Title
Expressive Law: Framing or Equilibrium Selection?

Authors
Bohnet, Iris
Cooter, Robert

Publication Date
2001-03-09

Expressive Law: Framing or Equilibrium Selection?

Iris Bohnet* and Robert Cooter**

Second Draft, 9 March, 2001

Abstract

Law deters, educates, and coordinates. Economists typically focus on deterrence and sociologists often focus on education. Since deterrence causes marginal changes and education confronts relatively stable preferences, economists and sociologists may overlook law's largest effects on behavior. We hypothesize that law's largest effects come from coordination. Normative systems have multiple equilibria, so announcing a new law can change expectations and cause behavior to jump from one equilibrium to another. To demonstrate this possibility, we ran games with inter-dependent payoffs in which we simulated a law by telling experimental groups that choosing Left will result in a "penalty" ten percent of the time. We set the penalty too small to change the equilibria among rationally self-interested actors, thus eliminating deterrence effects. First we ran a PD game in which the penalty does not affect the uniquely dominant strategy of each player. In these circumstances, any change in behavior caused by announcing the penalty must result from more people acting morally and renouncing self-interest. In fact, announcing the penalty did not change the number of moral actors. Next we repeated the experiment in a crowding game with a uniquely dominant strategy for each player and obtained the same results as in the PD game. Finally, we ran a coordination game with multiple equilibria. Announcing the penalty caused jumps in behavior from one equilibrium to another. The power of the penalty to coordinate increased as the proportion of actors decreased whose cooperation was needed to tip the system to a Pareto-superior equilibrium.

Keywords: Coordination, prisoner's dilemma, equilibrium selection, framing, law and economics.

---

* Kennedy School of Government, Harvard University, 79 JFK Street, Cambridge, MA02138, email: Iris_Bohnet@Harvard.edu

** Boalt Law School, University of California at Berkeley, Berkeley, CA94720, email: rdc@law.berkeley.edu

We thank Dorothea Kuebler for her help in conducting the experiments and the participants of the 1999 American Law and Economics Association meetings in New Haven, the 2000 Economic Science Association meetings in Tucson and seminars at Berkeley and at Georgetown for their helpful comments. The financial support of the Earl Warren Legal Institute at the University of California Berkeley and the Kennedy School of Government Dean's Research Fund is greatly appreciated.

JEL codes C72, C91, K42

INTRODUCTION

Law deters, educates, and coordinates. Economists typically focus on deterrence and sociologists often focus on education. We hypothesize that law's largest effects come from coordination. Normative systems have multiple equilibria, so announcing a new law can change expectations and cause behavior to jump from one equilibrium to another. This effect on expectations is especially likely in a society where citizens regard laws as credible commitments for collective action.

To demonstrate that law can have large coordination effects, we study games with inter-dependent payoffs. We simulated announcing a law by telling experimental participants that choosing one of the strategies (labeled "Left") will result in a small "penalty" ten percent of the time. Announcing a penalty could cause deterrence, education, and coordination. We set the penalty too small to change the equilibria among rationally self-interested actors, thus eliminating deterrence effects. The word "penalty" frames choosing Left as wrongdoing, which could cause actors who value morality to change their behavior, independent of the penalty's size. Specifically, framing could induce people to internalize the law and to stop doing wrong. In addition, as a penalty implies a potential loss, decreases in the frequency of doing wrong can also attributed to loss aversion (Kahneman and Tversky 1979). Finally, the announcement of a penalty might help players to coordinate their behavior.

We tested for framing and coordination effects in three kinds of games. First we ran a prisoner's dilemma (PD) game in which the penalty does not affect the uniquely dominant strategy of each player. In these circumstances, any change in behavior caused by announcing the penalty must result from more people acting morally or altruistically. In fact, announcing the penalty had no lasting effect on the number of moral or altruistic actors.

Next we repeated the experiment in a crowding game characterized by negative externalities between the agents' strategy choices. Like the PD game, the crowding game has a uniquely dominant equilibrium, but, unlike in the PD game, the equilibrium is interior rather than at a corner. The crowding game yielded the same results as the PD game. Specifically, announcing the penalty had no lasting effect on the number of moral or altruistic actors.

Finally, we repeated the experiment in a coordination game characterized by positive complementarities between the agents' strategy choices. The coordination games has multiple Nash equilibria that are Pareto-ranked. Agents prefer one equilibrium to the other but they may be unable to coordinate on it. The announcement of a penalty can help the players select an equilibrium. We found that announcing the penalty caused jumps in behavior from one equilibrium to another. Jumps from the Pareto-inferior equilibrium to the Pareto-dominant equilibrium after introduction of the penalty indicates that the law serves as an equilibrium selection principle.

These findings are potentially important for legal scholars and lawmakers. Economic theory and empirical research focus on law’s deterrence effects. Since deterrence is marginal and coordination is non-marginal, economists may be concentrating on relatively small effects. Sociological theory and empirical research often focus on law’s educative effects. Since preferences are relatively stable, sociologists may be concentrating on relatively small effects. If coordination effects are large, then lawmakers should focus on building credibility so that enacting a new law will cause behavior to jump in the desired direction. Credibility requires lawmakers to restrain themselves and promulgate only those new laws that will cause citizens to jump to a new equilibrium, rather than enacting futile laws that cause citizens to fall back to the original equilibrium.

The paper is organized as follows: Part II, the next part, discusses the theoretical frameworks helpful for evaluating the role of the announcement of a penalty and related experimental evidence. Part III outlines the experimental design. Part IV presents our experimental results. The paper concludes with Part V.

II. EDUCATION AND COORDINATION

The introduction a small, non-deterring legal sanction has two possible effects, education and coordination. It may affect preferences due to the re-framing of the decision context (i) and it may affect expectations due to the introduction of a new equilibrium selection principle (ii). GROUP SIZE EFFECTS TO BE INTRODUCED. (I HAVE TO CHECK THE THEORETICAL LIT. ON TIPPING POINTS).

(i) Framing: Change of preferences

When introducing a penalty, we make two changes to the representation of the choice problem. First, an expected penalty implies a potential loss when choosing LEFT rather than RIGHT. According to Subjective Expected Utility theory, such a transformation of payoffs should yield the same preferences ("descriptive invariance", see Tversky and Kahneman 1986). However, according to Prospect Theory (Kahneman and Tversky 1979), a change in reference points could lead to preference reversals, motivating loss averse individuals to choose RIGHT in the penalty game but not in the control treatment. Secondly, the penalty also comes with a moral judgement. LEFT is no longer one of two neutral strategies, but the strategy where one can incur psychological cost, such a guilt. If subjects experience such cost, the penalty we introduced could affect outcomes in all our games.[1]

While possible framing effects of the law have not been experimentally investigated yet, framing effects in the context of the prisoner's dilemma and public goods games more generally have been studied widely. Ross and Ward (1996) examined whether normative aspects of the presentation, such as the name of the game, influence individuals' willingness to cooperate in a two-person PD. They framed the PD once as the "Wall Street Game" and once as the "Community Game" and found that subjects cooperated twice as much in the Community Game than in the Wall Street Game. While about one third of the players cooperated in the first round of the Wall Street Game, more than two thirds selected to cooperate in the Community Game. The authors conclude: "Further research will be required to determine exactly why the particular label attached to the game exerted so large an effect – that is, to what extent the label influenced subjects directly (i.e. determined the way subjects felt they ought to play) and to what extent it influenced them indirectly (i.e. by changing their expectations about how the other player would expect them to play)." (Ross and Ward 1996, p. 108).

