning, the ability to take what has been learned in one game and apply it in another related game, is an integral but largely unexplored aspect of learning in games. Virtually all papers on learning in games employ a stationary environment in which agents have no relevant experience with related games. However, suppose these agents must learn across a series of related games rather than facing a sequence of identical games. Without substantial cross-game learning, what has been learned with one set of parameter values may have to be completely relearned when there are changes in the underlying structure of the game. Convergence results that hold in stable environments may therefore cease to be valid in changing environments. More generally, we cannot understand how individuals will react to new or changed institutions, critical issues in applying game theory to real world settings, without an understanding of how subjects draw upon experiences with games related to their new situation.

The relevant psychology literature suggests we should be pessimistic about the possibility of positive cross-game learning, but this issue has been little explored in economic contexts. The experiments presented below study aspects of the environment that we have good reason to believe will affect cross-game learning: the use of meaningful versus abstract context and team versus individual play.

An important distinction between conventional practices in economic and psychology experiments is the use of abstract labels in the former versus natural (meaningful) labels in the latter. Economists' predilection for abstract labels is largely driven by methodological concerns – if a game is stripped down to its essential mathematical features there is no chance for labels to elicit unintended meanings with a resultant loss of experimental control. There are no anticipated costs to this methodology since, from the vantage point of standard economic theory, the game's mathematical structure is all that matters.[1] However, psychologists have found that meaningful labels can have a dramatic positive effect on the ability of subjects to solve problems involving deductive reasoning, even when the labels are unrelated to situations that subjects have directly experienced. Psychologists posit that this effect relates to the cognitive processes underlying deductive reasoning. An increasing body of work within cognitive psychology favors the view that subjects primarily rely on mental models to solve deductive reasoning problems rather than relying on pure logic. That is, given a set of premises, rather than fully reasoning

[1] However, see Jehiel (2005), Huck, Jehiel and Rutter (2006), and Gilboa and Schmeidler (1995) for alternative approaches that take account of context within standard game theoretic models.

through to conclusions that are consistent with these premises, individuals develop a simplified model of the situation and evaluate what conclusions are most likely given the premises and their mental model.[2] Within this framework, meaningful (rather than abstract) context can improve subjects' ability to solve deductive reasoning problems, provided the context is appropriate to the situation at hand, by providing appropriate short-cuts to reaching valid conclusions (Johnson-Laird, 1999).

Given that learning in games is at least partially a process of deductive reasoning, the preceding implies that the use of meaningful context that is appropriate to the situation at hand can speed up the emergence of strategic play across games. Experiment 1 explores this issue. Subjects play a simplified version of Milgrom and Robert's (1982) entry limit pricing. Strategic play in this game involves an incumbent monopolist attempting to deter entry by signaling it will be a tough competitor for a potential entrant. Past experiments have shown that strategic play only emerges gradually in this game, with most monopolists initially ignoring the strategic implications of their choices on entrants' responses (Cooper, Garvin and Kagel, 1997a, b). The limit pricing game provides a rich environment for studying cross-game learning as with small changes in the payoff tables, the game converges to either a pooling equilibrium or a separating equilibrium. This allows us to confront subjects with closely related games that require quite different strategic actions. Further, strategic play is clearly identifiable, making it easy to measure the extent to which there is, or is not, cross-game learning.

In Experiment 1 subjects are initially confronted with a version of the limit pricing game where play reliably converges to a pooling equilibrium. The payoff tables are then changed so only separating equilibria are present. Playing strategically in this new game requires a change in actions but draws on the same underlying logic as strategic play in the original game. When the game is presented with abstract context *negative* cross-game learning is observed – following the crossover, less strategic play is observed than occurs with subjects who have no previous experience with the same game. In contrast, with meaningful context we observe strong *positive* cross-game learning: in the first periods following the crossover, subjects with previous

[2] For example, consider the following example (Johnson-Laird, 1999): "All of the Frenchmen are wine drinkers. Some of the wine drinkers are gourmets. Therefore some of the Frenchmen are gourmets." This syllogism is invalid. If subjects relied on pure logic, they should recognize this, but many do not. This is quite comprehensible within a mental models framework. If I believe that Frenchmen are representative of wine drinkers, the conclusion is almost certain to be true. This example also serves to illustrate the importance of context – if "Frenchmen" is replaced by "steelworkers", the example becomes far less compelling.

experience in the pooling equilibrium exhibit almost three times as much strategic play as inexperienced subjects. Within the mental model framework, the use of meaningful labels provides a superior framework for thinking through the implications of the change in entrants' payoffs. The concluding section of the paper discusses the basis for this superior framework.

Experiment 2 explores the effect of *changing* the (meaningful) context by changing the labels used to frame the game along with "superficial" changes in the games structure. These changes leave the underlying game structure isomorphic to the original game from a theorist's point of view. As such, subjects aren't asked to apply the underlying logic of the game to a new setting, but instead must realize that their previous experience remains relevant. The change in labels generates a pause in subjects' learning compared to a control group that faces the same "superficial" changes in the game's structure but no change in the (abstract) context. This is consistent with the mental model view of deductive reasoning as subjects must not only adjust to the 'superficial" changes in the game's payoffs, but also realize that their existing mental model of the game applies to the new context. Changing the labeling muddies the waters sufficiently that subjects have trouble drawing on their previous experiences. Experiment 2 suggests that moving from the entry limit pricing game to, for example, Spence's (1973) education game is unlikely to generate positive cross-game learning (psychologists refer to this as "far transfer") since subjects are unlikely to realize the relevance of models developed for understanding the first game when faced with the second.

Given the disruption in the learning process from changing context in Experiment 2, we explore the effects of the same change using two subject teams as players. Previous research shows that two player teams are far more strategic than individuals in the limit pricing game (Cooper and Kagel, 2005). Experiment 2 provides a perfect opportunity for extending this result. Unlike individuals, we find that the *change* in meaningful context has no effect on the learning process for teams. Team dialogues show that teams see through the change in labels as essentially producing "no change" in the nature of the game. Further, the superior strategic play of teams beats the demanding "truth wins" norm (Lorge and Solomon, 1955), indicating that team play generates significant synergies in the development of strategic play. Given the frequency with which economic decisions are made in a team or group framework and the infrequency with which team play is studied in economic experiments, it is important to confirm that the earlier results extend to a different group of subjects and a different game structure.

The paper is organized as follows: Section I reports on the design and procedures underlying Experiment 1, followed by the results and a brief discussion that motivates Experiment 2. Section II reports on the design, procedures, and results from Experiment 2. Section III reports on the procedures underlying the teams treatment, followed by the results for the teams treatment which we refer to as Experiment 3. Section IV provides a summary of the main results and discusses a variety of issues, including differences identified in the role of meaningful context on initial versus cross-game learning.

I. Experiment 1: Learning Transfer from a Pooling to a Separating Equilibrium: The games studied here are based on a simplified version of Milgrom and Roberts' (1982) entry limit pricing model. We employ a stylized version of the model that focuses on the signaling aspects of the game.

The game is played between an incumbent monopolist (M) and a potential entrant (E). The game proceeds as follows: (1) M observes its type, high cost (MH) or low cost (ML). The two types are realized with equal probability with this being common knowledge. (2) M chooses one of seven output levels (quantities). M's payoff, shown in Table 1a, is contingent on its type, the output level chosen, and E's response. (3) E sees M's output, but not M's type, and either enters or stays out. This asymmetric information, in conjunction with the fact that it is only profitable to enter against MHs provides an incentive for strategic play (limit pricing) by Ms. E's payoff depends on M's type and on E's decision, but not on M's choice.

Experiment 1 focuses on subject's ability to transfer what they have learned from a game starting with high cost Es (Table 1b) to a game with low cost Es (Table 1c). The game with high cost Es (Table 1b) supports both pure strategy pooling and separating equilibria, with past research showing that inexperienced subjects reliably converge towards a pooling equilibrium at output level 4. A pooling equilibrium at 4 is supported by the fact that (i) given the 50-50 distribution of MLs and MHs types, E's expected value of OUT is greater than IN (250 versus 187) so that pooling deters entry and (ii) out-of equilibrium beliefs that any deviation involves an MH type with sufficiently high probability to induce entry.[3] This pooling equilibrium is achieved by a stylized learning process whereby MHs start out choosing 2 with MLs choosing 4. There are substantially higher levels of entry on 2 than on 4, with the entry rate differential such

[3] There are pooling equilibria at output levels 1-5, each of which is supported by out-of-equilibrium beliefs that any deviation from the output level in question represents an MH with sufficiently high probability to induce entry. Of these, output levels 4 and 5 satisfy the Cho-Kreps intuitive criteria (see Cooper, Garvin, and Kagel, 1997a, b).

that MHs achieve higher expected earnings from choosing higher output levels (3, 4 or 5) right from the start. This, in turn, induces MHs to imitate MLs, with the out-of-equilibrium beliefs that support the pooling equilibrium at 4 developed on the basis of this early history of play.

[Insert Table 1 here]

Switching Es payoffs to Table 1c destroys any pure strategy pooling equilibrium, as given the prior probability of high and low cost type Ms, the expected value of IN now exceeds OUT (350 versus 250). This leaves two pure strategy separating equilibria. In both of these MHs choose 2 and are always entered on. MLs always choose 6 in one separating equilibrium and 7 in the other, with MLs not entering in either case. With MLs choosing 6 or 7, MHs cannot profitably imitate them since 2 dominates 6 and 7 for MHs. Out-of-equilibrium beliefs supporting these equilibria are that any deviation involves an MH type with sufficiently high probability to induce entry.[4]

Going from games with high cost Es (Tables 1a and 1b) with play converging on a pooling equilibrium at 4 to the game with low cost Es (Tables 1a and 1c) presents subjects with a difficult challenge. Limit pricing in the game with high cost Es involves MHs imitating MLs, while limit pricing in the game with low cost Es involves MLs clearly distinguishing themselves from MHs. Although the concept underlying limit pricing is the same in both games (manipulating Es' beliefs to make it appear more likely that the monopolist is a low cost type) the actions used to achieve this end are quite different.

In the analysis we will refer to limit pricing by MHs in games with high cost Es (choice of output levels 3, 4 and 5) as strategic play, and to limit pricing by MLs in games with low cost Es (choice of output levels 5, 6 or 7) as strategic play. This is clearly not the only possible definition of strategic play since what constitutes strategic play depends critically on Ms' beliefs about Es' responses to their actions. However, the evidence accumulated from past experiments makes it clear that Ms initial choices (which are overwhelmingly at output 2 for MHs and 4 for MLs; see, for example, Figures 6 and 7 below) involve attempts to maximize their payoffs *ignoring* the effect of their choices on Es potential responses (Cooper and Kagel, 2005). Once Ms begin to consider the effect of their choices on Es responses, their choices almost exclusively

[4] There are also mixed strategy equilibria, the most prominent one consisting of MHs choosing 2 with probability .80 and 5 with probability .20 and MLs always choose 5. Of these three equilbiria, only the pure strategy separating equilibrium with MLs choosing 6 satisfies the Cho-Kreps intuitive criteria.

involve MHs choosing 3-5 in games with high cost Es and MLs choosing 5 or 6 in games with low cost Es.

Experimental Method: All of the sessions in Experiment 1 involved what will be referred to as 1x1 games in which individual subjects played against each other in their role as Es or Ms. Each session employed between 12 and 16 subjects.[5] For inexperienced subject sessions, a common set of instructions were read out loud, with each subject having a written copy.[6] Subjects had copies of both Ms' and Es' payoff tables and were required to fill out short questionnaires to insure their ability to read them. After reading the instructions, questions were answered out loud and play began with a single practice round followed by more questions. At the beginning of experienced subject sessions an abbreviated version of the full instructions were read out loud with each subject having a written copy.

Before each play of the game the computer randomly determined each M's type and displayed this information on Ms' screens.[7] The screen also showed the payoff tables for both types with the payoff table for the player's own type displayed on the left. After making their choices the program automatically highlighted Ms' possible payoffs and required that the choice be confirmed. Once *all* Ms had confirmed their choices, each M's choice was sent to the E they were paired with. Es then decided between IN and OUT with their possible payoffs highlighted on their computer screen, and were asked to confirm their choices.

Following each play of the game subjects learned their own payoff as well as the payoff for the E they were paired with, as well as Ms type. In addition, the lower left-hand portion of each player's screen displayed the results of all pairings: M's type, M's output, and E's response ordered by output levels from highest to lowest. The screen automatically displayed the three most recent periods of play, with a scroll bar available to see all past periods.

Subjects rotated between roles with Ms (Es) becoming Es (Ms) every 6 games for inexperienced subject sessions, and every 4 games for experienced subject sessions. We refer to a block of 12 (8) games in an inexperienced (experienced) session as a "cycle." Within each half-cycle, each M was paired with a different E for each play of the game. Inexperienced subject sessions had 24 games divided into two 12 game cycles. Experienced subject sessions had 32

[5] A smaller session size (10 subjects) was used once to avoid losing difficult to obtain experienced subjects.

[6] A copy of the instructions is available at http://www.econ.ohio-state.edu/kagel.

