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Equilibrium selection in similar repeated games: experimental evidence on the role of precedents

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Abstract We report on an experiment examining behavior and equilibrium selection in two similar, infinitely repeated games, Stag Hunt and Prisoner's Dilemma under anonymous random matching. We are interested in the role that historical precedents may play for equilibrium selection between these two repeated games. We find that a precedent for efficient play in the repeated Stag Hunt game does not carry over to the repeated Prisoner's Dilemma game despite the possibility that efficient play can be sustained as an equilibrium of the indefinitely repeated game. Similarly, a precedent for inefficient play in the repeated Prisoner's Dilemma game does not extend to the repeated Stag Hunt game. We conclude that equilibrium selection between similar repeated games may have less to do with historical precedents and might instead depend more on strategic considerations associated with the different payoffs of these similar repeated games.

Keywords Repeated games · Coordination problems · Equilibrium selection · Similarity · Precedent · Beliefs · Experimental economics

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1 Introduction

One of the most important uses of the experimental methodology is to address questions of equilibrium selection in environments with multiple equilibria. As Van Huyck and Battalio (2008, pp. 454–455) observed:

> When these equilibria can be Pareto ranked it is possible for historical accident and dynamic process to lead to inefficient equilibria, that is coordination failure. Consequently, understanding the origin of mutually consistent behavior is an essential complement to the theory of equilibrium points. The experimental method provides a tractable and constructive approach to the equilibrium selection problem.

Numerous experimental studies have explored questions of equilibrium selection in both static and repeated (dynamic) games (for surveys see, e.g., Ochs 1995 or Camerer 2003, Chapter 7). Typically, such questions of equilibrium selection have been studied using a *single* game played just once or repeatedly, though there are some papers exploring equilibrium selection across *different* games as discussed below in the related literature section. The novel question that we explore in this paper is whether equilibrium selection in one *indefinitely* repeated game with a multiplicity of equilibria serves as a *precedent* for equilibrium selection in a *similar*, indefinitely repeated game also having a multiplicity of equilibria.[1] That is, we are interested in whether there is a transfer of precedent to new, but similar indefinitely repeated game settings. This question is of real-world importance as it is likely that the payoffs that players face in indefinitely repeated strategic settings change over time and thus it is of interest to understand whether and how players adapt their behavior to such changes. It is also of interest to understand mechanisms that might support the play of efficient equilibria in such repeated game environments where many other equilibria are possible, and precedent (or history) is one mechanism that is relatively under-explored in the literature.[2] Our paper takes a first step toward addressing this question by combining the theory of repeated games with experimental economics methods.

In particular, we consider indefinitely repeated play of versions of the 2×2 game $\Gamma[T]$ shown in Table 1. In this game, the variable T denotes the "temptation" payoff. Our treatments consist of different values for T and the size of these

[1] We use the terminology "indefinitely repeated" rather than "infinitely repeated" to refer to the type of repeated games we can play in the laboratory. Infinitely repeated games cannot be implemented in the laboratory but indefinitely repeated games involving a constant probability that the game continues from one round to the next, *can* be implemented in the laboratory. One can interpret the continuation probability as the discount factor of the infinitely repeated game.

[2] Other well-known equilibrium selection mechanisms include risk dominance, payoff dominance, or embedding the coordination game in some kind of incomplete information setting e.g., as in the global game approach of Carlson and van Damme (1993).

Table 1 The 2×2 stage game $\Gamma[T]$ used in the experiment

	X	Y
X	20, 20	0, T
Y	T, 0	10, 10

differences provides us with a similarity measure across games. Note that if $T > 20$, $\Gamma[T]$ is a Prisoner's Dilemma game as strategy Y (equivalent to defection) is a dominant strategy for both players in the one-shot game. When $10 \le T < 20$, $\Gamma[T]$ is a Stag Hunt coordination game with two Pareto rankable pure strategy equilibria: the cooperative, efficient equilibrium where both players play X and the inefficient equilibrium where both players play Y. Under this same Stag Hunt game parameterization there also exists a mixed strategy equilibrium where X is played with probability $\frac{10}{30-T}$. Our experiment will employ these two types of stage games, Prisoner's Dilemma and Stag Hunt. All payoff values other than T in $\Gamma[T]$ will be held constant across all of our experimental treatments; that is, treatments consist only of variations in T or in the order of the two types of stage games played, Prisoner's Dilemma (PD) or Stag Hunt (SH).

We study play of $\Gamma[T]$ for various values of T under anonymous random matching and indefinite repetition. This is a stark though empirically plausible environment that has been the subject of much study with regard to mechanisms for sustaining social norms of cooperation among strangers. Here the mechanism we consider is *historical precedent*. Indefinitely repeated play of $\Gamma[T]$ means that there can be multiple equilibria in the Prisoner's Dilemma version of the stage game even under the random anonymous matching protocol for certain parameterizations of the environment. In particular, given the parameterizations of the PD versions of the game that we study and the continuation probability for indefinite play, it is possible to support play of the most efficient equilibrium where all players play the cooperative action X via the social norm, contagious strategy of Kandori (1992); the specific details are provided in Appendix A of the supplementary material. Prior research, for example, by Duffy and Ochs (2009) suggests that coordination on play of this efficient equilibrium when the stage game is a PD is not easily achieved under anonymous random matching in large populations of size 6 or 14, though it may have some success in smaller populations of size 4, as in Camera and Casari (2009). Here, we consider a population of size 10 and ask whether prior coordination on play of *a pure strategy equilibrium* in the repeated Stag Hunt game, either the inefficient equilibrium where all play Y or the cooperative, efficient equilibrium where all play X, serves as precedent for equilibrium selection in a subsequent repeated Prisoner's Dilemma game involving the *same* population of 10 players holding the anonymous random matching protocol fixed across the two repeated games. We also explore this same question in the reverse order, i.e., whether a precedent for equilibrium selection in the repeated Prisoner's Dilemma game carries over to the subsequent repeated Stag Hunt game again among the same population of anonymous, randomly matched players.

Our main finding is that the role of historical precedents for equilibrium selection in the indefinitely repeated games that we study appears to be surprisingly limited. For small changes in the value of the temptation payoff parameter T, we observe large swings in the frequency with which players play the two actions available to them. More precisely, the frequency of play of the cooperative action, X, is stochastically larger in repeated play of the stage game with a Stag Hunt parameterization than in repeated play of the game with a Prisoner's Dilemma parameterization, regardless of the order in which these two types of games are played. These swings in the frequency of cooperative play are associated with significant changes in subjects' beliefs about the cooperative play of others. These beliefs are largely influenced by players' initial propensities to cooperate as well as by the frequency of cooperative encounters in the early rounds of play.

There is, however, some variation in the frequency of play of action X and beliefs about cooperation as the temptation parameter is varied, and there are some instances where a precedent of coordination on the inefficient, all play Y equilibrium in the repeated Prisoner's Dilemma game carries over to the subsequent Stag Hunt game or the reverse (a precedent of the inefficient all Y play equilibrium in the repeated Stag Hunt game carries over to the subsequent Prisoner's Dilemma game) though such transfer of precedent is not the norm. Importantly, we never observe that a precedent of efficient play, where all play X (as is often achieved in the repeated Stag hunt game), carries over to the subsequent Prisoner's Dilemma game. We conclude that equilibrium selection results for indefinitely repeated games may be rather limited in scope to the particulars of the stage game being played and that there appears to be limited use of equilibrium selection precedents between similar indefinitely repeated games.

2 Related literature

The importance of precedent or "history" as a selection device in coordination games was first established experimentally by Van Huyck et al. (1991). They studied behavior in an average opinion game where n players were repeatedly asked to choose a number from the set of integers 1–7 inclusive. The player's common payoff function was decreasing in the distance to the median of all numbers chosen. Thus, there were 7 pure Nash equilibria, and Van Huyck et al. found that the median number choice in repeated play was equal to the median number chosen in the very first period which served as a historical precedent. Devetag (2005) documented that a precedent of efficient play in a critical-mass game transfered in most cases to the Pareto-efficient equilibrium in a seven-choice minimum-effort game. Similarly, Cason et al. (2012) provide evidence that play of the Pareto-optimal equilibrium in a median-effort game influences play in a subsequent minimum-effort game.

Another strand of the literature focuses on equilibrium selection and learning when subjects face several similar games in sequence. For example, Rankin et al. (2000) studied play of a sequence of similar Stag Hunt games that differed in terms of the payoffs and action labels presented in each repetition. They report that payoff dominance rather than risk dominance was the most frequent selection criterion adopted by subjects. Huck et al. (2010) and Grimm and Friederike (2012) considered learning spillovers in a multiple-games environment with varying complexity and feedback for subjects. They provide evidence that subjects tend to extrapolate behavior from strategically similar games.[3]

A few papers investigate how behavior spreads across strategic contexts when playing multiple games simultaneously in a finitely-repeated setting. For example, Bednar et al. (2012) demonstrated that in comparison to a single-game environment playing two games simultaneously alters behavior in predictable ways depending on the complexity of these games. However, Cason et al. (2012) found no evidence for spillovers in two simultaneously played coordination games that differ in complexity. Behavioral spillovers are also not common when subjects simultaneously play two identical games (Falk et al. 2013).