Evidence on the relevance of loss aversion in public goods games is not conclusive. Fleishman et al. (1988), Messick et al. (1993) and Rutte et al. (1987) find no significant effects due to framing. Andreoni (1995), Cooksen (2000), Park (2000) and Sonnemans et al. (1998) find higher cooperation rates in a positive frame where contributions to the public good provide positive externalities to others (or are labeled as a gift) than in the negative frame where purchasing the private good imposes

[1] In addition, if people overweight small probabilities as assumed by Prospect Theory, the perceived expected cost is even larger.

negative externalities on others. In a similar set-up, Brewer and Kramer (1986) get the opposite results.

A first look at the differences in designs suggests that researchers were more likely to find framing effects, the more the game studied resembled a coordination game.[2] The game employed by Sonnemans et al. (1998), for example, is a step-level public goods/bads game with multiple Pareto-ranked equilibria structurally similar to a coordination game. Andreoni's (1995) public goods experiments focus on the relevance of positive versus negative complementarities. He concludes: "People are significantly more willing to cooperate in a public goods experiment when the problem is posed as a positive externality rather than as a negative externality." (p. 13) Finally, Cooksen (2000) also reports higher contribution levels in the positive frame but rejects the notion of preference change. She tests for restart effects in repeated games where contributions were described as a gift to others (rather than neutrally). While she finds decreasing cooperation rates over time, contributions jump to significantly higher levels whenever the game is re-started (after a break of 30 seconds). She asks "Why would a "warm-glow" gradually cool down with repetition and then suddenly re-kindle after re-start?" (p. 69) and suggests that her description may "make plain the players' dependence on each other's goodwill" (p. 58).

We suspect that many games, which we would describe as n-person prisoner's dilemma or public goods games when focussing on monetary payoffs only, in fact are perceived as coordination games by the subjects. The gift exchange frame by Cooksen

---

[2] Farrell and Rabin (1996) point out that the results for two-person prisoner’s dilemma games more generally suggest that experimental participants perceived them as coordination games where psychological benefits of "both cooperating" induced the players to cooperate if the other did but not otherwise.

(2000) as well as the positive externality frame by Andreoni (1995) propound positive complementarities and thus, multiple equilibria.

(ii) Equilibrium selection: Change of expectations

Introducing a penalty implies sending a message to the participants and thus, can be interpreted similarly as allowing for pre-play communication. Both are "cheap talk" and theoretically should not affect behavior in either the PD or the crowding game but may coordinate expectations on particular equilibria in coordination games. In the latter, the introduction of a small penalty by the experimenter can be interpreted as a common information assignment to all agents by some "arbitrator" or "referee" (Brandts and MacLeod 1995, Kohlberg and Mertens 1986, Van Huyck et al 1992). While not affecting the structure of the game, the law may help agents to coordinate their expectations. Even though the argument seems intuitively appealing, Van Huyck et al. (1992) and Farrell and Rabin (1996) elaborate why deviations from an assignment made by an arbitrator can still occur (e.g. a "babbling" equilibrium where messages are ignored and coordination is not achieved always exists). In our framework, an important condition for the law to serve as equilibrium selection principle would thus be that the social meaning of punishment is shared. Results could vary in different cultures and societies, making the degree to which a small fine helps people coordinate on a specific equilibrium an empirical question.

While there exits a large experimental literature on non-binding pre-play communication in prisoner's dilemma and more generally public goods games, suggesting that "talk is not cheap",[3] the empirical conditions for communication to

[3] For a meta-analysis comparing over 100 experimental studies allowing for communication, see Sally (1995).

affect behavior are not given in our set-up. Higher cooperation rates are typically found for two-way, face-to-face communication rather than for either one-way or written communication (Charness 1998, Frohlich and Oppenheimer 1998).[4] As the announcement of a penalty resembles one-way anonymous communication, we cannot conclude that it should exhibit any effect in the prisoner's dilemma or the crowding game based on prior evidence.

Experimental evidence on the relevance of assignments in various coordination games suggests that payoff-dominant equilibrium points are credible assignments while payoff-dominated equilibria are not (Van Huyck et al. 1992). As a penalty is imposed on strategy LEFT, the Pareto-inferior action, and as the social meaning of punishment (generally) is negative in our culture, we expect the introduction of such a law to coordinate agents on the Pareto-dominant equilibrium.[5]

(iii) Group size or threshold effects THEORY ON TIPPING POINTS TO BE INCLUDED BY IRIS

Why, however, is a coordination device required to decrease strategy uncertainty in coordination games with a focal point as salient as a Pareto-dominant equilibrium? When multiple equilibrium points are Pareto-ranked, it seems plausible to apply the

---

[4] Bohnet and Frey (1999) show for prisoner’s dilemma (and for dictator and ultimatum games) that the identifiability of one’s counterpart(s) is crucial for increasing other-regarding behavior such as cooperation or fairness. For the importance of personal compared to computer communication in bargaining games, see Radner and Schotter (1989) and Valley et al. (1998).

[5] Preplay communication also helps overcome coordination failures. While Cooper et al. (1992) report two-way communication to be especially effective, Charness (1998) finds significant increases in coordination success in the same bilateral stag hunt game even with one-way communication.

concept of payoff-dominance to resolve the strategic uncertainty about others' play. As a payoff-dominant equilibrium is not strictly Pareto-dominated by any other equilibrium points, it could provide a natural focal point, especially if it is unique (Harsanyi and Selten 1988, Schelling 1960). Prior experimental evidence on coordination games suggests that considerations of efficiency indeed help people coordinate on the payoff-dominant equilibrium in small groups. However, in larger groups (six or more members), coordination failures are the typical outcome.[6] While the experimental literature so far has focused on the size of the groups, we wonder whether this emphasis is warranted. Is the number of group members really responsible for coordination failures or is it the percentage of subjects required to lead to a collapse to the inefficient equilibrium? In weak-link coordination games, one person can destroy the efficient outcome independent of the size of the group. This means that in groups of 2, 50% of the group has to deviate from playing the payoff-dominant action while in groups of 10, the inefficient outcome results if only 10% of the group members deviate. Increases in group size thus imply decreases in the threshold leading to inefficiency. If agents are concerned about the "riskiness" of an equilibrium point (Harsanyi and Selten 1988), a threshold at 10 percent seems quite a bit riskier than one at 50%. That is, while it becomes harder to coordinate on the efficient equilibrium with more people, it may not be the number of group members determining coordination success or failure but rather the riskiness implied by the location of the tipping point. In weak-link coordination games, the riskiness increases

---

[6] See for the empirical relevance of various selection criteria in coordination games, Cooper (1990). For the comparison between groups of 2 and 14 (or 16) members in weak-link coordination games, see Van Huyck et al. (1990), between groups of 3 and 6, Knez and Camerer (1994) and for groups of 9, Cachon and Camerer (1996). An exception to large-group coordination failures is reported by Weber (1998). He shows

with group size, in our design it decreases with group size. We thus hypothesize that the smaller the percentage of subjects required to tip behavior to the Pareto-dominant equilibrium is, the more efficient outcomes we observe.