[7] We employed a block-random design so that the number of high and low cost types is equal (or as close to equal as possible) in each round.

games, divided into four 8 game cycles. The number of games in a session was announced in the instructions.[8]

Subjects were recruited through announcements in undergraduate classes, posters placed throughout the Ohio State University campus, advertisements in the campus newspaper, and direct e-mail contact with students. This resulted in recruiting a broad cross section of the undergraduate student body along with a few graduate students. Sessions lasted a little under two hours. Subjects were paid $6 for showing up on time. Earnings averaged $26 per subject in inexperienced subject sessions, including the show-up fee. Earnings were generally higher in experienced subject sessions, averaging $33 per subject, largely as a result of playing more games.

At the end of the inexperienced sessions, subjects were asked if they were interested in returning for a second session. Experienced subject sessions generally took place about a week after the inexperienced subject sessions. Subjects from different inexperienced subject sessions were mixed in the experienced sessions. Econometric analysis indicates that there are no systematic differences between the choices of subjects who returned for an experienced subject session and those who did not.

Experimental Design: The experimental design calls for comparing limit pricing *following* the crossover from games with high cost Es (Tables 1a and b) to games with low cost Es (Tables 1a and c) to control sessions in which inexperienced subjects confront Tables 1a and c right from the start. We will say that *positive learning transfer* has occurred if limit pricing by MLs in the cross-over sessions exceeds the limit pricing by inexperienced MLs in the control sessions. Correspondingly, *negative learning transfer* will have occurred if limit pricing by MLs in the cross-over sessions is less than the limit pricing by inexperienced MLs in the control sessions; with *no learning transfer* if neither of these two conditions holds.

All subjects in the cross-over sessions had participated in one inexperienced subject session with high cost Es (payoff Tables 1a and 1b). Each cross-over session began with one cycle of the game with high cost Es, followed by three cycles of the game with low cost Es (payoff Tables 1a and 1c). At the time of a crossover, all subjects were given written copies of the new payoff tables. A brief set of instructions were read out loud indicating that the basic

[8] There are a small number of exceptions to these general procedures. Two inexperienced subject 1x1 sessions with the low cost Es and abstract context used three 12 game cycles rather than two so that a number of the subjects in subsequent experienced subject sessions are more experienced than would normally be the case.

structure of the game was the same as before but that payoff tables had changed. The total number of additional games to be played was announced as well.[9] For sessions with a crossover from the quantity to the price game, labels were changed as well. Any changes in the framing were noted in the instructions and on the new payoff tables subjects received at the time of the crossover.

Sessions were conducted using either an "abstract" or a "meaningful" context. The abstract context uses abstract terms throughout. Monopolists were referred to as "A players," with the two types being referred to as "A1" and "A2" types. Potential entrants were described as "B players." The strategy choices of monopolists were referred to solely as choices of numbers with no concrete meaning attached to them and the responses of entrants are referred to as either "X" or "Y." Nothing in the instructions or payoff tables suggests any relationship between the limit pricing game and some situation outside the lab.

In contrast, meaningful context used natural terms and labels to frame the game. Thus, monopolists were referred to as "existing firms," with the two types being "high cost" and "low cost" firms respectively. Potential entrants were referred to as the "other firm" deciding between "entering this industry or some industry." M's choices were explicitly characterized as "output" or "price" levels (depending on the game being played). We purposely avoided terms which could elicit strong emotional responses – for example, we did not refer to Ms as "monopolists" to avoid the negative connotations associated with this label.

No subject was ever switched between abstract context and meaningful context or vice versa. The sole difference between abstract and meaningful context session was the framing of the materials. Otherwise, the procedures, payoffs, and interfaces were identical.

Table 2 reports the number of experimental sessions and subjects in Experiment 1.

[Insert Table 2 here]

Experimental Results: Our discussion of the results focuses on MLs' behavior for the cycles immediately following the crossover compared to inexperienced subject play in the control sessions. Figure 1 displays behavior for the cycle immediately before and after the cross-over in Experiment 1 (first and second rows of data, respectively). The left hand column shows data from sessions using abstract context; the right hand column data from sessions with meaningful

[9] There was no forewarning of the changeover at the beginning of the cross-over sessions.

context. Numbers in parentheses below the "output" levels reported on the horizontal axis are entry frequencies for each output level.

[Insert Figure 1 here]

Looking at the first row of Figure 1, a pooling equilibrium at output level 4 has started to emerge with both abstract and meaningful context. Convergence is somewhat stronger with abstract context where 100% of play by MLs is at 4 and 77% of MHs are choosing 3 or 4, both of which involve limit pricing. With meaningful context, 91% of ML play is at output level 4 and 58% of MHs are limit pricing. The lower frequency of strategic play by MHs with meaningful context largely reflects lower incentives as the entry rate differential between 2 and 4 is 60% with abstract context versus 38% with meaningful context. Further, the few cases where MLs chose output level 5 with meaningful context (less than 5%) are matched by the same frequency with which other MLs chose output levels 2 or 3, indicating that overall MLs were no closer to separating with meaningful compared to abstract context.

The difference between abstract and meaningful context following the crossover can be seen from the second row of Figure 1. With abstract context, play by MLs is virtually unchanged, with 88% of MLs continuing to choose 4. Limit pricing by MLs is barely more common (7%) than play of output levels lower than 4 (5%). Under meaningful context, choice of 4 remains the modal choice for MLs (60%), but there has been a substantial shift toward strategic play (35% choosing 5 or 6).

[Insert Figure 2 here]

Panels a and b in Figure 2 compare behavior in the cross-over sessions to controls (inexperienced MLs) for the abstract sessions (panel a) and the sessions with meaningful context (panel b). Cycles for the crossover sessions are counted from the time of the crossover so that "Cycle 1" corresponds to the first cycle following the crossover. For data from the inexperienced control sessions, cycles are reported from the beginning of play so that "Cycle 1" corresponds to the first cycle of inexperienced subject play.[10]

Figure 2 shows that the use of meaningful context changes the nature of play following the crossover. With abstract context, previous experience with high cost Es retards the

[10] We report 3 cycles for the controls. Cycles 1 and 2 are from the inexperienced control sessions. Cycle 3 is taken from the first cycle of play as experienced subjects, both in the control sessions and the crossover sessions from Experiment 2 (this is equivalent to control session data as subjects have only played the quantity game with low cost Es).

development of limit pricing by MLs (as compared with the controls), but with meaningful context MLs' limit pricing far exceeds the level observed in the control sessions.

[Insert Table 3 here]

The preceding observations are based on visual inspection of the data. Table 3 reports the results of probit regressions examining performance by MLs following the crossover. Each observation corresponds to a single play by an ML. The dependent variable is a dummy for whether the ML chose to limit price (e.g. chose output level 5, 6, or 7).[11] The base is play in the first cycle following the crossover for MLs with abstract context. Independent variables include dummies for the cycle (counting from the time of the crossover), interactions between the cycle dummies and a dummy for meaningful context, and, as a control for the incentives to play strategically, the entry rate differential between 4 and 6.[12] To control for repeated observations, the standard errors are corrected for clustering at the player level (Moulton, 1986; Liang and Zeger, 1986).[13]

Model 1 is a basic regression establishing the treatment effects. The variables of primary interest are "Meaningful Context * Crossover Cycle 1" which captures the difference between abstract and meaningful context sessions in the first cycle following the crossover. The estimated parameter is positive and statistically significant at the 1% level. The difference between abstract and meaningful context remains large and significant at the 1% level for the second cycle following the crossover as well. Only in the third cycle following the crossover does strategic play with abstract context increase to the point that the difference between abstract and meaningful context is no longer statistically significant.

Model 2 adds the entry rate differential as a dependent variable. This has a strong positive effect, easily significant at the 1% level. Including the entry rate differential reduces both the magnitude and statistical significance of the parameter estimate for "Meaningful Context * Crossover Cycle 1" so that it just fails to achieve statistical significance at the 10%

[11] Choice of 7 is quite limited but included here for completeness.

[12] See the online Appendix for exactly how this entry rate differential is calculated as well as discussion of its relevance and robustness compared to alternative measures for Ms incentives to limit price.

[13] As an alternative to controlling for clustering at the player level, we have run regressions with controls for clustering at the session level (see the online Appendix). There is clear evidence of session level effects in the data, but the results reported in the text are robust to controlling for clustering at the session level. It vastly simplifies the exposition in the text to ignore these effects.

level.[14] The "Meaningful Context * Crossover Cycle 2" variable remains statistically significant (at the 5% level) and "Meaningful Context * Crossover Cycle 3" now achieves statistical significance at the 10% level. Using a log-likelihood test for joint significance, the three context dummies are significant at the 1% level ($\chi^2 = 24.00$; 3 d.f., $p < .01$).

The probits reported on Table 4 test whether the positive/negative learning transfer observed under the different treatments reported in Figure 2 are statistically significant. Once again the dependent variable is a dummy for the use of strategic play by an ML. The independent variables are dummies for the cycle of play and interactions between the cycle dummies and a crossover dummy. Thus, the variable "Crossover Cycle 1" captures the difference between the first cycle of play in inexperienced control sessions and the first cycle following the crossover. Analogous differences are captured by "Crossover Cycle 2" and "Crossover Cycle 3." Specifications with and without entry rate controls are reported. Once again the standard errors have been corrected for clustering at the player level.

[Insert Table 4 here]

Absent controls for entry rate differences, the parameter estimate for the "Crossover Cycle 1" variable, the time period of greatest interest, is positive and statistically significant at the 1% level for meaningful context and negative and statistically significant at the 5% for abstract context. Controlling for entry rate differences, the results are essentially unchanged with abstract context, but the coefficient value for "Crossover Cycle 1" although still positive is halved, and is no longer statistically significant at conventional levels. The negative cross-game learning with abstract context carries over to the second cycle following the cross-over, with the positive cross-over effect for meaningful context carrying over to cycles 2 and 3 following the cross-over. Both of these results are robust to introducing controls for entry rate differences and for clustering at the session level (see the online Appendix; http://www.econ.ohio-state.edu/kagel/LearningTransfer_Appendix.htm).

Conclusion 1: *The use of meaningful context fosters positive cross-game learning for 1x1 sessions in moving from a pooling to a separating equilibrium. The difference between abstract and meaningful context in 1x1 games is both qualitative as well as quantitative, as abstract context results in negative cross-game learning. These conclusions are robust to controls for entrants' behavior.*

---

[14] The reduced parameter estimate for "Meaningful Context * Crossover Cycle 1" in Model 2 is driven by the high entry rate for output level 6 (50%) in the first cycle following the crossover with abstract context versus 20% with meaningful context. However, the entry rate for abstract context is based on only four observations, so that it probably is not an accurate proxy for subjects' beliefs.

Discussion of Experiment 1: There are a number of reference points in both the psychology and economics literature against which to compare the results reported here. The psychology literature on learning transfer - the ability to take knowledge acquired in one setting and successfully apply it in another related setting – suggests that the ability to generalize across games cannot be taken for granted. The consensus from this literature is that positive transfer usually fails except when environments are perceived to be quite similar. This failure follows in part from subjects' inability to recognize underlying concepts that allow them to generalize between settings (Gick and Holyoak, 1980; Perkins and Salomon, 1988; Salomon and Perkins, 1989).

There are significant differences between the underlying structure and procedures of the experiment reported here and the experiments used to study learning transfer in psychology. Psychology studies of learning transfer tend to be one shot in nature, both in terms of what was initially learned and in terms of the new learning environment. In contrast, as economists we are concerned with whether agents, having adjusted *over time* to an equilibrium in one game, will be able to adjust more quickly *over time* to a new equilibrium in a related game. Additionally, what subjects have learned in many psychology studies is algorithmic in nature (e.g. what is the best method of solving a logic problem) as they typically involve solving a puzzle. Our experiment involves a game of strategic interaction where, ironically, successful play often involves psychological insights (e.g. is my opponent trying to fool me).

The preceding helps explain why we observe greater learning transfer (with meaningful context) than the psychology literature would lead us to expect, but these differences do not account for the difference in cross-game learning between abstract and meaningful context. To help explain this, we look to the psychology literature on deductive reasoning. That literature provides clear evidence for context effects impacting on deductive reasoning. Perhaps the most famous example of this is Wason's four card selection problem (1966). In this problem subjects are shown four cards lying on a table top. They are told (for example) that each card has either an A or a K on one side and either a 4 or a 7 on the other. The four cards are arranged so that each of the four possibilities (A, K, 4, or 7) is facing up on one card. Subjects are then asked to select two cards to determine whether the statement "all cards with an A on one side must have a 4 on the other side" is true or false. A "rational" subject should always select the cards showing an A and a 7 – the other two cards are useless in verifying the truth of the statement. In fact, only about 10% of subjects select the correct cards in this abstract version of the four card selection problem.[15] In contrast, when Wason changed the content of the problem to a sensible everyday generalization, a majority of people made the correct selection (Wason and Johnson-Laird, 1972), with this increase holding up for a variety of generalizations (see Dominowski, 1995 and Johnson-Laired, 1999, for reviews of this literature).

