Directional Behavioral Spillover and Cognitive Load Effects in Multiple Repeated Games*

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Abstract

In this study, we use a novel design to test for directional behavioral spillover and cognitive load effects in a set of multiple repeated games. Specifically, in our experiment, each subject plays a common historical game with two different matches for 100 rounds. After 100 rounds, the subject switches to a new game with one match and continues playing the historical game with the other match. This design allows us to identify the direction of any behavioral spillover. Our results show that participants exhibit both *behavioral spillover* and *cognitive load* effects. First, for pairs of Prisoners' Dilemma and Alternation games, we find that subjects apply strategies from the historical game when playing the new game. Second, we find that those who participate in a Self Interest game as either their historical or new game achieve Pareto efficient outcomes more often in the Prisoners' Dilemma and Alternation games compared to their control counterparts. Overall, our results show that, when faced with a new game, participants use strategies that reflect both behavioral spillover and cognitive load effects.

Keywords: multiple games, repeated games, behavioral spillovers, cognitive load, entropy.
JEL Classification: C72, C91, D03

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1 Introduction

Laboratory experiments in economics are often compared to a wind-tunnel (Plott 1994). That is, economists first develop a new mechanism with superior theoretical properties and then assess its performance relative to theory and what it was created to do (Chen and Plott 1996). If the mechanism performs well in the laboratory, it is potentially put to use in the real world. However, when a new mechanism or game is implemented among real people in the real world, the way people behave is influenced not only by the mechanism but also by their previous experience playing similar games.

Furthermore, previous research has found that game behavior is also influenced by the distinct sets of experiences or cases that lead communities to draw different analogies when constructing strategies (Gilboa and Schmeidler 1995). This stream of research has policy implications for institutional interventions by suggesting that taking behavioral influences into account may lead to greater intervention success. Our research lends insight into this area by examining how individual behavior is influenced by behavior in previous interventions.

Our research also contributes to the field of game theory and experimental economics. Within this field, studies have suggested that social scientists may need to consider the full ensemble of games that an individual faces when examining behavior in one particular game (Samuelson 2001, Bednar and Page 2007). Indeed, multiple game experiments can provide deeper insights into both individual and collective behavior in decision-making scenarios (Section 2). On an individual level, these experiments provide a laboratory in which subjects find themselves in a more cognitively-taxing environment, one that resembles real world situations in which multiple stimuli compete simultaneously for a person's attention. On a collective level, these experiments may contribute to an institutional explanation for behavioral variations in the play of common games.

In our study, we conduct a series of multiple game experiments and find both behavioral path dependence and cognitive load effects. We further identify two effects of the multi-game treatments at both the strategic and outcome level: first, the multiple game environment creates a cognitive load that affects subjects' ability to respond optimally, and second, subjects apply behaviors from one game to another – what we call a *behavioral spillover* effect. That is, we find that behaviors spill over from one game to another, but only when both games require a sufficient cognitive load. By contrast, if a preceding game requires little cognitive effort, we find that a participant's performance in the subsequent game improves compared to controls. This latter finding differs from that found in earlier experiments using a simultaneous game design (Bednar, Chen, Liu and Page 2012, Cason and Gangadharan 2013).

In our experimental design, individuals begin by playing an identical game with two opponents. After 100 rounds, we replace one of those games with a new game that they play with one opponent while continuing to play the historical game with their other opponent. In comparison, those in the control condition continue to play the historical game throughout all rounds of the experiment. This design allows us to test for the direction of any behavioral spillover effect. Specifically, we can observe whether players apply behavior learned from the first 100 rounds to their behavior the second set of rounds. This design also allows us to test how the size of that spillover depends on the cognitive load required by each game.

Our design further allows us to test for both path dependent behaviors and outcomes (Page 2006). The former arise when behavior in the first game influences behavior in the second game. The latter arise when that behavior leads to a different outcome. Note that the former need not imply the latter. For example, subjects who play an alternation game might later cooperate in a Prisoners' Dilemma game by playing Tit for Tat, whereas subjects who first play a game that promotes selfish behavior might subsequently cooperate by playing Grim Trigger. Each example shows that a participant chooses to cooperate in the subsequent game (outcome), but the behavior (reason for doing so) differs.

We also test whether an individual's play in the new game is influenced by her play in the historical game, and whether the new game can influence the play in the continued historical game, a form of reverse path dependence (Bednar, Page and Toole 2012a). Evidence shows that playing multiple games simultaneously can induce behavioral spillovers across games whereby individuals choose similar strategies in the two games (Bednar et al. 2012, Cason, Savikhin and Sheremeta 2012, Cason and Gangadharan 2013). Past experimental research also finds that participating in multiple games increases an individual's cognitive load, thus preventing her from choosing efficient or even equilibrium behaviors.

By adding games sequentially, we can test for behavioral spillovers from one game to another as opposed to a crossover effect between games. Further, by having subjects continue to play the historical game with one of their opponents, we guard against the potential that an individual may clear her cognitive register related to the first game. In designs where individuals first play one game and then another, it would be possible for a subject to forget the first game. That cannot happen here. Last, by including games that require different levels of cognitive effort, we can test how the accumulation or diminution of cognitive load influences the spillover effect. The results from our experiment show a behavioral spillover effect only when both games require a high cognitive load. To measure cognitive load, we follow the approach in Bednar et al. (2012) and measure the entropy of action pairs.[1] Doing so, we find that subjects who play a historical game with a high cognitive load (high entropy), such as Strong Alternation, are more likely to use an alternation strategy when they next play a game like the Prisoners' Dilemma. As noted, these findings hold at both the outcome and repeated game strategy level.

Conversely, we find that subjects who play a game with a minimal cognitive load such as Self Interest do not exhibit a selfish strategy when they next play the Prisoners' Dilemma. Instead, they are more likely to play Pareto efficient outcomes in the Prisoners' Dilemma game, as compared to the control group. One interpretation of these findings is that subjects who are not cognitively taxed in one game can thus focus more on the second game, where they then play more efficiently. Alternatively, the findings may reflect the case where the norm of efficiency spills from one context to the other (North 2005). This would imply a transfer of learning at the level of norm (Cooper and Kagel 2008). However, comparing our results to those of earlier experiments with identical payoffs suggests this is not the case.

Furthermore, when the new game introduced after round 100 imposes a low cognitive load (low entropy), we find that subjects are more likely to decrease their level of selfish play in the remaining historical game, exhibiting a feedback effect from the new game.

To gain greater insight into our findings, we examine whether there is a transfer of learning at the individual repeated game strategy level, and find results consistent with those from our outcome level analyses. First, we observe that subjects transfer their chosen game strategy from the historical game to the new game, and their learned strategy from early to later rounds of the historical game. Second, we find a difference between our control and treatment groups in the

[1] To measure the behavioral variation in a game, we apply a standard entropy measure to the outcome distributions (Shannon 1948), which we will explain in detail in Section 4.

likelihood of transferring repeated game strategies. For example, we find that treatment subjects are less likely to transfer the repeated game strategy from the Self Interest game to the new game. Third, we also find a difference between our control and treatment groups in the type of strategy transferred. For example, we find that treatment subjects who begin with the Self Interest game are less likely to keep using a selfish strategy when they then play the Strong Alternation game.

To frame our experimental findings, we focus on our contributions to the growing body of research on multiple games and transfer of learning. This literature provides a clearly-delineated context within which we can interpret our specific findings on the size and direction of the effects (Bednar et al. 2012, Cason and Gangadharan 2013). However, we also consider our contributions to the literature on institutional path dependence and cultural effects (Bednar, Jones-Rooy and Page 2015, Bednar and Page 2017) in the conclusion, where we suggest avenues for future research.

As one of the pioneers of experimental economics, Professor Charles Plott has designed numerous experiments testing the performance of various institutions in the laboratory, with topics ranging from industrial organization (Plott 1982), financial markets (Lei, Noussair and Plott 2001), public economics (Plott, Rullère and Villeval 2011), to political science (Fiorina and Plott 1978). Our research continues and complements his path-breaking work by introducing behavioral influences from institutional context and demonstrating their effects on the performance of new institutions.

The remainder of the paper is organized as follows. In Section 2, we review the relevant literature. Section 3 presents the experimental design. In Section 4, we present our analysis and results. Section 5 discusses the results and concludes.

