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Cooperation, but No Reciprocity: Individual Strategies in the Repeated Prisoner's Dilemma†

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In the repeated prisoner's dilemma, predictions are notoriously difficult. Recently, however, Blonski, Ockenfels, and Spagnolo (2011)—henceforth, BOS—showed that experimental subjects predictably cooperate when the discount factor exceeds a particular threshold. I analyze individual strategies in four recent experiments to examine whether strategies are predictable, too. Behavior is well summarized by "Semi-Grim" strategies: cooperate after mutual cooperation, defect after mutual defection, randomize otherwise. This holds both in aggregate and individually, and it explains the BOS-threshold: Semi-Grim equilibria appear as the discount factor crosses this threshold, and then, subjects start cooperating in round 1 and switch to Semi-Grim in continuation play. (*JEL* C72, C73, C92, D12)

Many economic interactions are long-run relationships. The welfare generated in long-run relationships primarily depends on whether agents manage to sustain mutual cooperation. Since this applies to all kinds of relationships, including personal, industrial, and multinational ones, the emergence of cooperation has been studied extensively. The theoretical results are well known for underlining the diversity of potential equilibrium outcomes. This diversity is theoretically robust to impatience, renegotiation, and various notions of equilibrium refinement. In line with the theoretical diversity, experimental analyses showed that the existence of cooperative equilibria is necessary but not sufficient for cooperative behavior to emerge (e.g., Dal Bó 2005).

Recently, Blonski, Ockenfels, and Spagnolo (2011)—henceforth, BOS—and Dal Bó and Fréchette (2011)—henceforth, DF—showed that an empirically *predictive* condition for cooperation exists nonetheless. In experiments on the repeated prisoner's dilemma, they showed that cooperation sets in once the discount factor δ exceeds a certain threshold. It is currently unclear whether this threshold has a strategic interpretation, such as guaranteeing existence of a particular class of equilibria. The

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purpose of the present paper is twofold, to show that individual strategies are similarly predictable and to provide a strategic interpretation of the BOS-threshold. I show that across treatments in four recent experiments on the repeated prisoner's dilemma, strategies have a simple, common structure: cooperate with high probability if both players cooperated in the previous round, with low probability after mutual defection, and with intermediate but equal probabilities if exactly one player cooperated in the previous round—regardless of who had cooperated. I call such strategies *Semi-Grim*, theoretically characterize the Markov perfect equilibria (MPEs) in Semi-Grim strategies, and show that laboratory subjects switch to cooperation in round 1 and to Semi-Grim in the continuation simultaneously at a level of δ that is roughly equal to where the Semi-Grim MPEs appear. The Semi-Grim MPEs appear as δ crosses the BOS-threshold, which in turn explains the observations in BOS and DF.

In addition to reanalyzing the data of BOS and DF, which in total contain 15 treatments where δ is comparably near the BOS-threshold, I consider Duffy and Ochs (2009) and Fudenberg, Rand, and Dreber (2012), which contain one treatment each where δ is substantially above the BOS-threshold. This provides two robustness checks. The remaining treatments of their experiments are nonstandard repeated games with either fixed rematching or exogenous noise. The analyzed dataset contains about 30,000 decisions of 550 subjects in 4 different experiments and 17 different treatments.

I find that laboratory behavior is effectively organized if we distinguish behavior in round 1 and behavior in the continuation game. In round 1 of each game, the cooperation rate depends strongly on the treatment parameters. Cooperation starts to occur systematically (in 50 percent of the games) almost exactly as δ crosses the BOS-threshold. In contrast to round-1 behavior, the continuation strategies are fairly constant across treatments. They are well described by 1-memory Markov strategies and have the outlined Semi-Grim structure. Individually, however, subjects switch strategies as δ increases. Starting with Always-Defect, they move via Grim toward Semi-Grim. This is compatible with the observation that the "average strategy" (i.e., the average of all individual strategies) is Semi-Grim in most treatments. As δ crosses the BOS-threshold, about 50 percent of the subjects play Semi-Grim MPE strategies, and further above the threshold they play Semi-Grim MPE strategies almost exclusively.

Both strategies and expected payoffs in repeated games thus turn out to be more predictable than previously thought, at least to the degree that mixed strategies are predictable. This has a variety of implications. The recurrence of nontrivially mixed strategies suggests that some form of equilibrium refinement is applied consistently across experiments and in fact continents. It therefore seems reasonable to expect that the results appropriately generalized apply to more general repeated games. A refinement concept potentially explaining Semi-Grim is robustness to both imperfect monitoring and utility perturbations, as I outline briefly below. The usage of mixed strategies even after mutual cooperation contrasts with the conventional focus on trigger strategies. It indicates that at least some of the subjects do not "misconduct" (defect after mutual cooperation) accidentally but willfully in the sense of strategic randomization.[1] The observation that the cooperation rates are equal after mixed

[1] The observation that the strategies are mixed also explains that the sucker's payoff (unilateral cooperation) is of strategic relevance, as postulated by BOS, since it implies that one may end up cooperating unilaterally. It also

histories contradicts Tit-for-Tat and suggests that reciprocity is less relevant in long-run interactions than previously hypothesized. The results also imply predictability of expected payoffs, i.e., of *relationship values*. This is not implied by existing results in noncooperative game theory, but it is intuitive[2] and generally assumed in related work such as the marriage problem following Gale and Shapley (1962) and in search and matching theory (see, e.g., Mortensen and Pissarides 1994).[3] Thus, results confirming the predictability of relationship values may help solidify the foundation of related theoretical and empirical work.

Section I provides background information. Section II provides a first look at the data. Section III analyzes individual behavior and Section IV concludes. The online Appendix contains all parameter estimates and various robustness checks.

I. Related Literature and Background Information

This section provides background information on the repeated prisoner's dilemma and reviews theoretical and experimental results that relate to the subsequent analysis. A repeated prisoner's dilemma proceeds for an infinite number of rounds. In each round, players simultaneously decide whether to cooperate (c) or defect (d). Future payoffs are discounted using the factor $\delta \in (0, 1)$. Following Roth and Murnighan (1978), laboratory experiments implement infinitely repeated games with discount factor δ as indefinitely repeated games with continuation probability δ after each round. The current theoretical literature on repeated games considers general classes of constituent games, but many ideas where originally developed in the repeated prisoner's dilemma and generalized in a second step. The current experimental literature largely focuses on the repeated prisoner's dilemma,[4] partly for the lack of potentially generalizable results. The purpose of the present paper is to analyze a potentially general pattern in individual behavior.

Experimental analyses usually consider generalized prisoner's dilemmas (PDs) as in panel A of Figure 1. Fixing scale and level of the stage game payoffs, we obtain a two-dimensional continuum of stage games. Let me standardize by setting $p_{dd} = 1$ and $p_{cd} = 0$, as in panel B of Figure 1 and in the best-known examples. Three examples are as follows:

Γ_1:

	c	d
c	2, 2	0, 3
d	3, 0	1, 1

Γ_2:

	c	d
c	3, 3	0, 4
d	4, 0	1, 1

Γ_3:

	c	d
c	3, 3	0, 5
d	5, 0	1, 1

The best-known examples are Γ_1 and Γ_2. In comparison, Γ_2 induces higher incentives for long-run cooperation $(p_{cc} - p_{dd})$ than Γ_1, while the gain from unilateral

explains that individual choices may sometimes appear lenient (Grim only after two defections, "giving the benefit of the doubt" after the first defection) or forgiving (Tit-for-Two-Tats, Two-Tits-for-Tat), as observed by Fudenberg, Rand, and Dreber (2012). This is discussed in detail below.

[2] In practice, the first ten minutes suffice to predict the relationship value (Sunnafrank and Ramirez 2004) and even "love at first sight" is empirically stable (Barelds and Barelds-Dijkstra 2007).

[3] For example, if learning relationship values would require long test runs, then efficiency of matching as in the (theoretical) marriage problem would be practically impossible.

[4] Exceptions are analyses of repeated trust games (e.g., Engle-Warnick and Slonim 2004, 2006a,b), and of repeated oligopoly (e.g., Dufwenberg and Gneezy 2000 and Huck, Müller, and Normann 2001).

Panel A. Prisoner's dilemma (PD)

	c	d
c	p_{cc}, p_{cc}	p_{cd}, p_{dc}
d	p_{dc}, p_{cd}	p_{dd}, p_{dd}

Panel B. "Standardized" PD

	c	d
c	a, a	$0, b$
d	$b, 0$	$1, 1$

FIGURE 1. PRISONER'S DILEMMA GAMES ($p_{dc} > p_{cc} > p_{dd} > p_{cd}$ and $b > a > 1$)

defection ($p_{dc} - p_{cc}$) is held constant. Γ_3 induces higher incentives for unilateral defection than Γ_2, while the gain from long-run cooperation is held constant. Finally, between Γ_3 and Γ_1, Γ_1 models interactions with lower incentives to invest into relationships, which is captured by $p_{dd} - p_{cd}$ in relation to the other payoff differences.[5]

Thus, even the simple class of standardized PDs allows us to study three intuitive factors for the emergence of long-run cooperation: incentives to cooperate, incentives to defect, and incentives to invest. Subgame perfect equilibria sustaining long-run cooperation in the repeated PD exist if the Grim strategy is an equilibrium strategy. Grim prescribes to start with cooperation, and subsequently, to cooperate if both players cooperated in the previous round and to defect otherwise. However, factors such as the long-run gain from cooperation and the short-run gain from unilateral defection, while being of intuitive relevance, are not of strategic relevance if Grim is an equilibrium strategy, and the incentive to invest is never of relevance with respect to the existence of equilibria sustaining cooperation. Blonski, Ockenfels, and Spagnolo (2011) argue that subgame perfection does therefore not capture our intuition of play in repeated games.

Relatedly, subgame perfection has been criticized for being compatible with many counterintuitive outcomes in repeated games. In fact, if players are sufficiently patient, *any* individually rational outcome may be sustained along the equilibrium path (Friedman 1971; Fudenberg and Maskin 1986).[6] The behavioral strategies associated with these Folk theorems are called "trigger" strategies: players distinguish cooperation phases and punishment phases, in the cooperation phases they stick with a predefined plan,[7] while any deviation from the plan triggers the punishment phase. During the punishment phase, they play the Nash equilibrium (in PDs) for t rounds and t is sufficiently large to render unilateral deviation unprofitable. Afterward, play reverts to the cooperation phase. Along the equilibrium path, both players stick to the plan.

[5] One may "invest" by cooperating unilaterally. The lower p_{cd}, the lower are the incentives to invest. Now set $p_{cd} = -1$ in Γ_3 and restandardize the payoffs to arrive at Γ_1. The sequence of games is

	c	d
c	3, 3	−1, 5
d	5, −1	1, 1

	c	d
c	4, 4	0, 6
d	6, 0	2, 2

	c	d
c	2, 2	0, 3
d	3, 0	1, 1

[6] In the repeated PD, an outcome is individually rational if the average payoffs of both players are greater than their Nash payoffs. Similar Folk theorems apply, for example, for players who discount future payoffs by more than infinitesimal amounts (Abreu, Pearce, and Stacchetti 1990; Abreu, Dutta, and Smith 1994; Stahl 1991); and for the more general class of stochastic games where the constituent game is not constant (Dutta 1995).