Data from Wason's four card selection problem, as well as a host of other psychology experiments, is totally *inconsistent* with the notion that deductive reasoning depends only on formal rules of inference and that semantic content has no role to play in deductive reasoning (Johnson-Laird, 1999). As an alternative, psychologists posit that reasoning is mediated by manipulation of mental models, simplified representations of the problem at hand (Johnson-Laird and Byrne, 1991; Polk and Newell, 1995). Given a set of premises (e.g. known facts about a setting), individuals reach conclusions not through formal reasoning but rather by determining which possibilities are most consistent with the premises *given their mental model*.[16] The use of mental models provides individuals with short cuts (hopefully useful short cuts) in thinking about problems. Unlike models as usually conceived of by economists, mental models often have very limited domain, with the domain determined as much by context as by the mathematical structure of a problem.[17] Within the mental models framework, the use of meaningful context can improve deductive reasoning either by stimulating subjects to establish a better mental model of the situation at hand or by helping subjects realize that the domain of an existing mental model can be extended to the current situation. Applying the preceding to Experiment 1, we hypothesize that the use of meaningful context that is relevant to the task at hand increases strategic play by helping subjects establish a better mental model of the situation,

[15] The most frequent error is selection of the card with a 4 rather than the card with a 7.

[16] The mental model approach is similar to case-based reasoning (Gilboa and Schmiedler, 1995). According to case-based reasoning, thinking has nothing to do with logic but is instead based on memories of previous performance in similar situations. This approach is relevant for the cross-game learning effects we report on here. Johnson-Laird (1999) points out that the drawback to this approach is that it offers no immediate explanation of the ability to reason about the unknown. Our goal here is not to try and distinguish between the mental model approach and case-based reasoning as both have very similar implications for the cross-game learning found here.

[17] For example, successful executives in the construction industry presumably have a useful mental model of how to bid on projects, as evolutionary forces would quickly eliminate those who don't. This mental model need not draw on the underlying theory of common value auctions, nor does it need to be applicable (in the minds of executives) to any setting other than bidding on construction projects. Thus, as observed by Dyer and Kagel (1996), construction executives have no advantage when faced with a common value auction that is framed in abstract terms as opposed the concrete conditions under which they usually operate.

particularly of Es'choices, thereby fostering appropriate short-cuts in the reasoning process. The precise nature of the mental model that we believe is at work here is discussed below.

The economics literature on learning transfer is quite sparse. Ho et al. (1998) explicitly test for transfer in two closely related dominant solvable games, and find no transfer from the first game to the second.[18] Results from common value auctions are somewhat mixed. Kagel (1995) finds that prior experience with first-price sealed-bid common-value auctions reduces the severity of the winner's curse compared to inexperienced bidders in an ascending-price common-value auction. But bidders with experience in ascending price auctions do no better than inexperienced subjects in first-price sealed bid auctions. Kagel and Levin (1986) find that subjects who have learned to avoid the winner's curse in auctions with small numbers of bidders succumb once again when playing with larger numbers of bidders. However, fully absorbing the adverse selection effects inherent to common value auctions is no mean task, and would seem considerably harder than identifying strategic responses in signaling games.

Cooper and Kagel (2005) study the same environment as Experiment 1 with two person teams acting as a single player with abstract context. Unlike the results found here for individuals, there was strong positive cross-game learning with teams. Thus, the added power of deductive reasoning generated by team play helps to overcome the negative cross-game learning found with abstract context in 1x1 games. Further, after controlling for entry rate differences, we are unable to reject a null hypothesis that the extent of the positive cross-game learning found here with meaningful context is at the same level as in the team crossover sessions with abstract context.[19] Under any measure, the extent of the positive cross-game learning in 1x1 games with meaningful context is impressive, particularly given the negative cross-game learning found with abstract context.

Analysis of dialogues between teammates in the teams experiment links positive transfer in Experiment 1 to the development of strategic empathy – the ability of Ms to think from Es' point of view and therefore anticipate that the change in Es payoffs will increase entry rates

---

[18] Weber and Rick (2006) report somewhat more success in generating positive transfer in this game when subjects play the game a number of times *without* feedback compared to with feedback. They relate this to differences between "implicit" learning from feedback alone versus "explicit" learning whereby individuals come to obtain meaningful cognitive representations of underlying concepts and relationships.

[19] See the probits reported in the online Appendix.

substantially at the old pooling output level.[20] This is the first step in the adjustment process needed to generate a separating equilibrium (Cooper and Kagel, 2005). The similarity of results reported for meaningful context with individuals suggests that a similar mechanism is at play; namely meaningful context, when it is relevant to the situation, enhances this kind of strategic empathy.

Experiment 2 explores the effect of "superficial" changes in the structure of the signaling game with low cost Es that leave the equilibrium outcomes unaltered along with a change in the (meaningful) context used to describe the game. According to the mental model point of view, the change in context along with the "superficial" changes in the game's structure will adversely affect the learning process if the change in context obscures the relevance of past experience with essentially the same game. And indeed this is what we observe. Experiment 3 then goes on to look at these same changes in games played by two person teams. Teams once again outperform individuals, with the change in context having no adverse effect on the teams' high level of strategic play.

II. Experiment 2: The Effect of Changes in Structure and Context on Cross-Game Learning: Experiment 2 explores the effect in 1x1 games of changing the meaningful context/labeling used to frame the game along with "superficial" changes in the games structure, but which leave the underlying economic structure isomorphic to the original game. To do this we constructed the "price" game shown in Table 5. To get Ms' payoffs in the price game, subtract 50 from the payoffs in Table 1a and then multiply by .86. Further, some of the payoffs for MHs' dominated strategies were adjusted to maintain negative payoffs, as previous work indicates that this is an important cue to Es that these strategies won't be used by MHs, which plays an important role in the development of strategic play by MLs (Cooper, Garvin, and Kagel, 1997b). The payoff tables are then flipped from top to bottom, and the location of low cost types' payoffs are flipped from right to left.[21] Es' payoffs are obtained from Table 1c by adding 25 to low cost Es' payoffs and then multiplying by .8. The positions of the two columns are

[20] A similar point is made by Cooper and Kagel (in press) who develop a formal model of learning that includes "sophisticated" learners – subjects who explicitly model the learning and decision making processes of other subjects. A substantial and growing proportion of sophisticated learners is sufficient to explain the positive cross-game learning observed in that experiment.

[21] A linear transformation, without flipping, doesn't require players to change actions in order to continue playing strategically. Without forcing changes in actions it is difficult to determine whether any meaningful transfer occurs, as opposed to simple inertial on each player's part.

flipped. From a game theoretic point of view, the price game and the quantity game are identical. None of the equilibrium predictions are affected by the transformation (once we control for the flipping of the payoff tables), nor are the incentives necessary to induce strategic play affected. That is, the price game is theoretically isomorphic to the quantity game with low cost Es, but is not obviously identical from the subjects' point of view.

[Insert Table 5 here]

Experimental Design and Procedures: Experiment 2 employs both abstract and meaningful context cross-over sessions. The abstract cross-over sessions changed the screen layout and payoffs as noted in Tables 5a and b but did not change the labels or the framing of the game. The meaningful context cross-over sessions did the same but in addition changed the framing of the game and the labels so that existing firms were choosing over "prices" as opposed to "quantities." Our basic empirical strategy here is to compare the growth of strategic play following the cross-over between the abstract and context treatments. In addition, we have experienced subject data for the game with abstract context while holding payoffs the same to serve as controls against which to compare the growth in strategic play with the cross-over sessions.[22] In all other respects Experiment 2 uses the same procedures as Experiment 1.

Table 6 shows the number of experimental sessions and subjects per session underlying the analysis. In the analysis itself Ms' choices in the price game have been set equal to the corresponding output levels in the quantity game. For example, price level 6 is transformed to output level 2. This transformation eases comparison between the quantity and price games.

[Insert Table 6 here]

Experimental Results: Figure 3 displays the relevant data for Experiment 2: The first and second cycles of experienced subject play for the controls (left most column – top and bottom row respectively) along with the corresponding data for the abstract cross-over sessions (middle column) and the meaningful context cross-over sessions (right most column). The crossover sessions with abstract context show much the same pattern as the controls immediately following the crossover: The level of strategic play by MLs increases and, more noticeably, strategic play shifts from output level 5 to 6. Indeed there is little to distinguish between the control sessions and the crossover sessions with abstract context both in terms of the overall development of

[22] These are the abstract control sessions from Experiment 1.

strategic play (choice of 5 or 6) as well as the movement towards output level 6. Six definitively distinguishes play of MLs since it is dominated for MHs and is virtually never chosen by them.

The same cannot be said for the crossover sessions with the change in meaningful context. Following the crossover, the proportion of strategic play remains almost unchanged. While there is some movement towards output level 6, the movement is not as extreme as observed in either the control or crossover sessions with abstract context.

[Insert Figure 3 here]

Figure 4 graphs the frequency of limit pricing by MLs (choice of 5, 6, or 7) in the two crossover treatments along with the controls. The crossovers take place between Cycles 1 and 2. The proportion of strategic play rises between Cycles 1 and 2 for both the control and abstract context crossover sessions. This increase is far more pronounced in the crossover sessions reflecting greater incentives to play strategically as MLs; the entry rate differential between 4 and 6 is 52% in the abstract crossover sessions versus 38% in the control sessions. In contrast, in the crossover sessions with the change in meaningful context, the proportion of strategic play by MLs increases by less than one percent (38.5% versus 39.1%) between Cycles 1 and 2. The incentives to play strategically here are in-between those reported above, with an entry rate differential of 47%. In cycle 3 limit pricing has almost completely caught up to the levels reported for the controls and the abstract context cross-over treatment.[23] The effect of the change in meaningful context on subjects' ability to transfer learning between the quantity and price games could best be described as a stall in the learning process.

[Insert Table 7 here]

Table 7 reports the results of probit regressions dealing with these crossover effects. The data set includes all observations from the experienced subject sessions with a crossover between the quantity and price game (both abstract and meaningful context) as well as the experienced subject control sessions (with abstract context). Each observation corresponds to a single play by an ML. The dependent variable is a dummy for whether an ML chose to limit price (i.e., chose 5, 6, or 7). The base for these regressions is the first cycle of play in the abstract context crossover sessions. To capture changes over time, dummies for Cycles 2 – 4, Cycles 3 – 4, and Cycle 4 are included as independent variables. Due to the overlapping structure of these

[23] The entry rate differential between 4 and 6 is essentially the same in cycle 3 – 59% for the meaningful context cross-over case, 55% for the abstract cross-over sessions, and 52% in the control sessions.

dummies, the resulting estimates measure *differences* between cycles. For example, the dummy for Cycles 3 – 4 measures how much strategic play by MLs increases between Cycle 2 and Cycle 3. These time dummies are also interacted with dummies for the meaningful context crossover sessions and the abstract context control sessions. The entry rate differential between 4 and 6 constitutes a final independent variable, capturing incentives to play strategically as an ML. Standard errors are corrected for clustering at the individual subject level.

Model 1 does not control for entry rate differences. The critical variable identifying a possible stall in the learning process for the meaningful context sessions is "Meaningful Context * Crossover Cycles 2 – 4." This variable captures a difference in differences; how does the difference between the first and second cycles' levels of strategic play differ between abstract and meaningful context crossover sessions? The estimate is negative, as expected, but fails to achieve statistical significance at standard levels. Moreover, the estimate for "Abstract Context * Control Cycles 2 – 4," measuring the difference between the crossover effect with abstract context and the corresponding difference for the control sessions, is almost as large in magnitude (although again not statistically significant).

Model 2, with the entry rate differential added as an independent variable, yields different results. The estimate for the entry rate differential is large and positive. The estimate for "Meaningful Context * Crossover Cycles 2 – 4" is larger in absolute value and easily achieves statistical significance at the 5% level. While not statistically significant, the difference between this estimate and the estimate for "Abstract Context * Control Cycles 2 – 4" has increased substantially. Thus, once we control for the differing incentives to play strategically, the statistical analysis supports the conclusion that the change in meaningful context generates a stall in the learning process compared to the same "superficial" changes in payoffs, but no change in context.

In examining Figure 3, the effect of context on cross-game learning impacts on differences between output levels 5 and 6 as strong, if not stronger, than on the overall level of limit pricing. As an alternative specification, we have run probit models identical to Models 1 and 2 except with a dummy for choice of output levels 6 or 7 as the dependent variable. We have also run ordered probit models with the same specifications as Models 1 and 2 with the output level itself as the dependent variable. For both of these models, absent controls for the entry rate differential, the estimate for "Meaningful Context * Crossover Cycles 2 – 4" is negative and statistically significant.[24] The size and statistical significance of the parameter estimate increases in both cases with inclusion of the entry rate differential. The effect of abstract versus meaningful context impacts as strongly on *how* MLs play strategically as on *whether* they play strategically.

The negative effect of changing meaningful context on cross-game learning between the quantity to price games is modest.[25] Increasing our confidence that we are capturing a real phenomenon rather than a statistical anomaly are similar results from an earlier experiment conducted at the University of Pittsburgh. The results for this experiment are summarized in the on-line Appendix. Consistent with the results reported above, we find that the use of a changing meaningful context leads to a stall in the growth of MLs' strategic play when subjects are switched between the quantity and price games, as well as a more dramatic shift to limit pricing at output level 6 with abstract context. This experiment used a somewhat different computer interface than the one employed here, a different subject population, and modest changes in the payoff tables. The consistency of the results between the two experiments adds to the robustness of the results reported.