In work more related to this paper, Knez and Camerer (2000) found that a precedent of efficient play in a seven choice minimum-effort coordination game increased cooperation in a subsequent, finitely-repeated seven choice prisoners' dilemma game. Peysakhovich and Rand (2015) report on an experiment where, in a first stage, subjects are anonymously and randomly matched to play an indefinitely repeated prisoner's dilemma game that is parameterized to facilitate adoption of either a social norm of cooperation or a social norm of defection. In a subsequent second stage, subjects are anonymously matched to play a number of different *one-shot* games, e.g., a public goods game, an ultimatum game, a dictator game and a trust game. They report that subjects who experienced a social norm of cooperative play in stage 1 are more likely to play pro-socially in the subsequent stage 2 one-shot games as compared with subjects who experienced a social norm of defection in stage 1.

Theoretical work on the role of history as a selection device across similar coordination games can be found in Steiner and Stewart (2008) and Argenziano and Gilboa (2012). Steiner and Stewart propose a form of similarity-based learning by myopic agents and show how such behavior can lead to contagious play of a unique equilibrium across similar games that have different equilibria when examined in isolation. Argenziano and Gilboa (2012) propose a dynamic model in which the history of play of prior, similar coordination games affects players' beliefs about play in current period coordination games according to a similarity measure between past and present games. They show that the unique equilibrium to which players converge across similar games can be different depending on prior histories of play of those similar coordination games. We find inspiration for our approach in this work, in particular the notion that similarity measures are important for players' beliefs about how different games will be played.

[3] There is also a related literature that investigates how changes in institutional details can affect equilibrium play in the same game. For example, Brandts and Cooper (2006) explored the salience of equilibrium payoffs, Ahn et al. (2001), Bohnet and Huck (2004) or Duffy and Ochs (2009) studied reputation building using different matching protocols and Knez (1998) or Weber (2006) explored a change in group size. In contrast to this literature, we keep the institutional details fixed and consider changes in off-equilibrium payoffs so as to focus on behavior in consecutively played similar games.

Our first point of departure from the existing experimental literature is that we examine the role of history or precedents in equilibrium selection across similar *indefinitely* repeated games which serve as proxies for infinitely repeated games.[4] The previous literature has focused on the role of precedents for one-shot or finitely repeated games. However, many real-world strategic interactions are not one-shot encounters or finitely repeated games; it is more frequently the case that strategic interactions are indefinitely rather than finitely repeated. Further, indefinitely repeated games are more generally associated with equilibrium selection problems; in the finitely repeated Prisoner's Dilemma game studied for example by Knez and Camerer (2000), or in the one-shot games studied by Peysakhovich and Rand (2015) there is no theoretical equilibrium selection question as "defection" by all players is a dominant strategy in the second stage game(s) that they considered. By contrast, in the indefinitely repeated games we study in this paper, there are always multiple equilibria and hence questions of equilibrium selection and transfer are more natural.

A second point of departure of our work from other experimental studies of learning across games is that we use an explicit measure of similarity for the games we study which enables us to examine whether variations in the similarity of the stage games matters for the role of historical precedents as an equilibrium selection device. More precisely, we say that games $\Gamma[T]$ and $\Gamma[T']$, are more similar in payoff terms the smaller is $|T - T'|$, a definition that is also found, for example, in Steiner and Stewart (2008).[5] We note that unlike Knez and Camerer (2000) or Peysakhovich and Rand (2015) we are not changing the number of actions that players have available between the different games they face. Furthermore, the two pure strategy equilibria of our indefinitely repeated games—where all play X or all play Y—are the same and involve the same payoffs across our two types of games; we are only changing the off-diagonal temptation payoff, T.

[4] Indefinitely repeated games have been studied in the laboratory e.g., by Dal Bò (2005) Duffy and Ochs (2009), Camera and Casari (2009), Dal Bò and Fréchette (2011), Blonski et al. (2011) and Fudenberg et al. (2012) among others. These papers all consider equilibrium selection within the same class of indefinitely repeated Prisoner's dilemma games, and do not consider, as we do in this paper, whether the equilibrium selected in an indefinitely repeated Prisoner's dilemma game carries over to a similar but different class of indefinitely repeated game, in our case, the Stag Hunt game.

[5] An alternative notion of "structural similarity" for normal-form games is given by Germano (2006) who defines similarity between two games using the geometry of the best response correspondences. According to that criterion, the prisoner's dilemma and stag hunt stage games that we study are not structurally similar. However, as we are exploring *indefinitely repeated* versions of these normal form stage games and the parameterizations that we study for the indefinitely repeated prisoner's dilemma game allow for play of both of the pure strategy equilibria in the repeated stag hunt game, we believe that payoff similarity, rather than structural similarity, is the more relevant similarity concept for our purposes. See for example, Dufwenberg et al. (2010) or Mengel and Sciubba (2014) for experimental evidence of learning spillovers in "structurally similar" normal-form games.

3 Experiment and hypotheses

3.1 Design

Our experiment makes use of the stage game $\Gamma[T]$ shown in Table 1. Our main treatment variable involves changes in the temptation payoff, T, which creates variation in the similarity of the stage games. In each experimental session we consider just two different values for this temptation payoff, T and T' and we use the difference $|T - T'|$ as a measure of the similarity of the two stage games. More specifically, one treatment pair of parameter values for the stage games consists of $T = 10$ and $T' = 30$ and the other treatment pair consists of $T = 15$ and $T' = 25$. Thus, the stage-game pair $\Gamma[15]$ and $\Gamma[25]$ with difference $|T - T'| = 10$ is more similar than the stage-game pair $\Gamma[10]$ and $\Gamma[30]$ with difference $|T - T'| = 20$. Recall that the stage game is a Prisoner's Dilemma (PD) game for $T > 20$ and a Stag hunt (SH) game for $10 \leq T < 20$, where the choice Y is weakly (strictly) risk-dominant for $T = 10$ ($T = 15$).

In addition to differences in the temptation payoff parameter, a second treatment variable is the order in which the two indefinitely repeated games are played. We chose to vary the order of play of the stage games so as to consider whether there were any order effects. In one treatment order, the indefinitely repeated Stag Hunt game is played first followed by the indefinitely repeated Prisoner's Dilemma game. In the other treatment order, the indefinitely repeated Prisoner's Dilemma game is played first followed by the indefinitely repeated Stag Hunt game.

In practice, each session begins with several supergames of the PD version or the SH version of the game. We then switch the value of T twice (at the start of new supergames only). That is, subjects who started out playing several supergames involving the PD game next played several supergames involving the Stag Hunt game and completed the session by playing several supergames involving the original PD game. We denote treatments using this order of play as *PD[T]-SH[T]* where T is the corresponding temptation payoff. For example, in Treatment *PD30-SH10* we start with the PD, $T = 30$ game and then switch to the SH, $T = 10$ game before switching back once more to the PD, $T = 30$ game. Similarly, subjects who started out playing several supergames involving the SH game next played several supergames involving the PD game and finished the session by playing several supergames involving the original SH game. Following the same logic as before, these treatments are denoted as *SH[T]-PD[T]*.

The infinite horizon supergame was constructed as follows. Following play of the stage game, subjects took turns rolling a six-sided die.[6] If the die roll came up 1, 2, 3, 4 or 5 the stage game was repeated. If the die roll was a six, the supergame was ended. Therefore, the probability of continuation of a supergame, p, was set to 5 / 6 and the expected number of rounds in each supergame from any round reached is $1/(1 - p)$ or 6. This random termination procedure is equivalent to an infinite horizon where the discount factor attached to future payoffs is 5 / 6 per round (see,

[6] Allowing subjects to roll a die provides the most credible means of establishing the indefiniteness of the repeated game.

e.g., Roth and Murnighan (1978) who originated this methodology). Once a supergame ended (i.e., a six was rolled), another supergame would begin with the same matching protocol and the same population of 10 players used in all previous supergames of the experimental session. Subjects were also informed that at the beginning of each new supergame, the value of T could possibly change, but that the value of T would remain constant for all rounds of a supergame.

At the start of each session, subjects were randomly assigned to a fixed matching group of size 10. Subjects remained in this same matching group, with the same 9 other players in all rounds of all supergames played in the session and this fact was public knowledge. In each round of a supergame subjects were randomly and anonymously paired with a member of their matching group. Subjects were informed of the two possible payoff matrices (either $\Gamma[10]$ and $\Gamma[30]$ or $\Gamma[15]$ and $\Gamma[25]$) and were instructed that the payoff matrix would not change over the course of each indefinitely repeated sequence of rounds (supergame). However, subjects were instructed that at the start of each new sequence (supergame) the payoff matrix *could* change. Indeed, to make such a change even more apparent, the payoff matrix for the Stag Hunt game was shown in a red color and the payoff matrix for the Prisoner's Dilemma game was shown in a black color on subjects' computer screens.[7] The duration of each supergame was common across matching groups in each session but differed across sessions.

Table 2 provides details about all of our experimental sessions. In total, we recruited 140 subjects from the undergraduate population at the University of Pittsburgh for three h sessions.[8] Sessions consisted of 10 or 20 subjects, but as noted above, subjects always remained in a single matching group of size 10. Our aim in each session was to conduct approximately 90 total rounds of play and to divide these rounds up so that approximately 1/3 of the rounds involved play of the first game (Stage 1) the next 1/3 involved play of the second game (Stage (2) and the final 1/3 involved play of the first game once again (Stage 3). Thus, we aimed to get approximately 30 rounds of play with a given payoff matrix before we changed to the other payoff matrix. Given that the average length of each supergame is 6 rounds, our goal was satisfied by playing, on average, 5 sequences (supergames) with one game (first stage) before we changed the payoff matrix. We repeated the goal of roughly 30 rounds (around 5 supergames) for the second game (second stage) and also for the third and final stage (third stage) where the same game was played as in the first stage. As noted above, subjects were completely unaware of our objective of two game changes or of the duration of each of the three stages. They were only instructed that a change could occur at the start of each new supergame (after a die roll of a 6). The last three columns of Table 2 indicate some variation in the number of supergames, or "sequences" and the total number of

[7] Thus in the very first round of each new supergame it was very evident to subjects whether or not the payoff parameter, T, of the stage game had changed.