[7] A plan may include defection, such as the plan to cycle through $(c, c), (c, d), (d, d), (c, c), \ldots$

An approach to refine the set of subgame perfect equilibria relates to the observation that trigger strategies contradict a behavioral observation of Fudenberg, Rand, and Dreber (2012): at least some of the subjects defect and risk conflict willfully. To be clear, the cooperation phase in trigger strategies may include planned defection, but Folk theorems and trigger strategies do not explain "conflicts" in the strict sense. In this context, I say that action $s \in \{c, d\}$ triggers conflict if the continuation payoff after playing s is lower for the player in question than the continuation payoff following an alternative action s'. The punishment phase in trigger strategies represents a conflict, but it is off the equilibrium path. Arguably, conflict is an off-equilibrium phenomenon for some people, perhaps following random trembles, but this is not true for everybody in all situations, as documented by Fudenberg, Rand, and Dreber (2012).

There is a well-known set of mixed equilibria, called belief-free equilibria (Ely, Hörner, and Olszewski 2005), explaining conflict. These equilibria are in 1-memory Markov strategies, i.e., in strategies where the probability of cooperation in a given round depends only on the choices made in the previous round. The choices in the previous round constitute the *state*, and in the repeated PD, five 1-memory states are possible: $\emptyset, (c, c), (c, d), (d, c), (d, d)$, where $\emptyset$ denotes the state in the first round. A 1-memory strategy $\boldsymbol{\sigma}$ defines cooperation probabilities $\sigma_{\emptyset}, \sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}$ for each of these states. The notation for asymmetric states is that σ_{cd} denotes the cooperation probability if the player in question cooperated in the previous round, and σ_{dc} denotes the cooperation probability if the player in question defected in the previous round. I will make frequent use of this notation.

Surprisingly, neither the restriction to 1-memory strategies nor that to belief-free equilibria restricts the set of equilibrium outcomes in the repeated PD. The basic insight, originally described in Ely and Välimäki (2002) and further developed by Bhaskar, Mailath, and Morris (2008) and Press and Dyson (2012), is that two simple conditions on a player's Markov strategy $\boldsymbol{\sigma}$ suffice to ensure that his opponent is indifferent in all states. Since a mixed strategy offers four degrees of freedom per player ($\sigma_{\emptyset}$ is not free in this sense), this leaves two degrees of freedom for a player to affect his opponent's payoff while ensuring her indifference in all states. This is possible for both players, and thus we obtain a continuum of (mixed) 1-memory Markov perfect equilibria where the players "set" each other's payoff. In the repeated PD, this continuum covers the range of Folk theorem outcomes. In turn, it follows that the restriction to 1-memory Markov strategies does not refine the range of equilibrium outcomes in repeated PDs. It also follows that these equilibria explain conflict: since every player is indifferent between playing c and d, where d always yields the higher stage game payoff, playing d must yield the lower continuation payoff—and thus it triggers conflict in the above sense, while being on the equilibrium path. Further, as the construction ensures that players are indifferent in all states, their best responses are independent of their beliefs about the opponent's history. These "belief-free" equilibria (Ely, Hörner, and Olszewski 2005) are robust to imperfect monitoring. In turn, requiring robustness to imperfect monitoring does also not refine the set of equilibrium outcomes, but it may explain conflict.

An alternative approach to equilibrium refinement is by requiring robustness to payoff perturbations (which are private information). A Markov perfect equilibrium is called purifiable if it is robust to generic payoff perturbations (Doraszelski and

Escobar 2010). Pure equilibria are trivially purifiable, which implies that Folk theorems are robust in this sense. In turn, requiring that equilibria be belief-free and robust to generic payoff perturbations is prohibitive—no belief-free equilibrium is purifiable and vice versa (Bhaskar, Mailath, and Morris 2008). This observation, that equilibrium refinement in repeated PDs is either ineffective (each approach on its own) or prohibitive (both approaches combined), is a recurring theme in theoretical analyses. So far, equilibrium refinement therefore offers little guidance with respect to the prediction of strategies or outcomes in repeated PDs (for an overview, see Mailath and Samuelson 2006).

In contrast, arguments leaning on evolutionary stability suggest that Tit-for-Tat (TFT) and Win-Stay-Lose-Shift (WSLS) might be particularly robust strategies. Tit-for-Tat emerged as the winner of the tournaments of Axelrod (1980a,b), in which he invited game theorists to submit strategies for a (finitely) repeated PD. It prescribes to start with c in round 1 and to imitate the opponent in all subsequent rounds. TFT is strictly reciprocal, it is at least as old as the Old Testament ("eye for an eye, tooth for a tooth"), and it is "unbeatable" if $\delta \approx 1$ (Duersch, Oechssler, and Schipper 2014). That is, playing against TFT, a player's payoff may exceed the TFT-player's payoff by no more than a unilateral defection—and to get the TFT-player to return to cooperation, one has to give up the advantage that the unilateral defection yielded in the first place. One argument against widespread use of TFT relates to the fact that it is not a subgame-perfect equilibrium. After a unilateral defection, two TFT players enter a $(c, d) - (d, c)$ cycle, and perpetually cycling this way is not optimal. Players benefit from breaking out by playing c after (c, d), and indeed children are taught early on to be "the bigger person" when such cycles emerge. Thus, people are trained to deviate from Tit-for-Tat by cooperating after (c, d) with positive probability. A related argument is made by Nalebuff (1987).

Nowak and Sigmund (1993) and Imhof, Fudenberg, and Nowak (2007) show that in evolutionary processes where players may tremble with small probability, TFT will invade populations of All-Defect players, but once All-Defect players are extinct, Win-Stay-Lose-Shift (WSLS) players replace the TFT players as they may return to cooperation after accidental defections. WSLS prescribes to start with c, to play c after (c, c) and (d, d), and to play d otherwise. WSLS is a simple learning rule, though not universal (Olton and Schlosberg 1978), and in repeated PDs, it is unstable if one round of punishment does not suffice to render unilateral defection unprofitable (e.g., in Γ_1 above). Thus, WSLS in this strict form cannot be a general principle of behavior in repeated PDs. Weaker forms of WSLS, where $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1, 0, 0, \alpha)$ with $\alpha \in [0, 1]$, can be equilibria in rather general circumstances, however.

The testable predictions that can be derived from the theoretical literature include the condition for the existence of equilibria sustaining cooperation, and whether cooperating subjects play Grim, TFT, or WSLS. Early experimental research, such as Roth and Murnighan (1978) and Murnighan and Roth (1983), showed that cooperation increases when equilibria sustaining cooperation exist. The existence of cooperative equilibria did not seem to yield substantial or even robust cooperation, however. Dal Bó (2005) points out shortcomings of early experiments (such as playing against the experimenter) and conducted a novel experiment comparing finitely and infinitely repeated games. There, behavior was largely in line with the

Table 1—Overview of Widely Discussed Strategies

	$\sigma_\emptyset$	σ_{cc}	σ_{cd}	σ_{dc}	σ_{dd}	Code
Always defect	0	0	0	0	0	(0, 0, 0, 0)
Always cooperate	1	1	1	1	1	(1, 1, 1, 1)
Tit-for-Tat	1	1	0	1	0	(1, 0, 1, 0)
Win-Stay-Lose-Shift	1	1	0	0	1	(1, 0, 0, 1)
Grim	1	1	0	0	0	(1, 0, 0, 0)

theoretical predictions. In line with the nonuniqueness of equilibrium outcomes, Dal Bó (2005) found that existence of equilibria sustaining cooperation is necessary but not sufficient for cooperation to emerge. As indicated above, equilibria sustaining cooperation exist if Grim strategies are equilibrium strategies, and they are if $\delta \geq (p_{dc} - p_{cc})/(p_{dc} - p_{dd})$ in the above notation. The resulting question, whether a threshold for δ that is predictive of cooperation existed nonetheless, has been answered in the affirmative by Blonski, Ockenfels, and Spagnolo (2011) and Dal Bó and Fréchette (2011). They showed that cooperation occurs predictably if δ exceeds a threshold significantly stronger than existence of cooperative equilibria, namely if

$$\delta \geq \frac{p_{dc} + p_{dd} - p_{cd} - p_{cc}}{p_{dc} - p_{cd}} =: \delta^*. \tag{1}$$

BOS obtained this threshold from axioms establishing a balance between the three factors for long-term cooperation reviewed above: the incentive to cooperate $(p_{cc} - p_{dd})$, the incentive to defect $(p_{dc} - p_{cc})$, and the incentive to invest $(p_{dd} - p_{cd})$. Unfortunately, their analysis provides little insight into related equilibrium strategies,[8] and indeed, it is unclear whether δ^* has a strategic interpretation in the sense of being the threshold for the existence of some class of equilibria. Finally, DF as well as Fudenberg, Rand, and Dreber (2012) analyzed individual strategies and both found that of the candidate strategies reviewed above (Table 1), no particular strategy is used systematically across treatments.

In relation to this background, let me preview my results. Behavior in experiments is well-organized by a class of mixed strategies that I call Semi-Grim. There is a continuum of belief-free equilibria in Semi-Grim strategies. These equilibria exist if δ exceeds the BOS-threshold, which implies that they are compatible with the intuitive relevance of the three factors of long-term cooperation and that they explain the experimental result that cooperation sets in at this threshold. By virtue of being belief-free equilibria, these Semi-Grim equilibria are also compatible with the observation that some subjects willfully trigger conflict and thus they amalgamate many of the ideas expressed in the literature.

[8] The axioms relate more closely to concepts in cooperative game theory. In short, they are: invariance with respect to linear payoff transformations; monotonicity of cooperation in δ; cooperation disappears as $p_{cd} \rightarrow -\infty$; cooperation occurs if and only if a condition $\mu \geq 0$ where μ is additively separable with respect to $p_{cc} - p_{dd}, p_{dc} - p_{cc}, p_{dd} - p_{cd}$; and the two differences $p_{dc} - p_{cc}$ and $p_{dd} - p_{cd}$ have the same weight in this condition.

II. A First Look at the Data

Typical concerns about the replicability of experimental results relate to the possible relevance of cohort effects and to the multiple-testing problem.[9] In order to mitigate these concerns, I analyze behavior in four recent experiments and use adjusted p-values following Wright (1992). The four experiments and their designs are reviewed in Table 2. Blonski, Ockenfels, and Spagnolo (2011) conducted an experiment with ten different treatments to test their δ^*-criterion for the emergence of cooperation in the repeated PD. Dal Bó and Fréchette (2011) conducted an experiment with six different treatments and a particularly large number of repeated games per session (up to 77) to study the evolution of cooperation as subjects accumulate experience. In contrast to BOS, their experiment provides observations for an alternative subject pool, additional treatments, and for subjects with a larger level of experience. Finally, Fudenberg, Rand, and Dreber (2012)—henceforth, FRD—study repeated PDs when actions are implemented with exogenous noise and Duffy and Ochs (2009)—henceforth, DO—study repeated PDs with either fixed or random matching. Each of their experiments contained one treatment with a "standard" repeated PD. These treatments implemented comparably high discount factors. They complement the treatments of BOS and DF, which tend to focus on setups close to the BOS-threshold for cooperation, and thus provide robustness checks.