Conclusion 2: *In 1x1 games, the change in meaningful context in conjunction with the change in the "superficial" structure of the game generates a stall in the learning process compared to when only the structure changes. This negative effect is weaker than the positive effect of meaningful context on cross- game learning in Experiment 1. The change in meaningful context impacts as strongly on how MLs play strategically as on whether they play strategically, as the development of a clear distinguishing signal (choice of 6) is delayed with the change in meaningful context. Results from an earlier experiment yield similar results.*

Discussion: The results of Experiment 2 indicate that the use of meaningful context does not necessarily lead to greater cross-game learning, especially if the context is changing. The observed stall in learning is consistent with the mental model view of the role of meaningful context on the impact of cross-game learning. In the meaningful context cross-over treatment, subjects have two things to adjust to when the crossover occurs (labels and along with the "superficial" changes in the payoff table) as opposed to only one thing (the "superficial" changes in the payoff table) in the abstract context cross-over treatment. If the change in labels increases the perceived difference between the quantity and price game, learning could be slowed either

---

[24] For the probit model with choice of 6 or 7 as the dependent variable, z = 1.65 (p < .10). In the ordered probit, z = 2.13 (p < .05).
[25] The online Appendix shows that these results are not robust to clustering at the session level.

because subjects try to formulate a new mental model of the game (rather than realizing that the old model remain relevant) or because subjects are less likely to treat their previous experience as relevant. Either interpretation suggests that really radical differences between signaling games – say from the industrial organization context used here to a labor model context like Spence's (1973) education game – are unlikely to generate much in the way of positive cross-game learning as both the underlying structure and the entire context of the game have changed.[26] Examination of this conjecture is a subject for another paper.

III. Experiment 3 – Team versus Individual Play in Experiment 2: The results from Experiment 2 provide an opportunity to check for the robustness of the superiority of strategic play in two-person teams versus individuals in signaling games reported in (Cooper and Kagel, 2005). That is, will teams suffer a stall in the learning process similar to the one reported in Experiment 2, and will strategic play in teams beat the demanding "truth wins" norm relative to the 1x1 games?

Experimental Design and Procedures: In what follows the term "player" refers to an agent in the limit pricing game. A player is a single subject in the Experiments 1 and 2, but consists of two subjects in the team (2x2) sessions. Team sessions used between 20 and 28 subjects.[27] Team pairings were determined randomly by the computer at the beginning of each session. Matches could not be preserved between inexperienced and experienced subject sessions due to attrition and mixing of subjects from different sessions.[28]

Teammates were able to communicate and coordinate their decisions using an instant messaging system with full knowledge that these messages would be recorded, but with no other team having access to their messages. In addition to instructing subjects that the instant messaging system was intended to be used for coordinating their decisions, subjects were told to be civil to each other and to not use any profanity, nor to identify themselves. Otherwise, subjects were given no instruction about what messages to send. The message system was open continuously, and messages were time stamped with the period of the game being played.

[26] This is what psychologists would refer to as "far transfer" as opposed to the "close transfer" between the price and quantity games in Experiment 2 and the change from high to low cost Es in Experiment 1 (see Salomon and Perkins, 1989).

[27] The smaller number of players in the team sessions was unavoidable since the 2x2 treatment must be run in multiples of 4 subjects (two pairs) and the lab only has 30 work stations.

[28] In three of the inexperienced subject team sessions the software had to be restarted, which necessitated new team pairings. Two of these restarts were due to software crashes; the third was due to the session running beyond its advertised time, necessitating the release of some subjects.

Exactly the same payoff tables, with essentially the same computer interface, were employed in the team sessions as reported in Tables 1 and 5 above. When teams made choices the relevant payoff table on the screen had a column labeled “partner’s choice” on the left and a column labeled “my choice” on the right. When a subject entered a choice, the possible payoffs were highlighted in blue. When a subject’s partner entered a choice, the possible payoffs were highlighted in pink. Once choices coincided, possible payoffs were highlighted in red, at which point teammates had 4 seconds to change their choice before it was binding. Teams started out with 3 minutes to coordinate their choices, with a countdown clock shown on their screens. If teams failed to coordinate within this time constraint, the dialogue box was closed and one teammate was randomly selected as “leader” with his choice implemented unilaterally. Disagreements of this sort were rare. Each team member got the full payoff associated with their team’s choice, with the same conversion rate as used in the 1x1 games. Thus, there were no differences in subjects’ financial incentives between the 1x1 and 2x2 games. Other than these differences, everything was the same between the 2x2 and 1x1 sessions.

Team sessions are limited to those using meaningful context, beginning with inexperienced subjects playing the quantity game. Subjects were brought back in different combinations, played a single cycle of the quantity game followed by three cycles of the price game, just as in Experiment 2. Table 8 reports the number of session and subjects in each session. Reasons for not conducting a parallel series of sessions with abstract context will become apparent shortly.

[Insert Table 8 here]

Experimental Results: Figure 5 shows the level of strategic play in the cross-over sessions for teams versus the 1x1 games with meaningful context reported in Experiment 2. The first thing to note is the huge difference in the level of strategic play prior to the crossover between the 2x2 and 1x1 games. Virtually all teams have mastered strategic play prior to the crossover with 94% of ML teams playing strategically compared to 49% in the 1x1 games. Following the crossover to the price game the proportion of strategic play increases to 99% for the 2x2 games. The pause in the development of strategic play by MLs observed in 1x1 sessions with meaningful context is not present here.

[Insert Fig 5 here]

Given that strategic play with teams was close to 100% *prior to* the cross-over and exhibited no downturn following the cross-over, there was no scope for cross-over sessions with teams to exhibit superior performance with abstract rather than meaningful context. As such we did not conduct a parallel series of abstract cross-over sessions. Rather, in the analysis that follows we compare the team sessions with the corresponding 1x1 sessions, and report what the team dialogues have to tell us about how teams viewed these "superficial" changes in the game's structure. Before doing this we summarize what we have found so far.

Conclusion 3: *Teams are close to 100% strategic play prior to the crossover from the quantity to the price game. Teams do not exhibit any pause in the development of strategic play following the crossover from the quantity to price games.*

Figures 6 and 7 compare the detailed evolution of play between the 1x1 and 2x2 games with meaningful context. Recall that in both cases the change from the quantity to price game occurs between the first and second cycle of experienced subject play. In the cycle just prior to the cross-over, play by MLs in the 2x2 games is consistent with a separating equilibrium with over 90% of all choices at output level 6. However, there are large numbers of MHs choosing output level 4 rather than 2, in part because the entry rate differential between 2 and 4 makes it more profitable to do so.[29] Despite the change in context and payoff values play continues to move smoothly towards the efficient separating equilibrium in the first cycle following the crossover, with an increasing portion of MHs choosing output level 2. This evolution continues in experienced cycles 3 and 4 leading to an extremely clean separating equilibrium. In contrast, in the 1x1 games learning stalls following the crossover and a clean separating equilibrium has yet to emerge by the last cycle of experienced subject play.

[Insert Figs 6 and 7 here]

Table 9 reports a sampling of team dialogues during the first full cycle of play following the cross-over to the price game, primarily from the first period following the crossover. These fall into two main categories – those in which subjects explicitly recognized the changes in the "superficial" structure of the game (50% of the teams) and those in which this was not mentioned, with subjects locking into the new equilibrium directly (44.1%). This is not to say that teams in this second category failed to recognize the superficial changes in the structure of the game. Rather, they did not discuss it, locking in directly to the relevant equilibrium choices.

[29] The minimum entry rate differential to make 4 more profitable than 2 is 13%, much less than the entry rate differential of 32.5% observed in the first cycle of experienced subject play.

The remaining 5.9% (2 out of 34 teams) represent one team that did not figure things out immediately, failing to play strategically in their first opportunity, and one team in which one member was clearly confused by the change for several periods, with the team's play dictated by the subject who understood the changes.[30]

[Insert Table 9]

Ex ante, we expect strategic play by MLs to develop more rapidly for teams than individuals. In learning to play strategically, we can think of MLs as solving a "common purpose" problem. These are problems for which there is a unique answer (e.g. distinguish yourself from MHs by choosing 5, 6 or 7) which once discovered is easy to explain to one's partner. For common purpose problems teams should do better than individuals since for any random pair of subjects the team should be as able to solve the problem as soon as its most able member would *acting alone*. Psychologists use this criteria - solving the problem as soon as the most able team member would acting alone (the "truth wins" benchmark) - in judging whether or not teams are superior to individuals.[31] Beating the truth wins benchmark indicates that positive synergies are generated by team play. Falling short of it is consistent with reduced effort due to free-riding, coordination problems involved in combining team members' contributions, or process loss, as the concentration and energy expended communicating with teammates may subtract from the mental resources available for solving the problem at hand. The prevailing wisdom in the psychology literature holds that "... freely interacting groups *very* rarely exceed, sometimes match, and usually fall below" the truth wins baseline (Davis, 1992, p. 7, italics in the original).

Figure 8 compares team play against the truth wins benchmark in the game for both inexperienced and experienced team play, including the cross-over from the quantity to the price game with its change in context. The truth wins benchmark is shown by filled diamonds with the bars to either side indicating the 90% confidence interval based on the simulations conducted. Strategic play in teams equals the truth win benchmark in the first cycle of inexperienced subject

---

[30] Reading the dialogues we identified at least one case, or possibly two (depending on how one interprets the dialogues), in which one member of the team appeared to be confused at first but was enlightened immediately by his/her partner.

[31] Mathematically, if the probability of an individual working alone solving the problem equals p, the probability P of a randomly selected group with r members solving the problem is the probability that the random sample contains at least one successful member: $P = 1 - (1-p)^r$.

play, falling within the 90% confidence interval, and exceeds it thereafter.[32] These results replicate those reported for games with low cost Es and abstract context (Cooper and Kagel, 2005); only here teams beat the truth wins benchmark more consistently and by a larger margin.

[Insert Fig 8 here]

Conclusion 4: *Examination of team dialogues suggest that almost all teams saw completely through the change from the quantity to price games. Comparing strategic play by MLs between the 2x2 and 1x1 games, teams meet or beat the demanding truth wins benchmark throughout, consistent with team play generating positive synergies.*

IV. Summary and General Conclusions: This paper looks at cross-game learning, or what psychologists would refer to as learning transfer. Learning transfer is important since as Fudenberg and Kreps (1988) note:

> ". . . it seems unreasonable to expect the exact same game to be repeated over and over; put another way, if we could only justify the use of Nash analysis in such situations, we would not have provided much reason to have faith in the widespread applications that are found in the literature. Faith can be greater if, as seems reasonable, players infer about how their opponents will act in one situation from how opponents acted in other, similar situations."

The psychology literature on learning transfer is quite discouraging in that it typically shows zero or even negative transfer except for situations that are quite similar (see, for example, Salomon and Perkins, 1989). Our experiments are designed to explore when positive cross-game learning *will* and *will not* occur. Experiment 1 finds that meaningful context can play an important role in promoting strong positive cross-game learning. This role for context falls completely outside the typical domain of inquiry in economics, for which the only thing that matters is the underlying mathematical structure of the problem, but is consistent with research in psychology showing that meaningful context can substantially improve deductive reasoning (Wason and Johnson-Laird, 1972).

The psychology literature offers several models of the cognitive process underlying deductive reasoning, with mental models being the currently favored hypothesis. Within this approach, meaningful context can foster positive cross-game learning by helping individuals

[32] Because of clustering in the data, simulations are needed to correctly calculate the error bars. The simulation procedures are described in the online Appendix along with probits showing that the differences in the level of strategic play for MLs between the 2x2 and 1x1 games are statistically significant in all cycles of play with and without controls for entry rate differentials.

generate a better mental model of the situation, facilitating short-cuts in the reasoning process that are appropriate to the question at hand. The results of previous studies (Cooper and Kagel, 2005 and in press) indicate that positive cross-game learning in Experiment 1 is linked to strategic empathy, the ability to think from others' point of view and therefore anticipate that the change in payoffs will change Es' behavior. Considering strategic empathy as a mental model, meaningful context that is appropriate to the situation at hand appears to stimulate its development, thereby improving cross-game learning.

Results for 1x1 games with meaningful context in Experiment 2 show that meaningful context is not a panacea for fostering cross-game learning. A *change* in context, in conjunction with changes in the "superficial" structure of the game, generates a stall in the learning/adjustment process compared to sessions in which only the superficial structure changed. From the mental model point of view the change in context presented an impediment to adjusting to the change in the game's structure because the change in context left subjects unaware of, or confused, regarding the relevance of their pre-crossover cross-over model of the situation.

Sessions with 2x2 teams show that virtually all teams see directly through the change in context in Experiment 2, with no stall or fallback in the learning process. We are unaware of any results on cross-game or cross-situation learning in the psychology literature for teams. But these results, along with the results reported in Cooper and Kagel (2005) on cross-game learning in Experiment 1 with an abstract context, show that at least in these two cases team play contributes to positive cross-game learning. The present results also provide additional evidence of the superiority of team decision making over individuals in signaling games.

There are a number of open questions regarding the role of teams in fostering strategic play in games. These include the scope of these effects, the role of two heads in fostering synergies as opposed to the fact that the team's treatment requires team members to articulate and think harder about the problem at hand, the impact of increasing team size, and the role of changing team membership. All of these issues are currently under investigation.

There are several issues that remain to be discussed. First, the large increase in strategic play found with meaningful context following the crossover in Experiment 1 is *not* found in inexperienced subject play. Cooper and Kagel (2003) find that meaningful context leads to marginally increased levels of strategic play compared to abstract context among inexperienced subjects in the quantity game with low cost entrants. What accounts for the far stronger effect in the cross-over treatment? We suspect there is simply little scope for differential effects among inexperienced subjects. In 1x1 games strategic play by low cost monopolists develops very slowly, only emerging via repeated interactions through a trial and error learning process. In these early stages, there is so much for subjects to learn that is foreign to them that there is little scope for deductive reasoning processes to play a role. In the cross-over treatment in Experiment 1 subjects should have formed some sort of a working mental model from their previous play that is available to be deployed in response to the change in entrant's payoffs, with meaningful context both improving the available mental model and increasing the chance that it will be accessed.