2 Literature Review

As mentioned, our research contributes directly to the literature in experimental economics and psychology examining the respective roles of framing, transfer of learning, behavioral spillovers, and cognitive load in the context of multiple games. These experimental findings also underpin theoretical research showing cultural effects on institutional performance by providing a cognitive mechanism in the form of behavioral spillovers (Aoki 2001, Bednar and Page 2017). Within the multiple games experimental literature, researchers use either a sequential or simultaneous experimental design. In the sequential design, subjects first play one game and then play a second game. By contrast, in the simultaneous design, individuals play multiple games concurrently, with either the same opponent or different opponents.

Using a sequential design, Albert, Guth and Kirchler (2007) find that individual contributions in the first stage of a charity donation significantly affect their opponents' choice in the second stage of a Prisoner's Dilemma game by serving as a signal of an individual's likelihood of cooperating. In another sequential study, Bettenhausen and Murnighan (1985, 1991) find that the cooperation norm can be transferred between different Prisoner's Dilemma games. In other studies, Van Huyck, Battalio and Beil (1991), Devetag (2005), and Cason, Lau and Mui (2013) find a positive transfer of learning between different coordination games. In particular, Cason et al. (2013) find the existence of a "teacher" player who teaches her matched opponent to coordinate by sacrificing her individual payoff in initial periods of game play. Additionally, Cason et al. (2012) find that the behavioral spillover from a median- to minimum-effort game helps individuals achieve better coordination in the minimum-effort game.

Studies have also found a positive transfer of learning between games with different payoff structures. For example, Knez and Camerer (2000) find a positive cooperation transfer when players move from finitely repeated coordination games to finitely repeated Prisoner's Dilemma games. However, they find that this positive transfer disappears when the Pareto efficient outcomes are labeled differently. Similarly, Ahn, Ostrom, Schmidt, Shupp and Walker (2001) find that the efficient play utilized in playing repeated coordination games improves cooperative play in the subsequent one-shot Prisoner's Dilemma game, especially for fixed-matching pairs. Cooper and Kagel (2008) likewise find a positive learning transfer from one signaling game where both pooling and separating equilibria exist to a second signaling game which supports only separating equilibria. Finally, Kagel (1996) finds a positive transfer of learning in the continuous strategy space of auctions, but only in the direction from a first-price sealed bid auction to an English auction.

It is worth noting that the transfer of learning from one game to the next can sometimes produce lower payoffs as behavioral stickiness may end up stifling incentives. For example, Cason and Gangadharan (2013) find that competitive behaviors developed in double auction settings actually lower the likelihood of cooperation in a subsequent public goods game. In another study, Kagel and Levin (1986) find that bidders who learn to avoid the winner's curse in a first-price sealed bid auction with four bidders will continue to bid similar amounts even when the number of bidders increases from four to seven. As a result, they earn negative profits.

In a study of sequential games, Mengel and Sciubba (2010) find that when individuals play a subsequent game strategically similar to their first game, they converge to a Nash equilibrium more quickly in the subsequent game. However, they exhibit slower convergence if the two games are strategically different, indicating that learning to play a game by iterated elimination of dominated strategies slows the ability to converge in a subsequent coordination game. They further find a significant labeling effect between different coordination games, suggesting that action labeling can be an effective tool for inducing efficient outcomes in coordination games. Finally, while most of the sequential design literature shows the existence of learning transfer, some studies, such as Duffy and Fehr (2017), find no evidence of a strategy transfer when players move from a Prisoners' Dilemma to Stag Hunt game.

In addition to those studies using a sequential design, there are several that examine learning transfer when games are played simultaneously. These studies find a transfer of learning but not necessarily greater efficiency. For example, McCarter, Samak and Sheremeta (2014) find that the amount of group contribution in one voluntary contribution mechanism public goods game has a negative impact on individual contributions in a second voluntary contribution mechanism when the games are played simultaneously.

To identify spillovers that increase efficiency requires a theory for the direction spillovers take. Savikhin and Sheremeta (2013) find that participation in a voluntary contribution public goods game decreases overbidding in a simultaneously played lottery contest. In this case, being co-operative in the public goods game attenuates potential ineffective competitiveness in the lottery setting. Overall, they find that direction of the spillover depends on the strategic uncertainty of the games. This result echoes an earlier finding by Bednar et al. (2012), who show that, for multiple pairs of games, the direction of the behavioral spillover tends to be from lower to higher entropy games. For example, when the Prisoners' Dilemma (PD) game is paired with a Self Interest (SI) game in which the dominant strategy Nash equilibrium and the Pareto efficient outcome involve defecting, they find that subjects are more likely to choose to defect in the PD game than are their control counterparts.

Our experimental design is unique in that it draws from both the sequential and simultaneous design literature. Like sequential experiments, our design gives one game temporal precedence. However, unlike sequential experiments, the historical game also continues to be played as a multiple game experiment. This design enables us to identify the direction of flow in any potential behavioral spillover. Specifically, our design allows us to test if game A affects B (and vice versa) depending on how the games are ordered.

3 Experimental Design

In this section, we describe the specific games included in our study and provide a detailed description of our experimental procedures.

3.1 The Games

As we are interested in path dependency in a multiple game setting, we choose three games from the games outlined in Bednar et al. (2012): the Prisoner's Dilemma (PD), the Strong Alternation (SA), and the Self Interest game (SI). These individual games belong to a class of two-person two-action games that contain a self-regarding action, S, and a cooperative action, C. The first game is a standard Prisoner's Dilemma game, where the stage game has a dominant strategy equilibrium, (S, S), which is Pareto dominated by (C, C).

Prisoner's Dilemma: (PD)

	C	S
C	7, 7	2,10
S	10,2	4,4

In the second game, Strong Alternation (SA), while (S, S) remains the dominant strategy equilibrium for the stage game, agents maximize their joint payoff by alternating between the off diagonals, (C, S) and (S, C). In the Strong Alternation game, constructing an alternating behavior pattern requires substantial coordination and cognitive effort.

Strong Alternation: (SA)

	C	S
C	7, 7	4,14
S	14,4	5,5

In the final game, Self Interest (SI), the dominant strategy equilibrium, (S, S) is Pareto efficient, unlike PD or SA. As such, SI is the easiest game to play.

		C	S
Self Interest:	C	7, 7	2,9
(SI)	S	9,2	10,10

Although these three games have the same dominant strategy Nash equilibrium, (S,S), for the stage game, they have different joint payoff maximizing outcomes: (C,C) in PD, alternating between (C, S) and (S,C) in SA, and (S,S) in SI.

In the infinitely repeated setting, the minmax payoff is 4 for PD, 5 for SA, and 9 for SI. Based on the Folk Theorem (Friedman 1971, Abreu and Rubinstein 1988), the average payoff in the repeated game should weakly dominate the minmax payoff. Therefore, for PD and SA, all three outcomes are subgame perfect, including cooperation, (coordinated) alternation, and selfish, for both players. The convex hull of a vector set, which refers to the four payoff vectors in stage games, characterizes the set of subgame perfect Nash equilibrium outcomes. By contrast, in SI, the only subgame perfect Nash equilibrium outcome is (S, S). Given that PD and SA both have a large number of repeated game equilibria, subjects might attempt to coordinate on an equilibrium, which may or may not be efficient.

3.2 Experimental Procedure

Our experiment consists of four control sessions, each of which consists of an ensemble of the same games, and 12 treatment sessions, each of which consists of a pair of the same historical game in the first 100 rounds, and a new game-historical game pairing that begins after round 100. This experimental design enables us to determine the effects of the historical game on behavior in the new game, and vice versa.

The control sessions follow a common protocol for infinitely repeated game ensembles in the laboratory. Specifically, each of our PD and SA games consists of two 12-player sessions. Within each session, at the beginning of the experiment, each player is randomly matched with two other participants, both of whom will be her matches for the entire experiment. Subjects are informed about the fixed-matching protocol at the beginning of the experiment. During the experiment, each player firsts plays the same game with each of her matches. For example, in the PD control, a player plays one PD with her left match, and another PD with her right match. This design allows us to control for the ensemble feature in each session. Our specific matching protocol is: $\underbrace{4-2-1-3}$,

$\underbrace{6-5-7-8}$, $\underbrace{10-9-11-12}$. Using this procedure, we have three independent groups that comprise each session, where each subject has a unique left and right match, respectively. This yields six independent observations for our PD and SA control groups, respectively. Note that we do not include SI control sessions since there is little behavioral variation whenever SI is played, either alone or as part of an ensemble (Bednar et al. 2012).

In each session for both our control and treatment groups, each subject first plays both games for 200 rounds. After round 200, games have a 90% chance of continuing to the next round. We implement an infinitely repeated game, with a discount factor of 1 for the first 200 rounds, and 0.9 thereafter. Doing this ensures that (C, C) is one of the repeated game equilibria outcomes in the PD and SA games.