[8] The experiment was computerized using z-Tree (Fischbacher 2007). Subjects were paid their game payoffs in cents (US$) from all rounds of all supergames played. Total earnings for subjects averaged about $17 (including a $5 show-up fee), and sessions typically lasted about 90 min. For more procedural details see Appendix B of the supplementary material. Instructions are found in Appendix C of the supplementary material.

Table 2 Overview of sessions

Treatment	Session (Chronological order)	#Subjects	#Groups		#Sequences (#Rounds)		
					Stage 1	Stage 2	Stage 3
PD30-SH10	1	10	1		6 (27)	8 (31)	6 (33)
SH10-PD30	2	10	1		6 (30)	5 (32)	9 (28)
PD25-SH15	3	10	1		5 (30)	6 (28)	8 (31)
PD25-SH15	4	20	2		4 (35)	7 (42)	2 (10)
SH15-PD25	5	20	2		3 (28)	4 (29)	6 (33)
PD30-SH10	6	20	2		3 (37)	7 (30)	6 (26)
SH10-PD30	7	20	2		3 (33)	6 (46)	5 (21)
PD25-SH15	8	10	1		4 (47)	9 (30)	3 (20)
SH15-PD25	9	20	2		8 (19)	4 (24)	7 (21)
	Total	140	14	Avg.	4.7 (31.8)	6.2 (32.4)	5.8 (24.8)

rounds played in all supergames (sequences) which is, of course, due to the randomness of the length of the supergames drawn according to the die rolls.[9]

3.2 Hypotheses

Our experiment was designed to test two main hypotheses. The first hypothesis concerns the role of precedents.

Hypothesis 1 A precedent for equilibrium selection in game $\Gamma[T]$ carries over to a similar game, $\Gamma[T']$, where $T' \neq T$ and all other elements of the two games are held constant.

By equilibria, we have in mind the two, population-wide pure-strategy equilibria, where all play X or all play Y, though we recognize that there exist other equilibria in the environment that we study. If play settles on or near one of these two pure equilibria in the first of the two games played (as it often did) then we hypothesize that this precedent serves as a criterion for selecting the *same* pure equilibrium in the second, similar game despite the change in the temptation value, T. Our second hypothesis pertains to variations in the similarity between the two games as measured by the difference in the temptation payoff values.

Hypothesis 2 The more similar are the two games (i.e., the smaller is $|T - T'|$), the greater is the role played by precedents for equilibrium selection between the two games.

The more similar are the two stage games, the more likely it is that a precedent for equilibrium selection in one game carries over to the other similar game. The logic here follows from a simple continuity argument; if the difference between the two temptation payoffs were 0, the games would not differ at all and thus once an

[9] In the experimental instructions we refer to a "supergame" as an indefinite "sequence" of rounds. In the remainder of the paper we use the terms "supergame" and "sequence" interchangeably.

equilibrium was selected the population of players would likely remain at that same equilibrium forever after. However, as the difference in the temptation values grows, the strategic incentives of the two games are more different and precedents for equilibrium selection in one game may become weaker for equilibrium selection in the other, similar game. Thus we predict that precedents may play a greater role in our $T = \{15, 25\}$ treatment where the two stage games are more similar in payoff incentives as compared with our $T = \{10, 30\}$ treatment where the two stage games are more different in payoff incentives.

4 Aggregate results

4.1 First round behavior

We begin our analysis with a detailed look at first-round choices and the beliefs of subjects. Table 3 shows the average cooperation rate (average choice of *X*) and beliefs about others' play of *X* in the first round of the first stage in column 1.

Table 3 reveals two noteworthy observations. First, beginning with the first round of the first supergame in stage 1, we observe that cooperative play (i.e., the choice of action *X*) is lower in *PD30-SH10* where subjects start with the Prisoner's Dilemma game (PD) than in *SH10-PD30* where subjects start with the stag-hunt game (SH) (0.60 vs. 0.87). According to Fisher's exact test, the hypothesis that cooperative play is equally likely in both the PD and SH games can be rejected ($p = 0.039$). A similar pattern emerges for *PD25-SH15* and *SH15-PD25*. Despite the greater similarity between the two stage games, the cooperation rates are at least as high and in most cases higher in the SH than in the PD. On average, the cooperation rate in the first round of the first supergame of stage 1 is about 0.43 in *PD25-SH15* as compared with about 0.75 in *SH15-PD25*. Using Fisher's exact test, we can reject the hypothesis of an equal distribution of cooperative play ($p = 0.006$). These differences in cooperation rates are also reflected in subjects' belief statements, see Table 3, column 4. Subjects expect less cooperative choices (*X*) from others in the PD games than in the SH games.[10]

Second, while subjects seem to react to incentives (i.e., to changes in *T*), these reactions are not universal. It is the case that initial cooperation rates are lower in the PD, $T = 25$ game (0.43) than in the PD, $T = 30$ game (0.60) and also lower in the SH, $T = 15$ game (0.75) than in the SH, $T = 10$ game (0.87), but neither difference is statistically significant according to Fisher's exact tests ($p > 0.22$). Thus, small differences in *T* that do not change the strategic incentives, that is changes representing either a lower incentive to deviate from cooperation in the PD game or a higher risk from choosing *X* in the SH game, are not initially taken into account by subjects.[11]

[10] Subjects expect less cooperation in *PD30-SH10* than in *SH10-PD30* ($p < 0.01$, two-sided t-test) as well as less cooperation in *PD25-SH15* than in *SH15-PD25* ($p < 0.01$, two-sided t-test).

[11] These findings are corroborated by subjects' beliefs. On average, subjects expect cooperative play to be approximately the same in the PD, $T = 30$ game (0.56) as in the PD, $T = 25$ game (0.63) ($p > 0.31$, two-sided t-test). In the SH games, subjects' expectations are on average closer to actual observed behavior. Indeed, they expect a bit more cooperation in the SH, $T = 10$ game (0.88) than in SH, $T = 15$ game (0.76). This difference in beliefs is marginally statistically significant ($p = 0.094$, two-sided t-test).

Table 3 Frequency of cooperation, coordination and expected cooperation in the first round and all rounds

Treatment	Group	Choice *X*, round 1			Belief *X*, round 1			Choice *X*, all rounds			Coordination *XX* or *YY*, all rounds		
		Stage 1 (1)	Stage 2 (2)	Stage 3 (3)	Stage 1 (4)	Stage 2 (5)	Stage 3 (6)	Stage 1 (7)	Stage 2 (8)	Stage 3 (9)	Stage 1 (10)	Stage 2 (11)	Stage 3 (12)
PD30-SH10	1	0.70	0.80	0.40				0.14	0.96	0.17	0.83	0.94	0.74
	2	0.60	0.90	0.10	0.57	0.94	0.22	0.04	0.99	0.02	0.92	0.99	0.97
	3	0.50	1.00	0.10	0.56	0.83	0.26	0.15	1.00	0.06	0.79	1.00	0.88
	Avg.	0.60	0.90	0.20	0.56	0.89	0.24	0.11	0.99	0.09	0.85	0.98	0.85
SH10-PD30	4	0.80	0.40	1.00				0.98	0.10	1.00	0.97	0.81	1.00
	5	0.90	0.30	1.00	0.90	0.48	0.99	0.98	0.02	0.99	0.96	0.96	0.98
	6	0.90	0.50	1.00	0.86	0.50	1.00	1.00	0.09	1.00	0.99	0.83	1.00
	Avg.	0.87	0.40	1.00	0.88	0.49	0.99	0.99	0.07	1.00	0.97	0.87	0.99
PD25-SH15	7	0.70	0.80	0.30	0.69	0.71	0.37	0.30	0.89	0.13	0.59	0.79	0.77
	8	0.40	0.70	0.50	0.59	0.52	0.37	0.05	0.78	0.18	0.90	0.62	0.72
	9	0.10	0.40	0.20	0.56	0.37	0.18	0.04	0.09	0.03	0.93	0.84	0.94
	10	0.50	0.80	0.40	0.68	0.71	0.44	0.06	0.91	0.10	0.89	0.84	0.87
	Avg.	0.43	0.68	0.35	0.63	0.58	0.34	0.10	0.63	0.12	0.84	0.77	0.81
SH15-PD25	11	0.70	0.10	0.40	0.74	0.18	0.12	0.24	0.02	0.11	0.64	0.97	0.85
	12	0.80	0.40	0.80	0.79	0.57	0.88	0.97	0.08	0.99	0.94	0.86	0.98
	13	0.70	0.30	0.80	0.73	0.38	0.62	0.71	0.08	0.80	0.48	0.88	0.71
	14	0.80	0.30	0.80	0.77	0.58	0.83	0.85	0.03	0.95	0.72	0.94	0.90
	Avg.	0.75	0.28	0.70	0.75	0.10	0.75	0.68	0.05	0.67	0.71	0.91	0.87

4.2 Is there a precedent transfer between similar games?

In this section, we investigate whether a precedent for equilibrium selection in game $\Gamma[T]$ carries over to a similar game, $\Gamma[T']$. Table 3 shows the frequency of cooperative play (i.e., the choice of *X*) in each group across all rounds in a stage along with the averages in the respective treatment in columns 7–9. In the last three columns (10–12) we report the aggregate frequency of *coordination* in each stage, i.e., the play of either *XX* or *YY* by randomly matched pairs of subjects.