Table 3 presents estimates of the average of individual 1-memory strategies $\boldsymbol{\sigma} = (\sigma_\emptyset, \sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd})$, or "average strategy" for short. The average strategies are estimated in linear probability models with subject-level random effects.[10] It provides a first, preliminary picture of the strategies used in experiments. Following BOS, DF, and FRD, I focus on the second halves of these experiments, when learning supposedly has stabilized (as discussed shortly). This eliminates treatment 3 of BOS, which was played only in first halves. Treatment 6 of BOS contains virtually no cooperation, which implies that a comparison of cooperation rates contingent on histories is impossible there. Table 3 therefore comprises 16 treatments in total.

Across treatments and experiments in Table 3, the average strategy has a surprisingly constant structure, approximately $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (0.9, 0.3, 0.3, 0.1)$. Recall that σ_{cd} denotes the probability that a player cooperates when his most recent action was c and his opponent's most recent action was d. I refer to strategies of the form $\sigma_{cc} > \sigma_{cd} = \sigma_{dc} > \sigma_{dd}$ as *Semi-Grim*. Similar to Grim, the probability of returning to mutual cooperation after defection is rather low, but Semi-Grim is milder than Grim in that returning to mutual cooperation is possible, in particular after unilateral defection.

[9] The multiple-testing problem refers to the fact that if sufficiently many hypotheses are tested, some of them are bound to be rejected with statistical significance. Such results are not robust across studies.

[10] The linear probability models regress the probability of cooperation (either 0 or 1 in each round) on all 1-memory histories including $\emptyset$ (without intercept). This approach yields the most transparent estimates of the cooperation probabilities while controlling for subject-level random effects. Random effects are used to control for unobserved heterogeneity in the frequency of cooperation. Subjects that cooperate more frequently find themselves more often in states such as (c, c) and (c, d). Observations in these states are thus more likely to be from subjects that tend to be cooperative, and observed cooperation rates in these states may thus be biased toward cooperation. Subject-level random effects help correct for that. To further accommodate for the panel character of the data, all p-values and standard errors are bootstrapped by resampling at the subject level, as described in the note to Table 3. The qualitative results do not change if the random effects are dropped (see the online Appendix).

Table 2—Overview of the Datasets and Treatment Parameters

	Game parameters			Sizes of the datasets			
Treatment	b	a	δ	Subj.	$\frac{\text{Games}}{\text{Subj.}}$	$\frac{\text{Decis.}}{\text{Subj.}}$	Decis.
Blonski, Ockenfels, and Spagnolo (2011)							
6	1.429	1.286	0.5	20	8	17	340
2	1.25	1.125	0.75	20	8	42	840
7	1.429	1.286	0.75	20	8	25	500
4	2.5	1.5	0.75	20	8	27	540
8	1.429	1.286	0.875	20	5	34	680
5	2.5	1.5	0.875	20	5	46	920
9	2.4	1.8	0.75	20	8	38	760
1	3	2	0.75	20	8	30	600
10	4.667	3	0.75	20	8	34	680
Dal Bó and Fréchette (2011)							
1	2.923	1.538	0.5	44	30–36	55–80	2,988
3	2.923	2.154	0.5	50	36	62–81	3,614
2	2.923	1.538	0.75	44	14–17	70–103	3,606
5	2.923	2.769	0.5	46	34–39	67–78	3,398
4	2.923	2.154	0.75	38	12–24	49–75	2,524
6	2.923	2.769	0.75	44	15–18	59–83	3,140
Duffy and Ochs (2009), "random rematching" treatment							
	3	2	0.9	56	4–7	28–103	3,276
Fudenberg, Rand, and Dreber (2012), "no-noise" treatment							
	5	4	0.875	48	3–5	24–42	1,800

Note: The "game parameters" are standardized as in panel B of Figure 1.

The stability of average 1-memory strategies across treatments and experiments has not been pointed out in the literature. The only references related to this observation that I found are in Rapoport and Mowshowitz (1966), who observed $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (0.81, 0.43, 0.37, 0.22)$, which is discussed briefly also in Erev and Roth (2001), and in Bruttel and Kamecke (2012), who elicit strategies via three different methods (hot play, strategy method, and a Moore procedure following Engle-Warnick and Slonim 2004, 2006a) and obtain, but do not discuss, strategies of the form $\sigma_{cc} > \sigma_{cd} \approx \sigma_{dc} > \sigma_{dd}$ in their logistic regressions (see their 2006a, Table 4). However, the robustness of Semi-Grim strategies on average does not guarantee that any individual subject plays a Semi-Grim strategy. This motivates my first research question.

Question 1: *Do subjects play Semi-Grim also individually, not just on average?*

My second research question relates to the observation that $\sigma_{cd} = \sigma_{dc}$ is rejected in just 1 of the 16 treatments. The differences in the cooperation rates $|\sigma_{cd} - \sigma_{dc}|$ are mostly around than 10 percentage points or less.[11] These observations stand in

[11] The differences $\sigma_{cd} - \sigma_{dc}$ are almost perfectly balanced if we consider the raw probabilities (dropping the random effects), as shown in the online Appendix. The online Appendix also contains several robustness checks suggested by a reviewer. I computed the actual average payoffs for each action in each state from the data, accounting for continuation play, and then computed the "empirical incentive to cooperate" as the difference in expected payoffs between playing c in a given state and playing d. In 15 of the 16 treatments, the incentives to cooperate in state (c, d) are equal to the incentives to cooperate in state (d, c), i.e., they do not differ significantly (the exception is treatment 5 of DF). Thus, the incentives are indeed similar in these two states, corroborating that the cooperation rates are. I verified this by computing the expected payoffs using the estimated 1-memory strategies, which concur. However, in treatments with low δ, in particular in DF's data, the incentives to cooperate are negative across all five

TABLE 3—OVERVIEW OF THE EXPERIMENTS: THE ESTIMATED 1-MEMORY STRATEGIES

Treatment	Probability of cooperation after 1-memory histories								
	$\hat{\sigma}_{\emptyset}$		$\hat{\sigma}_{cc}$		$\hat{\sigma}_{dc}$		$\hat{\sigma}_{cd}$		$\hat{\sigma}_{dd}$
Blonski, Ockenfels, and Spagnolo (*2011*)									
6									
2	0.087	$\ll$	0.973	$\approx$	0.349	$\approx$	0.232	$\gg$	0.006
7	0.250	$\ll$	1.009	$\gg$	0.450	$\approx$	0.345	$\gg$	0
4	0.169	$\approx$	0.865	$\gg$	0.049	$\approx$	0.058	$\approx$	0.027
8	0.470	$\ll$	0.945	$\gg$	0.347	$\approx$	0.287	$\gg$	0.03
5	0.420	$\ll$	0.922	$\gg$	0.383	$\approx$	0.217	$\gg$	0.056
9	0.494	$\ll$	0.981	$\gg$	0.180	$\approx$	0.185	$>$	0.011
1	0.406	$\ll$	0.905	$\gg$	0.281	$\approx$	0.328	$\gg$	0.01
10	0.775	$\approx$	0.890	$\gg$	0.277	$\approx$	0.177	$>$	0.061
Dal Bó and Fréchette (*2011*)									
1	0.062	$<$	0.795	$\gg$	0.428	$\approx$	0.207	$\gg$	0.031
3	0.194	$\ll$	0.811	$\gg$	0.383	$\approx$	0.255	$\gg$	0.118
2	0.263	$\ll$	0.899	$\gg$	0.414	$\approx$	0.300	$\gg$	0.041
5	0.408	$\ll$	0.929	$\gg$	0.244	$\approx$	0.319	$\gg$	0.072
4	0.768	$<$	0.945	$\gg$	0.548	$>$	0.364	$\gg$	0.167
6	0.955	$\approx$	0.98	$\gg$	0.383	$\approx$	0.303	$\gg$	0.106
Duffy and Ochs (*2009*), *"random rematching" treatment*									
	0.696	$\ll$	0.957	$\gg$	0.380	$\approx$	0.342	$\gg$	0.134
Fudenberg, Rand, and Dreber (*2012*), *"no-noise" treatment*									
	0.825	$\ll$	0.949	$\gg$	0.483	$\approx$	0.466	$\gg$	0.171

Notes: The cooperation probabilities $\hat{\sigma}_{\emptyset}$, $\hat{\sigma}_{cc}$, $\hat{\sigma}_{cd}$, $\hat{\sigma}_{dc}$, $\hat{\sigma}_{dd}$ are estimated in linear probability models with subject-level random effects (standard errors and p-values are provided as supplementary material). The relation signs indicate the p-values of statistical tests of the equality of the respective probabilities (p-values are bootstrapped and adjusted as described below): "$\gg,\ll$" indicate $p < 0.003$, "$>,<$" indicate $p < 0.05$, and "$\approx$" indicates p-values greater than 0.05. As σ_{cd}, σ_{dc} mostly do not differ significantly, relative frequencies of $\sigma_{cd,dc}$ have been pooled in tests against either σ_{cc} or σ_{dd}. The treatment numberings for BOS and DF differ from the stage game numberings in their original articles by also distinguishing differences in discount factors. Treatment #3 from BOS is missing above, as it has not been played in the second half of a session.

Bootstrap: I bootstrap p-values to account for the panel structure of the data and in this, I follow Dal Bó and Fréchette (2011) and Fudenberg, Rand, and Dreber (2012). For each treatment, I obtain 10,000 bootstrap samples by randomly sampling the original number of subjects with replacement (all resampled subjects are assigned different labels). For each bootstrap sample b, I obtain the estimates x_b from the random effects regression model. To define the p-value of the null hypothesis that some statistic s is zero (above, s would be the difference of two coefficients), let s_b denote its value in sample b and let s_0 denote its original value. The p-value of the two-sided test is $\frac{1}{2R}\#\{b : |s_b - \bar{s}| > |s_0|\} + \frac{1}{2R}\#\{b : |s_b - \bar{s}| \geq |s_0|\}$, where $\bar{s}$ is the mean of (s_b) and R the number of samples. For discussion and references, see Fox (2008, ch. 21).

Adjusted p-values: To accommodate for the multiplicity of comparisons within treatments, I adjust the bootstrapped p-values (Wright 1992) using the Holm-Bonferroni method (Holm 1979) within treatments. As a result, if a treatment is considered in isolation, the 0.05-level indicated by "$>,<$" is appropriate. If all 16 treatments are considered simultaneously, the corresponding Bonferroni correction requires to further reduce the threshold to $0.003 \approx 0.05/16$, which corresponds with "$>,<$".