III. EXPERIMENTAL DESIGN

We run three different games with dichotomous choices called "L" and "R", in two treatment conditions. The first condition was the control treatment; in the sanction treatment, we introduced a small expected sanction of 20 cents for choosing one of the two strategies. In all games, the marginal cost of choosing one instead of the other strategy is held constant in both treatment conditions. Game 1 is an n-person prisoner’s dilemma in which the marginal cost of choosing R (cooperation) rather than L (defection) was $2, independent of how many people chose to cooperate. Figure 1 presents the payoffs graphically.[7] In the sanction treatment, two changes were made. First, in order to hold the marginal cost of cooperation constant, 20 cents was added to all payoffs for choosing L. Secondly, the following sentences were added to the instructions:

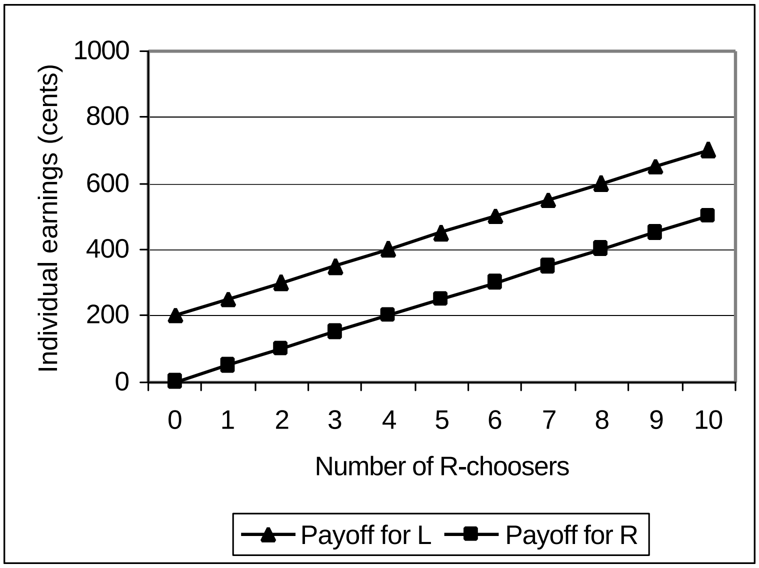
"Note: Choosing L will be punished. The penalty for choosing L instead of R is 200 cents. The penalty will be enforced with a probability of 0.1. After you have made your choice, we will determine whether the penalty will be enforced. There is a deck of 9 black and 1 red cards. One of the participants can pick a card. If the card is black, the penalty will not be enforced. If the card is red, the penalty will be enforced."

---

that when groups are started small and additional players are added slowly enough, even large groups with 12 members can avoid coordination failures.

[7] Table 1 (appendix A) shows the payoff table for an 11-person game as presented to experimental participants in the control treatment.

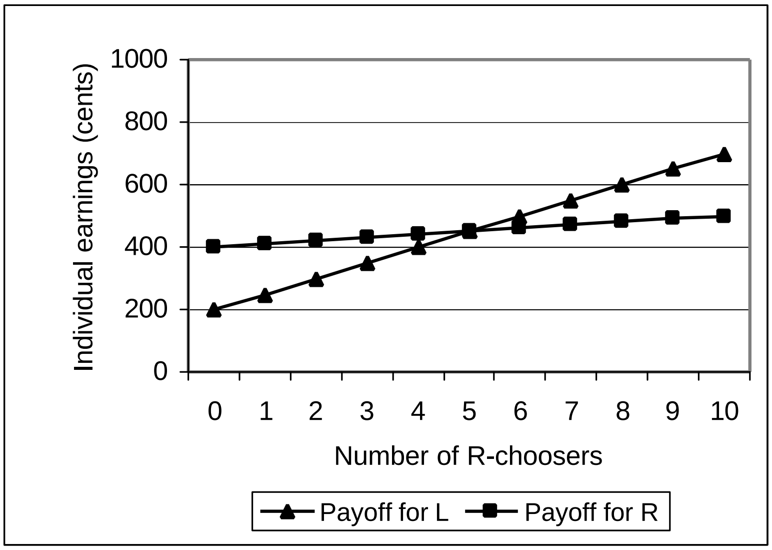
Figure 1: The 11-person prisoner's dilemma game[8]



Game 2 is the crowding game with negative complementarities and a stable interior equilibrium. It represents a situation in which every additional person choosing R produces negative externalities for all other R-choosers. In our design, these are 40 cents and accumulate until the sixth potential R-chooser is indifferent between choosing R or L, creating the stable interior equilibrium. Subjects are thus confronted with a payoff table in which choosing R pays when most people do not choose R, whereas choosing L pays when most people choose R. While the payoffs for choosing L are the same as in the prisoner's dilemma game, the marginal cost for switching strategies is $2 for the last R-chooser (or the last L-chooser) only (see figure 2, and payoff table 2 in appendix A).

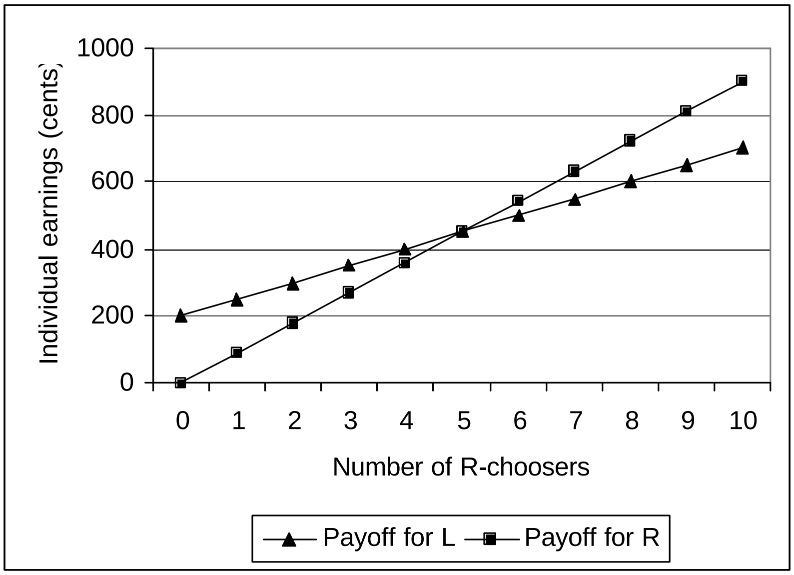
[8] Figure 1 describes the payoffs as they present themselves to the marginal 11th player.