In each treatment session, during the first 100 rounds, each subject likewise plays a historical game with both of her matches. However, after round 100, she switches to a new game with one of her matches while continuing the historical game with her other match. We use each game as a historical game and as a new game, generating six treatment groups in total. Furthermore, as the two games for a given participant are displayed side by side, we conduct two independent sessions for each game ensemble, changing the order of the display to avoid any potential order effect. That is, if a player always makes decisions from left to right, we have a balanced number of observations for each order.

Furthermore, at the beginning of a session, subjects in the treatment groups are told that one of the historical games will be replaced by a new game after 100 rounds. At this time, we also explain the payoff matrix of the new game. Note that there is no announcement during the experiment when the game switch is made. Subjects are informed about their two matches' decisions and their payoffs in each game at the end of each round. To mimic the perfect recall setting, each subject is provided with a history window indicating her decision as well as those of her matches in all previous rounds.

Table 1 reports the features of our experimental sessions, including the game played, the number of players in each session (n), the number of independent groups for each control session, the ensemble of games, the number of players in each session, and the number of independent groups in each ensemble treatment session.

Overall, 16 independent computerized sessions were conducted in the Behavioral Economics

Table 1: Features of Experimental Sessions

Ensemble Control			Ensemble Treatment		
Game	n	Groups	(Left, Right)→ (Left, Right)	n	Groups
PD	12	3	(PD, PD)→(PD, SA)	12	3
PD	12	3	(PD, PD)→(SA, PD)	12	3
			(PD, PD)→(PD, SI)	12	3
			(PD, PD)→(SI, PD)	12	3
SA	12	3	(SA, SA)→ (SA, PD)	12	3
SA	12	3	(SA, SA)→(PD, SA)	12	3
			(SA, SA)→(SA, SI)	12	3
			(SA, SA)→(SI, SA)	12	3
			(SI, SI)→ (SI, PD)	12	3
			(SI, SI)→(PD, SI)	12	3
			(SI, SI)→(SI, SA)	12	3
			(SI, SI)→(SA, SI)	12	3
Total	48	12		144	36

and Cognition Experimental Lab at the University of Michigan between February 2009 and October 2013, yielding a total of 192 subjects. We used z-Tree (Fischbacher 2007) to program our experiments. Our subjects were students from the University of Michigan, recruited by email from a subject pool for economic experiments.[2] Participants were allowed to participate in only one session. Each session lasted approximately 90 minutes, with the first 10 minutes used for instructions. The exchange rate was set to 100 tokens for \$1. In addition, each participant was paid a \$5 show-up fee. The average earnings per participant (including the show-up fee) were \$35 for those in the treatment sessions and \$32 for those in the control sessions. A copy of the experimental instructions are included in Appendix B. Data are available from the authors upon request.

4 Results

In this section, we first report our outcome results in Subsection 4.1. We then report our results for the repeated game strategies for our control and treatment conditions in Subsection 4.2.

[2]Graduate students from the Economics Department are excluded from the list.

Throughout our analysis, we treat each group of four subjects as an independent observation. Given the random stopping point for each session, different sessions last for a different number of rounds. Therefore, we report our analysis for the first 100 rounds and the second 100 rounds to make the different treatments comparable.[3] We use the notation (Game 1, Game 2) to represent sessions with the same game ensemble but different position displays. For example, treatment (SA,SA)→(SA,SI) includes both (SA,SA)→(SA,SI) and (SA,SA)→(SI,SA).

Finally, following Bednar et al. (2012), we measure the outcome uncertainty for each game using entropy and consider this to be a proxy for the cognitive effort required to play the game. Specifically, we define the *entropy* of a random variable X with a probability density function, $p(x) = Pr\{X = x\}$, as $H(X) = -\sum_x p(x) \log_2 p(x)$, which measures the amount of stochastic variation in a random variable that assumes a finite set of values (Shannon 1948). In the context of 2×2 games, the minimum entropy of zero indicates perfect convergence to one outcome, whereas the maximum entropy of two indicates uniform outcome distribution across all four cells. In general, a higher entropy indicates that subjects either do not coordinate or are playing a sophisticated pair of strategies with multiple outcomes.

4.1 Outcome Level Analysis

We first examine the behavioral variation in the historical game between the control and treatment conditions. Table 2 (3) reports the aggregate distribution of outcomes in PD (SA) control and treatments for the first and second 100 rounds, respectively. These results show that, in both our PD (SA) control and treatment groups, the average entropy for the historical game significantly decreases from the first 100 to the second 100 rounds ($p \leq 0.05$, one-sided signed rank tests), suggesting that outcome distribution in the historical game becomes less dispersed in the second 100 rounds, likely due to individual learning.

We next compare the distribution of our three outcomes, cooperation (CC), selfish (SS) and alternation (ALT), between the control and treatment conditions. Here, we are especially interested in the joint-payoff maximizing outcomes for each game, i.e., cooperation in PD, selfish in SI, and alternation in SA, as well as the conditions under which transfer of learning occurs.

[3] We also repeat all analyses replacing the second 100 rounds with all rounds beyond 100, and find similar results.

Table 2: Outcome Distribution and Entropy in PD Control and Treatments

Control (PD, PD)	First 100 Rounds					Second 100 Rounds					
	Historical Game 1			Historical Game 2			Historical Game 1			Historical Game 2	
	C	S		C	S		C	S		C	S
C	38.17	10.92	C	34.58	12	C	47.92	6.25	C	43.58	5.42
S	9.58	41.33	S	9.83	43.58	S	4.17	41.67	S	3.83	47.17
Average Entropy	1.25					0.57					
(PD,PD)→(PD,SA)	First 100 Rounds					Second 100 Rounds					
	Historical Game 1			Historical Game 2			Historical Game 1			New Game SA	
	C	S		C	S		C	S		C	S
C	42.75	12.08	C	44.75	11.33	C	57.92	6.92	C	25.83	19.17
S	15.17	30	S	10.42	33.5	S	8.42	26.75	S	20.42	34.58
Average Entropy	1.34						0.68			1.29	
(PD,PD)→(PD,SI)	First 100 Rounds					Second 100 Rounds					
	Historical Game 1			Historical Game 2			Historical Game 1			New Game SI	
	C	S		C	S		C	S		C	S
C	18.42	13.33	C	22.17	10.5	C	34.75	2.5	C	0.58	0.5
S	9.5	58.75	S	11.92	55.42	S	3.75	59	S	0.67	98.25
Average Entropy	1.05						0.41			0.11	

Notes: The average entropy for both games is the mean of a pair's average entropy between two games.
The average entropy for each game in the second 100 rounds of treatments is the mean of a pair's entropy in a game.

Table 3: Outcome Distribution and Entropy in SA Control and Treatments

Control (SA, SA)	First 100 Rounds						Second 100 Rounds				
	Historical Game 1			Historical Game 2			Historical Game 1			Historical Game 2	
	C	S		C	S		C	S		C	S
C	9.33	22.5	C	11.92	19.25	C	1.08	24.08	C	9.25	19.42
S	31.58	36.58	S	22.75	46.08	S	36.33	38.5	S	23.75	47.58
Average Entropy	1.22						0.81				
(SA,SA)→(SA,PD)	First 100 Rounds						Second 100 Rounds				
	Historical Game 1			Historical Game 2			Historical Game 1			New Game PD	
	C	S		C	S		C	S		C	S
C	14.25	23.67	C	8.75	26.67	C	14.25	34.42	C	46.67	12.92
S	32.83	29.25	S	27.75	36.83	S	36.5	14.83	S	13.83	26.58
Average Entropy	1.53						1.09			0.74	
(SA,SA)→(SA,SI)	First 100 Rounds						Second 100 Rounds				
	Historical Game 1			Historical Game 2			Historical Game 1			New Game SI	
	C	S		C	S		C	S		C	S
C	11.58	15.42	C	14.58	17.75	C	28.67	22	C	0	0.67
S	19.33	53.67	S	23.83	43.83	S	22.17	27.17	S	3.33	96
Average Entropy	1.38						1.02			0.19	

Notes: The average entropy for both games is the mean of a pair's average entropy between two games.

The average entropy for each game in the second 100 rounds of treatments is the mean of a pair's entropy in a game.