Second, we would be remiss if we left the reader with the idea that meaningful context (as long as it is not changed) will generally improve performance in games. One can readily imagine situations where the use of meaningful context triggers an inappropriate mental model which short-circuits the reasoning process, resulting in suboptimal actions. Indeed, something like this seems to have been at work in Burns (1985) comparison of professional wool buyers with student subjects in a market game. In that experiment the wool buyers appear to have used the meaningful context to trigger rules of thumb that were more appropriate to the detailed structure of the game that they were familiar with from their field experience, but which were not present in the laboratory.

As noted in the introduction, cross game learning plays a critical role in determining whether convergence results developed for stable environments (e.g. identical games are played repeatedly) will apply to settings where the game changes over time. Because play in field settings is inherently framed in a meaningful context, the results of Experiment 1 suggest that convergence results should apply even if the underlying game in a field setting isn't stable. If, as we conjecture, meaningful context improves cross-game learning in Experiment 1 by making it more likely that subjects have developed a relevant and useful mental model of the game, cross-game learning should be strong as long as the same concepts remain relevant.

The results of Experiment 2 suggest limits on this ability to learn across games. "Far transfer" is unlikely to occur – individuals who have gained experience with one game are unlikely to be any better than novices at a different game framed in a different context even if the same strategic insights are germane. Understanding basic concepts does little good when individuals fail to realize their continued relevance. The failure of cross game learning in Experiment 2 is rather mild, but we conjecture it would be worse if subjects were switched between games where both the context and the mathematical structure were changed in non-trivial ways even if similar concepts applied. For example, we hypothesize that giving subjects a lecture about Akerlof's model of lemons (Akerlof, 1970) would not reduce the incidence of the winner's curse in common value auctions even though the concept of adverse selection is relevant in both settings.

Finally, in light of the varied effects of meaningful context, we are frequently asked what format we advise experimenters to employ – meaningful or abstract context? There is unfortunately no pat answer to this. As already noted our experience has been that there are, at best, only small differences between the use of meaningful and abstract context for inexperienced subjects first playing signaling games of this sort (Cooper and Kagel, 2003). Thus, our conjecture at this point is that meaningful context will have its greatest role to play when exploring behavior relative to the comparative static predictions of the theory. Beyond that our research unambiguously shows that experimenters and economists in general need to be aware that semantic content can affect behavioral outcomes, as opposed to the usual assumption among economists that the only thing that matters is mathematical structure of the problem. The potential sensitivity of subject behavior to context may account for unanticipated effects in how games are actually played. If using meaningful context we have one clear suggestion -- use as neutral a formulation as possible so as not to generate unwanted meaning responses from experience outside the laboratory. Beyond this, we need a firmer grasp of why and in what settings the use of meaningful context is likely to matter before we can offer methodological advice with confidence.

References

Akerlof, George A. (1970).”The Market for Lemons: Quality Uncertainty and the Market Mechanism.” *Quarterly Journal of Economics*, 89, 488-500.

Burns, P. (1985). “Experience and Decision Making: A Comparison of Students and Businessmen in a Simulated Progressive Auction,” in Vernon L. Smith (ed.) *Research in Experimental Economics, Vol 3*, Greenwich: JAI Press, 139-53.

Cho, I. and D. Kreps (1987). “Signaling Games and Stable Equilibria,” *Quarterly Journal of Economics*, 102, 179-221.

Cooper, David J., Susan Garvin and John Kagel (1997a). "Adaptive Learning vs. Equilibrium Refinements in an Entry Limit Pricing Game," *Economic Journal*, 107, 553-575.

Cooper, David J., Susan Garvin and John Kagel (1997b). "Signaling and Adaptive Learning in an Entry Limit Pricing Game," *Rand Journal of Economics*, 28, 662-683.

Cooper, David J. and John H. Kagel (2003). “The Impact of Meaningful Context on Strategic Play in Signaling Games,” *Journal of Economic Behavior and Organization*,50, 311-37.

Cooper, David J. and John H. Kagel (2005). “Are Two Head Better than One? Team versus Individual Play in Signaling Games,” *American Economic Review*, *95* (3), 477 - 509.

Cooper, David J. and John H. Kagel (in press). “Learning and Transfer in Signaling Games,” *Economic Theory*.

Davis, James, H. (1992). “Some Compelling Intuitions About Group Consensus Decisions, Theoretical and Empirical Research, and Interpersonal Aggregation Phenomena: Selected Examples, 1950-1990,” *Organizational Behavior and Human Decision Processes*, 52(1), 3-38.

Dyer, Douglas and Kagel, John H. (1996). “Bidding in Common Value Auctions: How the Commercial Construction Industry Corrects for the Winner’s Curse,” *Management Science*, 42(10), 1463-1475.

Dominowski, R.L., (1995). “Content Effects in Wason’s Selection Task,” in S.E. Newstead and J. St. B.E. Evans, (Eds.), *Perspectives on Thinking and Reasoning*. Lawrence Erlbaum Associates, Hillsdale, N. J., 41-65.

Fundenberg, Drew and David M. Kreps (1988). “A Theory of Learning, Experimentation, and Equilibrium in Games,” Mimeo, MIT.

Gick, Mary L. and Keith K. Holyoak (1980). "Analogical Problem Solving," *Cognitive Psychology*, 12, 306-355.

Gilboa, Itzhak and Schmeidler, David (1995). “Case Based Decision Theory,” *Quarterly Journal of Economics*, 110, 605-639.

Huck, Steffen, Jehiel, Philippe, and Rutter, Tom (2006). “Information Processing, Learning and Analogy-Based Expectations: An Experiment,” unpublished manuscript.

Ho, Teck-Hua, Camerer, Colin, and Weigelt, Keith (1998). “Iterated Dominance and Iterated Best-Response in *p*-Beauty Contests,” *American Economic Review*, 88(4), 947-69.

Jehiel, Philippe (2005). “Analogy-Based Expectation Equilibrium,” *Journal of Economic Theory*, 123, 81-104.

Johnson-Laird, P. N. (1999). “Deductive Reasoning,” *Annual Review of Psychology*, 50, 109-135.

Johnson-Laird, P. N. and Byrne, R. M. J. (1991). *Deduction*, Hillsdale, NJ: Earlbaum.

Kagel, John (1995). “Cross-Game Learning: Experimental Evidence from First-Price and English Common Value Auctions,” *Economic Letters*, 49, 163-70.

Kagel, John and Levin, Dan (1986). “The Winner’s Curse and Public Information in Common Value Auctions,” *American Economic Review*, 76(5), 894-920.

Kreps, David W. and Robert Wilson (1982). "Sequential Equilibria," *Econometricia*, 50, 853-894.

Liang, Kung-Yee, and Scott L. Zeger (1986), “Longitudinal Data Analysis Using Generalized Linear Models,” *Biometrika*, 73(1), 13-22.

Lorge, Irving and Solomon, Herbert (1955). “Two models of group behavior in the solution of eureka-type problems.” *Psychometrika*, 20(2), 139-48.

Milgrom, Paul and John Roberts (1982). "Limit Pricing and Entry Under Incomplete Information: An Equilibrium Analysis," *Econometrica*, 50, 443-459.

Moulton, Brent (1986). "Random Group Effects and the Prevision of Regression Estimates," *Journal of Econometrics*, 32, 385-397.

Perkins, David N. and Gavriel Salomon (1988). "Teaching for Transfer," *Educational Leadership*, 46, 22-32.

Polk, T. A. and Newell, A. (1995). “Deduction as Verbal Reasoning,” *Psychological Review*, 102, 533-566.

Salomon, Gavriel and David N. Perkins (1989). "Rocky Roads to Transfer: Rethinking Mechanisms of a Neglected Phenomenon" *Education Psychologist*, 24, 113-142.

Spence, A. Michael (1973). "Job Market Signaling," *Quarterly Journal of Economics*, 87(3), 355-374.

Wason, P.C. (1966). “Reasoning,” in B.M. Foss, (ed.), New Horizons in Psychology, I. Penguin Books, Baltimore, MD., 135-151.

Wason, P. C. and Johnson-Laird, P. N. (1972), *The Psychology of Deduction: Structure and Content*, Cambridge, MA: Harvard University Press.

Weber, Roberto A. and Rick, Scott (2006). “Meaningful Learning and Transfer in Learning in Games,” unpublished manuscript.

Table 1a - Quantity Game
Existing Firm's Payoffs as a Function of Other Firm's Choice
(A Player's Payoffs as a Function of B Player's Choice)

High Cost *(A1)*		
Output *(A's choice)*	**Enter** *(B's choice)*	
	This (x)	**Other** *(y)*
1	150	426
2	168	444
3	150	426
4	132	408
5	56	182
6	-188	-38
7	-292	-126

Low Cost *(A2)*		
Output *(A's choice)*	**Enter** *(B's choice)*	
	This (x)	**Other** *(y)*
1	250	542
2	276	568
3	330	606
4	352	628
5	334	610
6	316	592
7	213	486

Table 1b
Other Firm's Payoffs
(B's Payoffs)

Other Firm Enters *(B's Choice)*	**Existing Firm's Type** *(A's Type)*		Expected Payoff[a]
	High Cost *(A1)*	**Low Cost** *(A2)*	
This *(x)*	300	74	187
Other *(y)*	250	250	250

Note: Information on expected payoffs was not displayed on subjects' payoff tables.

Table 1c
Other Firm's Payoffs
(B's Payoffs)

Other Firm Enters *(B's Choice)*	**Existing Firm's Type** *(A's Type)*		Expected Payoff[a]
	High Cost *(A1)*	**Low Cost** *(A2)*	
This *(x)*	500	200	350
Other *(y)*	250	250	250

Labeling in bold applies to meaningful context. Labeling in italics and in parentheses applies to abstract context.

[a] This information was not provided as part of payoff tables.

Table 2
Summary of Sessions Included in Experiment 1[1]

Session Type	Session Type	Context	Previous	Games		# Sessions	# Subjects
				Rounds	Payoffs		
Inexperienced Low Cost Entrants	Controls	Abstract	None	1 – 24	1a & 1c	8	128
Inexperienced Low Cost Entrants	Controls	Meaningful	None	1 – 24	1a & 1c	4	62
Experienced Low Cost Entrants	Controls	Abstract	Quantity Game Low Cost Entrants	1 – 8	1a & 1c	6	83
Experienced Low Cost Entrants	Controls	Meaningful	Quantity Game Low Cost Entrants	1 – 8	1a & 1c	4	52
Experienced High & Low Cost Entrants	Crossover High → Low Cost	Abstract	Quantity Game High Cost Entrants	1 – 8	1a & 1b	4	50
				9 – 32	1a & 1c		
Experienced High & Low Cost Entrants	Crossover High → Low Cost	Meaningful	Quantity Game High Cost Entrants	1 – 8	1a & 1b	3	44
				9 – 32	1a & 1c		

[1] For most of the experienced control sessions, subjects were crossed to another game after the eighth round as part of Experiment 2.

Table 3
Comparing Crossover Effects Going from High to Low Cost Entrants

Probit Regressions: Dependent Variable is Strategic Choice by MLs
(816 obs, 136 players)

Variable	Model 1	Model 2
Constant	-1.471*** (.217)	-.948*** (.292)
Crossover Cycle 2	.781*** (.239)	.307 (.317)
Crossover Cycle 3	1.596*** (.238)	1.150*** (.277)
Meaningful Context * Crossover Cycle 1	1.091*** (.289)	.638* (.352)
Meaningful Context * Crossover Cycle 2	1.010*** (.283)	.829*** (.295)
Meaningful Context * Crossover Cycle 3	.316 (.283)	.487* (.271)
Entry Rate Differential		1.732*** (.557)
Log Likelihood	-449.64	-431.03

Standard errors corrected for clustering at the player level.

* statistically significant at the 10% level
** statistically significant at the 5% level
*** statistically significant at the 1% level

Table 4[1]
Crossover from High to Low Cost Entrants: Comparing Outcomes Against within Treatment Controls

Probit Regressions: Dependent Variable is Strategic Choice by MLs

Variable	Model 1	Model 2	Model 3	Model 4
Data Set	Abstract Context, 1x1		Meaningful Context, 1x1	
# Subjects	178		106	
# Observations	1239		744	
Constant	-.907*** (.107)	-.700*** (.112)	-1.106*** (.154)	-.888*** (.156)
Inexperienced Cycle 2	.602*** (.112)	.326*** (.121)	.302 (.187)	.164 (.181)
Experienced Cycle 1	.788*** (.151)	.204 (.187)	.814*** (.213)	.454* (.247)
Crossover Cycle 1	-.634** (.242)	-.701** (.282)	.727*** (.245)	.398 (.274)
Crossover Cycle 2	-.385* (.232)	-.596*** (.225)	1.214*** (.254)	.855*** (.310)
Crossover Cycle 3	.244 (.236)	.327 (.233)	.733*** (.276)	.850*** (.271)
Entry Rate Differential		1.206*** (.254)		.980** (.444)
Log Likelihood	-705.37	-661.81	-410.69	-405.01

Standard errors corrected for clustering at the player level.