Figure 2: The 11-person crowding game



In game 3, choosing R pays when most people choose R, and choosing L pays when most people choose L. This is a typical coordination game with positive complementarities. The same tipping point as in game 2 applies: If 5 people choose R, the sixth player is indifferent between choosing R and L. However, here the equilibrium is unstable. Every R-chooser produces positive externalities of 40 cents for every other R-chooser. The payoffs for choosing L are again identical to the other two games and the difference between the payoffs of the two strategies is $2 for the two corner solutions (see figure 3, and payoff table 3 in appendix A).

Figure 3: The 11-person coordination game



As we wanted to test for situations in which there were enough R-choosers present to tip behavior to the Pareto-dominant equilibrium as well as for situations in which the players were stuck at the Pareto-inferior equilibrium in the control version of the coordination game, we varied the size of the groups and thereby the location of the tipping point. We had no priors about the percentage of R-choosers required to tip behavior to the Pareto-dominant equilibrium. Holding the interior equilibrium constant at 6 R-choosers, for groups of 11 players, the tipping point is at 55 percent R-choices. By decreasing the size of the groups and holding the absolute number of subjects required to tip behavior to the Pareto-dominant equilibrium constant, we increase the relative threshold. For groups of 10, the threshold is at 60 percent, for groups of 9 at 67 percent, for groups of 8 at 75 percent and for groups of 7 at 86 percent R-choices.

While this procedure is similar to the one employed in weak-link coordination games,[9] in which the effect of the group size has been studied so far, there is one crucial difference: In the latter, the number of subjects required to lead to a collapse to the Pareto-inferior equilibrium is held constant. It is always 1, independent of the size of the group, implying that with an increasing group size the relative threshold for tipping behavior to the Pareto-dominant equilibrium becomes higher and higher.

In the sanction treatments of games 2 and 3, the same changes were made as in the prisoner's dilemma: the payoffs for choosing L were adjusted to hold the marginal cost of choosing one instead of the other strategy constant and the paragraph explaining the sanction was added. Table 4 summarizes the experimental design and indicates the number of experimental subjects who participated in each cell. Overall, 454 individuals participated in these experiments.

Table 4: Experimental design (n=group size, N=number of participants)

Treatments	n=11	n=10	n=9	n=8	n=7
Game 1: Prisoner's dilemma					
Control			N=27	N=24	N=21
Sanction			N=27	N=32	N=21
Game 2: Crowding					
Control			N=27	N=24	N=21
Sanction			N=27	N=24	N=21

[9] See van Huyck et al. (1990), Cachon and Camerer (1996), Knez and Camerer (1994) and Weber (1998).

Game 3: Coordination					
Control	N=33	N=30	N=27	N=24	N=28
Sanction			N=27	N=24	N=28

In order to get a feeling for behavior in the coordination game, we first run the control treatment for all five group sizes. It turned out that groups with tipping points of 60 percent or lower (11- and 10-person groups) were able to coordinate on the Pareto-dominant equilibrium even without the help of an additional equilibrium selection principle. As introducing sanctions does not make much sense here, we continued with the groups which did not manage to fully coordinate on the payoff-dominant equilibrium in the control treatment. These are the groups with 9, 8 and 7 participants. Each game was repeated five times. However, this was not announced to the participants. They were only told that the game would be repeated several times. Subjects were randomly allocated to new groups after each round. Due to our large group sizes, a true one-shot treatment was not possible. However, subjects did not know the code numbers of other group members at any time. While individual reputation building was not possible, group contagion effects cannot be excluded. The experiments were run double-blind, with neither the experimenter nor other subjects being able to identify individual decisions.[10] After the experimental instructions had been distributed, we also read them aloud to make sure that they were common knowledge. The experiments were run with students from various universities in the

[10] The experimental procedure described in Bohnet and Frey (1999) was used. For the experimental instructions, see appendix B.

greater Boston area.[11] They received a show-up fee of $5 and earned approximately $10 in the experiment. Their earnings in two randomly chosen rounds determined payment. The experiment took 45 minutes.

IV. RESULTS

In the following, the main findings are presented.

Observation 1: There is no evidence for a change in preferences due to the announcement of a non-deterring sanction.

We focus on the two games with unique Nash equilibria. The likelihood of choosing R, i.e. of cooperating, is not higher in prisoner's dilemma games where a sanction was announced than in the control treatments. Basically, round-by-round comparisons do not reveal any significant differences between cooperation rates independent of the size of the group and the round of the game.[12] The only exception is round 3 in groups with 7 participants where significantly more subjects cooperated in the control than in the penalty treatment ($chi^2=6.9$, $p<0.01$). This pattern, while not significant in other rounds, is representative of the behavior in the two treatments more generally. Typically, cooperation rates in the control treatment are higher than in the penalty treatment.[13] Our data looks very similar to standard one-shot repeated prisoner's

---

[11] We thank the Harvard Business School for the recruitment of the participants. Subjects were recruited by announcements in student newspapers in various universities in the Boston area and signed up electronically for experiments.

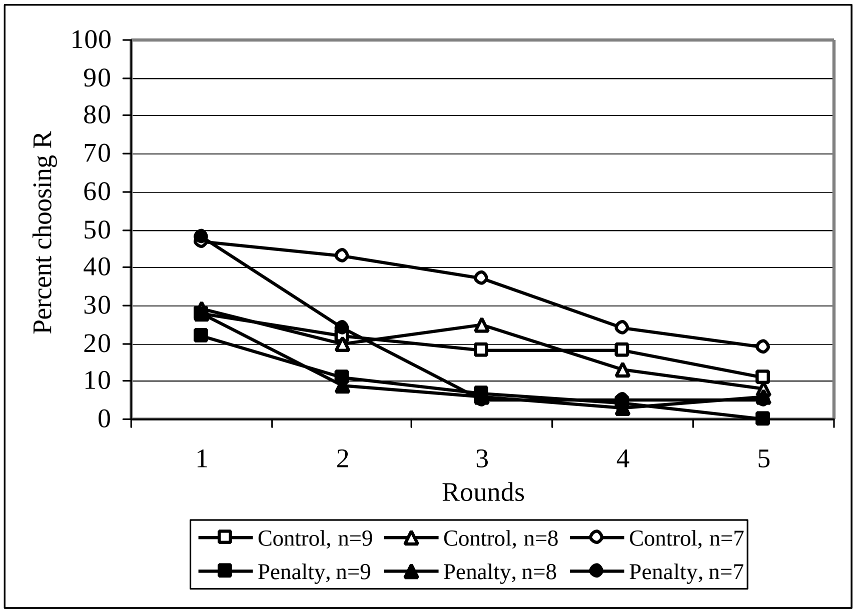
[12] We treat individual subjects as independent observations here but acknowledge that this is a second-best solution and that comparing group level data would be preferable. However, given our group sizes and our reluctance to aggregate over rounds, this would decrease our sample size most significantly, rendering any test meaningless.