Robustness: The online Appendix contains two sets of alternative estimates of the average strategies, using, e.g., ordinary least squares (OLS) (i.e. without random effects). The qualitative results are robust to the estimation method and even more balanced with respect to $\sigma_{cd} \approx \sigma_{dc}$ using the alternative methods.

contrast to the notion of reciprocity: if subjects would be more likely to respond in kind than not, the probability to cooperate after (d, c) should be higher than the probability to cooperate after (c, d). The observation that $\sigma_{cd} \approx \sigma_{dc}$ holds in most treatments, and regardless of whether σ_{cd} and σ_{dc} are high or low, suggests that

states. This suggests that at least some subjects have wrong beliefs about the continuation strategies of their opponents, possibly due to a false consensus effect. This will be acknowledged in the analysis below.

behavior in the repeated PD is not significantly shaped by reciprocity—but perhaps solely by strategic reasoning. This raises the question if Semi-Grim strategies are indeed strategically stable in the sense of Markov perfect equilibrium (MPE) for payoff-maximizing players, or whether weaker solution concepts or more general utility functions are required to explain their occurrence.

Question 2: *Are Semi-Grim strategies compatible with Markov perfect equilibrium?*

My last two research questions are closely related. Even if Semi-Grim MPEs exist in some treatments, they would hardly exist for all stage games and continuation probabilities. Table 3 suggests that Semi-Grim is played in all treatments, however. The possible observation that subjects play Semi-Grim MPEs when they exist would be less informative if they also played Semi-Grim strategies when no such MPEs exist (although there are possible explanations for such phenomena, see, e.g., Samuelson 2001).

Question 3: *Does playing Semi-Grim strategies relate to the existence of Semi-Grim MPEs?*

Finally, even if the continuation strategy is known to be a Semi-Grim MPE strategy, equilibrium play in round 1 is not uniquely determined. At a minimum, round 1 could be equated with any other state, which implies that there tend to be three different round-1 equilibria for any Semi-Grim continuation equilibrium. A conventional assumption in the literature has been $\sigma_{cc} = \sigma_{\emptyset}$, but as Table 3 shows, the null $\sigma_{cc} = \sigma_{\emptyset}$ is rejected in most treatments. Thus, we cannot pool first-round behavior and choices after (c, c) when we estimate strategies. This raises the question whether we can predict cooperation in round 1. In fact, the main difficulty in predicting repeated-game behavior seems to be the prediction of round-1 cooperation—the continuation strategies are largely constant.

Question 4: *Does cooperation in round 1 relate to the existence of Semi-Grim MPEs?*

The finding that subjects play Semi-Grim strategies would explain several results in the literature, in particular of DF and FRD. They both found that Tit-for-Tat (TFT) was not used systematically, which is compatible with $\sigma_{cd} \approx \sigma_{dc}$ and Semi-Grim. Similarly, $\sigma_{dd} \approx 0$ explains why Win-Stay-Lose-Shift (WSLS) had generally been insignificant in their analyses. Finally, FRD estimated that the population weights associated with particular strategies fluctuate across treatments, including strategies such as Tit-for-2-Tats (TF2T: play c unless partner played d in both of the last 2 rounds), 2-Tits-for-Tat (2TFT: play c unless partner played d in either of the last 2 rounds), Lenient Grim2 (play c until 2 consecutive rounds occur in which either player played d, then play d forever), and corresponding 3-memory strategies. This is compatible with Semi-Grim, which randomizes after mixed histories fairly uniformly in FRD's treatment, and then exactly these paths of play will materialize depending on how the chips fall.

III. Analysis of Individual Strategies

The analysis proceeds in two main steps, first a general classification of the individual strategies, and second an evaluation of the relation to Markov perfect equilibrium (MPE). Finally, I analyze how behavior depends on whether MPEs in Semi-Grim strategies exist.

As indicated, I follow BOS, DF, and FRD by focusing on the second halves of the experimental games per session, when learning supposedly stabilized.[12] In order to verify whether learning has stabilized indeed, I extend the analysis of Table 3 to allow for linear time trends in the cooperation rates. All details are provided in the online Appendix. The main results are that time trends in σ_{cc}, σ_{cd}, and σ_{dd} are never significant and that the time trend in σ_{dc} is significant in just 1 of the 16 treatments (treatment 6 of DF). Thus, continuation strategies indeed do not change much over time, and the standard assumption that each subject chooses a fixed strategy for (at least) the second halves of the experiment appears valid.[13] In turn, the time coefficient of $\sigma_{\emptyset}$ (round-1 cooperation) is significant in several treatments, and in the aggregate dataset of DF. This evolution of cooperation in round 1 has been analyzed in detail by Dal Bó and Fréchette (2011), and in an Appendix of Fudenberg, Rand, and Dreber (2012). My analysis of the Semi-Grim continuation strategies thus complements their findings on round-1 cooperation.

Briefly, let me also report on two checks of the validity of the focus on 1-memory histories. First, I estimate another set of models containing the 2-memory histories. This allows me to verify whether subjects systematically deviate from the 1-memory strategies after particular 2-memory histories. None of the 2-memory histories is of significant relevance in more than 2 of the 16 treatments, and even in conjunction, controlling for all 2-memories does not improve goodness-of-fit as measured by Bayes information criterion (BIC; details are provided as in the online Appendix). This suggests that the focus on 1-memory strategies is sufficient. Partially, though, the insignificance of 2-memory histories seems to be due to the fact that the number of observations for any particular 2-memory history is relatively small. For this reason, I additionally analyze whether the 1-memory strategies adapt to the opponents' percentages of cooperation in the first half of the session (data from the first half are not otherwise used in the analysis).[14] If the subjects' behavior would significantly adapt to their opponents' cooperativeness, which is hidden information, one could conclude that the focus on 1-memory strategies is invalid. This is not the case, though. Behavior significantly adapts to the opponent's cooperativeness in just 1 of the 16 treatments.[15] Therefore, I conclude that focusing on 1-memory strategies is appropriate in the sense that 1-memory strategies seem to capture most of the behavioral regularities in the datasets considered.

[12] To be precise, BOS analyzed approximately the last two-thirds of interactions in their sessions, while DF and FRD analyzed the last one-third of interactions. FRD mentioned their results are robust to alternatively using the last one-half of interactions. Thus, taking the second half of interactions per session is approximately the average assumption, which I apply consistently to all datasets.

[13] FRD also showed that strategy weights estimated under the alternative assumption that subjects pick strategies independently across interactions yielded qualitatively similar results for their data.

[14] This robustness check has been proposed by a reviewer.

[15] In turn, the own cooperativeness in the first half of the session significantly affects behavior (in the second half) in most treatments. This confirms the asserted subject heterogeneity (see footnote 10) and reaffirms the use of subject-level random effects and subject-level bootstrapping.

A. *Do Subjects Play Semi-Grim also Individually, Not Just on Average?*

First, I determine to which degree subjects use the strategies discussed in the literature, or Semi-Grim, without yet imposing an equilibrium constraint. To this end, I estimate the latent weights of six prototypical strategies, only one of which is Semi-Grim. The other five candidate strategies generalize the well-known strategies reviewed in Table 1, and in conjunction, they allow to reconstruct a Semi-Grim population without any individual playing a Semi-Grim strategy.

The six candidate strategies are as follows. First, a constant strategy $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (\alpha_1, \alpha_1, \alpha_1, \alpha_1)$, $\alpha_1 \in [0,1]$; it contains Always-Defect and Always-Cooperate as special cases. Second, a generalized Grim strategy $(1, \alpha_2, \alpha_2, \alpha_2)$; it contains Grim and Always Cooperate as special cases. Third, a generalized TFT strategy $(1, 0, \alpha_3, 0)$. Fourth, a generalized Win-Stay-Lose-Shift strategy $(1, 0, 0, \alpha_4)$. Fifth, a generalized cooperative strategy $(1, \alpha_5, 0, 0)$; it is behaviorally equivalent to "always cooperate" for $\alpha_5 = 1$, and in conjunction with strategies $(1, 0, \alpha_3, 0)$ and $(1, 0, 0, \alpha_4)$ it allows to reconstruct the Semi-Grim population. Sixth, Semi-Grim $(1, \alpha_6, \alpha_6, 0)$ itself. In order to be able to relate the identified strategies to strategies discussed in the literature, I restrict the cooperation probabilities $\alpha_3, \alpha_4, \alpha_5$ to be in $[0.5, 1]$. For example, the restriction $\alpha_3 \geq 0.5$ enforces that a strategy classified as generalized TFT has to be closer to TFT than to Grim in terms of say Euclidean distance. The restrictions on α_4 and α_5 serve similar purposes for generalized WSLS and the generalized cooperative strategy, respectively. Subject to these constraints, all strategy weights and parameters $(\alpha_1, \ldots, \alpha_6)$ are treated as free parameters and estimated by maximum likelihood, as described next.[16]

An efficient approach to estimate the population weights of the six strategies is finite mixture modeling (McLachlan and Peel 2000). In the experimental literature, it is standard practice since Stahl and Wilson (1994, 1995), and it was adopted by both DF and FRD. My analysis adopts this specification, including the standard assumption that all subjects are ex ante identical with respect to their probability distribution over strategies. I will find that most subjects indeed play Semi-Grim strategies. Partially, this may be the result of trembling-hand mistakes, however. A TFT player, for example, may cooperate with positive probability also after (c, d) due to trembling-hand mistakes. In order to avoid misclassifying such players as Semi-Grim, I explicitly allow for trembling-hand mistakes, similarly to DF and FRD.[17] Formally, let $\sigma_{s,s'} \in [0,1]$ denote a type's theoretical cooperation probability in state (s, s'). I assume subjects implement their strategy with a trembling hand following Selten (1975): the probability to cooperate in state (s, s') is bounded below by γ and above by $1 - \gamma$; it thus satisfies $\min\{1 - \gamma, \max\{\gamma, \sigma_{s,s'}\}\}$, where $\gamma \in (0,1)$ is constant across subjects. In addition, I assume that randomization is independent across subjects, interactions, and histories. This approach yields the

[16] In total, there are $3^4 = 81$ possible "prototypical" strategies $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd})$ where each cooperation probability $\sigma_{s,s'}$ is equal to either 0, 1, or some parameter α. I focus on the strategies discussed in the literature, which suggests they are the most plausible ones, and as hypothesized Semi-Grim.

[17] The online Appendix also contains a strategy classification assuming there is no noise (which is feasible, as the constant strategy is fully mixed). In relation to the results reported here, either the results without noise are similar or the no-noise model fits highly significantly worse. This suggests that a model specification with noise is indeed appropriate.

same estimates of the population weights as DF's and FRD's approach under their restriction to pure strategies, and it provides a straightforward generalization to mixed strategies.[18]

Following Biernacki, Celeux, and Govaert (2000), I identify strategies with insignificant weights using the ICL-BIC (integrated classification likelihood-Bayes information criterion). The weights of the remaining strategies are of interest in my analysis. Finally, all standard errors are bootstrapped. For each treatment, I obtain 1,000 bootstrap samples by randomly sampling the original number of subjects with replacement, and the standard errors are determined as standard deviation of the MLE estimates across these samples.[19] Appendix A provides all technical details.