We first report evidence for behavioral spillovers from the historical game on the new game introduced after round 100. Our findings align with previous experimental research and reveals behavioral spillovers across games (Section 2). Here, when we add a cognitively demanding game to an existing cognitively demanding game, we would expect the behavior from the first game to spill over and contaminate behavior in the new game. As subjects only play 100 rounds of the new game, the relevant comparison will be between the first 100 rounds of the new game control setting versus the 100 rounds when a new game is added in a treatment. This leads to our first hypothesis:

Hypothesis 1 (Behavior spillover from a high-entropy historical game to a high-entropy new game)**.** *When a high-entropy new game is added to a high-entropy historical game, outcomes in the first 100 rounds of the new game will be more likely to match the efficient outcome of the historical game than if the new game is played by itself.*

To test Hypothesis 1, we consider only those sequences of games involving PD and SA. We first compare outcomes for the first 100 rounds of the SA control to the 100 rounds of the SA when it is added after players participate in the PD game. In this situation, we expect that participant outcomes in the SA when it is a new game added to PD will show greater cooperation than when SA is played in the control condition. This is consistent with the idea that cooperative behavior, an inefficient strategy in SA, will spill over from PD to SA.

Likewise, we also need to compare outcomes for the first 100 rounds of the PD control to the 100 rounds of PD when it is added to the SA game. By the same reasoning, we expect to see a greater likelihood of alternation in PD when it is a new game than when it is not. If so, this would indicate a behavioral spillover effect, since alternation as a PD strategy is both inefficient and more cognitively taxing than cooperation.

Result 1 (Behavior spillover from a high-entropy historical game to a high-entropy new game)**.** *When SA is introduced as a new game in (PD,PD)→(PD,SA), subjects cooperate significantly more in SA compared to the first 100 rounds in our SA control sessions (0.26 vs. 0.11,* $p = 0.035$*, one-sided permutation test). Likewise, when PD is introduced as a new game in (SA,SA)→(SA,PD), subjects alternate weakly more in PD compared to the first 100 rounds in our PD control sessions (0.20 vs. 0.07,* $p = 0.094$*, one-sided permutation test).*

Support. *Tables 2 and 3 report the respective outcome distributions for our PD and SA control and treatment groups. The outcome distribution for SA as a new game in (PD,PD)→ (PD,SA) is reported in the middle panel of Table 2, whereas that for SA in the first 100 rounds of our SA control sessions is reported in the top panel of Table 3. Statistics supporting the second half of the result are reported similarly in Tables 3 and 2, respectively.*

By Result 1, we reject the null in favor of Hypothesis 1. We find further evidence in support of Hypothesis 1 when we compare new-game outcomes across treatment groups. For example, we find that alternation is used as a PD strategy more when PD is the new game added to (SA,SA)→(SA,PD) compared to when it is the new game added to (SI,SI)→(SI,PD) (0.20 vs. 0.04, $p = 0.083$, one-sided permutation test).

Table 4: Outcome Distribution and Efficiency in New Games

SA in the first 100 rounds for control and second 100 rounds for treatments				
	SS	CC	ALT	Efficiency
SA Control	41	11	32	0.534
(PD,PD)→(PD,**SA**)	35	26	29	0.525
(SI,SI)→(SI,**SA**)	16	27	49	0.705
Control vs. (PD,PD)→(PD,**SA**)	0.293	0.035	0.424	0.469
Control vs. (SI,SI)→(SI,**SA**)	0.011	0.028	0.159	0.071
(PD,PD)→(PD,**SA**) vs. (SI,SI)→(SI,**SA**)	0.059	0.427	0.097	0.059
PD in the first 100 rounds for control and second 100 rounds for treatments				
	SS	CC	ALT	Efficiency
PD Control	42	36	7	0.505
(SA,SA)→(SA,**PD**)	27	47	20	0.645
(SI,SI)→(SI,**PD**)	18	67	4	0.772
Control vs. (SA,SA)→(SA,**PD**)	0.198	0.299	0.095	0.220
Control vs. (SI,SI)→(SI,**PD**)	0.047	0.028	0.061	0.036
(SA,SA)→(SA,**PD**) vs. (SI,SI)→(SI,**PD**)	0.338	0.132	0.088	0.238

Notes: The last three rows in each panel report p-values of one-sided permutation tests.

We next outline our hypothesis when the historical game is the low-entropy SI game. Previous studies of simultaneous multiple games find a behavior spillover effect from low- to high- entropy games (Bednar et al. 2012). However, here subjects first played only SI games and achieved efficient outcomes. We hypothesize that this would free up cognitive resources to focus on the more cognitively demanding game. This leads to our second hypothesis:

Hypothesis 2 (Behavior spillover from a low-entropy historical game to a new high-entropy game)**.** *When a new high-entropy game is added to a low-entropy historical game, outcomes in the first 100 rounds of the new game will exhibit more Pareto efficient play than if the new game is played by itself.*

To test Hypothesis 2, we compare outcomes for the first 100 rounds of the SA (PD) control group to the 100 rounds of SA (PD) when it is added to the SI game. Our results show that the proportion of Pareto efficient outcomes increases in both cases, but only significantly so for the PD game.

Table 4 reports the outcome distribution and efficiency in the SA (upper panel) and PD (lower panel) control and treatment groups, where each is the respective new game. By convention, we define normalized efficiency as follows:

$$\text{Efficiency} = \frac{\text{Actual joint payoffs} - \text{Minimum joint payoffs}}{\text{Maximum joint payoffs} - \text{Minimum joint payoffs}}.$$

Result 2 (Behavior spillover from a low-entropy historical game to a new high-entropy game)**.** *When PD is introduced as a new game in (SI,SI)→(SI,PD), we find that subjects cooperate significantly more compared to the first 100 rounds in PD control sessions (0.67 vs. 0.36, $p = 0.028$, one-sided permutation test), leading to greater efficiency for PD in the treatment versus control group (0.772 vs. 0.505, $p = 0.036$, one-sided permutation test).*

Support. *The bottom panel of Table 4 presents the outcome distributions and efficiency results (last column) for the first 100 rounds of the PD control as well as the first 100 rounds of PD as a new game in (SI,SI)→(SI,PD). The corresponding p-values are presented in the second-to-last row.*

Likewise, the results in the top panel of Table 4 for when SA is added as a new game show that subjects alternate more in the SA treatment group compared to the first 100 rounds of SA control. However, this difference is not significant (0.49 vs. 0.32, $p = 0.159$, one-sided permutation test). Consequently, we find that efficiency in the SA treatment versus control group is only marginally higher (0.705 vs. 0.534, $p = 0.071$, one-sided permutation test).

By Result 2, we reject the null in favor of Hypothesis 2. Specifically, Result 2 suggests that first playing a low-entropy game subsequently enables individuals to achieve Pareto efficient outcomes more quickly in the second game.

In addition to our results regarding the impact of a high- or low-entropy historical game on subsequent behavior in a new game, we are interested in whether cognitive load impacts efficiency in the historical game. Here, we expect that introducing a new game with low entropy allows subjects to free up cognitive resources which they can then devote to the remaining historical game. Thus, we expect that the increased availability of cognitive resources should result in more efficient outcomes in the second 100 rounds of the historical game. This leads to our third hypothesis.

Hypothesis 3 (New game effect: Cognitive load reduction)**.** *When replacing a historical game with a new low-entropy game, the increase in Pareto efficient outcomes in the remaining historical game should be greater than the increase achieved by the control group.*

In other words, Hypothesis 3 implies that, in PD (SA) treatments, when SI replaces one of the PD (SA) games, the proportion of CC (ALT) in the remaining PD (SA) should be higher than that in the corresponding control group.

Table 5 reports the outcome distributions for the treatment and control groups when SA (upper panel) and PD (lower panel) are the respective historical games, while Table 6 reports the corresponding efficiency measures. To measure the new game effect on the remaining historical game, we compare the increment of each measure by subtracting the proportion in the first 100 rounds from that in the second.