[13] While the differences are small, they are in line with the "crowding out" of voluntary cooperation induced by small fines found by Fehr and Gaechter (2000),

dilemma games, with a decline over time to cooperation rates close to the equilibrium prediction (see Ledyard 1995). The results for all group sizes are presented in figure 4.

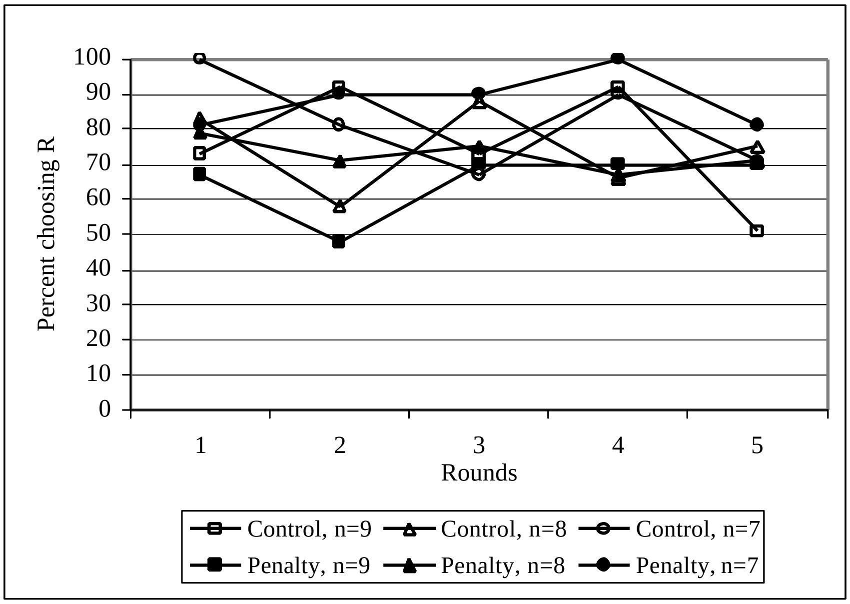
Gneezy and Rustichini (2000), and more generally by Frey and Oberholzer-Gee (1997).

Figure 4: Percentage of R-choices in the prisoner's dilemma games



The same pattern as in the prisoner's dilemma is found in the crowding games. Announcing a small sanction while holding the marginal cost of switching strategies constant, does not affect aggregate behavior. Practically none of the differences are significant. Round-by-round comparisons reveal one significant difference in round 2 of 9-person groups where more subjects choose R in the control than in the penalty treatment (chi^2=12.8, p<0.01). Other than that, all groups stay close to the equilibrium independent of the treatment and the round. In 9-person groups with an equilibrium point at 67% R-choices, 76% of the subjects choose R in the control and 65% in the penalty treatment on average. In 8-person groups, with the equilibrium point at 75% R-choices, 74% of the subjects choose R in the control and 73% in the penalty treatment on average. Finally, in 7-person groups, with an equilibrium at 86% R-choices, R is chosen by 82% of the subjects in the control and by 88% in the penalty treatment on average. No time trend can be observed. Figure 5 presents the results graphically.

Figure 5: Percentage of R-choices in crowding games

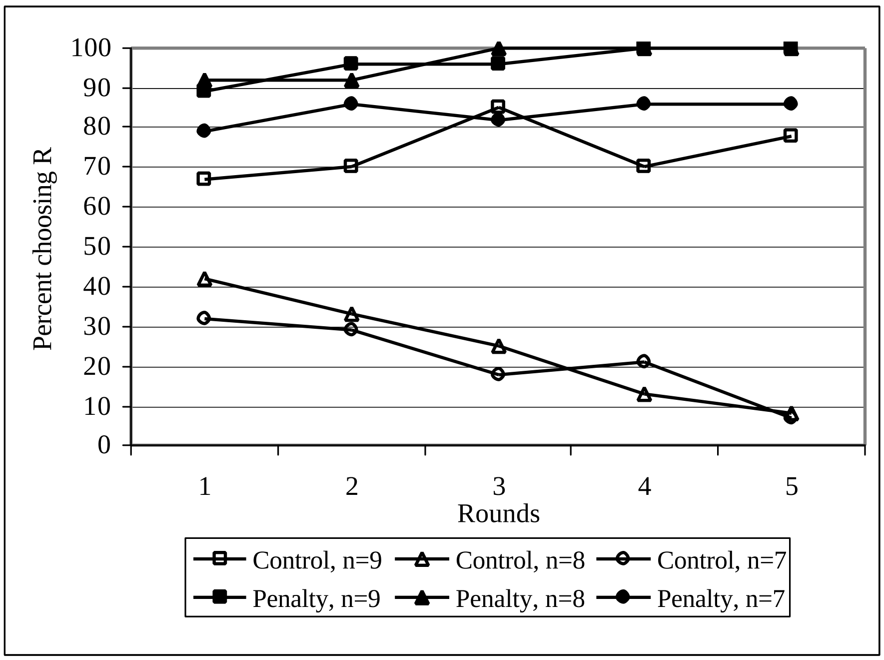


Observation 2: There is evidence for a change in beliefs due to the announcement of a non-deterring sanction.

The likelihood of choosing R is higher in the sanction treatments than in the control sessions. Round-by-round comparisons reveal significant differences for groups of 7 or 8 participants, with interior equilibrium points at 86% and 75% R-choices respectively. While the differences are already significant in the first rounds, subjects in 8-person groups are able to coordinate on the Pareto-dominant equilibrium in the sanction treatment by round 3 while coordination fails in the control treatment. In 7-person groups, the introduction of the sanction does not take the strategic uncertainty completely away. While the sanction significantly increases the likelihood of choosing R, there remain 4 people (out of 28) who keep choosing L in rounds 4 and 5. As subjects are already quite likely to choose R in 9-person groups where the percentage of subjects required to tip behavior to the Pareto-dominant equilibrium is

67%, the differences are only marginally significant here (r.1: p=0.05, r.2: p=0.10, r.3: p=0.16, r.4: p<0.01, r.5: p<0.01). However, by round 4, the groups in the sanction treatment but not in the control treatment have coordinated on the Pareto-dominant equilibrium.

Figure 6: Percentage of R-choices in coordination games



Observation 3: There is no evidence that the size of the group affects the likelihood of coordination on the Pareto-dominant equilibrium. There is evidence that the smaller the percentage of subjects required to tip behavior to the Pareto-dominant equilibrium is, the more likely coordination failure is avoided.