The results, estimated strategy weights and parameters, are presented in Tables 4 and 5. First, let me summarize the results of Tables 4 and 5 by simply counting which components have majority weight in the various treatments. Note that in contrast to the analysis of cooperation rates presented in Table 3, which was feasible only in the 16 treatments with cooperation, individual strategies can be analyzed in all 17 treatments played in second halves of sessions.

RESULT 1 (Semi-Grim Strategies): *In* $14/17$ *treatments*, *the majority of subjects play Semi-Grim strategies*, *and in* $12/17$ *treatments*, *at least* 80 *percent of the subjects play Semi-Grim. It is the only strategy type that is detected in more than* 50 *percent of the treatments. The only other strategy that has more than* 10 *percent weight in more than* $3/17$ *treatments is Generalized Grim* $(1,\alpha,\alpha,\alpha)$, *which is borderline Semi-Grim.*

Subjects overwhelmingly play strategies with Semi-Grim structure. They do not play mixtures of say $(1,0,\alpha_3,0)$, $(1,0,0,\alpha_4)$, $(1,\alpha_5,0,0)$, which on average would yield something akin to Semi-Grim. With the exception of Treatment 6 in BOS, where subjects do virtually not cooperate, Semi-Grim has positive weight in all cases. In 12 of these 16 treatments, its weight is above 80 percent. In these cases, alternative strategies can be considered residual. All four treatments where the Semi-Grim weight is below 80 percent can be found in the DF dataset. This appears to reflect a mild cohort effect. Overall, however, the results suggest that subjects' behavior is well described by Semi-Grim strategies, even at the individual level. I verify the validity of this conclusion by two sets of robustness checks. Ex post, the most challenging alternative strategy appears to be $(\sigma_{cc},\sigma_{cd},\sigma_{dc},\sigma_{dd}) = (1,\alpha_1,\alpha_2,0)$, which I call Asymmetric Semi-Grim. This strategy nests Generalized TFT and Generalized Coop as special cases (for $\alpha_1 = 0$ and $\alpha_2 = 0$, respectively), which were found to be empirically relevant, and it nests symmetric Semi-Grim (for $\alpha_1 = \alpha_2$), the symmetry of which may therefore serve as the null

[18] An alternative approach would be to assume that types play their type strategy with probability $1-\gamma$, $\gamma \in (0,1)$, and that they randomize uniformly with probability γ (in each round, taking independent draws). I do not adopt this approach, because it slightly biases mixed strategies (which I consider undesirable when these strategies are supposed to form MPEs) and because it would introduce an interdependence between γ and $(\alpha_1,\ldots,\alpha_6)$ when the latter are free parameters (which weakens their separate identifiability). The results should be fairly similar, however, as the estimated noise level γ will be small in all treatments.

[19] The model parameters are the weights ρ_k of the significant components, the respective strategy parameters α_k, and the noise parameter γ. In almost all cases, the estimates are strictly in the interior of $[0,1]$, i.e., they are not close to the boundaries. Thus bootstrapping their standard errors is generally feasible. The exceptions concern 4 of the 86 standard errors in Tables 4 and 5, where caution is required.

Table 4—Estimated Weights (ρ) and Randomization Parameters (α) of Prototypical Strategies (σ_{cc}, σ_{cd}, σ_{dc}, σ_{dd})

Treat	γ	Constant $(\alpha,\alpha,\alpha,\alpha)$ Weight α	Gen. Grim $(1,\alpha,\alpha,\alpha)$ Weight α	Gen. Coop. $(1,\alpha,0,0)$ Weight α	Gen. TFT $(1,0,\alpha,0)$ Weight α	Gen. WSLS $(1,0,0,\alpha)$ Weight α	Semi-Grim $(1,\alpha,\alpha,0)$ Weight α	LL
Blonski, Ockenfels, and Spagnolo (2011)								
6	0.002	1	—	—	—	—	—	−15.3
	(0.001)	(−)						
		0.017	—	—	—	—	—	
		(0.014)						
2	0.005	—	—	—	—	—	1	−43.5
	(0.004)						(−)	
		—	—	—	—	—	0.3	
							(0.105)	
4	0	—	0.05	—	—	—	0.95	−19
	(0)		(0.048)				(−)	
		—	0.533	—	—	—	0.026	
			(0)				(0.026)	
7	0.004	—	—	—	—	—	1	−48
	(0.004)						(−)	
		—	—	—	—	—	0.387	
							(0.066)	
8	0.008	—	0.05	—	—	—	0.95	−74.8
	(0.006)	—	(0.049)				(−)	
			0.8	—	—	—	0.229	
			(0)				(0.06)	
5	0.013	0.1	—	—	—	—	0.9	−156.1
	(0.008)	(0.065)					(−)	
		0.219					0.314	
		(0.072)					(0.068)	
1	0.016	—	—	—	—	—	1	−99.2
	(0.012)						(−)	
		—	—	—	—	—	0.314	
							(0.081)	
9	0.01	—	—	—	—	—	1	−77
	(0.007)						(−)	
		—	—	—	—	—	0.186	
							(0.067)	
10	0.073	0.052	—	—	—	—	0.948	−180
	(0.028)	(0.06)					(−)	
		0.454	—	—	—	—	0.216	
		(0.035)					(0.072)	

Notes: Bootstrapped standard errors are provided in parentheses (in the four cases where $\alpha \approx 0$ is estimated, these standard errors are not guaranteed to be consistent). Irrelevant components are identified (and then eliminated, as indicated by "−") based on the ICL-BIC information criterion, as described in the Appendix. The right-most weight, usually that of the Semi-Grim component, is simply the difference of the remaining weights to 1. Thus, it is not a model parameter and is not assigned a standard error.

hypothesis in corresponding tests. I test the validity of symmetry in Semi-Grim in two ways, with respect to the primary component in the population and with respect to a possible secondary component, and the goodness-of-fit does not improve significantly by allowing for asymmetry in a single case.[20] In addition, I estimated

[20] I do so based on estimates of finite mixture models allowing for Asymmetric Semi-Grim *instead of* symmetric Semi-Grim and for Asymmetric Semi-Grim *in addition to* symmetric Semi-Grim. The first test allows me to verify

Table 5—Estimated Weights (ρ) and Randomization Parameters (α) of Prototypical Strategies (σ_{cc}, σ_{cd}, σ_{dc}, σ_{dd})

Treat	γ	Constant $(\alpha,\alpha,\alpha,\alpha)$ Weight α	Gen. Grim $(1,\alpha,\alpha,\alpha)$ Weight α	Gen. Coop $(1,\alpha,0,0)$ Weight α	Gen. TFT $(1,0,\alpha,0)$ Weight α	Gen. WSLS $(1,0,0,\alpha)$ Weight α	Semi-Grim $(1,\alpha,\alpha,0)$ Weight α	LL
Dal Bó and Fréchette (2011)								
1	0.006	—	0.045	—	0.069	—	0.885	−187
	(0.004)		(0.034)		(0.046)		(−)	
		—	0.553	—	0.997	—	0.276	
			(0.128)		(0.043)		(0.064)	
3	0.033	0.355	0.14	—	0.157	—	0.348	−513
	(0.018)	(0.076)	(0.048)		(0.063)		(−)	
		0.008	0.647	—	0.88	—	0.446	
		(0)	(0.071)		(0.186)		(0.081)	
2	0.027	—	0.325	—	0.141	—	0.533	−562.6
	(0.024)		(0.07)		(0.061)		(−)	
		—	0.005	—	0.952	—	0.508	
			(0)		(0.06)		(0.046)	
5	0.004	—	0.299	0.402	—	—	0.299	−337.3
	(0.006)		(0.1)	(0.109)			(−)	
			0.137	0.97	—	—	0.552	
			(0.092)	(0.246)			(0.114)	
4	0.026	—	0.226	0.146	—	—	0.628	−401.8
	(0.022)		(0.088)	(0.073)			(−)	
		—	0.703	0.873	—	—	0.358	
			(0.104)	(0.141)			(0.054)	
6	0.017	0.023	0.074	—	—	—	0.902	−322.2
	(0.007)	(0.026)	(0.078)				(−)	
		0.5	0.796	—	—	—	0.227	
		(0)	(0.122)				(0.044)	
Duffy and Ochs (2009), "random rematching" treatment								
	0.028	0.144	—	—	—	—	0.856	−815.7
	(0.023)	(0.053)					(−)	
		0.34	—	—	—	—	0.385	
		(0.048)					(0.032)	
Fudenberg, Rand, and Dreber (2012), "no-noise" treatment								
	0.021	0.083	—	—	—	—	0.917	−354.7
	(0.015)	(0.051)					(−)	
		0.419	—	—	—	—	0.434	
		(0.17)					(0.063)	

Notes: Bootstrapped standard errors are provided in parentheses (in the four cases where $\alpha \approx 0$ is estimated, these standard errors are not guaranteed to be consistent). Irrelevant components are identified (and then eliminated, as indicated by "−") based on the ICL-BIC information criterion, as described in the Appendix. The right-most weight, usually that of the Semi-Grim component, is simply the difference of the remaining weights to 1. Thus, it is not a model parameter and is not assigned a standard error.

finite mixtures of entirely unrestricted 1-memory strategies. The results concur.[21]

the null that the primary Semi-Grim component is symmetric ($\sigma_{cd} = \sigma_{dc}$) and the second test allows me to verify the same null for a possible secondary Semi-Grim component. These tests strongly support the symmetry $\sigma_{cd} = \sigma_{dc}$ in Semi-Grim in the sense that it is never rejected in favor of asymmetry. For details, let me refer to Tables 30 and 31 in the online Appendix.

[21] Details can be found in Table 21 in the online Appendix. In all treatments, the largest identified component (i.e., the strategy with the largest weight in the population) is a component where the Semi-Grim hypothesis $\sigma_{cc} > \sigma_{dc} = \sigma_{cd} > \sigma_{dd}$ is not rejected and overall, just two components significantly violate this structure, accounting for 9.2 percent and 14 percent of the subjects in the respective treatments.

In conjunction, I conclude that individual behavior is indeed best described by the symmetric Semi-Grim strategies.

B. *Are Semi-Grim Strategies Theoretically Compatible with Markov Perfect Equilibrium?*

The observation that subjects' behavior is well-captured by Semi-Grim strategies leads me to research question 2: are there Markov perfect equilibria in Semi-Grim strategies? And, to be addressed further below, how does behavior relate to these equilibria? Neither of these questions has an obvious answer, as mixed equilibria of repeated PDs are hardly ever explicitly constructed. The only example I have found is by Doraszelski and Escobar (2010, p.379), who construct an equilibrium of the form $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (\alpha, 0, 0, 0)$.