Figures 1 and 2 display the frequency of cooperation in the four treatments. The top panels of Figs. 1a and 2a present data on the aggregate frequency of cooperative play in each round of a stage of the groups that started with the PD in stage 1 (treatment *PD30-SH10* and *PD25-SH15*). The bottom panels of Figs. 1a and 2a present the aggregate frequency of cooperation in each round of a stage of the groups that started with the SH in stage 1 (treatment *SH10-PD30* and *SH15-PD25*). In Figs. 1a and 2a, the horizontal axis reports the consecutively numbered rounds within a *stage* so that a round number of 1 indicates the start of a new *stage*, i.e., a change in the temptation payoff, *T*. Note that the average duration of stages varied across groups (for details see Table 2). Thus, the aggregate frequencies in the later rounds of a stage in Figs. 1a and 2a do not necessarily include choices from all groups in a treatment, due to different supergame lengths in each stage.

The left (right) panels of Figs. 1b and 2b show the frequency of cooperative play for each group separately in *PD30-SH10* and *PD25-SH15* (*SH10-PD30* and *SH15-PD25*). Each stage consists of several sequences of rounds (or supergames) and within a stage the game matrix was always the same. That is, in *PD30-SH10* and *PD25-SH15* groups played several sequences of the PD in stage 1 followed by several sequences of the SH in stage 2 and finally several sequences of the PD in stage 3, whereas in *SH10-PD30* and *SH15-PD25* groups played several sequences of the SH in stage 1, then several sequences of the PD in stage 2 followed again by several sequences of the SH in stage 3. In Figs. 1b and 2b the horizontal axis reports consecutive round numbers across all sequences and phases of the session. The start of a new *sequence* (supergame) is indicated by gray dashed vertical lines while the start of a new stage is indicated by red dashed vertical lines.

Figures 1a and 2a indicate that the differences in first-round cooperation rates of the first stage between PD and SH extend to future rounds and become even more pronounced. Indeed, while the frequency of cooperation plummets toward zero within a few rounds in the stage 1 PD games (both in *PD30-SH10* and *PD25-SH15*), cooperative play either increases further in *SH10-PD30* reaching full cooperation in round 7 or is stable with a moderate downward trend toward a cooperation rate of 0.5 in later rounds of stage 1 in *SH15-PD25*. As evidenced in Table 3 and Figs. 1b and 2b, all but one group playing the SH first show substantially higher cooperation rates than groups starting out playing the PD game and thus, subjects have markedly different experiences in the beginning. In particular, they experience high rates of defection (i.e., choice of *Y*) in the stage 1, PD games and high rates of cooperation in the stage 1, SH games.

There is a dramatic change in behavior once the temptation payoff *T* is changed as shown in Figs. 1a and 2a. This change in *T* mostly occurs after about 30 rounds of

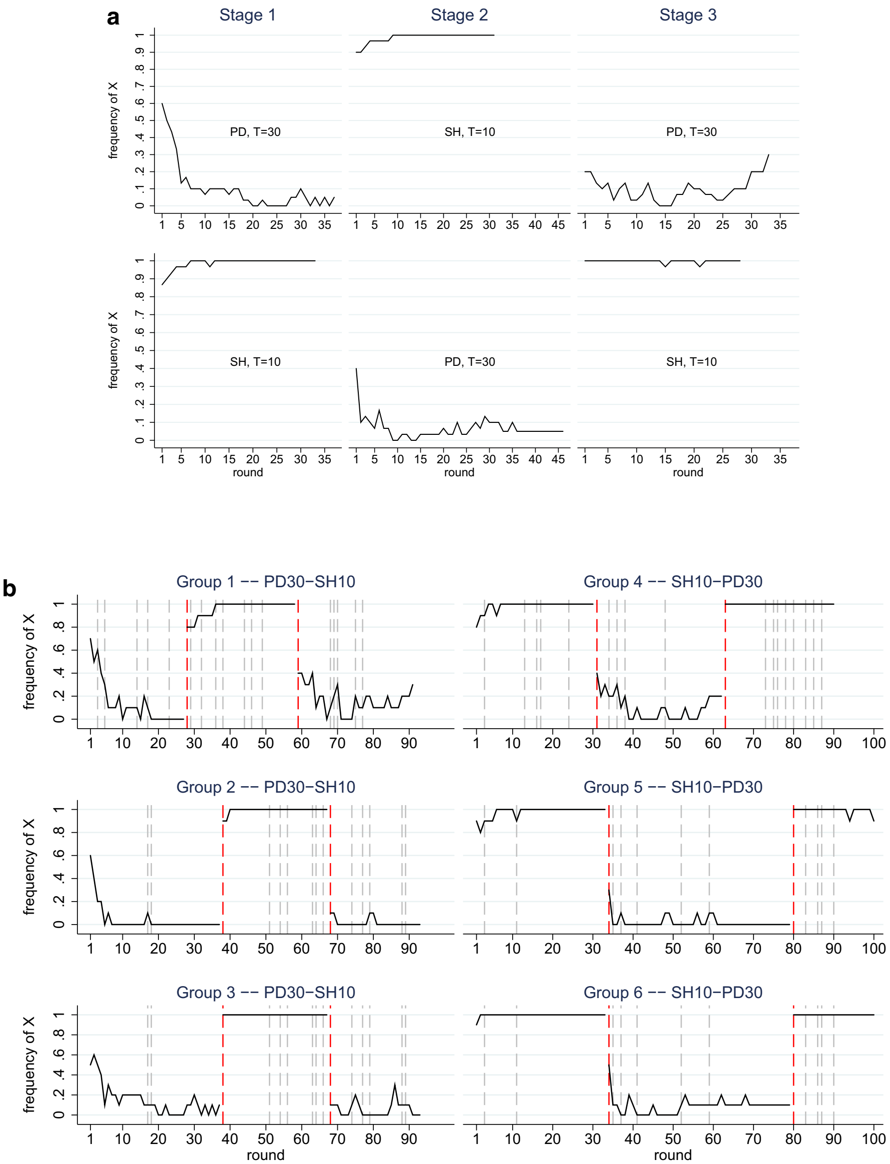

Fig. 1 Frequency of cooperation in PD30-SH10 and SH10-PD30, **a** aggregate frequency of cooperation in *PD30-SH10* (*top panel*) and *SH10-PD30* (*bottom panel*). **b** Aggregate frequency of cooperation at the group level—*PD30-SH10* (*left panel*) and *SH10-PD30* (*right panel*)

play. When T is lowered, either from 30 to 10 or 25 to 15, we observe a sudden increase of the frequency of cooperative play from below 0.1 in the last round of the last sequence of stage 1 to 0.9 (in *PD30-SH10*) and to about 0.7 (in *PD25-SH15*) in the first round of stage 2. Surprisingly, the same subjects who were not able to sustain cooperative play in the PD are rather confident, as indicated by their beliefs,

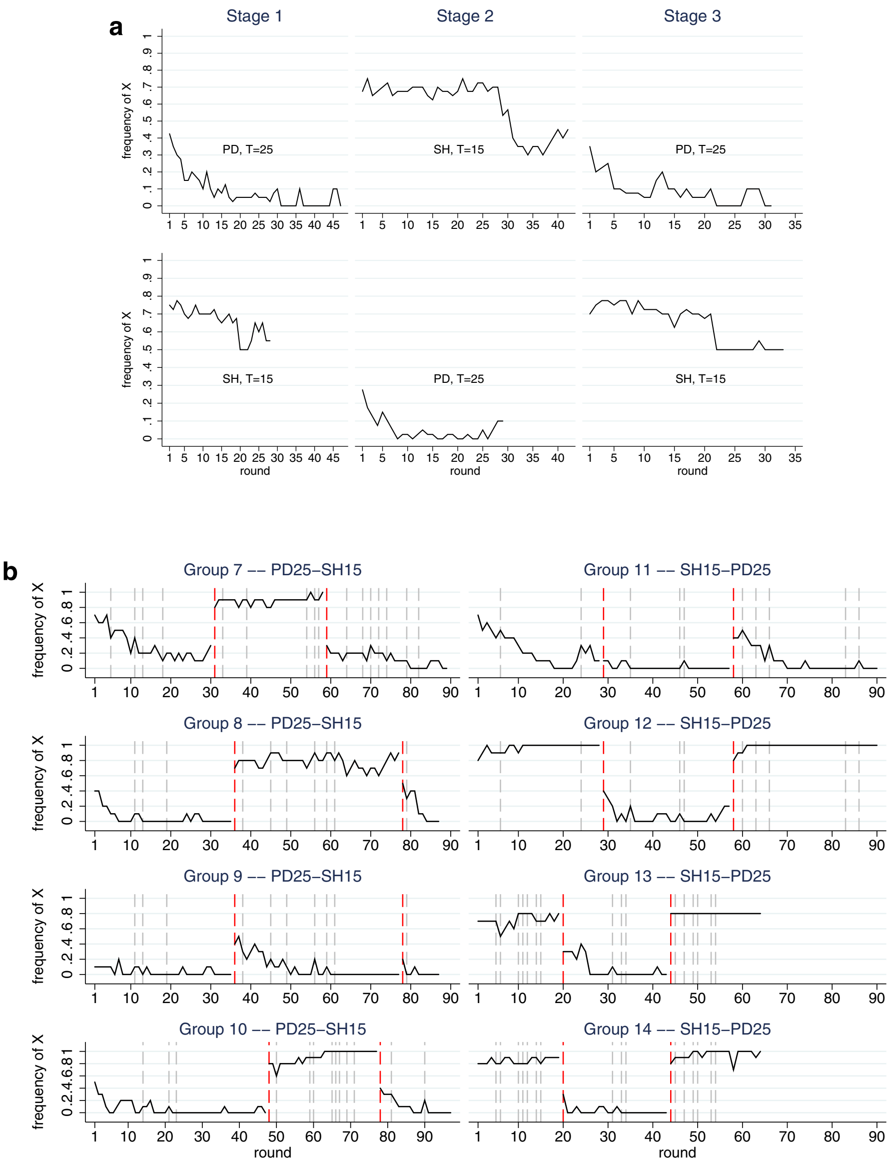

Fig. 2 Frequency of cooperation in PD25-SH15 and SH15-PD25, **a** aggregate frequency of cooperation in *PD25-SH15* (*top panel*) and *SH15-PD25* (*bottom panel*). **b** Aggregate frequency of cooperation at the group level—*PD25-SH15* (*left panel*) and *SH15-PD25* (*right panel*)