Coordination successes decrease when the relative share of R-choosers required to tip behavior to the efficient equilibrium increases. Round-by-round comparisons reveal significant differences between groups with equilibrium points at 67% or lower and the two other groups with equilibrium points at 75% and at 86%. Groups with low thresholds coordinate on the Pareto-dominant and groups with high thresholds on the Pareto-inferior equilibrium. By round 5, aggregate behavior has almost completely converged to one of the stable equilibria in all but in 9-person groups. In all the converging groups, there is either nobody or one person who "defects" from the equilibrium while there are between one and three people per group "defecting" in 9-person groups in round 5. Whether in the long run, aggregate behavior would converge to one of the equilibria in 9-person groups as well cannot be determined here. Table 5 summarizes the results.

Table 5: Probability of choosing R in the coordination game

Groups	**R. 1**	**R. 2**	**R. 3**	**R. 4**	**R. 5**
Size: n=11 - Interior equilibrium: 55%	76%	79%	82%	91%	94%
Size: n=10- Interior equilibrium: 60%	77%	77%	83%	87%	97%
Size: n= 9 - Interior equilibrium: 67%	67%	70%	85%	70%	78%
Size: n= 8 - Interior equilibrium: 75%	42%	29%	25%	13%	8%
Size: n= 7 - Interior equilibrium: 86%	32%	29%	18%	21%	7%

Coordination failures in groups where the tipping point requires a large percentage of R-choosers correspond to the results in weak-link coordination games (Van Huyck et al. 1990, Knez and Camerer 1994). Put differently, the smaller the percentage of people who can induce a collapse to the inefficient equilibrium, the riskier it is to choose the Pareto-dominant action. In our design, the share of people who can destroy coordination increases with the size of the group, making coordination easier in larger groups, while in weak-link coordination games it decreases with the size of the group, making coordination harder in larger groups. It is, thus, not group size which determines coordination success or failure but this threshold.[14]

V. CONCLUSIONS

Predictions based on multiple equilibria are difficult to falsify, and this fact makes coordination effects difficult to investigate econometrically. The fact that a phenomenon is difficult to investigate, however, does not make it any less real. Laboratory experiments provide sufficient control to investigate multiple equilibria, and our experimental results suggest that law's coordination effects are large. Our results extend to law the findings in more traditional coordination games such as network interaction, team production, and bank runs.

These experimental results support some findings on specific bodies of law. Empirical evidence on tax compliance suggests that deterrence cannot account for the wide variance between countries and sometimes regions or cities. Variables other than expected punishment are more helpful in accounting for observed differences in tax morale.[15] Several studies found that the perceived likelihood of others' tax paying

---

[14] There is one possible caveat to our interpretation: in our design, the payoff in the Pareto-dominant equilibrium increases with group size (from $5.40 in 7-person groups to $9.00 in 11-person groups). Compared to the constant payoff in the Pareto-inferior equilibrium of $2.00, coordination becomes comparatively more attractive. While the results for weak-link coordination games do not suggest that the payoff difference between the Pareto-dominant and the Pareto-inferior equilibrium matter (e.g. Van Huck et al. 1990 versus Weber 1998), we cannot exclude the possibility that the higher payoffs in larger groups helped people coordinate on the Pareto-dominant equilibrium.

[15] See Andreoni (1998) for a survey of the literature on tax morale and a discussion and rejection of arguments that the expected fine or risk aversion can account for the observed tax compliance.

significantly affects individual compliance.[16] These interdependencies suggest multiple equilibria, so coordination effects may account for the large differences in behavior from place to place. Similarly, police seldom enforce laws against smoking in public buildings, yet most people obey these laws in some countries and not in others. Similar arguments apparently apply to large differences in compliance with laws governing speeding on the highway, jay-walking, shop-lifting, or riding public transportation without paying. In general, coordination effects may account for large differences in compliance from place to place where expected sanctions are relatively constant.

This finding is potentially important for legal scholars and lawmakers. The economic analysis of law has focused on the power of sanctions to deter people from violating the law. Economist who study law debate about the magnitude of the elasticity of the relevant demand curves. Our experiments, however, suggest that deterrence effects may be far smaller than coordination effects. If our results are correct, the economic analysis of law must reorient itself towards powerful but elusive coordination effects.

Unlike economists, many sociologists express skepticism about the power of legal penalties to deter. In effect, sociologists debate whether or not the demand

[16] See Gordon (1989) for a theoretical model in the spirit of Akerlof (1980), where the proportion of the population believed to consider evasion to be morally wrong determines the psychic cost of evasion, Pommerehne et al. (1994) for simulation results where tax compliance depends, among other things, on the likelihood that others have paid their taxes in the previous period, and Sheffrin and Triest (1992) who analyze the 1987 Taxpayer Opinion Survey and find that perceiving other taxpayers as dishonest significantly decreases tax compliance. They argue: "Suppose, for example, that individuals who do not fully comply with the tax code experience more utility if aggregate noncompliance is higher. Perhaps this is because the guilt or stigma from noncompliance is eased when others are perceived to not comply as well. In this case, the relationship between individual and aggregate noncompliance can cause multiple equilibria." (p. 195)

curves relevant to law have *any* significant elasticity.[17] Conversely, many sociologists believe that the educative power of law can change preferences and induce altruism, whereas methodology commits most economists to the belief that preferences remain relatively stable as laws change. Most economists believe that self-interest anchors preferences.

Legal scholars disagree about the extent to which lawmakers can cause social change. Some argue that the law has an "expressive function" and, for example, that laws imposing desegregation in the southern states of the U.S. changed social norms developed to punish discrimination (Cooter 1998, Sunstein 1996, Ellickson 1996). Others are skeptical about the courts' ability to influence such phenomena as racial discrimination.[18] Our research suggests that expressing legal commitments is more likely to change behavior through coordination than education.

Coordination effects are especially relevant for lawmakers who often have to decide whether they should introduce a new legal rule knowing that the expected cost of violating the law will be too low to deter people from wrongdoing. To illustrate, should tax laws be introduced and speed limits imposed even though the probability of enforcement is too low to keep rational people from cheating or speeding? The more the lawmaker suspects that behavior in the underlying normative system has positive complementarities, the more inclined s/he should be to introduce a non-deterring legal sanction against "wrong-doing".

The expressive use of law to coordinate behavior depends on lawmakers' credibility. Lawmakers establish credibility by enacting laws that cause people to change their

---

[17] See Frank Zimring, Deterrence…

[18] Rosenberg (1993) argues that, e.g. *Brown v. Board of Education*, failed to integrate southern schools.

expectations in ways that events confirm. Thus credibility requires lawmakers to restrain themselves and promulgate only those new laws that will cause citizens to jump to a better equilibrium, rather than enacting futile laws that cause citizens to fall back to the original equilibrium. Using law to create focal points, thus, requires a substantial amount of information about payoffs and the structure of the game. Tax administrators, for example, need to worry about any factors that might shift social norms and perceptions regarding evasion. They need to be concerned about whether people perceive the tax game as a public goods game with a dominant strategy to defect or as a coordination game with multiple equilibria. In general the effect of non-deterring sanctions depends on the game people play.

