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Expressive Law: Framing or Equilibrium Selection?

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Abstract

Besides creating fear of sanctions, laws sometimes express collective commitments that can affect people's behavior in at least two ways. First, a law can frame an act as wrong and elicit intrinsic motivation to avoid doing it. Second, when people benefit from doing the same thing as others, a law can cause people to conform to it because they believe that others will do so. We investigate framing and coordination effects experimentally in three games: a prisoner's dilemma, a crowding game, and a coordination game. We simulate a law by telling subjects that one choice will result in a probabilistic penalty. In the prisoner's dilemma and the crowding game, announcing the penalty had no effect. In the coordination game, announcing the penalty caused behavior to jump to the Pareto-superior equilibrium. Our results suggest that law's largest effects come from equilibrium selection and not from preference change.

Keywords: Equilibrium selection, framing, expressive law, experimental law and economics, coordination, prisoner's dilemma.

JEL codes C72, C91, K42

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INTRODUCTION

Making a law, conventionally defined as an obligation backed by a sanction, can affect behavior in at least three ways. First, fear of sanctions deters some people from breaking the law. The theory of deterrence explains law's effects on behavior by the probability and severity of sanctions and the elasticity of demand curves.[1] Second, framing an act as illegal possibly elicits intrinsic motivation to avoid it. Many citizens comply with law from respect, not fear. The theory of framing explains law's effects on behavior by the intensity and malleability of respect for law. Third, a law ideally coordinates the behavior of people by helping them to predict what others will do. The theory of coordination explains law's effects on behavior by the credibility of the equilibrium selection principle it offers.[2]

Our experiments test the strength of two of these effects: framing and coordination. We build on an extensive literature concerning whether changes in the representation of choices influences decisions in games (for a review, see Camerer 1995). In one famous framing experiment, Ross and Ward (1996: 108) found significantly more cooperation from labelling a two-person prisoner's dilemma (PD) as the "Community Game" rather than the "Wall Street Game." They concluded with this question:

> "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)."

Our experimental design allows us to answer this question as applied to sanctions.

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[1] For a review of the "imperative theory of law" in Anglo-American jurisprudence, see Raz (1980), and for a recent survey on deterrence theory, see Polinsky and Shavell (2000).
[2] For three effects of legal obligations on behavior, see Cooter (2000a).

To simulate a legal sanction, we tell subjects in our experimental group that one choice will result in a probabilistic "penalty" (in dollars). To keep the expected payoffs constant, we offset the probabilistic penalty by a small increase in the certain payoff. The penalty does not affect the expected payoffs in dollars for each combination of strategies by the players. Any effect of the penalty on the experimental group must occur because players change their subjective valuation of the dollar payoffs (e.g. guilt decreases the dollar value of the penalized act), or because they change their beliefs about what others will do.

We used games with a unique Nash equilibrium where a sanction can only affect behavior by influencing preferences and games with multiple equilibria where introducing a sanction can serve as an equilibrium selection principle. We found that announcing a penalty in the prisoner's dilemma and "crowding games," which have a unique Nash equilibrium, had no effect. Intrinsic motivation was too weak to overcome the dominant, self-interested strategy. However, announcing a penalty for the "wrong" choice in coordination games caused behavior to jump from the Pareto-inferior to the Pareto-superior equilibrium, thus showing a strong coordination effect. Our results suggest that sanctions affect behavior more by changing beliefs than by changing preferences.

This experimental result conforms to our hypothesis about law in general. When navigating a world with cultural diversity, organized interest groups, and unreliable regulators, a critical reception of legal pronouncements preserves moral compass. Legal skepticism, however, inhibits law's ability to elicit intrinsic motivation. In contrast, coordination effects can operate when people are critical of law but believe that the law will create a focal point.

While our experiments only concern coordination and framing, we also believe that coordination dominates deterrence in law. Coordination causes behavior to jump from one equilibrium to another, which is a large change. In contrast, deterrence causes actors to equate the marginal benefits of wrongdoing and the marginal expected sanction. Marginal adjustments in behavior are relatively small. If this reasoning is correct, then neglecting coordination and focusing on deterrence causes economists to concentrate on law's small effects and neglect its large effects.[3]

The paper is organized as follows: Part I reviews the relevant scholarship on framing and equilibrium selection. Part II describes the experimental design. Part III presents our results. The paper concludes with Part IV.