* statistically significant at the 10% level
** statistically significant at the 5% level
*** statistically significant at the 1% level

[1] The results for Models 5 and 6 differ slightly from those reported in Cooper and Kagel (2005, Table A3) as we drop the experienced 2x2 control sessions and cluster at the team level both for consistency with the 1x1 regressions.

Table 5a - Price Game
Existing Firm's Payoffs as a Function of Other Firm's Choice
(A Player's Payoffs as a Function of B Player's Choice)

Low Cost *(A2)*		
Price *(A's Choice)*	**Enter** *(B's choice)*	
	This *(x)*	**Other** *(y)*
1	226	461
2	315	552
3	330	568
4	346	583
5	327	564
6	280	531
7	258	529

High Cost *(A1)*		
Price *(A's Choice)*	**Enter** *(B's choice)*	
	This *(x)*	**Other** *(y)*
1	-218	-75
2	-129	-22
3	91	200
4	157	394
5	172	409
6	187	425
7	172	409

Table 5b

Other Firm's Payoffs
(B's Payoffs)

Other Firm Enters *(B's Choice)*	**Existing Firm's Type** *(A's Type)*		Expected Payoff[a]
	Low Cost *(A2)*	**High Cost** *(A1)*	
This *(x)*	420	180	300
Other *(y)*	220	220	220

Labeling in bold applies to meaningful context. Labeling in italics and in parentheses applies to abstract context.

[a] This information was not provided as part of payoff tables.

Table 6
Summary of Sessions Included in Experiment 1

Session Type	Session Type	Context	Previous	Games		# Sessions	# Subjects
				Rounds	Payoffs		
Inexperienced Low Cost Entrants	Controls	Abstract	None	1 – 24	1a & 1c	8	128
Inexperienced Low Cost Entrants	Controls	Meaningful	None	1 – 24	1a & 1c	4	62
Experienced Low Cost Entrants	Controls	Abstract	Quantity Game Low Cost Entrants	1 – 32	1a & 1c	??	??
Experienced Low Cost Entrants	Controls	Abstract	Quantity Game Low Cost Entrants	1 – 32	1a & 1c	3	42
Experienced Low Cost Entrants	Crossover Quantity → Price	Abstract	Quantity Game High Cost Entrants	1 – 8 9 – 32	1a & 1c 5a & 5b	4	50
Experienced Low Cost Entrants	Crossover Quantity → Price	Meaningful	Quantity Game High Cost Entrants	1 – 8 9 – 32	1a & 1c 5a & 5b	4	54

Table 7
Crossover from Quantity to Price Game: 1x1 Sessions

Probit Regressions: Dependent Variable is Strategic Choice by MLs
(1116 obs, 140 players)

Variable	Model 1	Model 2
Constant	-.015 (.194)	-.208 (.211)
Cycles 2 – 4	.361** (.163)	.902*** (.201)
Cycles 3 – 4	.053 (.168)	-.116 (.190)
Cycle 4	.162 (.159)	-.296 (.239)
Meaningful Context * Crossover Cycles 1 – 4	-.277 (.266)	-.186 (.283)
Meaningful Context * Crossover Cycles 2 – 4	-.346 (.221)	-.516** (.226)
Meaningful Context * Crossover Cycles 3 – 4	.472* (.248)	.238 (.278)
Meaningful Context * Crossover Cycle 4	-.079 (.200)	.142 (.242)
Abstract Context * Control Cycles 1 – 4	-.208 (.275)	.077 (.292)
Abstract Context * Control Cycles 2 – 4	-.254 (.238)	-.341 (.257)
Abstract Context * Control Cycles 3 – 4	.261 (.232)	.032 (.273)
Abstract Context * Control Cycle 4	.071 (.234)	.171 (.272)
Entry Rate Differential		2.602*** (.588)
Log Likelihood	-739.25	-685.21

Note: The base is 1x1 abstract context crossover sessions.
Standard errors corrected for clustering at the player level.

* statistically significant at the 10% level
** statistically significant at the 5% level
*** statistically significant at the 1% level

Table 8
Summary of Sessions Included Experiment 3

Session Type	Player Type	Context	Previous	Games: Rounds	Games: Payoffs	# Sessions	# Subjects
Inexperienced Low Cost Entrants	Individual	Meaningful	None	1 – 24	1a & 1c	4	62
Inexperienced Low Cost Entrants	Team	Meaningful	None	1 – 24	1a & 1c	3	68
Experienced Crossover Quantity → Price	Individual	Meaningful	Quantity Game Low Cost Entrants	1 – 8 9 – 32	1a & 1c 5a & 5b	4	54
Experienced Crossover Quantity → Price	Team	Meaningful	Quantity Game Low Cost Entrants	1 – 8 9 – 32	1a & 1c 5a & 5b	3	68

Table 9
Sample Quotes from Team Dialogues

1. ***Explicitly recognize that these are essentially the same payoff tables, only the numbers and layout have changed:*** **50.0%** (17/34)

16: "it looks like about the same deal."
6: "yeah, they just changed "output" with "price" and the numbers finally changed"

19: "this is almost the reverse as the other table "
9: "yea it's the same idea just different numbers"

8: "ok 2 is the new 6 lol (laugh out loud)"

15: "well this is certainly a twist"
17: "haha yeah ill (sic) have to be careful to realize the numbers are switched around"

16: "if low cost, choose 2"
16: "it's the same thing pretty much, they're just trying to throw us off"

11: "I am guessing everything will flip around, high costs will pick 6 and low costs will pick 2"

2. ***Play strategically from the start but without any explicit reference to the payoff tables being essentially the same:*** **44.1%** (15/34)

5: "what do you think we should do?"
24: "its best no matter what to do 6 if you are high"
5: "yeah"
24: "but if we can trick them into thinking we're low by picking 4 then they will pick other"

2: "if we pick 2 for every low maybe they will catch on"
17: "do you wanna stick to 2 then?"
2: "yeah"
17: "if we get high cost we could go with 4 and then maybe they would choose other since 4 is the highest on the low side"
2: "that'll work"

1: "low we want to pick 2" "high pick 4"
…..
19: "we'll see how soon other firms start catching on to high's picking 4" "pretty quickly, I guess"
1: "haha we should try 3 or 6" [after being entered on when high cost and picking 4]

19: "2 for low, 6 for high"
3: "yeah"

Figure 1
Experiment 1: High Cost Entrant to Low Cost Entrant Crossovers

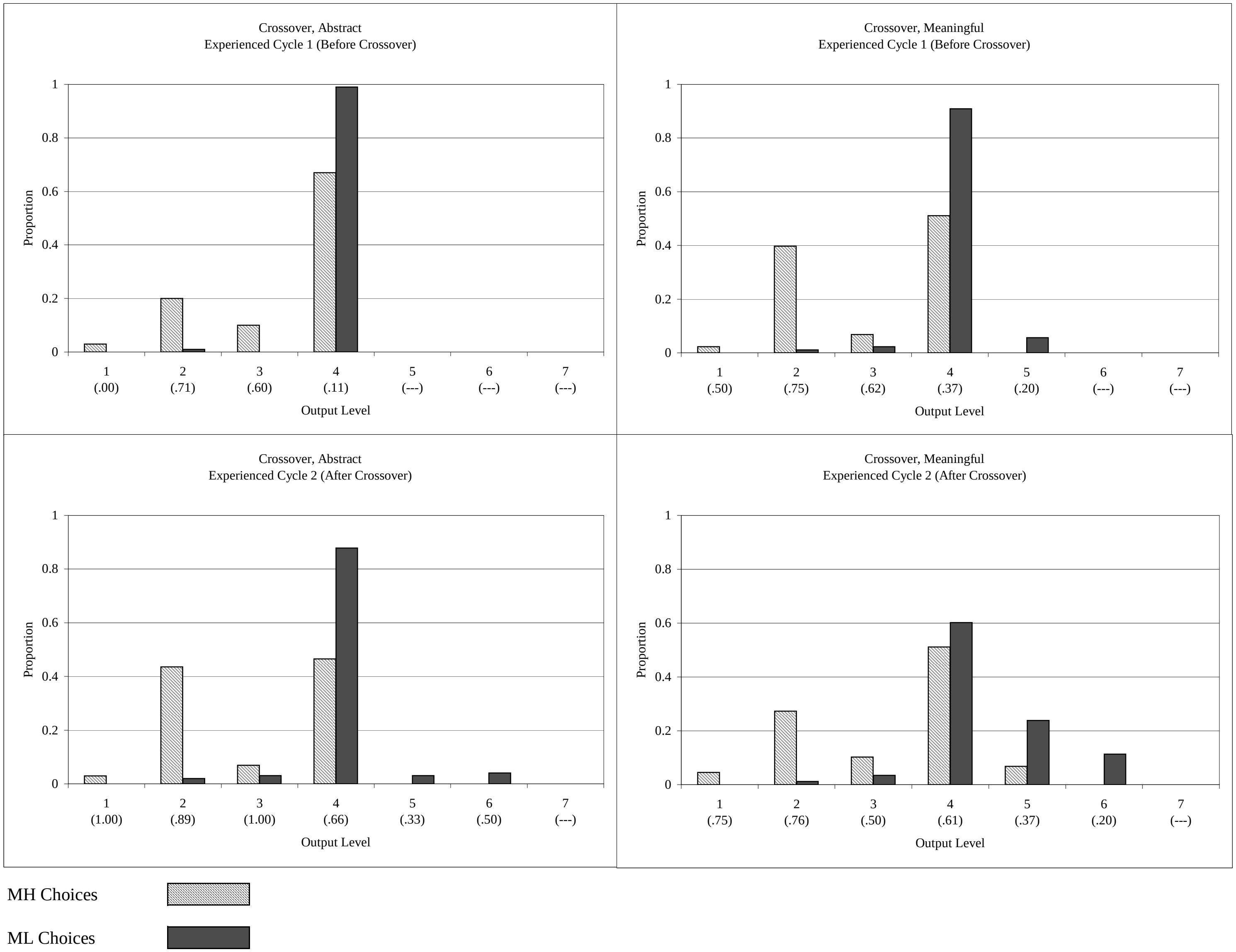
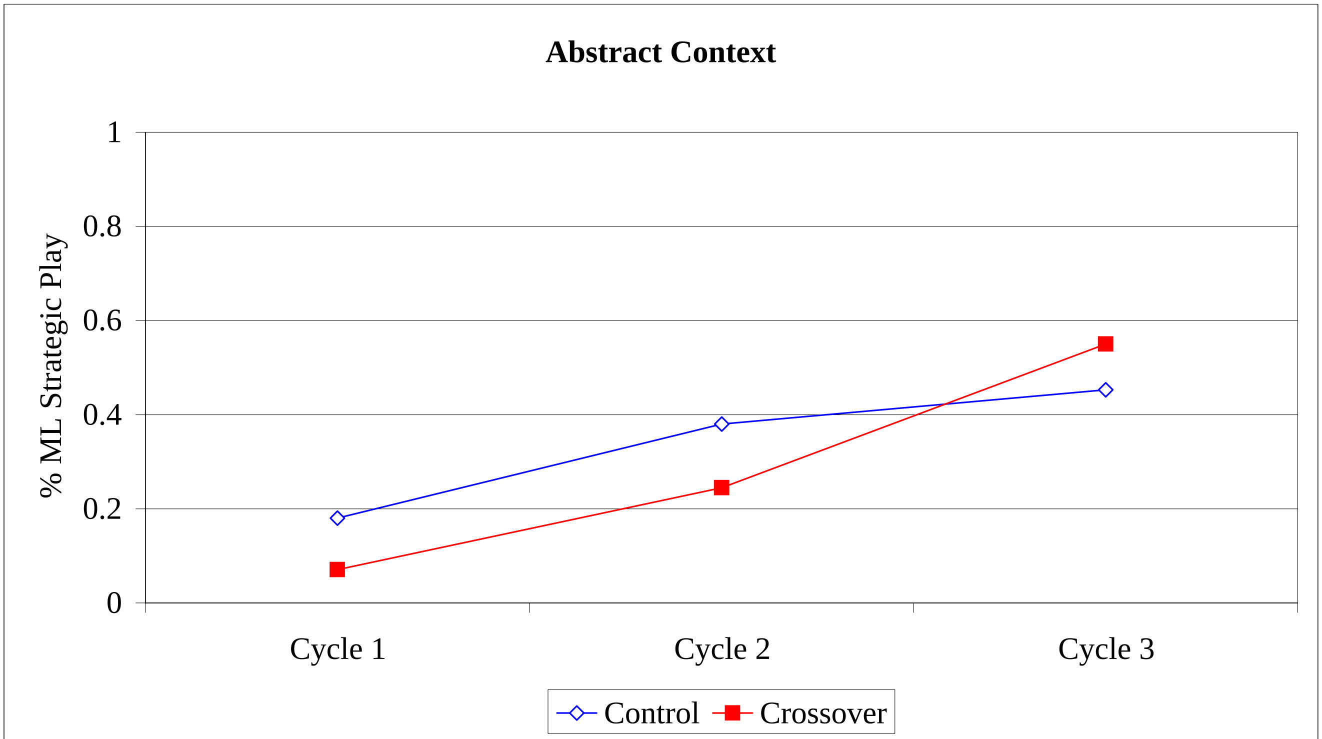