REST FROM EARLIER CONCLUSIONS:

We have run experiments that differ from past studies on equilibrium selection and framing in that our design allows us to differentiate between changes in beliefs and changes in preferences. In games with unique Nash equilibria, expressive law - a non-deterring sanction - can only affect behavior by influencing preferences. We examine the preference effect of a non-deterring sanction for "wrong-doing" in a prisoner's dilemma and in a crowding game with negative complementarities where we hold the marginal cost of cooperation constant across treatment conditions. In groups with 7, 8 and 9 members, low cooperation rates prevail in both, the control and the sanction treatment of the prisoner's dilemma. In the crowding game, behavior converges to the stable interior equilibrium over time independent of the sanction. Thus, we do not find any evidence for changes in preferences due to the introduction of a sanction.

To what degree the small sanction serves as an equilibrium selection principle is studied in coordination games. The sanction helps agents focus their beliefs on the Pareto-dominant equilibrium. While behavior collapses to the Pareto-inferior equilibrium in the control treatments, the sanction induces a jump (close) to the Pareto-superior equilibrium in groups of 7 and 8. In these groups, the percentage of subjects required to tip behavior to the Pareto-superior equilibrium is high, 86% and 75% respectively. The lower this threshold is, the more likely agents are to coordinate on the Pareto-dominant equilibrium without a sanction. With thresholds of 60% and lower, payoff-dominance is a salient selection principle. This result holds despite the fact that we increased the size of the group as we decreased the threshold. While earlier studies on coordination games focused on the absolute number of group members, we show that it is not the group size but the threshold, which is responsible for coordination failures.

Appendix A

Table 1: Payoff Table for the prisoner's dilemma game (n=11)

Number of persons choosing L	Outcome for L (cents)	Number of Persons choosing R	Outcome for R (cents)
0	--	11	500
1	700	10	450
2	650	9	400
3	600	8	350
4	550	7	300
5	500	6	250
6	450	5	200
7	400	4	150
8	350	3	100
9	300	2	50
10	250	1	0
11	200	0	--

Table 2: Payoff table for the crowding game (n=11)

Number of Persons choosing L	Outcome for L (cents)	Number of Persons choosing R	Outcome for R (cents)
0	--	11	500
1	700	10	490
2	650	9	480
3	600	8	470
4	550	7	460
5	500	6	450
6	450	5	440
7	400	4	430
8	350	3	420
9	300	2	410
10	250	1	400
11	200	0	--

Table 3: Payoff table for the coordination game (n=11)

Number of Persons choosing L	Outcome for L (cents)	Number of Persons choosing R	Outcome for R (cents)
0	--	11	900
1	700	10	810
2	650	9	720
3	600	8	630
4	550	7	540
5	500	6	450
6	450	5	360
7	400	4	270
8	350	3	180
9	300	2	90
10	250	1	0
11	200	0	--

Appendix B

Sample instructions for the sanction treatment in an 8-person coordination game

Welcome to this research project!
You are participating in a study in which you have the opportunity to earn cash. The actual amount of cash you will earn depends on your choices and the choices of the other persons in the study. At the end of the study, two rounds will be randomly selected and the amount you earned in these rounds will be added to the show-up fee of $5. In addition to these instructions, you receive an envelope containing

- a Code Number Form
- a Decision Form marked with your code number
- an envelope marked with your code number

What the study is about:
The study is on how people decide. You and 7 other persons have to choose between two alternatives, L and R. The payoff table tells you how much money you earn depending on what you choose and what the 7 other persons choose.

How the study is conducted:
The study is conducted anonymously and repeated as many rounds as possible in the remaining time. Participants are only identified by "code numbers". In order to guarantee privacy and anonymity, <u>do not show anyone your code number</u>! You are randomly matched with 7 persons present in this room in each round.

START

The table reads as follows:

If you and all other persons choose R, each of you earns 630 cents.
If 1 person chooses L and 7 persons R, choosing L earns 570 cents and choosing R 540 cents.
If 2 persons choose L and 6 persons R, choosing L earns 520 cents and choosing R 450 cents.
…
…
If 6 persons choose L and 2 persons R, choosing L earns 320 cents and choosing R 90 cents.
If 7 persons choose L and 1 person R, choosing L earns 270 and choosing R 0 cents.
If you and all other persons choose L, each of you earns 220 cents.

Payoff Table

Number of Persons choosing L	Outcome for L (cents)	Number of Persons choosing R	Outcome for R (cents)
0	--	8	630
1	570	7	540
2	520	6	450
3	470	5	360
4	420	4	270
5	370	3	180
6	320	2	90
7	270	1	0
8	220	0	--

Note: <u>Choosing L will be punished</u>. The penalty for choosing L instead of R is 200. This penalty will be enforced with a probability of 0.1.

Procedure:
The same procedure is repeated in all rounds.

Round 1:
Please carefully read the payoff table before making a choice. Indicate your choice for Round 1, L or R, on the decision form, put it back into the envelope and then into the box, which we will pass around.

We will now determine whether the penalty will be enforced or not. There is a deck of 9 black and 1 red card. One of the participants can pick a card. If the card is black, the penalty will not be enforced. If the card is red, the penalty will be enforced. End of round 1.

We will now determine your earnings according to your choice and the choices of the other persons, and privately inform each of you how much money you earned in this round. For this purpose, we will again pass the box around. Please take the envelope marked with your code number out of the box. It contains the decision form now also indicating your earnings. Do not tell or show anybody else your result.

The following rounds:
The exact same procedure as in Round 1 will be repeated. You are randomly matched with 7 persons in this room. Please indicate your choice for 'Round 2' on the decision form, put it into the envelope and then into the box which we will pass around. We will then determine whether the penalty will be enforced or not. Your earnings will be computed again and you will be privately informed how much money you earned in this round.

The end of the study will be announced when time is up. We then randomly decide which two rounds are relevant for your payment. You are informed on this. For your own records, please write down how much you earned in this study. Please put the decision form back into the envelope and then into the box. Keep your code number form!

END OF THIS STUDY. You are invited to collect your earnings right after the experiment.

If you have any questions, please address them to Iris_Bohnet@Harvard.edu

We thank you for participating in the study.

REFERENCES

Akerlof, George A. (1980). A theory of social custom of which unemployment may be one consequence. *Quarterly Journal of Economics* 94: 749-795.

Andreoni, James (1995). Warm-glow versus cold-prickle: The effects of positive and negative framing on cooperation in experiments. *Quarterly Journal of Economics* CX(1), 1-21.

Andreoni, James, Brian Erard and Jonathan Feinstein (1998). Tax Compliance. *Journal of Economic Literature* 36(June): 818-860.