that their opponents will choose the Pareto-efficient action once the temptation payoff *T* is lowered. Taking all rounds in a stage into account, we observe the same frequency of cooperative play in stage 1, SH games and stage 2 SH games (see Table 3, column 7 and 8). Thus, it seems that it does not matter whether subjects are inexperienced as in *SH10-PD30* or *SH15-PD25* or whether subjects experienced high rates of defection, and thus a high level of mistrust, before they play the SH as in *PD30-SH10* or *PD25-SH15*.

By contrast, when T is increased from 10 to 30 or from 15 to 25 at the beginning of stage 2, the frequency of cooperative play immediately drops from high to low rates of cooperation and never rebounds in subsequent rounds. Again, previous experience seems to have little effect on play in the stage 2 game. That is, despite the fact that subjects typically experienced very high cooperation rates in the SH games played throughout stage 1, the average cooperation rate in the stage 2, PD games is about as low as in the stage 1, PD games.

In stage 3 the frequency of cooperative play reverts to observed stage 1 levels in all treatments. That is, confidence in cooperative play is immediately lost when the temptation payoff T is increased and immediately restored when T is lowered. Clearly, the previous analysis suggests that there are no order effects, i.e., play is unaffected by previous experience in a similar game.

To what extent does the observed behavior reflect pure-strategy equilibrium play? In the last three columns of Table 3 we report the coordination rates for each stage, which measures the frequency of play of either *XX* or *YY* play within a matching group. While in most, though not all cases the coordination rates are above 0.7 in all stages, the underlying behavior in the PD and SH games is quite different. In the PD games, subjects coordinate in most cases quickly on mutual defection, *YY*, and mis-coordination arises through individual attempts to induce cooperative play in the group. However, these attempts remain largely unsuccessful since the frequency of *X* choices is too low to turn play around. By contrast, in the SH game, coordination primarily means play of the cooperative choice *X*. While in the less similar games in *PD30-SH10* and *SH10-PD30* coordination rates are higher in the SH than in the PD, they are lower when the two games are more similar. It seems that the risk dominant choice, *Y*, creates some tension between choosing *X* and *Y* in the SH, $T = 15$ game resulting in more frequent mis-coordination.

We now turn to a more detailed look at behavior at the group level (see Figs. 1b and 2b). While the aggregate analysis thus far suggests that there is no precedent for equilibrium selection transfer between the two games, this is not necessarily true for all groups. Indeed, we observe two instances where a precedent of inefficient equilibrium play does transfer between the different stage games. In both instances the cooperation rates in the SH game are substantially lower than what is found when other groups play the same SH game. In group 9 involving treatment *PD25-SH15* it appears that a precedent of mutual defection in the first PD supergames transfers to the SH supergames in stage 2. In the other group—group 11 involving treatment *SH15-PD25*—we observe that play converged toward the inefficient, all *Y* equilibrium in the stage 1, SH game. When the game was changed to a PD in stage 2, subjects continued to play *Y* and the frequency of cooperative play is virtually zero throughout this second stage. Notice, that in contrast to all other repeated PD games in a similar situation, the cooperation rate in the first round of stage 2 is very low, indicating almost no attempt for initial cooperation. Notice further for this same group 11 that the precedent for inefficient play continues to spill over into the third SH stage as well. Nevertheless, these incidences of precedent transfer are few, and we note that there is no evidence for a precedent of efficient play (of the all-X equilibrium) to spill over into a similar game.

Based on these observations, we reject *Hypothesis 1*. That is, we do not find strong evidence that a precedent for equilibrium selection in game $\Gamma[30]$ or $\Gamma[25]$ carries over to a similar game, $\Gamma[10]$ or $\Gamma[15]$, respectively. Similarly, there is not much in the way of support for a precedent transfer from game $\Gamma[10]$ or $\Gamma[15]$ to game $\Gamma[30]$ or $\Gamma[25]$. Apparently, once the incentives to deviate from cooperation are sufficiently low as in stage 2 of *PD30-SH10* and *PD25-SH15*, there is no scope for a transfer of a precedent of defection. In a similar vein, a precedent of cooperation does not transfer to a similar game in which the incentives to deviate from cooperation are high as in stage 2 of *SH10-PD30* and *SH15-PD25*.

4.3 Does higher similarity facilitate the transfer of precedents?

We have provided evidence that the value of T plays a crucial role for equilibrium selection and effectively leaves little room for a transfer of precedents. Our second hypothesis was that more similar games, i.e., games with a smaller difference $|T - T'|$, are more likely to facilitate a precedent transfer. To test this hypothesis we compare behavior across treatments, i.e., *PD30-SH10* and *SH10-PD30* versus *PD25-SH15* and *SH15-PD25*. Let us first concentrate on how different values of T affect behavior in either the PD or the SH. The previous analysis has shown that there is some initial cooperation in the PD, $T = 30$ game followed by a strong increase in defection. Surprisingly, a lower temptation payoff ($T = 25$) does not lead to less defection. Even though the gain to deviating from the cooperative choice when $T = 25$ is smaller than when $T = 30$, cooperation is not more easily sustained in the PD game with the lower value for T. Using a robust rank-order test we cannot reject the hypothesis of equal cooperation rates in stages 1, 2 and 3 of the PD $T = 30$ and PD $T = 25$ games (all p values $p = 0.114$).[12]

By contrast, a higher temptation payoff ($T = 15$) in the SH game does have an effect on the frequency of cooperative behavior. A higher T makes play of the cooperative action X less attractive (risk-dominated) and subjects appear to react to this circumstance with a lower cooperation rate when $T = 15$ as compared with when $T = 10$. The hypothesis of equal cooperation rates in stages 1, 2 and 3 of the SH $T = 10$ and SH $T = 15$ games can be clearly rejected according to a robust rank-order test (for all three stages $p \leq 0.029$).[13]

However, in contrast to *Hypothesis 2*, it does not appear that precedent plays a stronger role when the difference in the temptation payoffs of the two games is smaller, so that the two games are more similar. While cooperation in the SH, $T = 15$ game is indeed lower following a precedent of inefficient play in the PD, $T = 25$ game as compared with cooperation in the SH, $T = 10$ game following a precedent of inefficient play in the PD, $T = 30$ game, it is also the case that

[12] See Feltovich (2003) for a discussion of the robust rank-order test.

[13] It is important to note that this result does not depend on the inclusion of the two groups with a low cooperation rates (group 9 and 11). As Table 3 shows, cooperation rates in the SH, $T = 15$ game are in all stages lower than for the SH, $T = 10$ game for each group.

cooperation is lower when the SH, $T = 15$ game is the first game played as compared with the case where SH, $T = 10$ game is the first game played—see, e.g., Figs. 1 and 2. Thus it appears that risk dominance considerations rather than equilibrium selection precedents or similarity measures plays the more important role in understanding the choices made by subjects in our experiment.

On the other hand, in support of *Hypothesis 2*, we note that the only two instances in which a transfer of precedent for equilibrium selection took place were in treatments where the two games were more similar to one another. In particular, as noted earlier, we observed one instance (group 11) in *SH15-PD25* where a precedent of inefficient play in the SH transfered to the PD, and one instance (group 9) in *PD25-SH15*, where a precedent for inefficient play in the PD transfered to the SH. There are no instances where a precedent for equilibrium selection transfers between different games played in the *SH10-PD30* or *PD30-SH10* treatments. Finally, we note that we do not observe any cases where a precedent for efficient play (all *X*) in the SH transfers over to a subsequent PD; it seems that only precedents for inefficient equilibrium play (all *Y*) have some spillover effects in more similar repeated games.

5 Individual behavior across treatments

This section takes a closer look at individual behavior in order to shed some light on why precedents are often ineffective and do not generally transfer from game to game, even when $|T - T'|$ is small so that the games are more similar. We first consider cooperation frequencies at the individual level to explore any regularities in individual choice behavior. We then provide a more thorough analysis of elicited beliefs. That is, we look at best-response behavior and how well beliefs are calibrated to actual behavior in the first round of a particular sequence. Finally, we study choices in the first round of a new stage, i.e., stage 2 or 3. Utilizing the belief data we provide insights that help explain the sudden swings in behavior that accompany changes in *T*.