It can be shown straightforwardly, however, that a plethora of MPEs satisfying the Semi-Grim condition $\sigma_{cc} > \sigma_{cd} = \sigma_{dc} > \sigma_{dd}$ exists if δ is sufficiently large. There are up to two Semi-Grim MPEs that are regular (i.e., generically purifiable) in the sense of Doraszelski and Escobar (2010) and a continuum of belief-free MPEs satisfying the Semi-Grim condition. This continuum exists if and only if $\delta \geq \delta^*$, i.e., these equilibria indeed provide a strategic interpretation of the BOS-threshold. The continuum consists of all strategy profiles satisfying (using the notation of panel B of Figure 1)

$$\sigma_{cc} = \frac{(b\delta - b + a - 1)r + b - a + 1}{b\delta} \tag{2}$$

$$\sigma_{cd} = \sigma_{dc} = \frac{(b\delta - b + a - 1)r + 1}{b\delta}$$

$$\sigma_{dd} = \frac{(b\delta - b + a - 1)r}{b\delta}$$

for some $r \in [0, 1]$. To restrict the degrees of freedom used in the subsequent analysis, I focus on the Semi-Grim MPEs that are obtained if players pick equilibria satisfying two of the three best-known selection criteria: efficiency ($\sigma_{cc} = 1$), robustness to (extreme-value) utility perturbations (RUP), and robustness to imperfect monitoring (RIM). At the threshold $\delta = \delta^*$, an MPE satisfying all three criteria exists—it is unique and in Semi-Grim strategies.[22] As δ increases, the equilibrium splits up into three distinctive Semi-Grim equilibria, one of which is efficient and RIM, another one is efficient and RUP, and the third one is RIM and RUP.

[22] Bhaskar, Mailath, and Morris (2008) show that no MPE satisfies RIM and generic RUP, i.e., no belief-free MPE is robust to generic payoff perturbations. There are belief-free MPEs that are robust to payoff perturbations with given distributions, however. I focus on the belief-free MPEs that are robust to perturbations with extreme-value distributions. These MPEs are known as limiting *Markov logit equilibria* (MLE), where the limit is taken as noise approaches zero. Following McKelvey and Palfrey (1995), successful applications of logit equilibria in general include the centipede game (Fey, McKelvey, and Palfrey 1996); the traveler's dilemma (Capra et al. 1999); public goods games (Goeree, Holt, and Laury 2002); auctions (Goeree, Holt, and Palfrey 2002); monotone contribution games (Choi, Gale, and Kariv 2008); and beauty contests (Breitmoser 2012). Markov logit equilibria are defined in Breitmoser, Tan, and Zizzo (2010): further details are provided in Breitmoser (2013).

The Semi-Grim MPE that is efficient and RIM obtains for $r = 1$ in equation (2) and predicts

$$(3)\quad \sigma_{cc} = 1 \qquad \sigma_{cd} = \sigma_{dc} = \frac{b\delta - b + a}{b\delta} \qquad \sigma_{dd} = \frac{b\delta - b + a - 1}{b\delta}.$$

I refer to this equilibrium as the *efficient symmetric belief-free* MPE (or Eff Symm BF for short). Table 6 presents the respective equilibrium strategies in the experimental treatments. The component-wise median of these equilibrium strategies across treatments is $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1, 0.66, 0.66, 0.2)$. Players following this strategy are comparably forgiving and cooperate with high probability even after defections. In this way, they account for the (infinitesimal) possibility that imperfect monitoring (e.g., an attention lapse) has caused the defection.

The MPE satisfying RIM and RUP, to which I shall refer as *limiting-logit*, *belief-free* (LimLog BF) MPE, predicts strategies as in equation (2) with r solving

$$(4)\quad \left(\frac{\big((-b\delta + b - a + 1)r + b\delta\big)\big((b\delta - b + a - 1)r + 1\big)}{(b\delta - b + a - 1)r\big((-b\delta + b - a + 1)r + b\delta - 1\big)}\right)^{b-a}$$

$$= \frac{\big((b\delta - b + a - 1)r + b - a + 1\big)\big((b\delta - b + a - 1)r - b\delta + 1\big)}{(b\delta - b + a - 1)(r - 1)\big((b\delta - b + a - 1)r + 1\big)}.$$

For example, in case $b = a + 1$, the limiting-logit, belief-free MPE obtains for $r = 1/2$. Table 6 presents the predictions across treatments, again. In this case, the component-wise median across treatments is approximately $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (0.9, 0.5, 0.5, 0.1)$. Players following this strategy are also rather forgiving but trigger conflict with positive probability even after mutual cooperation.

The MPE that is efficient and RUP shall be called *efficient limiting-logit Semi-Grim* (Eff LimLog SG). It is a regular MPE[23] and predicts $\sigma_{cc} = 1$, $\sigma_{dd} = 0$, and

$$(5)\quad \sigma_{cd} = \sigma_{dc}$$
$$= \frac{\sqrt{2a(2\delta^2 - 4\delta + 1) + b^2(\delta - 1)^4 - 2ab(\delta - 1)^2 - 2b(\delta - 1)^2 + a^2 + 1} + b\delta^2 + (2 - 2b)\delta + b - a - 1}{2b\delta^2 + (-2b - 2a + 2)\delta}.$$

Table 6 again presents the predictions across treatments. The component-wise median across treatments is now $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1, 0.25, 0.25, 0)$. It characterizes players who are efficient but comparably unforgiving. The equilibrium exists under slightly weaker conditions than the belief-free Semi-Grim MPEs, but in all four experiments considered here, there is just one treatment (BOS treatment 7) where this limiting-logit Semi-Grim MPE exists and the belief-free Semi-Grim MPEs do not. I do therefore not analyze this subtle difference.

[23] The second regular Semi-Grim MPE exists only for high δ and is thus not relevant in this analysis.

TABLE 6—THE MIXED MPEs CONSIDERED IN THE CLASSIFICATION ANALYSIS
(*In addition, A-Def, Grim, and TFT are considered*)

	Mixed MPEs (σ_{cc}, σ_{cd}, σ_{dc}, σ_{dd})				
Treat	LimLog BF	Eff Symm BF	Eff LimLog SG	Asymm BF	W-WSLS
Blonski, Ockenfels, and Spagnolo (2011)					
6					(**1, 0, 0, 1**)
3					
2					
7	(1, 0.87, 0.87, 0)	(1, 0.88, 0.88, 0)	(**1, 0.67, 0.67, 0**)	(1, 0.86, 1, 0)	(**1, 0, 0, 0.84**)
4	(1, 0.5, 0.5, 0)	(1, 0.5, 0.5, 0)	(1, 0.5, 0.5, 0)	(**1, 0.33, 0.67, 0**)	
8	(**0.92, 0.8, 0.8, 0**)	(**1, 0.89, 0.89, 0.09**)	(**1, 0.33, 0.33, 0**)	(**1, 0.93, 0.5, 0**)	(**1, 0, 0, 0.57**)
5	(**0.96, 0.5, 0.5, 0.04**)	(**1, 0.54, 0.54, 0.09**)	(**1, 0.23, 0.23, 0**)	(**1, 0.71, 0.29, 0**)	
9	(**0.93, 0.59, 0.59, 0.04**)	(**1, 0.67, 0.67, 0.11**)	(**1, 0.3, 0.3, 0**)	(**1, 0.75, 0.42, 0**)	(1, 0, 0, 1)
1	(**0.94, 0.5, 0.5, 0.06**)	(**1, 0.56, 0.56, 0.11**)	(**1, 0.26, 0.26, 0**)	(**1, 0.67, 0.33, 0**)	
10	(**0.9, 0.43, 0.43, 0.14**)	(**1, 0.52, 0.52, 0.24**)	(**1, 0.13, 0.13, 0**)	(**1, 0.72, 0.17, 0**)	(1, 0, 0, 0.61)
Dal Bó and Fréchette (2011)					
1					
3	(1, 0.57, 0.57, 0)	(1, 0.57, 0.57, 0)	(1, 0.53, 0.53, 0)	(**1, 0.33, 0.87, 0**)	
2	(1, 0.42, 0.42, 0)	(1, 0.42, 0.42, 0)	(1, 0.46, 0.46, 0)	(**1, 0.14, 0.62, 0**)	
5	(**0.83, 0.73, 0.73, 0.04**)	(**1, 0.89, 0.89, 0.21**)	(**1, 0.33, 0.33, 0**)	(**1, 0.91, 0.57, 0**)	(**1, 0, 0, 0.54**)
4	(**0.89, 0.54, 0.54, 0.09**)	(**1, 0.65, 0.65, 0.19**)	(**1, 0.21, 0.21, 0**)	(**1, 0.78, 0.29, 0**)	(**1, 0, 0, 0.7**)
6	(**0.71, 0.64, 0.64, 0.18**)	(**1, 0.93, 0.93, 0.47**)	(**1, 0.14, 0.14, 0**)	(**1, 0.97, 0.19, 0**)	(**1, 0, 0, 0.23**)
Duffy and Ochs (2009)					
	(**0.87, 0.5, 0.5, 0.13**)	(**1, 0.63, 0.63, 0.26**)	(**1, 0.09, 0.09, 0**)	(**1, 0.89, 0.11, 0**)	(**1, 0, 0, 0.2**)
Fudenberg, Rand, and Dreber (2012)					
	(**0.73, 0.5, 0.5, 0.27**)	(**1, 0.77, 0.77, 0.54**)	(**1, 0.04, 0.04, 0**)	(**1, 0.95, 0.05, 0**)	(**1, 0, 0, 0.06**)
Median strategy					
	(0.90, 0.52, 0.52, 0.06)	(1, 0.66, 0.66, 0.20)	(1, 0.23, 0.23, 0)	(1, 0.75, 0.33, 0)	(1, 0, 0, 0.54)

Notes: The strategy components are reported in the order (σ_{cc}, σ_{cd}, σ_{dc}, σ_{dd}) and are computed using equations (2)–(7). An entry set in boldface type indicates that the respective MPE exists for the treatment parameters, it is set in plain type when it would exist for some $\delta' \leq \delta^{2/3}$, and an empty cell indicates that the MPE does not exist for the stage game payoffs and any discount factor $\delta' \leq \delta^{2/3}$. LimLog BF is the limiting-logit, belief-free MPE characterized in equations (2), (4), the Eff Symm BF is the efficient symmetric belief-free MPE in equation (3), Eff LimLog *SG* is the efficient limiting-logit Semi-Grim MPE in equation (5), Asymm BF is the asymmetric belief-free MPE in equation (7), and W-WSLS is the Weak WSLS MPE in equation (6).

Median strategy: For each mixed equilibrium, a median strategy (σ^m_{cc}; σ^m_{cd}; σ^m_{dc}; σ^m_{dd}) is determined. Focusing on the treatments where the equilibrium exists (i.e., the entries set in boldface type), the first component σ^m_{cc} is the median of all σ_{cc} across treatments, the second component σ^m_{cd} is the median of all σ_{cd} across treatments, and so on.

C. *Are Semi-Grim Strategies as Played by Subjects Compatible with Markov Perfect Equilibrium?*

These three Semi-Grim MPEs make very specific predictions across treatments. Thus, it is not obvious that a majority of subjects will again be estimated to play Semi-Grim strategies after accounting for the equilibrium constraint. In the subsequent analysis of the weights of equilibrium strategies, I assume that subjects play strategies that are best responses to themselves. Subjects are not aware of, or do not account for, the potential plurality of types. This is a reasonably simple assumption on how subjects may reduce the strategic uncertainty and it relates to the false-consensus effect (Ross, Greene, and House 1977) which is well-established in both psychology (Mullen et al. 1985) and experimental economics (e.g., Blanco, Engelmann, and Normann 2011).[24]

The strategy weights are estimated by finite mixture modeling with trembling-hand errors as above (see Appendix A), but the prototypical strategies considered above are replaced by their MPE counterparts. In addition to Semi-Grim, I consider the Always-Defect equilibrium $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (0,0,0,0)$ and four standard MPEs that are efficient $(\sigma_{cc} = 1)$ but not Semi-Grim. Their conjunction will again allow to reconstruct a Semi-Grim population without any individual playing Semi-Grim.