Result 3 (New game effect: Cognitive load reduction)**.** *After adding SI as a new game in (SA,SA)→(SA,SI), subjects show significantly less selfishness and significantly more cooperation in their historical game strategies, as compared to their control counterparts, leading to a marginally significant improvement in efficiency.*

Support. *The upper panel of Table 5 presents the outcome distributions for our SA control and treatment groups. Specifically, we find that the proportion of SS changes by 2 percentage points (p.p.) in the SA control group versus -22 p.p. when SI is added as the new game ($p = 0.006$, one-sided permutation tests). Likewise, we find that the proportion of CC changes by -5 p.p. in*

Table 5: Outcome Distribution in Historical Games

Treatment Name	First 100			Second 100			Second−First 100		
	SS	CC	ALT	SS	CC	ALT	SS	CC	ALT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SA Control	41	11	32	43	5	41	2	-5	9
(SA,SA)→(SA,PD)	33	12	35	15	14	65	-18	3	30
(SA,SA)→(SA,SI)	49	13	19	27	29	36	-22	16	16
Control vs. (SA,SA)→(SA,PD)	0.204	0.430	0.393	0.026	0.166	0.135	0.007	0.240	0.056
Control vs. (SA,SA)→(SA,SI)	0.199	0.377	0.167	0.096	0.030	0.398	0.006	0.007	0.241
(SA,SA)→(SA,PD) vs. (SA,SA)→(SA,SI)	0.009	0.407	0.058	0.119	0.149	0.107	0.311	0.136	0.165

Treatment Name	First 100			Second 100			Second-First 100		
	SS	CC	ALT	SS	CC	ALT	SS	CC	ALT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PD Control	42	36	7	44	46	4	2	9	-3
(PD,PD)→(PD,SA)	32	44	8	27	58	6	-5	14	-2
(PD,PD)→(PD,SI)	57	20	4	59	35	1	2	14	-3
Control vs. (PD,PD)→(PD,SA)	0.219	0.312	0.364	0.200	0.285	0.442	0.271	0.334	0.432
Control vs. (PD,PD)→(PD,SI)	0.194	0.183	0.129	0.254	0.294	0.022	0.495	0.309	0.343
(PD,PD)→(PD,SA) vs. (PD,PD)→(PD,SI)	0.050	0.099	0.158	0.068	0.154	0.081	0.251	0.491	0.314

Notes: The last three rows in each panel report p-values of one-sided permutation tests.

the SA control group versus 16 p.p. when SI is added as the new game ($p = 0.007$, one-sided permutation tests). We further find an insignificant increase in the use of alternation when SI is added as the new game (9 p.p. in the SA control and 16 p.p. when SI is added; $p = 0.241$, one-sided permutation test). Our results regarding efficiency in the upper panel of Table 6 show an increase of 1 p.p. in the SA control group versus an increase of 13.8 p.p. when SI is added ($p = 0.054$, one-sided permutation test).

Overall, Result 3 suggests that the introduction of a new low-entropy game, such as SI, mitigates potential convergence towards selfish play in the historical SA game. Specifically, we find an economically sizeable increase in the use of alternation in the historical SA game, albeit not a significant one.

Table 6: Efficiency in Historical Games

Treatment Name	First 100	Second 100	Second−First 100
SA Control	0.534	0.544	0.010
(SA,SA)→(SA,PD)	0.612	0.780	0.168
(SA,SA)→(SA,SI)	0.447	0.585	0.138
Control vs. (SA,SA)→(SA,PD)	0.168	0.068	0.037
Control vs. (SA,SA)→(SA,SI)	0.149	0.373	0.054
(SA,SA)→(SA,PD) vs. (SA,SA)→(SA,SI)	0.010	0.067	0.363
Treatment Name	First 100	Second 100	Second−First 100
PD Control	0.505	0.523	0.018
(PD,PD)→(PD,SA)	0.601	0.681	0.081
(PD,PD)→(PD,SI)	0.354	0.389	0.035
Control vs. (PD,PD)→(PD,SA)	0.247	0.231	0.286
Control vs. (PD,PD)→(PD,SI)	0.182	0.270	0.418
(PD,PD)→(PD,SA) vs. (PD,PD)→(PD,SI)	0.067	0.092	0.335

Notes: The last three rows in each panel report p-values of one-sided permutation tests.

In sum, our outcome analysis shows a significant transfer of learning from the historical game to the new game, with the nature of the transfer influenced by the cognitive requirements of the historical game. We find that subjects transfer strategy labels (efficient play) when the historical game imposes a high (low) cognitive load. We also observe horizontal transfers from the new game to the historical game, especially when the new game has low entropy.

4.2 Strategy Level Analysis

Building on our outcome level analysis, we next move to a discussion of our repeated game strategy level analysis. We first infer individual strategy types based on their behavior in the first 100 rounds. We then examine whether individuals transfer their repeated game strategies from the historical game to the new game as well as whether they change their historical game strategies after the new game is introduced.

4.2.1 First 100 Rounds: Inferring Strategy Types

To identify individual participants' strategy types, we follow previous protocol and match each subject's set of actions in the first 100 rounds to a set of repeated game strategies represented by an automaton, M_j, (Rubinstein 1986; Abreu and Rubinstein 1988; Engle-Warnick and Slonim 2006). Specifically, we have the automaton predict an agent's action in round t based on her and her match's previous actions. For example, a one-state automaton, e.g., Always Cooperate, will predict that an agent should cooperate in all rounds regardless of historical play. By contrast, a two-state automaton such as Tit-for-Tat, will predict that an agent should cooperate in the first round, but copy her match's action in round $t-1$ as her action in round t. For each subject i, we calculate the *fitting proportion* for each automaton, M_j, in the first 100 rounds of each game, defined as $F_{ip}(M_j) = \sum_{t=1}^{T=100} I(a_{ip}^t)/T$. The indicator function $I(a_{ip}^t, M_j^t) = 1$ if $a_{ip}^t \in \{C, S\}$, which is subject i's action in round t for game $p \in \{\text{left}, \text{right}\}$, can be perfectly predicted by M_j. Otherwise, $I(a_{ip}^t, M_j^t) = 0$.

Preliminary analyses show that 84% of our control group observations (93% in the PD treatments, 86% in SA, and 85% in SI) can be typed by one of the nine repeated game strategies discussed in Bednar et al. (2012). Table 12 and Figure 1 in Appendix A contain the description and representation of the nine repeated game strategies. These nine strategies can be categorized into three types: (1) cooperative strategies, including Always Cooperate (AC), Forgive Once (F1), Suspicious Forgive Once (sF1), Tit-for-Tat (TFT), and Suspicious Tit-for-Tat (sTFT); (2) selfish strategies, including Always Selfish (AS) and Grim Trigger (GT); and (3) alternation strategies, including Switch after Cooperate (SAC) and suspicious Switch after Cooperate (sSAC). The remaining unclassified strategies (16%) in each group have more than one best fitting strategy. We define these as “multiple-type” individual strategies.[4]

Tables 7 and 8 present the distribution of subjects’ strategy types in each game for our control and treatment groups, respectively, with the mode of each repeated game strategy in bold type. From these distributions, we see that the best fitting strategy in all groups, with the exception of the PD control, is Always Selfish (AS). For the PD control group, the best fitting strategy is Tit-for-Tat, consistent with previous findings (Bednar et al. 2012, Hanaki, Sethi, Erev and Peterhansl 2005). Our finding of a AS dominance in nearly all our groups may reflect subject reactions to the higher cognitive load imposed by playing two games. As a way of managing this cognitive load, subjects may be more likely to choose AS than they would be in a single game setting (Bednar et al. 2012). Additionally, we find that a substantial proportion of subjects have the same best fitting strategies between their left and right games,[5] suggesting that individuals have a tendency to choose not only the same action, but the same strategy across multiple games.

4.2.2 Round 100+: Transfer of Learning

In this section, we study the effects of both the historical and new games on subject behavior at the repeated game strategy level. Specifically, we examine whether individuals transfer the best fitting strategy they develop in the first 100 rounds of game play to the new game (between-game transfer) as well as to the historical game played after the first 100 rounds (within-game transfer). We also examine whether there is a difference in learning transfer at the strategy level between our control and treatment groups.

If a subject has the same best fitting strategy in a game from the first 100 rounds to rounds 101-150, we identify this observation as a successful **transfer of repeated game strategies**. To determine if subjects transfer their strategies, we first examine if a player who adopts strategy i in a historical game, i.e., strategy i is the best fitting strategy for this game in the first 100 rounds, adopts the same strategy when this game is replaced by a new game after round 100, i.e., the player keeps strategy i as the best fitting strategy for the new game in rounds 101-150. In particular, we are interested in examining whether different historical games have a different impact on the likelihood

[4]If an observation has either AC or AS as the best fitting strategy along with other best fitting strategies, we simply categorize it as an AC (AS) type. This simplification enables us to decrease the proportion of multiple-type strategies.

[5]38% subjects in PD (SA) control sessions have the same best fitting strategies. For subjects in treatments, 48% of them have the same best fitting strategies in PD, 29% in SA, and 88% in SI treatments.