5.1 Cooperation at the individual level

Table 4 shows the frequency of cooperation at the individual level. More precisely, Table 4 reports the number of subjects (cumulative frequencies) whose cooperation rates fall into one of eight ranges, using data from each stage separately. From the previously shown evidence, we know that cooperation is, of course, more prevalent in the SH games than in the PD games. Nevertheless, Table 4 reveals some heterogeneity in individual behavior within the two games. For example, there are a few subjects cooperating in between 10 and 25% of rounds in the PD games. In the SH games individual cooperation rates are often above 75%, though when risk dominance considerations come into play cooperation rates below 50% are more common.

Although a sizable share of subjects change their action at least once, there are some subjects who never change their action throughout a stage as evidenced by the number of subjects who fall into category $X = 0$ or $X = 1$ in Table 4. That is, they never play X in PD games or they always play X in SH games. About 39% (45%) of subjects never play X in the PD, $T = 30$ (PD, $T = 25$). Thus, subjects who start out playing cooperatively in the PD games eventually experience uncooperative behavior at some point, which induces them to change to defection as well as leading to the observed downward spiral of cooperation in both parameterizations of the PD game. Indeed, there is a strong negative correlation between the number of subjects who always defect and the cooperation rate within a group (Spearman's $\rho = -0.66$, $p < 0.01$). In the SH game, individual behavior is more differentiated between the two parameterizations. While about 89% of subjects stick to their action of X throughout a stage of the SH, $T = 10$ game, only 57% (45% always play X and 12% always play Y) do so in the SH, $T = 15$ game.

This evidence suggests that a sizeable share of subjects cannot be dissuaded from playing a particular action and they stick with their choice throughout a stage. While such stickiness in behavior would seem to be an excellent prerequisite for the transfer of a precedent for equilibrium selection, we do not observe such transfers in less similar games ($|T - T'| = 20$), where such inertia is more common than in more similar games ($|T - T'| = 10$). In fact, if anything, less inertia appears more fruitful for a precedent transfer. In more similar games, we observe more switching between choices in the SH due to risk dominance considerations and the only instances of precedent transfers of equilibrium selection.

5.2 Best response behavior and accuracy of beliefs

Given that there is little evidence that a precedent of equilibrium selection transfers between two similar games, we should also observe that subjects' beliefs about cooperative play and possibly their best-response behavior and the accuracy of their beliefs depend on game similarity. Table 5 gives a detailed overview of best-response rates and the accuracy of beliefs for each stage and for both treatments and both orders.[14]

Notice first that following the logic of Kandori (1992), cooperation in the infinitely repeated game can be a best response if a player believes that all the other players in his group will cooperate (see Appendix A of the supplementary material for a more formal treatment of this logic).[15] It follows that a subject is off-the-equilibrium path, i.e., plays a non–best response in the PD if he plays X but does *not* expect cooperation from all other players. By contrast, in a SH, a non-best response can involve play of both X and Y, depending on beliefs.

[14] Recall that we elicited subjects' beliefs about cooperative play in a matching group in the first round of a sequence except in the first session of *PD30-SH10* and *SH10-PD30*.

[15] The idea behind Kandori's *contagious equilibrium* is that cooperation can be sustained only if all players cooperate because defection by a single player would initiate a contagious spread of defection within the entire community (group) and this process cannot be stopped by re-igniting cooperation.

Table 4 Individual frequencies of cooperation

Frequency of	Cumulative number of the 10 subjects whose frequency of cooperation falls below various thresholds for each group and stage.								
cooperation is:	Stage 1			Stage 2			Stage 3		
PD30-SH10	PD, $T = 30$			SH, $T = 10$			PD, $T = 30$		
	Group 1	Group 2	Group 3	Group 1	Group 2	Group 3	Group 1	Group 2	Group 3
$X = 0$	1 (0.1)	3 (0.3)	3 (0.3)	0 (0.0)	0 (0.0)	0 (0.0)	3 (0.3)	8 (0.8)	7 (0.7)
$0 < X \leq 0.05$	1 (0.2)	2 (0.5)	1 (0.4)	0 (0.0)	0 (0.0)	0 (0.0)	1 (0.4)	0 (0.8)	1 (0.8)
$0.05 < X \leq 0.10$	1 (0.3)	3 (0.8)	0 (0.4)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.4)	2 (1.0)	1 (0.9)
$0.10 < X \leq 0.25$	6 (0.9)	2 (1.0)	4 (0.8)	0 (0.0)	0 (0.0)	0 (0.0)	3 (0.7)	0 (1.0)	0 (0.9)
$0.25 < X \leq 0.50$	1 (1.0)	0 (1.0)	1 (0.9)	0 (0.0)	0 (0.0)	0 (0.0)	3 (1.0)	0 (1.0)	1 (1.0)
$0.50 < X \leq 0.75$	0 (1.0)	0 (1.0)	1 (1.0)	1 (0.1)	0 (0.0)	0 (0.0)	0 (1.0)	0 (1.0)	0 (1.0)
$0.75 < X \leq 1$	0 (1.0)	0 (1.0)	0 (1.0)	1 (0.2)	1 (0.1)	0 (0.0)	0 (1.0)	0 (1.0)	0 (1.0)
$X = 1$	0 (1.0)	0 (1.0)	0 (1.0)	8 (1.0)	9 (1.0)	10 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)
SH10-PD30	SH, $T = 10$			PD, $T = 30$			SH, $T = 10$		
	Group 4	Group 5	Group 6	Group 4	Group 5	Group 6	Group 4	Group 5	Group 6
$X = 0$	0 (0.0)	0 (0.0)	0 (0.0)	3 (0.3)	6 (0.6)	1 (0.1)	0 (0.0)	0 (0.0)	0 (0.0)
$0 < X \leq 0.05$	0 (0.0)	0 (0.0)	0 (0.0)	2 (0.5)	3 (0.9)	6 (0.7)	0 (0.0)	0 (0.0)	0 (0.0)
$0.05 < X \leq 0.10$	0 (0.0)	0 (0.0)	0 (0.0)	1 (0.6)	0 (0.9)	2 (0.9)	0 (0.0)	0 (0.0)	0 (0.0)
$0.10 < X \leq 0.25$	0 (0.0)	0 (0.0)	0 (0.0)	3 (0.9)	1 (1.0)	0 (0.9)	0 (0.0)	0 (0.0)	0 (0.0)
$0.25 < X \leq 0.50$	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.0)	0 (1.0)	0 (0.9)	0 (0.0)	0 (0.0)	0 (0.0)
$0.50 < X \leq 0.75$	0 (0.0)	0 (0.0)	0 (0.0)	0 (1.0)	0 (1.0)	1 (1.0)	0 (0.0)	0 (0.0)	0 (0.0)
$0.75 < X \leq 1$	2 (0.2)	3 (0.3)	1 (0.1)	0 (1.0)	0 (1.0)	0 (1.0)	0 (0.0)	1 (0.1)	0 (0.0)
$X = 1$	8 (1.0)	7 (1.0)	9 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	10(1.0)	9 (1.0)	10 (1.0)

Table 4 continued

PD25-SH15	PD, $T = 25$				SH, $T = 15$				PD, $T = 25$			
	Group 7	Group 8	Group 9	Group 10	Group 7	Group 8	Group 9	Group 10	Group 7	Group 8	Group 9	Group 10
$X = 0$	2 (0.2)	5 (0.5)	5 (0.5)	3 (0.3)	0 (0.0)	1 (0.1)	3 (0.3)	0 (0.0)	3 (0.3)	5 (0.5)	7 (0.7)	6 (0.6)
$0 < X \leq 0.05$	0 (0.2)	0 (0.5)	2 (0.7)	2 (0.5)	0 (0.0)	0 (0.1)	1 (0.4)	0 (0.0)	1 (0.4)	0 (0.5)	0 (0.7)	0 (0.6)
$0.05 < X \leq 0.10$	0 (0.2)	3 (0.8)	2 (0.9)	3 (0.8)	1 (0.1)	0 (0.1)	3 (0.7)	0 (0.0)	1 (0.5)	0 (0.5)	0 (0.7)	1 (0.7)
$0.10 < X \leq 0.25$	2 (0.4)	2 (1.0)	1 (1.0)	2 (1.0)	0 (0.1)	0 (0.1)	2 (0.9)	0 (0.0)	4 (0.9)	2 (0.7)	3 (1.0)	1 (0.8)
$0.25 < X \leq 0.50$	5 (0.9)	0 (1.0)	0 (1.0)	0 (1.0)	0 (0.1)	1 (0.2)	1 (1.0)	0 (0.0)	1 (1.0)	2 (0.9)	0 (1.0)	2 (1.0)
$0.50 < X \leq 0.75$	0 (0.9)	0 (1.0)	0 (1.0)	0 (1.0)	0 (0.1)	1 (0.3)	0 (1.0)	2 (0.2)	0 (1.0)	1 (1.0)	0 (1.0)	0 (1.0)
$0.75 < X \leq 1$	0 (0.9)	0 (1.0)	0 (1.0)	0 (1.0)	2 (0.3)	3 (0.6)	0 (1.0)	3 (0.5)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)
$X = 1$	1 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	7 (1.0)	4 (1.0)	0 (1.0)	5 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)