The four efficient MPEs that are not Semi-Grim are Grim, $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1,0,0,0)$, and three equilibrium strategies related most closely to the prototypical strategies considered before: (i) Tit-for-Tat $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1,0,1,0)$, which of course is not an MPE but included as it represents the conventional notion of reciprocity most explicitly; (ii) Weak Win-Stay-Lose-Shift (*W-WSLS*), which is an MPE of the form $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1,0,0,\alpha)$, namely with[25]

$$(6)\quad \sigma_{dd} = \frac{b\delta^2 - 2\delta - b + a + 1 - \sqrt{b^2\delta^4 - 2\left(b^2 - ab + b + 2a - 2\right)\delta^2 + 4\left(b - 1\right)\delta + \left(b - a - 1\right)^2}}{2\delta(b\delta^2 - a)};$$

and (iii) the unique belief-free MPE satisfying $\sigma_{cc} = 1$ and $\sigma_{dd} = 0$. I refer to it as *asymmetric belief-free* MPE, and it is characterized as

$$(7)\quad \sigma_{cc} = 1 \qquad \sigma_{cd} = \frac{(b-1)\delta - b + a}{(a-1)\delta} \qquad \sigma_{dc} = \frac{1-\delta}{(a-1)\delta} \qquad \sigma_{dd} = 0.$$

The asymmetric belief-free MPE is particularly interesting, as it maximizes the difference $\sigma_{cd} - \sigma_{dc}$ subject to being an MPE.[26] It is therefore most suitable as counterpart to TFT in reconstructing the Semi-Grim population without any individual playing Semi-Grim. Since $\sigma_{cc} = 1$ and $\sigma_{dd} = 0$ holds true as well, it also fits the remaining observations on individual behavior. The strategies predicted by all mixed MPEs are provided in Table 6.

As for the analytical set up, one issue is left to be resolved: every subject is associated with an equilibrium strategy, but which strategy should a subject use if the equilibrium corresponding with his type does not exist? This is trivial for strategies such as Grim, which invariantly predicts $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1,0,0,0)$. Subjects may play Grim regardless of whether it is an equilibrium strategy or not. As for mixed MPEs, however, the respective formulae such as equation (3) do usually not

[24] It also relates to the observation reported in footnote 11, namely that some of the subjects seem to have wrong beliefs about their opponents' continuation strategies.

[25] This Weak WSLS strategy is a more promising candidate strategy than the pure WSLS $(1,0,0,1)$ for two reasons. First, pure WSLS is an MPE only if one round of punishment suffices to retaliate unilateral deviations, which is the case only in few treatments. Secondly, pure WSLS attracted virtually no weight in the analyses of Dal Bó and Fréchette (2011) and Fudenberg, Rand, and Dreber (2012). Thus, replacing it by the less extreme Weak WSLS strategy improves its chances of attracting weight.

[26] All (symmetric) MPEs where the players randomize with different probabilities in the states (c,d) and (d,c) must be belief-free, and of those, the asymmetric belief-free MPE described in equation (7) maximizes $\sigma_{cd} - \sigma_{dc}$. Further details are provided in Breitmoser (2013).

yield probabilities (i.e., values between 0 and 1) for treatment parameters where the MPE does not exist. Thus, in the case of mixed MPEs, subjects simply cannot play the equilibrium strategy associated with their type if it is not an MPE for the treatment parameters in question.

One solution would be to set the weights of mixed equilibria to zero if the equilibrium in question does not exist. For example, the limiting-logit, belief-free equilibrium exists if $\delta \geq \delta^*$. We will see that subjects indeed play Semi-Grim strategies if $\delta \geq \delta^*$. Thus, setting the Semi-Grim weights to zero whenever $\delta < \delta^*$ would effectively assume the result that subjects start playing Semi-Grim at $\delta = \delta^*$. In order to investigate when subjects actually start playing strategies related to Semi-Grim MPEs, I therefore relax the existence conditions and allow subjects to pick their MPE even if it does not exist for the δ in question, as long as it exists for some $\delta' \leq \delta^{2/3}$. In these cases, I use the mixed MPE at the threshold of existence.[27] In addition to allowing me to analyze the transition to Semi-Grim MPEs more effectively, this approach levels the playing field between the various MPEs and TFT (which is never an MPE but always considered) and most of all W-WSLS, which otherwise exists in the smallest number of treatments. The results are robust to varying the threshold $\delta^{2/3}$, as I show in the online Appendix.

Tables 7 and 8 presents the estimated weights. Accounting for the noise in experimental data, there is a surprisingly stable overall pattern. If δ is far below the threshold, roughly if $\delta - \delta^* < -0.2$, subjects do not cooperate and play always defect. Around the threshold, roughly if $-0.2 \leq \delta - \delta^* \leq 0.1$, subjects switch to Grim and Semi-Grim strategies, with Semi-Grim MPEs attracting around 50 percent in aggregate. The particular distribution of weight between the Semi-Grim MPEs is rather uninformative if $\delta \approx \delta^*$, since the three Semi-Grim MPEs are quantitatively fairly similar around the threshold (as shown in Table 6). Above the threshold, roughly if $\delta - \delta^* > 0.1$, the subjects have switched (almost) completely to Semi-Grim MPEs and distribute fairly evenly across the three Semi-Grim MPEs. These are the treatments BOS-10, DF-4 and 6, as well as DO and FRD. Thus, I conclude that individual behavior is compatible with MPEs in Semi-Grim strategies.

D. *How Does Behavior Relate to the Existence of Semi-Grim MPEs?*

In this subsection, I address the related research questions 3 and 4. The previous subsection shows that if δ is above the BOS-threshold δ^*, where subjects have to pick two of the three selection criteria, they consistently sort into either of the three cases. This is plausible, as the different selection criteria characterize players with different personalities. The respective equilibria are more or less forgiving and lenient, and they also differ with respect to the tendency that players willfully trigger conflict. Alternative MPEs are only residual if $\delta > \delta^*$. In particular, Weak WSLS and the asymmetric belief-free MPE never attract any weight, and TFT is used only unsystematically (in 5/17 treatments), never attracting more than one-third of the weight.

[27] Allowing for $\delta' \leq \delta^{2/3}$ corresponds with shortening the period length up to 33 percent and increasing the discount factor appropriately.

Table 7—Estimated Weights of Equilibrium Strategies

Treat	$\delta - \delta^*$	A-Def	Grim	TFT LimLog BF	W-WSLS Eff Symm BF	Asymm BF Eff LimLog SG	γ	LL
Blonski, Ockenfels, and Spagnolo (2011)								
6	−0.3	1 (−)	—	—	—	NA		
				NA	NA	NA	0.02 (0.014)	−15.3
2	−0.15	—	0.671 (0.141)	0.329 (−)	NA	NA		
				NA	NA	NA	0.015 (0.007)	−59.6
4	−0.05	—	1 (−)	—	NA	—		
				—	—	—	0.02 (0.021)	−42.6
7	−0.05	—	—	—	—	—		
				—	—	1 (−)	0 (0.004)	−58.1
8	0.075	—	—	—	—	—		
				—	0.064 (0.061)	0.936 (−)	0.008 (0.006)	−77.4
5	0.075	—	—	—	NA	—		
				0.41 (0.115)	—	0.59 (−)	0 (0.008)	−155
1	0.083	—	0.378 (0.163)	—	NA	—		
				0.622 (−)	—	—	0 (0.002)	−97
9	0.083	—	—	—	—	—		
				—	—	1 (−)	0.01 (0.005)	−80.4
10	0.179	—	0.622 (0.145)	—	—	—		
				0.378 (−)	—	—	0.064 (0.033)	−184.8

Notes: The mixed MPEs are as described in Table 6. Bootstrapped standard errors are provided in parentheses. There are two rows of estimates per treatment. "NA" indicates that the respective MPE does not exist even after inflating δ up to $\delta^{2/3}$. "–" indicates that the MPE exists but attracts insignificant weight according to ICL-BIC. Since the right-most weight is not a parameter but a (usually sizable) residual, it is not assigned a standard error. The econometric procedure is described in the Appendix.

Figure 2 illustrates how the strategy weights relate to the difference $\delta - \delta^*$ and how they relate to the probability of observing cooperation in round 1. Panel A of Figure 2 shows that subjects switch to Semi-Grim equilibria almost exactly as δ crosses the threshold δ^*. Below the threshold, the total weight of Semi-Grim strategies is around 0, at the threshold it is around 50 percent, and above the threshold it is near 100 percent. The coefficients of the respective logistic model, provided in Table 9, show that the intercept is insignificant. That is, the hypothesis that the 50 percent line is reached exactly at $\delta = \delta^*$ is not rejected. The plot in relation to cooperation in round 1 is even more illustrative. Subjects switch to Semi-Grim almost exactly as they start cooperating in round 1, both visually in panel B of

Table 8—Estimated Weights of Equilibrium Strategies

Treat	$\delta - \delta^*$	A-Def	Grim	TFT LimLog BF	W-WSLS Eff Symm BF	Asymm BF Eff LimLog SG	γ	LL
Dal Bó and Fréchette (2011)								
1	−0.316	1 (−)	NA	NA	NA	NA		
				NA	NA	NA	0.1 (0.04)	−487.1
3	−0.105	—	0.458 (0.074)	0.196 (0.069)	NA	—		
				0.346 (−)	—	—	0.076 (0.023)	−604.5
2	−0.066	—	0.315 (0.073)	0.139 (0.063)	NA	—		
				—	—	0.546 (−)	0.027 (0.024)	−564.3
5	0.105	—	0.448 (0.078)	—	—	—		
				—	0.15 (0.058)	0.403 (−)	0.01 (0.009)	−337.9
4	0.145	—	—	—	—	—		
				0.199 (0.079)	0.415 (0.104)	0.386 (−)	0.006 (0.005)	−398.9
6	0.355	—	—	—	—	—		
				0.092 (0.045)		0.908 (−)	0.012 (0.006)	−324.2
Duffy and Ochs (2009), "random rematching" treatment								
	0.233	—	—	0.095 (0.049)	—	—		
				0.312 (0.076)	0.298 (0.081)	0.296 (−)	0 (0.003)	−822.2
Fudenberg, Rand, and Dreber (2012), "no-noise" treatment								
	0.475	—	—	0.137 (0.067)	—	—		
				0.165 (0.055)	0.423 (0.081)	0.275 (−)	0.005 (0.004)	−334.3

Notes: The mixed MPEs are as described in Table 6. Bootstrapped standard errors are provided in parentheses. There are two rows of estimates per treatment. "NA" indicates that the respective MPE does not exist even after inflating δ up to $\delta^{2/3}$. "–" indicates that the MPE exists but attracts insignificant weight according to ICL-BIC. Since the right-most weight is not a parameter but a (usually sizable) residual, it is not assigned a standard error. The econometric procedure is described in the Appendix.