Bohnet, Iris and Bruno S. Frey (1999). The sound of silence in prisoner's dilemma and dictator games. *Journal of Economic Behavior and Organization* 38, 43-58.

Bohnet, Iris, Bruno S. Frey and Steffen Huck (2001). More Order With Less Law: On Contract Enforcement, Trust and Crowding. *American Political Science Review*. Forthcoming March.

Brandts, Jordi and W. Bentley MacLeod (1995). Equilibrium Selection in Experimental Games with Recommended Play. *Games and Economic Behavior* 11, 36-63.

Brewer, Marilyn B. and Roderick M. Kramer (1986). Choice Behavior in Social Dilemmas: Effects of Social Identity, Group Size and Decision Framing. *Journal of Personality and Social Psychology* L, 543-549.

Cachon, Gerard P. and Colin F. Camerer (1996). Loss avoidance and forward induction in experimental coordination games. *Quarterly Journal of Economics* 111, 165-194.

Charness, Gary (1998). Pre-Play Communication and Credibility: A Test of Aumann's Conjecture. Working paper. Universitat Pompeu Fabra.

Cooper, R., D. DeJong, R. Forsythe and T. Ross (1990), Selection Criteria in Coordination Games: Some Experimental Results. *American Economic Review* 80, 218-233.

Cooper, R., D. DeJong, R. Forsythe and T. Ross (1992). Communication in Coordination Games. *Quarterly Journal of Economics* 53, 739-771.

Cooksen, R. (2000). Framing Effects in Public Goods Experiments. *Experimental Economics* 3(1), 55-79.

Cooter, Robert (1998). Expressive Law and Economics. *Journal of Legal Studies* XXVII(2), 585-608.

Cooter, Robert (2000). Do Good Laws Make Good Citizens? An Economic Analysis of Internalizing Legal Values. VA. **Law Review ….**

Farrell, Joseph and Matthew Rabin (1996). Cheap Talk. *Journal of Economic Perspectives* 10, 103-118.

Fehr, Ernst and Simon Gächter (2000). Do incentive contracts crowd out voluntary cooperation? Working paper. Institute for Empirical Economic Research, University of Zürich.

Fleishman, J. (1988). The Effects of Decision Framing and Other's Behavior on Cooperatioon in a Social Dilemma. *Journal of Conflict Resolution* XXXII, 162-180.

Frey, Bruno S. and Felix Oberholzer-Gee (1997). The Costs of Price Incentives: An Empirical Analysis of Motivation Crowding Out. *American Economic Review* 87(September), 747-755.

Frohlich, Norman and Joe Oppenheimer (1998). Some consequences of e-mail vs. face-to-face communication in experiments. *Journal of Economic Behavior and Organization* 35, 389-403.

Gneezy, Uri and Aldo Rustichini (2000). Pay Enough or Don't Pay at All. *Quarterly Journal of Economics* 115(August), 791-810.

Gordon, James P.F. (1989). Individual Morality and Reputation Costs as Deterrents to Tax Evasion. *European Economic Review* 33(4): 797-805.

Harsanyi, John and Reinhard Selten (1988*). A General Theory of Equilibrium Selection in Games*. Cambridge: MIT Press.

Kahneman, Daniel and Amos Tversky (1979). Prospect theory: An analysis of decision under risk. *Econometrica* 47: 263-291.

Knez, Marc and Colin Camerer (1994). Creating expectational assets in the laboratory: Coordination in weakest-link games. *Strategic Management Journal* 15: 101-119.

Kohlberg, E. and J.-F. Mertens (1986). On the Strategic Stability of Equilibria. *Econometrica* 54, 1003-1038.

Ledyard, John O. (1995). Public Goods: A Survey of Experimental Research. In: Kagel, John H. and Alvin Roth (eds.). *Handbook of Experimental Economics*. Princeton: Princeton University Press, 111-194.

Messick, D., S. Allison and C. Samuelson (1993). Framing and communication effects on group members' responses to environmental and social uncertainty. In Maital, S. and S.L. Maital (eds.). *Economics and Psychology*. Aldershot: Edward Elgar.

Park, Eun-Soo (2000). Warm-glow versus cold-prickle: a further experimental study of framing effects on free-riding. *Journal of Economic Behavior and Organization* 43(4), 405-421.

Pommerehne, Werner W., Albert Hart and Bruno S. Frey (1994). Tax Morale, Tax Evasion and the Choice of Policy Instruments in Different Political Systems. *Public Finance* 49(Supplement): 52-69.

Radner, Roy and Andrew Schotter (1989). The Sealed-Bid Mechanism: An Experimental Study. *Journal of Economic Theory* 48, 179-220.

Rosenberg, Gerald (1993). *The Hollow Hope: Can Courts Bring about Social Change?* **PLACE AND PUBLISHER.**

Ross, Lee and Andrew Ward (1996). Naïve Realism in Everyday Life: Implications for Social Conflict and Misunderstanding. In: Brown, T., E. Reed and E. Turiel (eds.). *Values and Knowledge*. Hillsdale, NJ: Erlbaum, 103-135.

Rutte, C.G., H.A.M. Wilke and D.M. Messick (1987). The Effects of Framing Social Dilemmas as Give-Some or Take-Some Games. *British Journal of Social Psychology* XXVI, 103-108.

Sally, David (1995). Conversation and cooperation in social dilemmas. *Rationality and Society* 7(1), 58-92.

Schelling, Thomas (1960). *The Strategy of Conflict*. Oxford: Oxford University Press.

Sheffrin, Steven M. and Robert K. Triest (1992). Can Brute Deterrence Backfire? Perceptions and Attitudes in Taxpayer Compliance. In: Slemrod, Joel (ed.). *Why People Pay Taxes*. Ann Arbor: University of Michigan Press.

Sonnemans, Joep, Arthur Schram and Theo Offerman (1998). Public good provision and public bad prevention: the effect of framing. *Journal of Economic Behavior and Organization* 34, 143-161.

Sunstein, Cass (1996). On the Expressive Function of the Law. 144 *University of Pennsylvania Law Review*, **PAGES**

Tversky, Amos and Daniel Kahneman (1986). Rational choice and the framing of decisions. *Journal of Business* 59: S251-S278.

Valley, Kathleen L., Joseph Moag and Max Bazerman (1998). A 'Matter of Trust': Effects of Communication on Efficiency and Distribution of Outcomes. *Journal of Economic Behavior and Organization* 34, 211-238.

Van Huyck, John B., Raymond C. Battalio and Richard O. Beil (1990). Tacit Coordination Games, Strategic Uncertainty, and Coordination Failure. *American Economic Review* 80(1), 234-248.

Van Huyck, John B., Ann B. Gilette and Raymond Battalio (1992). Credible Assignments in Coordination Games. *Games and Economic Behavior* 4, 606-626.

Weber, Roberto (1998). Coordination problems in growing firms: Insights from experiments. Working paper. California Institute of Technology.