Table 7: The Distribution of Subject Types in Control

Left Game	Cooperative Type						Selfish Type			Alternation Type			Multiple Best
	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total	Fitting
Control PD	17	4	0	**25**	17	63	8	21	29	0	0	0	8
Control SA	8	4	0	17	4	33	**29**	0	29	8	8	17	21
Right Game	Cooperative Type						Selfish Type			Alternation Type			Multiple Best
	AC	F1	SF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total	Fitting
Control PD	4	0	0	**29**	**29**	63	13	13	25	0	0	0	13
Control SA	4	0	0	17	4	25	**33**	13	46	8	0	8	21

Notes: (1) Cooperative strategies include Always Cooperate (AC), Forgive Once (F1), Suspicious Forgive Once (sF1), Tit-for-Tat (TFT) and Suspicious Tit-for-Tat (sTFT).

(2) Selfish strategies include Always Selfish (AS) and Grim Trigger (GT).

(3) Alternation strategies include Switch after Cooperate (SAC) and suspicious Switch after Cooperate (sSAC).

Table 8: The Distribution of Subject Types in Treatments

Historical Game 1	Cooperative Type						Selfish Type			Alternation Type			Multiple Best
	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total	Fitting
(PD,PD)→(PD,SA)	8	4	0	**29**	25	67	17	13	29	0	0	0	4
(PD,PD)→(PD,SI)	4	4	0	4	17	29	**33**	17	50	0	0	0	21
(SA,SA)→(SA,PD)	4	0	4	17	**21**	46	13	4	17	13	8	21	17
(SA,SA)→(SA,SI)	0	0	0	13	8	21	**33**	21	54	0	17	17	8
(SI,SI)→(SI,PD)	0	0	0	0	0	0	**75**	0	75	0	0	0	25
(SI,SI)→(SI,SA)	0	0	0	0	0	0	**92**	0	92	0	0	0	8
Historical Game 2	Cooperative Type						Selfish Type			Alternation Type			Multiple Best
	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total	Fitting
(PD,PD)→(PD,SA)	8	0	0	**29**	**29**	67	21	13	33	0	0	0	0
(PD,PD)→(PD,SI)	0	4	0	13	21	38	**38**	21	58	0	0	0	4
(SA,SA)→(SA,PD)	4	8	4	8	4	29	**25**	13	38	8	8	17	17
(SA,SA)→(SA,SI)	8	0	4	13	8	33	**29**	21	50	8	0	8	13
(SI,SI)→(SI,PD)	0	0	0	0	0	0	**71**	4	75	4	0	4	21
(SI,SI)→(SI,SA)	0	0	0	0	0	0	**96**	0	96	0	0	0	4

Notes: (1) Cooperative strategies include Always Cooperate (AC), Forgive Once (F1), Suspicious Forgive Once (sF1), Tit-for-Tat (TFT) and Suspicious Tit-for-Tat (sTFT).

(2) Selfish strategies include Always Selfish (AS) and Grim Trigger (GT).

(3) Alternation strategies include Switch after Cooperate (SAC) and suspicious Switch after Cooperate (sSAC).

(4) One subject in (SA,SA)→(SA,SI) has both AC and AS as the best fitting strategies and each is 50%.

(5) Historical Game 1 is the one played more than 200 rounds. Historical Game 2 is the one replaced by the new game after round 100.

Table 9: The Proportion of Transferring Strategies between Games

Transfer Strategies from (PD/SI) in the First 100 Rounds to SA in Round 101-150				
Treatment Name	Any Strategies	Cooperative	Selfish	Alternation
	(1)	(2)	(3)	(4)
(PD,PD)→(PD,SA)	54	50	50	NA
(SI,SI)→(SI,SA)	38	18	8	18
(PD,PD)→(PD,SA)vs. (SI,SI)→(SI,SA)	0.034	0.003	0.002	NA
Transfer Strategies from (SA/SI) in the First 100 Rounds to PD in Round 101-150				
Treatment Name	Any Strategies	Cooperative	Selfish	Alternation
	(1)	(2)	(3)	(4)
(SA,SA)→(SA,PD)	42	36	56	13
(SI,SI)→(SI,PD)	13	7	14	0
(SA,SA)→(SA,PD) vs. (SI,SI)→(SI,PD)	0.002	0.04	0.000	NA

Notes: (1) The last row in each panel reports one-sided p-values from tests of proportions with standard error clustered at the group level.
(2)We are not able to compute p-values for Alternation strategies either because of missing values or invariance of the outcome.

of transferring repeated game strategies. We are also interested in whether the type of transferred strategy is different between our control and treatment groups.

Result 4 (Repeated game strategy transfer to the new game)**.** *Compared to when PD or SA is the historical game, subjects are less likely to transfer the repeated game strategy to a new game when SI is the historical game.*

Support. *Table 9 presents the statistics for the proportion of successful transfer of repeated game strategies from the historical game to the new game. Column 1 presents the proportion of successful transfer of any strategy while Columns 2, 3, and 4 present the proportion of successful transfer of cooperative, selfish, and alternation strategies separately, conditional on that strategy being the best fitting strategy in the first 100 rounds. In Appendix A, we report the proportion of successful transfer for all nine repeated game strategies.*

Our results show that when SI is the historical game, the proportion of successful transfer is significantly lower than it is for PD or SA. Specifically, we find that subjects are less likely to transfer the repeated game strategy when SA is the new game and SI is the historical game, compared to PD (0.38 vs. 0.54, $p = 0.034$). This result continues to hold when we examine cooperative and selfish strategies separately (Cooperative: 0.18 vs. 0.5, $p = 0.003$; Selfish: 0.08 *vs. 0.5,* $p = 0.002$*). This result also holds in general when the new game is PD (0.13 vs. 0.42,* $p = 0.002$*) as well as when we examine cooperative and selfish strategies separately (Cooperative: 0.07 vs. 0.36,* $p = 0.04$*; Selfish: 0.14 vs. 0.56,* $p = 0.000$*). P-values are computed from one-sided tests of proportions.*

Result 4 suggests that subjects may be more likely to see the strategic difference between the new game and SI and thus be less likely to transfer the repeated game strategy from SI to the new game. It also suggests that the low cognitive load required by SI may free up more cognitive resources to devote to playing the new game. This echoes Result 2, where we find that subjects are more likely to reach Pareto optimal outcomes in the new game when the historical game is SI.

Table 10: The Proportion of Transferring Strategies within Games

Transfer Strategies from the First 100 Rounds to Round 101-150 of SA				
Treatment Name	Any Strategies	Cooperative	Selfish	Alternation
	(1)	(2)	(3)	(4)
SA Control	60	46	68	56
(SA,SA)→(SA,PD)	38	29	40	43
(SA,SA)→(SA,SI)	46	83	29	43
Control vs. (SA,SA)→(SA,PD)	0.037	0.205	0.073	0.322
Control vs. (SA,SA)→(SA,SI)	0.164	0.055	0.018	0.195
(SA,SA)→(SA,PD) vs. (SA,SA)→(SA,SI)	0.275	0.019	0.317	0.500
Transfer Strategies from the First 100 Rounds to Round 101-150 of PD				
Treatment Name	Any Strategies	Cooperative	Selfish	Alternation
	(1)	(2)	(3)	(4)
PD Control	40	40	33	50
(PD,PD)→(PD,SA)	42	35	57	NA
(PD,PD)→(PD,SI)	42	25	47	0
Control vs. (PD,PD)→(PD,SA)	0.423	0.375	0.114	NA
Control vs. (PD,PD)→(PD,SI)	0.369	0.108	0.087	NA
(PD,PD)→(PD,SA)vs. (PD,PD)→(PD,SI)	0.500	0.273	0.277	NA

Notes: (1) The last three rows in each panel report one-sided p-values from tests of proportions with standard error clustered at the group level.
(2) We are not able to compute p-values for Alternation strategies in the bottom panel because of either missing values or invariance of the outcome.

In addition to examining whether subjects transfer their repeated game strategies to the new game, we examine whether they transfer strategies within the same game from the first 100 to later rounds. In particular, we explore whether subjects transfer the different types of strategies in the historical game between our control and treatment groups. Table 10 reports the summary statistics for the within-game transfer of repeated game strategies.