SH15-PD25	SH, $T = 15$				PD, $T = 25$				SH, $T = 15$			
	Group 11	Group 12	Group 13	Group 14	Group 11	Group 12	Group 13	Group 14	Group 11	Group 12	Group 13	Group 14
$X = 0$	1 (0.1)	0 (0.0)	1 (0.1)	1 (0.1)	7 (0.7)	3 (0.3)	3 (0.3)	5 (0.5)	5 (0.5)	0 (0.0)	2 (0.2)	0 (0.0)
$0 < X \leq 0.05$	1 (0.2)	0 (0.0)	0 (0.1)	0 (0.1)	2 (0.9)	3 (0.6)	3 (0.6)	4 (0.9)	0 (0.5)	0 (0.0)	0 (0.2)	0 (0.0)
$0.05 < X \leq 0.10$	0 (0.2)	0 (0.0)	0 (0.1)	0 (0.1)	0 (0.9)	0 (0.6)	1 (0.7)	0 (0.9)	0 (0.5)	0 (0.0)	0 (0.2)	0 (0.0)
$0.10 < X \leq 0.25$	2 (0.4)	0 (0.0)	1 (0.2)	0 (0.1)	1 (1.0)	4 (1.0)	3 (1.0)	1 (1.0)	3 (0.8)	0 (0.0)	0 (0.2)	0 (0.0)
$0.25 < X \leq 0.50$	6 (1.0)	0 (0.0)	0 (0.2)	0 (0.1)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	2 (1.0)	0 (0.0)	0 (0.2)	0 (0.0)
$0.50 < X \leq 0.75$	0 (1.0)	1 (0.1)	2 (0.4)	1 (0.2)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (0.0)	0 (0.2)	1 (0.1)
$0.75 < X \leq 1$	0 (1.0)	1 (0.2)	3 (0.7)	3 (0.5)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	2 (0.2)	0 (0.2)	3 (0.4)
$X = 1$	0 (1.0)	8 (1.0)	3 (1.0)	5 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	0 (1.0)	8 (1.0)	8 (1.0)	6 (1.0)

Table 5 Aggregate best response behavior and accuracy of beliefs

Treatment	Group	Best response			Accuracy		
		Stage 1	Stage 2	Stage 3	Stage 1	Stage 2	Stage 3
PD30-SH10	1						
	2	0.77	1.00	0.98	0.22	0.02	0.08
	3	0.77	0.99	0.93	0.16	0.01	0.13
	Avg.	0.77	0.99	0.96	0.19	0.02	0.10
SH10-PD30	4						
	5	0.97	0.92	1.00	0.14	0.17	0.00
	6	0.97	0.88	1.00	0.02	0.18	0.00
	Avg.	0.97	0.90	1.00	0.08	0.17	0.00
PD25-SH15	7	0.68	0.85	0.82	0.22	0.18	0.18
	8	0.90	0.66	0.60	0.18	0.22	0.30
	9	0.95	0.83	0.90	0.14	0.25	0.15
	10	0.82	0.81	0.70	0.21	0.16	0.23
	Avg.	0.83	0.79	0.78	0.19	0.20	0.20
SH15-PD25	11	0.70	0.95	0.73	0.30	0.07	0.29
	12	0.87	0.88	0.90	0.18	0.23	0.10
	13	0.76	0.90	0.80	0.33	0.09	0.27
	14	0.96	0.93	0.89	0.24	0.22	0.11
	Avg.	0.84	0.91	0.83	0.27	0.15	0.19

Given the previous evidence we have seen for *PD30-SH10* and *SH10-PD30*, it is therefore not surprising that best-response rates are lower in PD games than in SH games. In the SH we observe close to perfect best-response behavior in all stages, reflecting the fact that groups either start out in the cooperative equilibrium (stage 1) or quickly transition to play of the all *X* equilibrium when the stage 2 or 3 game is SH. In the PD games, however, best response rates are considerably lower. In particular, in the stage 1, PD game best response rates are below 80% reflecting a sizable amount of cooperation attempts in the first rounds of a PD sequence. In more similar games such as in *PD25-SH15* and *SH15-PD25* best response rates are quite similar across the PD and SH games. More precisely, best response rates in the PD games range from 0.6 to 0.95 and in the SH games they range from 0.66 to 0.96 and are more dispersed than in *PD30-SH10* and *SH10-PD30* in both cases.[16]

At first glance, it is striking how close beliefs are, on average, to actual behavior in each stage of *PD30-SH10* and *SH10-PD30*, while this is not so much the case when the two games are more similar as in *PD25-SH15* and *SH15-PD25* (see Table 3). The last three columns in Table 5 provide a more detailed look at how well individual beliefs are calibrated to opponents' actual behavior at the individual

[16] Note that low best response rates in the SH, $T = 15$ are not due to the two groups who coordinate on the inefficient all-*Y* equilibrium. Rather, it is the case that best response rates are in general lower in the SH, $T = 15$ than in the SH, $T = 10$.

level. The table displays the mean squared deviation of stated beliefs, which is calculated as $1/N\sum(b_i - a_j)^2$ where b_i is individual i's stated belief and a_j is the opponents' action (either 1 (X) or 0 (Y)). Notice that this measure is equivalent to a quadratic scoring rule. It is apparent that the accuracy of subjects' beliefs reflects the observed variation of choices in the two treatments. For example, it confirms that there is little scope for miscalibration in the SH, $T = 10$ game where play immediately reaches the cooperative outcome and is stable throughout the stages. The mean squared deviations in SH, $T = 10$ are between 0 and 0.14. Unsurprisingly, beliefs are more likely miscalibrated in the PD games where the mean squared deviations range between 0.08 and 0.22 for the PD, $T = 30$ game and between 0.07 and 0.30 for PD, $T = 25$ game. Miscalibrations of a similar magnitude can be observed for the SH, $T = 15$ game in which the mean squared deviations range between 0.10 and 0.33.

To put these numbers in perspective, notice that assigning roughly equal probabilities to the two actions would result in a squared deviation of 0.20.[17] Taking this value as a benchmark, the accuracy of stated beliefs is significantly higher when ($|T - T'| = 20$), but not when the two games are more similar ($|T - T'| = 10$).[18] The lower accuracy of beliefs when the games are more similar suggests that the lower (higher) temptation to deviate from cooperation in the PD (SH) creates greater strategic uncertainty about opponents' behavior. This increase in strategic uncertainty may also explain why we observe lower best response rates in the *PD25-SH15* and *SH15-PD25* treatments.

5.3 What determines the shift in behavior between stages?

While there is some inertia with respect to actions within stages, we have seen evidence for large swings in behavior between stages. More precisely, when the temptation payoff, T, is lowered after stage 1, i.e., when the PD turns into a SH, almost every subject switches from playing Y to playing X in treatment *PD30-SH10* (87%). This change is less dramatic in *PD25-SH15* where, on average, only two-thirds of subjects change their action choice. We can also reject the hypothesis of equal switching rates in the first round of stage 2 in *PD30-SH10* and *PD25-SH15* using a robust rank-order test ($p = 0.029$). The same pattern is true when T is lowered after stage 2, which is the case in *SH10-PD30* and *SH15-PD25*. In *SH10-PD30*, 90% of subjects switch from Y to X, whereas only 65% make the same switch in *SH15-PD25*. Again, we can reject the hypothesis of equal switching rates in the first round of stage 3 in *SH10-PD30* and *SH15-PD25* (robust rank-order test, $p = 0.057$).

Interestingly, when the temptation payoff T is increased after stage 1, i.e., a switch from SH to PD, on average 60% of subjects in *SH10-PD30* and 47% of

[17] Note that it is technically not possible to report equal probabilities, since subjects had to indicate how many of the other nine group members would choose cooperation (X). Thus, the squared deviation of 0.2 reflects the case where more weight −5 out of 9 others—is placed on the actual choice.

[18] A Wilcoxon signed-rank test comparing the mean squared deviations of all games having the same parameterization with the benchmark of 0.20 yields the following p values: $p = 0.046$ (PD, $T = 30$), $p = 0.027$ (SH, $T = 10$), $p = 0.58$ (PD, $T = 25$) and $p = 0.34$ (SH, $T = 15$).

Table 6 Regression: determinants of choice in first round after a game change

Dependent variable						
Choice = X	Change from PD to SH			Change from SH to PD		
$T = \{15, 25\}$	−0.103	−0.097	−0.110	0.088	0.058	0.062
	(0.089)	(0.097)	(0.078)	(0.079)	(0.095)	(0.084)
Belief	0.362***	0.395***	0.378***	0.627***	0.630***	0.654***
	(0.085)	(0.094)	(0.089)	(0.111)	(0.136)	(0.107)
Own 1st round cooperation	0.148**		0.182***	0.263***		0.276***
	(0.058)		(0.062)	(0.084)		(0.078)
Experienced cooperation		0.263	0.670**		0.009	−0.108
		(0.319)	(0.281)		(0.077)	(0.123)
Decision time	−0.008	−0.009*	−0.013**	−0.012***	−0.011**	−0.012***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.004)	(0.005)
N	120	120	120	120	120	120

* $p < 0.10$, **$p < 0.05$, *** $p < 0.01$

Probit regressions (average marginal effects) with robust standard errors clustered at the matching group level in parentheses. "Belief" is the elicited belief about others' play of X (cooperation) in the first round of a stage and is between 0 and 1. "Own 1st round cooperation" indicates whether a subject cooperated in the first round of the first sequence and "Experienced Cooperation" is the average cooperation rate of one's opponents in the previous stage. "Decision time" is the time needed for a decision in seconds. "$T = \{15, 25\}$" is an indicator variable which is one for *PD25-SH15* and *SH15-PD25*

subjects in *SH15-PD25* change their action choice. According to a robust rank-order test, this difference is not statistically significant ($p > 0.114$). When the same change in T occurs after stage 2, the switching rate is 80% in *PD30-SH10* and only 47% in *PD25-SH15*. Again, the difference is not statistically significant using a robust rank-order test ($p > 0.114$). The observation that the observed swings in behavior between stages are less pronounced in the more similar games provides some qualitative support for *Hypothesis 2*, which implicitly assumes that changes in behavior between stages are smaller when the games are more similar.