Figure 2
Strategic Play by MLs in High to Low Cost Entrant Crossovers



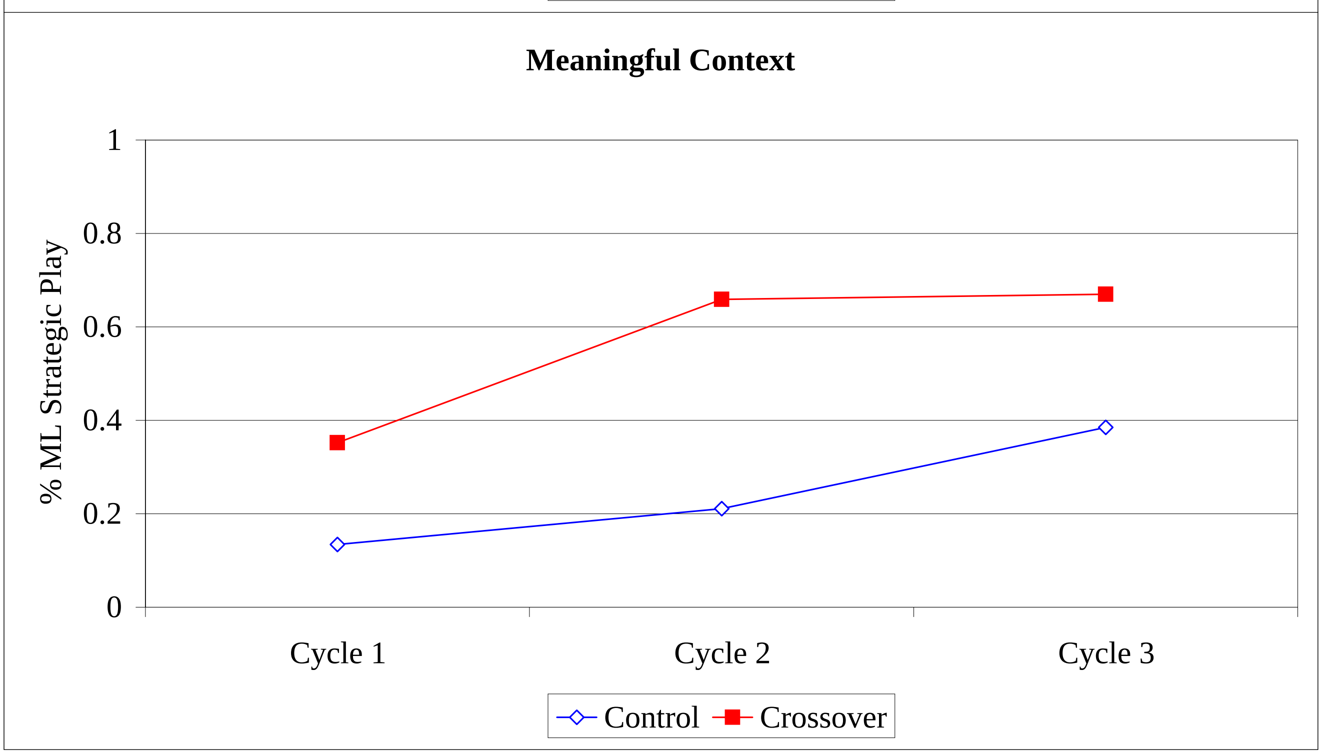
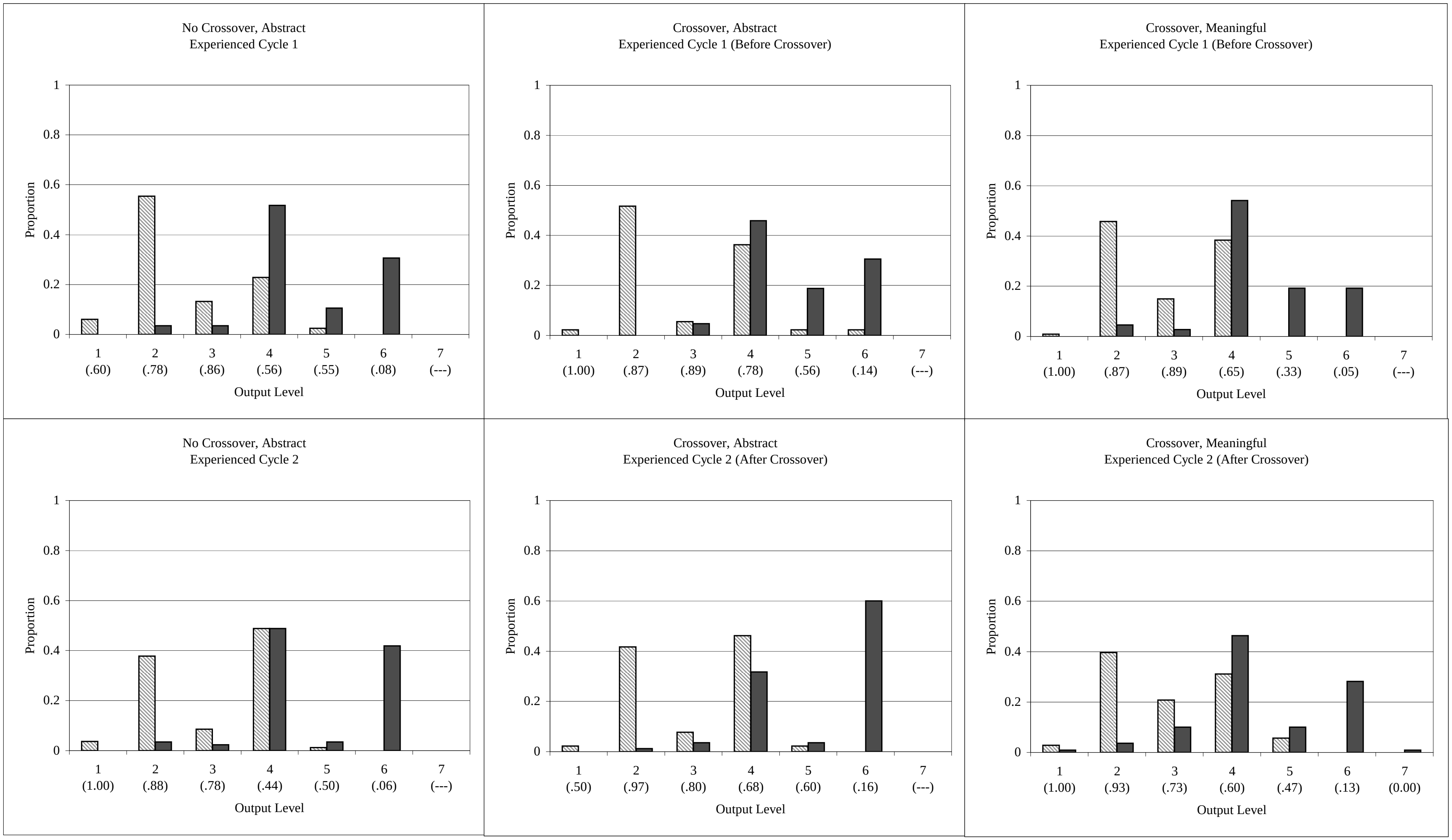


Figure 3
Quantity to Price Game Crossovers



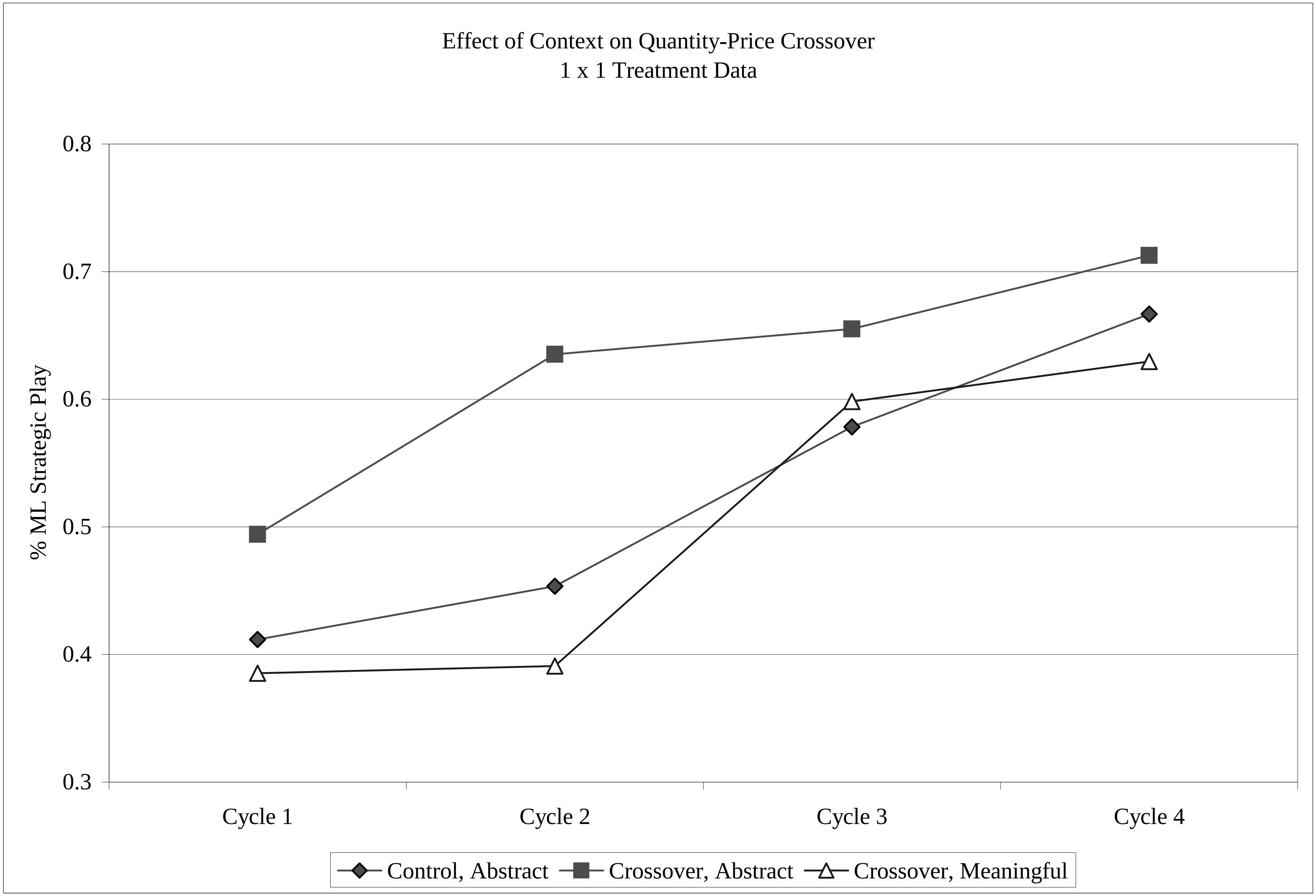
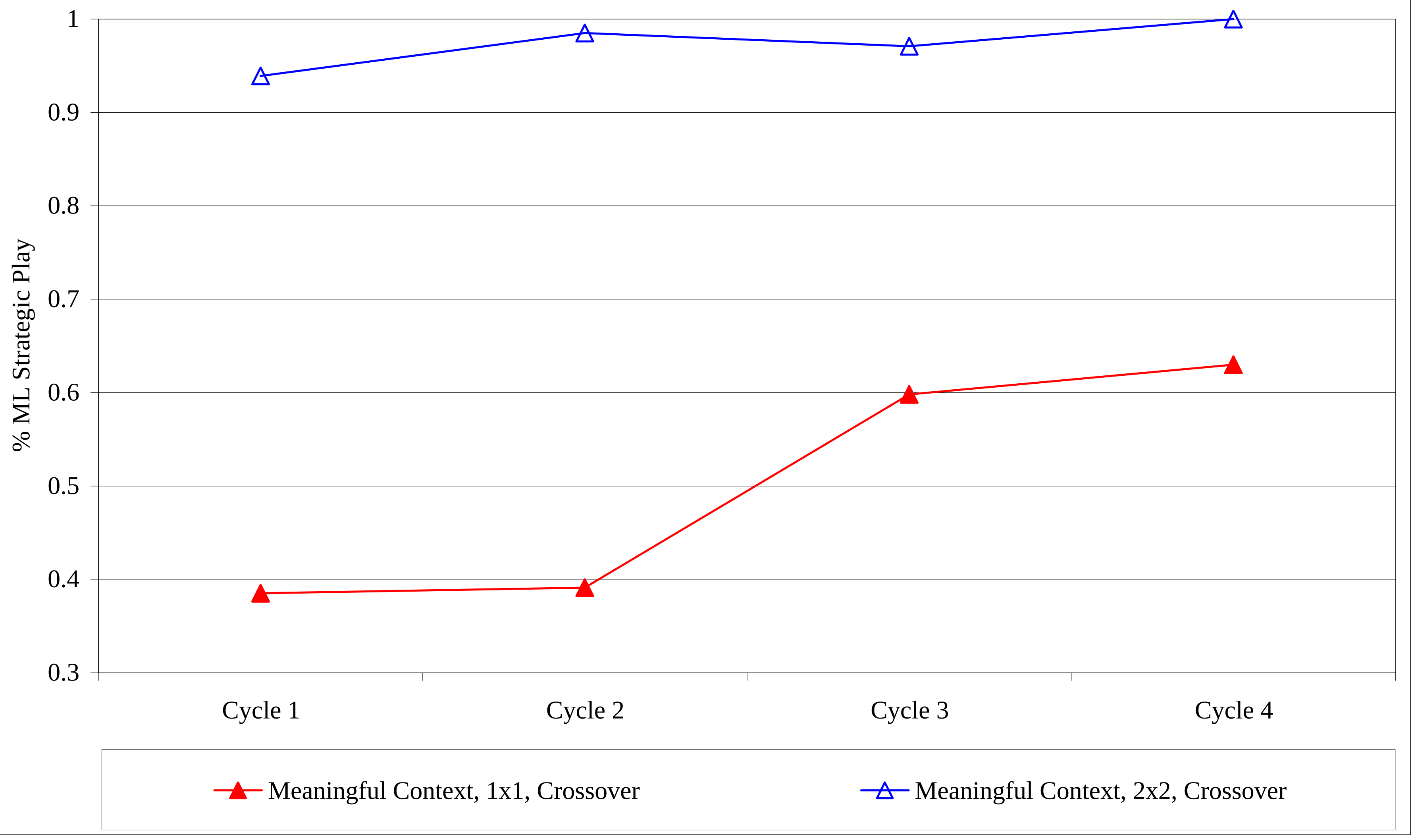