Result 5 (Repeated game strategy transfer within the historical game)**.** *Compared to our SA control group, subjects in (SA,SA)→(SA,SI) are less likely to use a selfish strategy in the historical game SA after round 100.*

Support. *The upper panel of Table 10 presents the proportion of successful transfer of repeated game strategies in the SA control and treatment groups. Consistent with the results from our outcome level analyses (Result 3), we find that subjects are significantly less (weakly more) likely to transfer selfish (cooperative) strategies in SA when SI is introduced after round 100, compared to our SA control group (selfish: 0.29 vs. 0.68,* $p = 0.018$*; cooperative: 0.83 vs. 0.46,* $p = 0.055$*, one-sided tests of proportions).*

Result 5 is consistent with the results from our outcome level analyses (Result 3). Furthermore, comparing the two treatment groups with the SA historical games, we find that the proportion of successful transfer of cooperative strategies in (SA,SA)→(SA,SI) is significantly higher than (SA,SA)→(SA,PD) (0.83 vs. 0.29, $p = 0.019$, one-sided tests of proportions). Comparing the two treatment groups with the PD historical games, we find that none of the results is significant, with the exception of a marginal significance between the control PD group and (PD,PD)→(PD,SI).[6]

In our final analysis, we use data from rounds 101-150 to update the best fitting strategy set identified from the first 100 rounds. Specifically, we consider a strategy i as remaining as the best fitting strategy if it belongs to the best fitting strategy set in both time blocks. If there is no intersection between the two sets, we use the best fitting strategy set in round 101-150 as the updated best fitting strategy set. We then use the updated best fitting strategy set to simulate individual choice in each game that we then use to compare with actual behaviors exhibited in rounds 151-200. The strategy with the highest fitting proportion becomes our final best fitting strategy.

In Table 11, we report the respective average fitting proportions for the finalized best fitting strategy in both our historical and new games. From Table 11, we see that the average fitting

[6]Compared to PD control, subjects are weakly more likely to keep selfish strategies in (PD,PD)→(PD,SI) (0.47 vs. 0.33, $p = 0.087$, one-sided test of proportions). However, it is worthwhile to note that the selfish strategy Grim Trigger can produce CC outcome, and this result does not contradict our outcome level analyses.

Table 11: The Fitting Proportion of the Finalized Best Fitting Strategy (Round 151-200)

	Historical Game	New Game
PD Control	94.04	
(PD,PD)→(PD,SA)	91.33	86.50
(PD,PD)→(PD,SI)	97.58	99.92
SA Control	92.87	
(SA,SA)→(SA,PD)	96.25	90.33
(SA,SA)→(SA,SI)	89.92	98.50
(SI,SI)→(SI,PD)	99.92	88.08
(SI,SI)→(SI,SA)	99.67	87.75

proportion for the PD game is usually higher than that for SA, suggesting that SA might be a more difficult game to play and to predict. Our results also show that, for both PD and SA, the average fitting proportion for the historical game is higher than that for the new game, suggesting that it is more difficult to transfer strategies between than within games.

5 Discussion

In this study, we use a novel experimental design to examine behavioral spillover and cognitive load effects across and between different types of games. In our experiment, we first have subjects play a game with two different matches for 100 rounds. We then have each subject play a new game with one of her matches while continuing to play the historical game with the other match. By manipulating which game is the historical game, we are able to identify the direction of any spillover effect.

Our study uses a sequential game design and thus complements previous research, including our own, on behavior spillover and cognitive load effects in simultaneous games. Similar to these studies, we find that cognitive load, measured as the entropy of action pairs, correlates with the presence of a spillover effect. Likewise, we find that when playing two high cognitive load games, individuals play similar strategies in each. As mentioned, our design allows us to manipulate which game will have a larger effect on the strategy chosen by making it the historical game. For example, we find that subjects who play SA as the historical game often choose an alternation strategy in PD even though alternation as a PD strategy is non-dominant, inefficient, and more complex than other strategies such as Titfor Tat or Grim Trigger. The finding that subjects adopt a more complex, less efficient behavior from a high-entropy historical game provides clear evidence for a direct spillover effect and illustrates the potential for path dependence in behaviors and outcomes.

Interestingly, our study shows no spillover effect from a low-entropy historical game (SI). When SI is the historical game, we find that subjects are more likely to choose the Pareto efficient outcome in both PD and SA. We also find that subjects more often converge to the Pareto efficient outcome in the historical game when SI is added as a new game. This finding is different from that obtained in simultaneous design studies and suggests the need for further research.

In addition to our outcome level analyses, we obtain results that show that this finding persists at the strategy level. When SI is the new game, we find that subjects are less likely to continue selfish strategies, e.g., always selfish, in the second 100 rounds of the historical game SA. When SI is the historical game, we find that subjects are less likely to transfer the selfish strategies used in SI to the new game.

While we focus on our contributions to the experimental literature, our research has real-world implications as well. For example, our research design mimics the sequential addition of institutions within economies and organizations. These additions necessitate new structures to manage the increases in scale and complexity. As these structures evolve, it is important to understand how behavioral spillovers from existing institutions impact the new entities. For example, if existing institutions exhibit a culture of cooperation or selfishness, it is of interest to consider whether the existing culture of behavior is transferred to the new institutional structure.

Our study also lends practical insight through our finding that people play multiple games differently than they do single games. This finding illustrates the need to examine behavior within a broader ecology, whether that ecology is a set of repeated games or a set of institutions. Indeed, the view that institutions function as an ensemble is supported by seminal works in economic history, political economics, and organization theory. Economic historians in particular have long emphasized the importance of seeing institutions as residing within an ecology or matrix of institutions (North 2005). In this view, one institution may produce trust, information, social structures, or behaviors that enable other institutions to perform better (Aoki 2001, 1994). Our research contributes to this perspective by providing insight into how behavior in one set of circumstances can carry over to other circumstances.

Evidence in the form of empirical analyses, cases studies, and failed development initiatives suggests that societies are not blank slates and that the weight of history has significant pull. The same institution can produce disparate outcomes across countries or even within the same country as shown in Putnam (1993)'s seminal study of Northern and Southern Italy. Similar phenomena occur within organizations through founder effects, where the behaviors of early leadership echoes across time and place (Boeker 1989).

Overall, our study uses a sequential design to demonstrate behavioral spillover and cognitive load effects. Our findings both complement and partly contradict earlier results on cognitive load involving multiple games in a simultaneous design setting. Our findings also suggest that experimental approaches to institutional design may need to consider the context within which the institution functions, i.e. the set of existing institutions. Finally, our findings show the potential for experimental methods to contribute to the literature on institutional interdependencies and cultural sway in understanding how institutions influence cultures and how cultures affect outcomes.

Appendix A. Analyses of Repeated Game Strategies

Table 12: The Description of Repeated Game Strategies

Strategy Category	Strategy	Strategy Code	Initial Action	Continued Play
Cooperative	Always Cooperate (AC)	M1	C	Always C
	Forgive Once (F1)	M2	C	Go to S if the match plays S and go to C when the previous period is S
	Suspicious Forgive Once (sF1)	M3	S	Go to S if the match plays S and go to C when the previous period is S
	Tit-for-Tat (TFT)	M4	C	Copy the match's previous action
	Suspicious Tit-for-Tat (sTFT)	M5	S	Copy the match's previous action
Selfish	Always Selfish (AS)	M6	S	Always S
	Grim Trigger (GT)	M7	C	C until the match plays S, then S forever
Alternation	Switch after C (SAC)	M8	C	After C, play S until the match plays C
	Suspicious Switch after C (sSAC)	M9	S	After C, play S until the match plays C