The regressions reported on in Table 6 provide some further understanding of the factors that may account for these large and sudden swings of behavior in the first round of a new stage. The dependent variable is a subject's choice of X in the first round of a stage. The first three columns in Table 6 focus on situations in which the game changes from a PD to a SH and while the last three columns report on situations where the game changes from a SH to a PD. We first note that there is no treatment effect in all specifications as evidenced by the insignificant coefficient on the T indicator variable. That is, after controlling for other independent variables, we find no support for *Hypothesis 2*.

From the regressions it is also apparent that beliefs are important: expecting more opponents to choose cooperative play is positively correlated with play of the cooperative action X. The coefficient on "Belief" is significantly positive in all regressions and the association is stronger for the transition from SH to PD. A second important factor is whether a subject cooperated in the first round of the session. These "cooperative" subjects are significantly more likely to choose *X* in the first round of a stage game change. Experience, on the other hand, seems to play only a minor role. Although an exposure to "more cooperative" opponents in the PD game is positively associated with cooperation in the first round of stage 2 or 3, it is only a significant factor when a subject's first round choice is also included in the regression, suggesting that *cooperation* experience is only important for first-round cooperators. Experience in SH games plays no role for choices when the game change occurs, which is not so surprising as subjects typically experience a high level of cooperation throughout the preceding SH games and thus the variation in play is usually lower than in the PD games. Interestingly, faster decisions, as measured by decision time, more likely result in cooperation, while defection appears to be a more deliberate (time-consuming) decision. The size of this decision time effect, however, is small and the statistical significance of it depends on which covariates are included in the regression.

We have seen that beliefs play an important role when the game changes and Table 3 indicates similar swings in beliefs as in choices. To get a clearer picture of the determinants of these swings in beliefs, we analyze how subjects' beliefs change when they encounter a particular game for the first time. That is, we are interested in the change between initial beliefs (prior to play of stage (1) and beliefs in the first round of a change in *T*. We concentrate here, in particular, on two factors that may contribute to a change in beliefs and that are not affected by a subject's experience within a stage: a subject's initial propensity to cooperate and the initial frequency of cooperative encounters in the first stage.

Let b_{1s} denote a subjects' belief about the number of others playing *X* in round 1 of stage $s = \{1, 2\}$ and denote the difference in such beliefs by $\Delta b = b_{12} - b_{11}$. First, Table 3 (columns 4 and 5) indicates that subjects expect less cooperation by others after an increase of *T* as $\Delta b = -0.39$ in *SH10-PD30* and $\Delta b = -0.34$ in *SH15-PD25*, whereas subjects expect more cooperation from others after a decrease in *T* as in *PD30-SH10* ($\Delta b = 0.33$) (but not in *PD25-SH15* ($\Delta b = -0.05$)). More important, however, is the significantly negative correlation between Δb and first stage, first-round choices in all treatments except *PD25-SH15*.[19] In fact, first-round defectors adapt their beliefs more drastically than do cooperators in the transition from the PD to the SH game, while the opposite holds for the transition from the SH to the PD game where first-round cooperators change their beliefs more drastically. This finding suggests that subjects whose initial choice turns out to be the predominant choice in later rounds (i.e., *Y* in PD games and *X* in SH games) are more sensitive to the strategic incentives of these games.

Second, to examine the impact of initial experience on subjects' changing beliefs, we consider the role of opponents' play in the first two rounds of stage 1. Notice that because of our random-matching procedure opponents' behavior in the first two rounds is not correlated with the past behavior of a subject. We expect that Δb is greater for subjects who experienced more cooperation in the first two rounds (i.e.,

[19] The correlation coefficient in *PD25-SH15* is -0.08 ($p = 0.62$), whereas coefficients range between -0.54 and -0.56 in *PD30-SH10*, *SH10-PD30* and *SH15-PD25* (p values <0.015).

they expect more cooperation in round 1 of stage (2) than subjects who were confronted with less cooperation. Indeed, this is what we find for all treatments. However, the correlation is significant only for treatments *PD25-SH15* and *SH15-PD25*.[20] That is, experiencing more initial cooperation has, on average, more impact on Δb when the games are more similar as in *PD25-SH15* and *SH15-PD25*.

These findings suggest, in line with what we have seen for choice behavior, that subjects' first-round choices influence their change in beliefs in the transition from one game to another, similar game. In more similar games, cooperative play by opponents in early rounds is also important for changes in beliefs. Again, this finding suggests that there is more scope for precedent transfers when there is more (expected) variation in play (as is, for example, is the case in the SH, $T = 15$ game as compared with the SH, $T = 10$ game).

6 Conclusion

We have conducted and reported on an experiment examining behavior and equilibrium selection in two similar, indefinitely repeated games, Stag Hunt and Prisoner's Dilemma under anonymous random matching. Our aim was to understand the role of historical precedents for equilibrium selection by varying the similarity across the two games. Arguably, the similarity of two games is an important consideration for effective transfers of precedent. For example, Fudenberg and Kreps (1988) noted "..., as seems reasonable, players infer about how their opponents will act in one situation from how opponents acted in other, similar situations."

Our main finding is that the role of precedent for equilibrium selection in the indefinitely repeated games that we study appears to be rather limited; the history of play in different but similar games appears to have little impact on behavior. Following small changes in the value of the temptation payoff parameter, there are large swings in the frequency with which players play the two actions available to them. More precisely, the frequency of play of the cooperative action is stochastically larger in SH parameterizations of the stage game than in PD parameterizations of the stage game, regardless of the order in which these two types of games are played and despite the fact that both pure equilibria of the SH can also be supported as equilibria under the PD using the contagious, society-wide grim trigger strategies of Kandori (1992). The swings in the frequency of cooperative play are associated with significant changes in beliefs about the cooperative play of others, which are largely influenced by players' initial propensities to cooperate as well as by the frequency of cooperative encounters early on.

One possible explanation for the lack of precedent transfer in our experiment is that our experimental design, emphasizing the change in the temptation parameter, T, as the sole difference between the two games, may have triggered an

[20] The correlation coefficients are 0.14 ($p = 0.54$) in *PD30-SH10*, 0.04 ($p < 0.87$) in *SH10-PD30*, 0.31 ($p = 0.054$) in *PD25-SH15* and 0.35 ($p = 0.026$) in *SH15-PD25*.

experimenter demand effect wherein subjects felt compelled to respond to the change by playing differently.[21] While we cannot rule out experimenter demand effects as a possible explanation for our results, there are good reasons to think that such effects are not solely responsible for the behavior that we observe. We note first that we do not find any order effects using our within-subject design; the behavior of subjects playing a PD game in stage 2 is similar to the behavior of subjects playing the same PD game in stage 1. This observation does not rule out demand effects, but it is reassuring that subjects behave in the same manner in situations where a demand effect coming from a change in the game played in stage 2 is possible and a situation where a demand effect is not possible, i.e., play of the same game in stage 1. Second, if "demand effects" are in fact responsible for what we find, they are weaker when games are more similar to one another. Recall that when the games are more similar, the shift of behavior between stages is less pronounced and there are even some instances of a precedent transfer. Finally, at the individual level, we observe not only that behavior changes as T changes but also that beliefs change as well and these belief changes are negatively correlated with first-round behavior. Specifically, first-round defectors in the PD games adapt their beliefs (i.e., expect more cooperation in the new game) more drastically than do first-round cooperators, while first-round cooperators in the SH adapt their beliefs more (expect less cooperation) than do first-round defectors. These differences suggest that type heterogeneity and belief updating in response to this heterogeneity are both playing some role in the transitions that are taking place.

There are several possible directions in which our analysis of the role of equilibrium selection precedents in repeated games might be profitably extended. First, one could consider changes in the value of T that are in an even smaller neighborhood of the threshold value that differentiates the SH from the PD to examine whether such smaller changes might give precedents even more weight. In addition, one might explore the role of precedents across similar games played in *continuous* time; Friedman and Oprea (2012), for instance, report varying but very high cooperation rates for different parameterizations of the Prisoner's Dilemma game played by subjects in continuous time. It would be of interest to consider play in continuous (or near continuous) time across similar classes of games including the Stag Hunt and Prisoner's Dilemma games or the Battle of the Sexes and Chicken games. Finally, we would ultimately like to endogenize the transition between the different stage games, as is done in stochastic Markov games, or to endogenize the value of the temptation parameter, T itself.[22] Specifically, one might suppose that the temptation payoff value, T, was a steadily increasing function of the time that players spent playing the efficient, XX outcome and, symmetrically, the temptation value T was a steadily decreasing function of the time that players spent

[21] For a discussion of such demand effects, see, e.g., Zizzo (2010) who defines experimenter demand effects as "changes in behavior by experimental subjects due to cues about what constitutes appropriate behavior."

[22] Vespa and Wilson (2015) have studied Markov games in the laboratory. Dal Bò et al. (2017) report on an experiment where subjects endogenously choose, via a majority rule vote, whether to participate in a Prisoner's dilemma game or an alternative (anti-PD game) where cooperation is a dominant strategy; they find that a slight majority (53.6%) vote for the PD game!

playing the inefficient *YY* outcome. In such a setting it would be possible to imagine cycles of coordinated play between the efficient and inefficient outcomes with transitions occurring endogenously according to some threshold value for *T*. We leave such extensions to future research.

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