Effect of Context on Quantity-Price Crossover
1 x 1 Treatment Data
% ML Strategic Play
0.8
0.7
0.6
0.5
0.4
0.3
Cycle 1
Cycle 2
Cycle 3
Cycle 4
Control, Abstract
Crossover, Abstract
Crossover, Meaningful

Figure 5
Strategic Play by MLs in Quantity to Price Game Crossovers -- Individuals vs. Teams



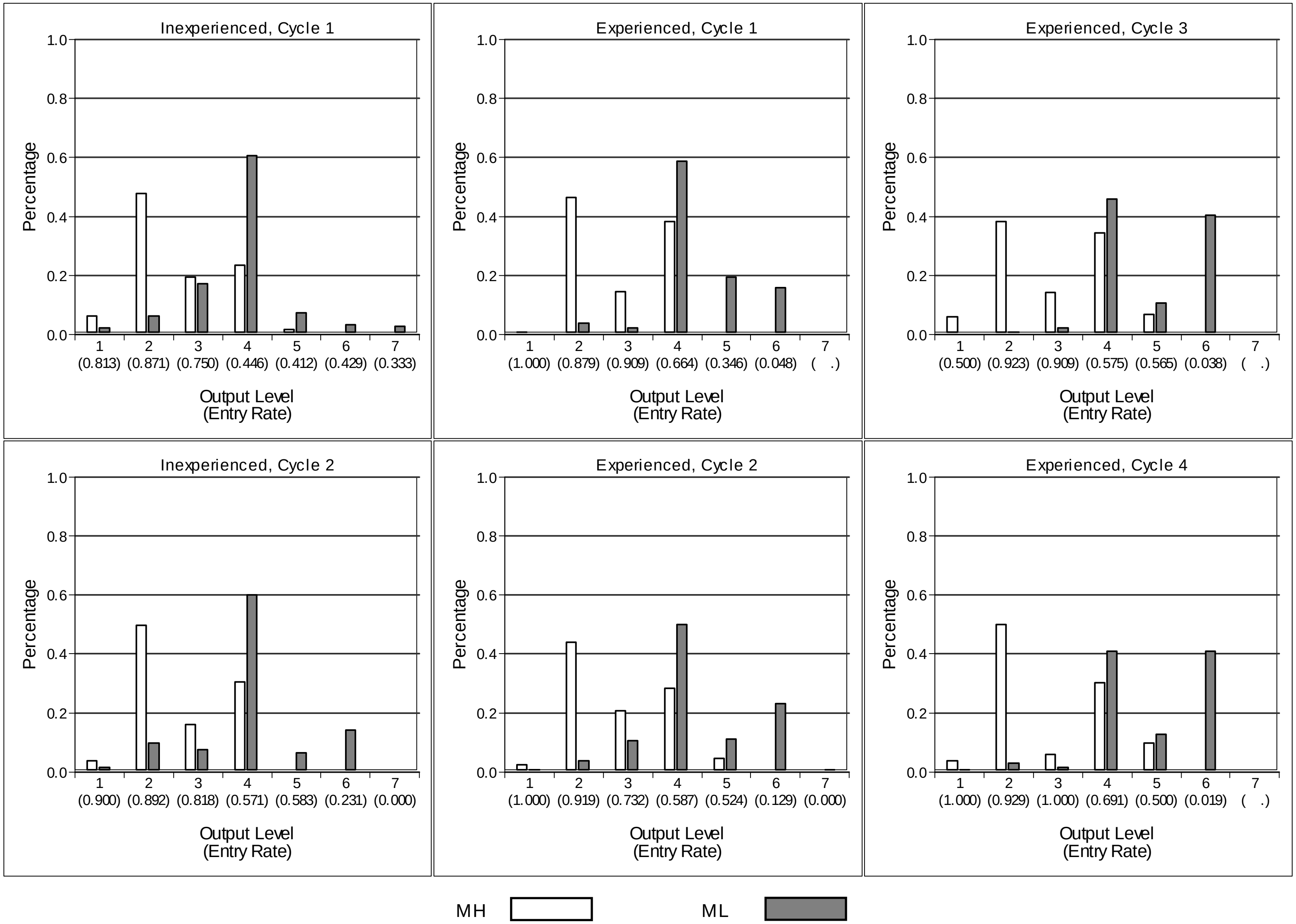


FIGURE 6. POOLED DATA FOR 1X1 SESSIONS WITH LOW COST ENTRANTS AND MEANINGFUL CONTEXT.

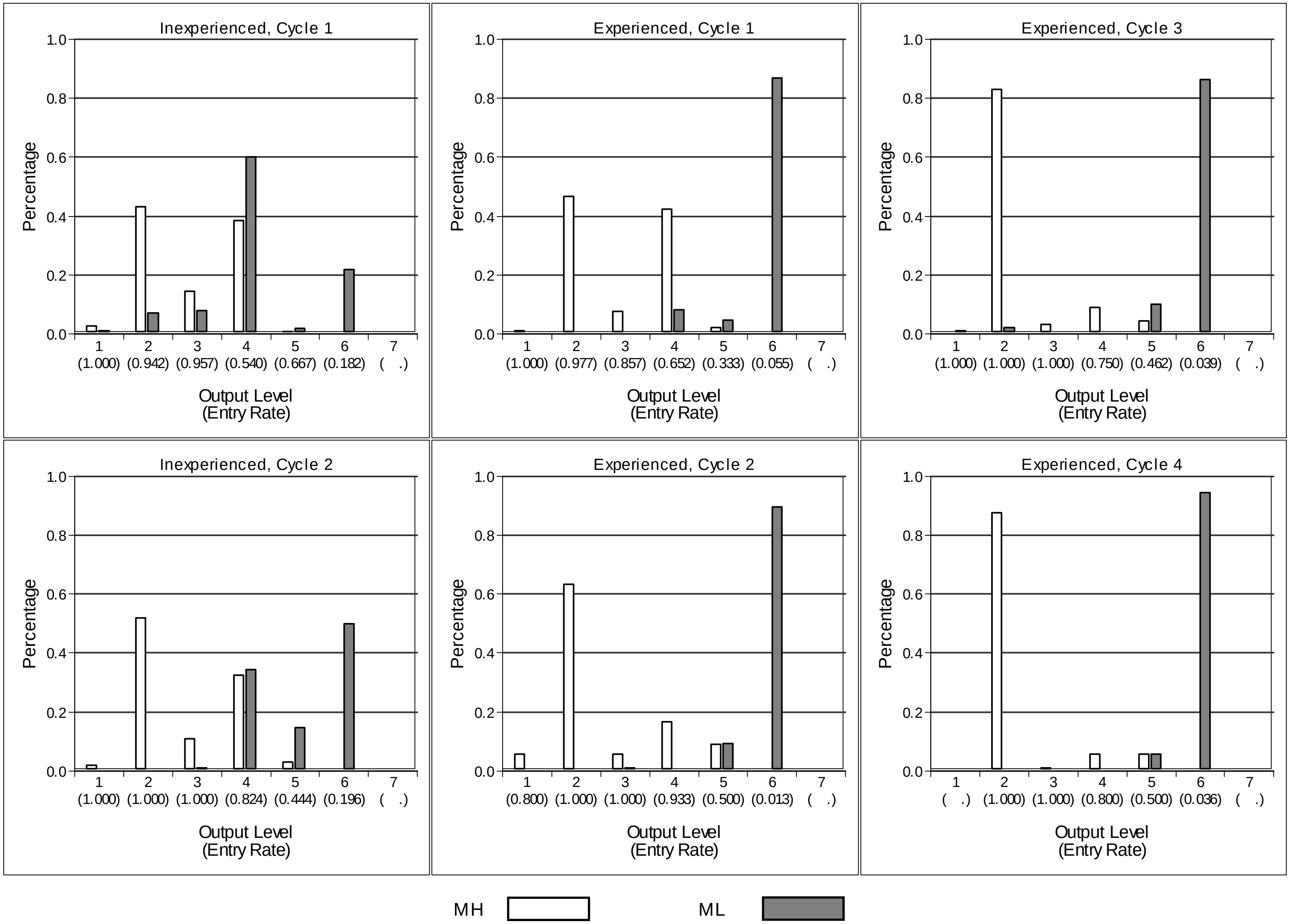


FIGURE 7. POOLED DATA FOR 2X2 SESSIONS WITH LOW COST ENTRANTS AND MEANINGFUL CONTEXT.

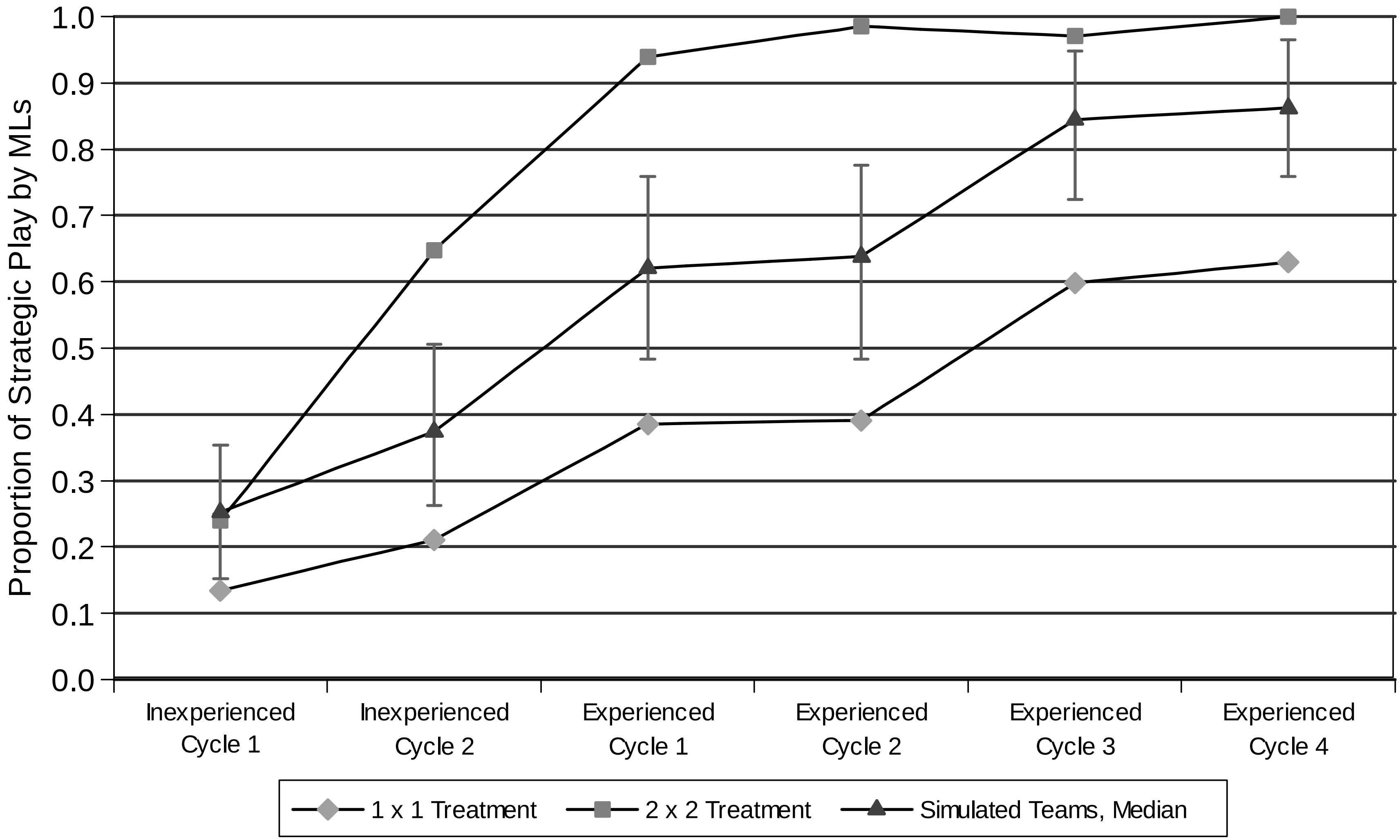


FIGURE 8. COMPARING THE DEVELOPMENT OF STRATEGIC PLAY FOR MLS IN 2X2 WITH 1X1 SESSIONS IN GAMES WITH LOW COST ENTRANTS AND MEANINGFUL CONTEXT