Figure 2 and econometrically as the relationship is virtually linear and the intercept is insignificant again (see Table 9). I summarize these observations as follows.

RESULT 2 (Semi-Grim MPEs): *There is a stable pattern across experiments that subjects switch to Semi-Grim strategies as they start cooperating in round 1, and both occur at the threshold* $\delta = \delta^*$ *where the Semi-Grim equilibria appear.*

IV. Discussion

The purpose of the paper was to analyze individual strategies in the repeated PD and to help explain the observation of Blonski, Ockenfels, and Spagnolo

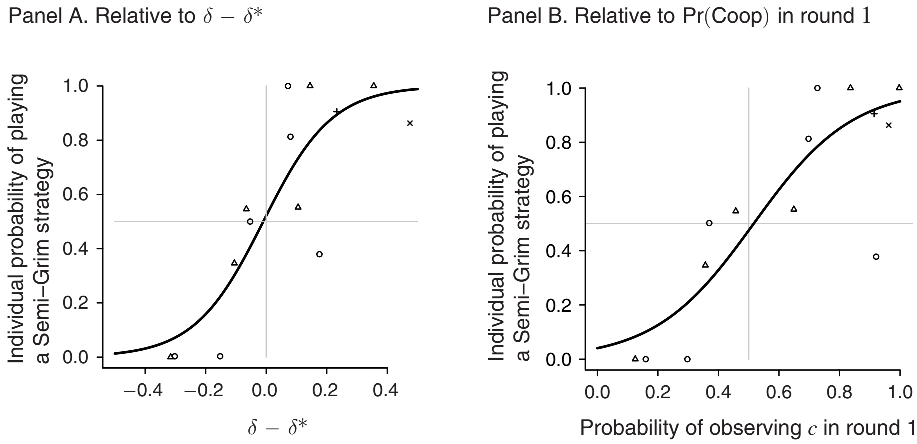

FIGURE 2. THE AGGREGATE SHARE OF SUBJECTS PLAYING SEMI-GRIM MPEs

TABLE 9—LOGISTIC REGRESSION OF PR(*Semi-Grim*) ON $\delta - \delta^*$ AND COOPERATION

	Intercept	Coefficient
Pr(Semi-Grim) | $\delta - \delta^*$	0.085	8.78*
	(0.289)	(2.034)
Pr(Semi-Grim) | Pr(Coop)	−0.101	6.13*
	(0.301)	(1.377)

(2011) and Dal Bó and Fréchette (2011). They observed that subjects start to cooperate when the discount rate δ reaches the seemingly abstract BOS-threshold $\delta^* = (p_{dc} + p_{dd} - p_{cd} - p_{cc})/(p_{dc} - p_{cd})$. The point of departure was the novel observation that the average strategies in most treatments of four recent experiments are mixed and exhibit a 1-memory Semi-Grim pattern: $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) \approx (1, \alpha, \alpha, 0)$ with $\alpha \in [0.2, 0.5]$.

My results suggest an explanation for both the Semi-Grim pattern and the predictiveness of the BOS-threshold. The main results are that Markov perfect equilibria in Semi-Grim strategies exist once the discount rate exceeds the BOS-threshold δ^*, and that subjects start to cooperate in round 1 and individually switch to Semi-Grim MPEs at this threshold. Three cases can be distinguished here. If the discount factor is far below the threshold δ^*, players hardly cooperate in round 1, and Semi-Grim thus predicts rare cooperation in subsequent rounds. In this case, subjects are most parsimoniously categorized as playing Always Defect. Around the threshold δ^*, the population is estimated to be a mixture of Grim and Semi-Grim MPEs, which have the structure $(\sigma_{cc}, \sigma_{cd}, \sigma_{dc}, \sigma_{dd}) = (1, 0, 0, 0)$ and $(1, \alpha, \alpha, 0)$, respectively. This yields a Semi-Grim structure $(1, \alpha', \alpha', 0)$ on average, just as observed. Above the threshold, most subjects play consistent with Semi-Grim MPEs, and they distribute fairly evenly across the three Semi-Grim MPEs associated with standard selection criteria (efficiency, RIM, and RUP). Their mixture of course yields Semi-Grim on average, again. Thus, it is not a single selection principle, but a mixture of three, and if the respective predictions are sufficiently different (above the threshold δ^*), subjects distribute evenly in all four experiments.

The Semi-Grim equilibria explain that individual choices may sometimes be lenient (retaliation only after two defections) or forgiving (Tit-for-Two-Tats, Two-Tits-for-Tat), as observed by Fudenberg, Rand, and Dreber (2012). As subjects cooperate with symmetric, intermediate probabilities $\alpha \in [0.2, 0.5]$ after mixed 1-memory histories, (c, d) and (d, c), they do not punish always nor do they punish for the same number of periods in all cases. Subjects appear to randomize and ex post, this yields lenient or forgiving behavior. Some subjects appear to randomize even after mutual cooperation, and in this sense, they willfully trigger conflict. This concurs with the verbal descriptions given by subjects in Fudenberg, Rand, and Dreber (2012) and suggests to relax the common focus on trigger strategies. The refinement concepts considered here induce differing degrees of leniency and willingness to trigger conflict and thus they relate to these personality characteristics in an intuitive way.

We observed that behavior is rather cooperative and efficient if the time horizon is sufficiently long (i.e., $\sigma_{\emptyset}, \sigma_{cc} \approx 1$ if δ is large). In long-run partnerships, potential explanations are that the choice of long-run partners is endogenous, that partners may fall in love (i.e., adopt highly altruistic preferences), or that social norms guide behavior. The first two explanations can be ruled out in laboratory experiments, and due to the anonymity of the laboratory interactions, the third explanation appears less appealing than it would be outside the laboratory. This suggests that strategic aspects such as equilibrium selection and refinement actually shape behavior more explicitly than perhaps previously hypothesized, which concurs with the observation that refinement concepts actually predict Semi-Grim strategies. These refinement requirements are robustness to imperfect monitoring and robustness to extreme-value utility perturbations, but further research is required to better understand the generality of their joint predictiveness.

The absence of direct reciprocity, $\sigma_{cd} = \sigma_{dc}$, marks a departure from the literature following Axelrod (1980a,b). This literature emphasizes the theoretical effectiveness of strategies such as Tit-for-Tat (TFT) and Win-Stay-Lose-Shift (WSLS) in evolutionary tournaments (Nowak and Sigmund 1993; Imhof, Fudenberg, and Nowak 2007). The initial inspection of the average choices, which revealed $\sigma_{cd} = \sigma_{dc}$ as well as $\sigma_{dd} \leq 0.1$, suggested that neither TFT nor WSLS would have substantial weight, and the analysis of individual strategies confirmed this impression. This observation accompanies Press and Dyson (2012), which recently showed that the "zero-determinant" strategies generalize TFT in that they are unbeatable by any opponent strategy (see also Duersch, Oechssler, and Schipper 2014). These zero-determinant strategies contain the belief-free equilibria (Ely, Hörner, and Olszewski 2005) as special cases. The finding that belief-free equilibria, in turn, help explain both the Semi-Grim pattern across experiments and the BOS-threshold thus connects several recent experimental and theoretical results on the repeated PD. Since belief-free equilibria exist in general repeated games, such results may be obtained in more general games as well. This appears to be a promising avenue for further research.

Appendix: Econometric Analysis of Individual Behavior

My approach adapts Dal Bó and Fréchette (2011) to enable the inclusion of mixed equilibria, but it is otherwise similar. Assuming $\sigma_{s,s'} \in [0,1]$ denotes a player's theoretical cooperation probability in a given state, the basic assumption is that the player cooperates with probability $\min\{1-\gamma, \max\{\gamma, \sigma_{s,s'}\}\}$, where $\gamma \in (0,1)$ is a free parameter. Randomization is independent across subjects, interactions, and histories. The population is a mixture of a finite number of components $k \in \mathcal{K}$, and each component k characterizes a strategy $\boldsymbol{\sigma}(k)$. Here, $\sigma_\omega(k)$ denotes the (perturbed) probability that members of component k cooperate in state $\omega \in \{cc, cd, dc, dd\}$, $\rho(k)$ denotes the weight of component k in the population, and $o_{s,t} \in \{0,1\}$ and $\omega_{s,t}$ denote choice and state of the decision number t of subject $s \in \mathrm{S}$, respectively ($o_{s,t} = 1$ denotes cooperation and $o_{s,t} = 0$ denotes defection). The log-likelihood of the model $\langle \boldsymbol{\sigma}, \rho, \mathcal{K} \rangle$ is

$$LL = \sum_{s \in \mathcal{S}} \log \sum_{k \in \mathcal{K}} \rho(k)\, L(s,k) \quad \text{with } L(s,k) = \prod_t \left(\sigma_{\omega_{s,t}}(k)\right)^{o_{s,t}} \cdot \left(1 - \sigma_{\omega_{s,t}}(k)\right)^{1-o_{s,t}}.$$

The likelihood is maximized jointly over all parameters $(\boldsymbol{\sigma}, \rho)$, first using the robust, gradient-free NEWUOA algorithm (Powell 2006) and secondly, verifying convergence using a Newton-Raphson algorithm. Standard errors are bootstrapped using 1,000 bootstrap samples obtained by randomly sampling the original number of subjects with replacement.

The model dimensionality $\mathcal{K}$ is estimated using ICL-BIC (integrated classification likelihood-Bayes information criterion), which is an entropy-based generalization of Bayes information criterion appropriate to discriminate finite-mixture models (Biernacki, Celeux, and Govaert 2000). Models with poorly distinguished components have high entropy, and in such cases, ICL-BIC recommends to eliminate a component. It is defined as

$$\text{(A1)} \qquad \textit{ICL-BIC} = -LL + D/2 \cdot \ln n + \mathrm{En}(\hat{\tau})$$

$$\text{with } \mathrm{En}(\hat{\tau}) = -\sum_{s \in \mathcal{S}} \sum_{k \in \mathcal{K}} \hat{\tau}_{sk} \ln \hat{\tau}_{sk} \quad \text{with} \quad \hat{\tau}_{sk} = \frac{\rho_k L(s,k)}{\sum_{k' \in \mathcal{K}} \rho_{k'} L(s,k')},$$

with n as number of subjects and D as number of parameters. Note that $\hat{\tau}_{sk}$ is the posterior probability that subject s belongs to component k. The precise procedure to estimate $\mathcal{K}$ is as follows. I start with the complete model containing all components and estimate all nested models where exactly one component is eliminated. I rank all the nested models according to their log-likelihood (LL) and start with the model with the lowest LL. If eliminating the respective component improves ICL-BIC, it is eliminated and the procedure restarts with the correspondingly reduced number of components. Otherwise, the next-ranked component is considered, and so on. The procedure stops if no component can be eliminated. Details on the intermediate outcomes and on the elimination order are provided as supplementary material.

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