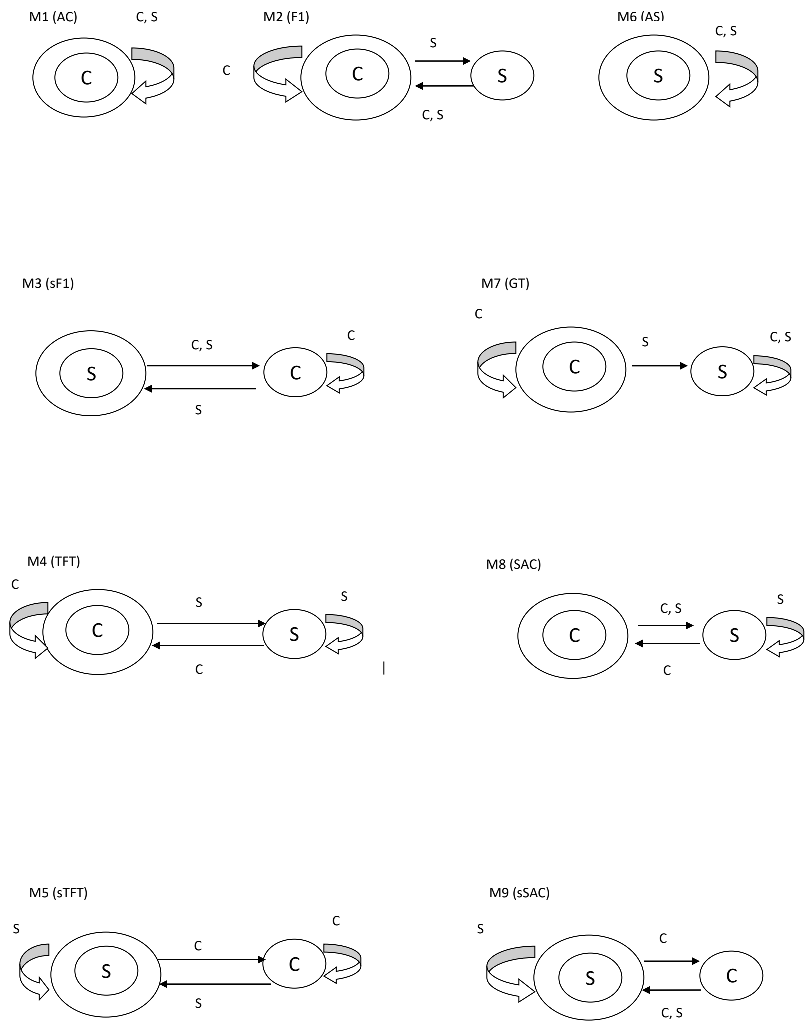

Figure 1: Automata representation of repeated game strategies

Table 13: The Proportion of Transferring Repeated Game Strategies in Control

Left Game	Any	Cooperative						Selfish			Alternation		
	Strategy	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total
Control PD	50	100	50	NA	50	25	53	100	17	38	NA	NA	NA
Control SA	63	50	100	NA	43	50	50	100	0	88	17	50	33
Right Game	Any	Cooperative						Selfish			Alternation		
	Strategies	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total
Control PD	29	0	0	NA	33	25	28	67	0	29	0	100	50
Control SA	58	100	0	0	14	75	42	75	0	55	60	100	86
Pooling	Any	Cooperative						Selfish			Alternation		
	Strategies	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total
Control PD	40	80	33	NA	41	25	40	80	10	33	0	100	50
Control SA	60	67	50	0	29	67	46	87	0	68	36	71	56

Notes: (1) Cooperative strategies include Always Cooperate (AC), Forgive Once (F1), Suspicious Forgive Once (sF1), Tit-for-Tat (TFT) and Suspicious Tit-for-Tat (sTFT).

(2) Selfish strategies include Always Selfish (AS) and Grim Trigger (GT).

(3) Alternation strategies include Switch after Cooperate (SAC) and suspicious Switch after Cooperate (sSAC).

Table 14: The Proportion of Transferring Repeated Game Strategies in Treatments

Within Game	Any	Cooperative						Selfish			Alternation		
Transfer	Strategy	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total
(PD,PD)→(PD,SA)	42	50	0	NA	63	0	35	100	0	57	NA	NA	NA
(PD,PD)→(PD,SI)	42	0	0	NA	33	25	25	75	14	47	0	0	0
(SA,SA)→(SA,PD)	38	0	100	0	20	40	29	33	50	40	50	25	43
(SA,SA)→(SA,SI)	46	NA	NA	NA	100	50	83	38	17	29	50	40	43
(SI,SI)→(SI,PD)	75	NA	NA	NA	0	90	56	94	0	71	0	90	56
(SI,SI)→(SI,SA)	92	NA	NA	NA	0	95	86	100	0	92	0	95	86
Between Game	Any	Cooperative						Selfish			Alternation		
Transfer	Strategies	AC	F1	sF1	TFT	sTFT	Total	AS	GT	Total	SAC	sSAC	Total
(PD,PD)→(PD,SA)	54	0	50	NA	56	43	50	80	20	50	NA	NA	NA
(PD,PD)→(PD,SI)	63	NA	0	NA	0	100	55	89	29	71	0	100	33
(SA,SA)→(SA,PD)	42	100	33	100	0	33	36	50	40	56	20	0	13
(SA,SA)→(SA,SI)	42	0	NA	0	20	50	25	100	20	67	0	50	17
(SI,SI)→(SI,PD)	13	NA	NA	NA	25	0	7	6	33	14	0	0	0
(SI,SI)→(SI,SA)	38	NA	NA	NA	100	14	18	9	0	8	0	19	18

Notes: (1) Cooperative strategies include Always Cooperate (AC), Forgive Once (F1), Suspicious Forgive Once (sF1), Tit-for-Tat (TFT) and Suspicious Tit-for-Tat (sTFT).

(2) Selfish strategies include Always Selfish (AS) and Grim Trigger (GT).

(3) Alternation strategies include Switch after Cooperate (SAC) and suspicious Switch after Cooperate (sSAC).

Appendix B. Experimental Instructions

We present the instructions for the (PD, PD)→(PD,SA) treatment. Instructions for other treatments are identical except for the specific game forms.

Name: PCLAB: Total Payoff:

Introduction

- You are about to participate in a decision-making process in which you will play games with two other participants. This experiment is part of a study intended to gain insight into certain features of decision processes. If you follow the instructions carefully and make good decisions, you may earn a considerable amount of money. You will be paid in cash at the end of the experiment.
- During the experiment, we ask that you please do not talk to each other. If you have a question, please raise your hand and an experimenter will assist you.

Procedure

- Matching: At the beginning of the experiment, you will be matched randomly with two other participants, both of whom will be your matches for the entire experiment. You will be matched with these same two people in all rounds. In the first 100 rounds, you will play two games with the same payoff structure with your matches. After round 100, you will play the same game as before with one of your matches, but play another new game with the other match.
- Roles: For each game, one player will be the row player, and the other will be the column player. However, since most people have an easier time reading the payoff matrices as row players, we transpose the matrix for all column players on their screen. Therefore, throughout the game, everyone can play as if they are the “row" players and their matches are the “column" players. The server will collect your decisions and match them into row’s and column’s decisions accordingly. For practical purposes, you will be a “row" player in all rounds, and your matches will be “column" players in all rounds.

- Actions: In each round, you and your two matches will simultaneously and independently make decisions in two games. One is the left game and the other is the right game. You will play the left game with one of your matches (Left Game Match) and play the right game with the other match (Right Game Match). In each game, the row player (you) will click either the Top (A) or the Bottom (B) button. The column player (your Left or Right Game Match) will choose either the Left (A) or Right (B) button. These choices determine which part of the matrix is relevant (Top Left, Top Right, Bottom Left, Bottom Right). In the first 100 rounds, your left game and right game have the same payoff structure. After round 100, your left game and right game will have different payoff structures.

- Interdependence: A player's earnings depend on the decision made by the player and on the decision made by his or her two matches as shown in the matrix below. In each cell, the row player's payoff is shown in red and the column player's payoff is shown in blue.

		Column Player			Column Player	
		Left (A)	Right (B)		Left (A)	Right (B)
Row	Top (A)	7, 7	2,10	Top (A)	7, 7	2,10
Player	Bottom (B)	10,2	4,4	Bottom (B)	10,2	4, 4

For example, in the first 100 rounds where you play the same game with two different matches, if the row player (you) choose Top (A) and the column player (your left game match) chooses Right (B) in the left game, then the row player (you) will get 2 points, while the column player (your left game match) will get 10 points in this game. Meanwhile, if the row player (you) chooses Bottom (B) and the column player (your right game match) chooses right (B) in the right game, then the row player (you) will get 4 points, and the column player (your right game match) will also get 4 points in this game. So as the row player in both games, you will get 6 points in this round in total.

		Column Player			Column Player	
		Left (A)	Right (B)		Left (A)	Right (B)
Row	Top (A)	7, 7	2,10	Top (A)	7, 7	4,14
Player	Bottom (B)	10,2	4,4	Bottom (B)	14,4	5, 5

After round 100, where you play two different games with the same two matches, if the row player (you) chooses Top (A) and the column player (your left game match) chooses Right (B) in the left game, then the row player (you) will get 2 points, while the column player (your left game match) will get 10 points in this game. Meanwhile, if the row player (you) chooses Bottom (B) and the column player (your right game match) chooses right (B) in the right game, then the row player (you) will get 5 points, and the column player (your right game match) will also get 5 points in this game. So as the row player in both games, you will get 7 points in this round totally.

- Rounds: You will first play the two games for 200 rounds. After round 200, whether the games will continue to the next round will depend on the results of the computer's random number generator. At the end of each round after round 200, with 90% chance, the games will continue to the next round. With 10% chance, the games stop.

- Earnings: Your earnings are determined by the choices that you and your two matches make in every round. Your total earning is the sum of your earnings in all rounds.

  The exchange rate is $1 for 100 points.

  You can round up your total earning to the next dollar. For example, if you earn $15.23, you can round it up to $16.

- History: In each round, your and your two matches' decisions in all previous rounds will be displayed in a history window.

We encourage you to earn as much money as you can. Do you have any questions?

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