I. REVIEW OF LITERATURE ON FRAMING AND EQUILIBRIUM SELECTION

Attaching a penalty lowers the payoff to the sanctioned act relative to unsanctioned acts. "Payoff" describes the sanction's effect in morally neutral language, whereas "penalty" implies that the act is wrong. We referred to a reduction

[3] To illustrate, textbooks in law and economics have almost nothing to say about law's coordination effects. See Cooter and Ulen (1999) or Posner (1998). Recent exceptions to this generalization are McAdams (2000) and Hay and Shleifer (1998) who argue that the potentially most significant benefit of public laws in emerging economies is that they become the focal point and can coordinate expectations even with little enforcement.

in payoffs in our experiments as a "penalty." We will review the literature on the question of how this change might effect preferences and beliefs.

(i) Framing: Change of preferences

Introducing a penalty changes the representation of the choice problem in two ways. First, "penalty" connotes wrongdoing, which might incur psychological cost such as *guilt*. Second, a "penalty" suggests a *loss* in dollar payoffs (even though an increase in the dollar value of certain payoffs offsets the probabilistic penalty). Subjective Expected Utility theory predicts no change in behavior from framing. If different descriptions of the choice task induced behavioral changes, the axiom of "descriptive invariance" would be violated (Tversky and Kahneman 1986). According to Prospect Theory (Kahneman and Tversky 1979), however, our transformation of payoffs could change the reference point causing loss averse individuals to avoid the "wrong" action in the experimental group and not in the control treatment. The two possible effects a penalty might have – guilt and loss aversion – operate in the same direction and could affect outcomes in all our games.[4]

Framing effects of penalties have not been investigated yet, whereas "guilt" induced by imposing *losses on others* has been studied more extensively. In public goods games, such guilt frames have been found to influence behavior less than "warm glow" frames (Andreoni 1995, Cookson 2000, Park 2000 and Sonnemans et al 1998).[5] In the guilt frame, subjects' decisions were typically framed as choosing a

[4] Given the comparatively small stakes in experiments, risk neutrality is the theoretically correct (Rabin 2000) and probably also behaviorally most accurate assumption (Harbaugh et al. 2002). The results in Harbaugh et al. (2002) also suggest that it is unclear whether people overweight small probabilities as assumed by Prospect Theory in actual choice tasks.

[5] Studies using hypothetical incentives have generally not found any effects of such framing (e.g. Fleishman et al. 1988, Messick et al 1993 and Rutte et al 1987). Brewer and Kramer (1986) found a "guilt" effect in a hypothetical common pool resource game.

private good hurting others; in the warm glow frame, subjects' decisions were generally framed as contributing to a public good benefiting others. Even though most of the studies employed standard linear public goods games,[6] people were more likely to cooperate when the problem was posed as a positive externality rather than as a negative externality. Such complementarities suggest multiple equilibria and pose a coordination problem.[7] Researchers were apparently more likely to find "framing effects" when their design created (the perception of) a game with multiple equilibria rather than a unique Nash equilibrium.[8] Perhaps the experimenters were actually observing coordination effects, not preference changes.

(ii) Equilibrium selection: Change of expectations

Announcing a probabilistic penalty that does not change the equilibrium (and is offset by an increase in the certain payoff) is "cheap talk". Despite being cheap, some forms of talk have been found to affect behavior in prisoner's dilemma and coordination games.[9] Unlike most experiments on cheap talk, however, our subjects do not talk to each other (or to the experimenter). Rather, the small penalty is introduced by an "external authority" (the experimenter), and thus can be interpreted as a "common information assignment" to all subjects (Brandts and MacLeod 1995,

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[6] Andreoni (1995), Cookson (2000) and Parks (2000) used standard linear public goods games while Sonnemans et al. (1998) used a step-level public goods game with multiple Pareto-ranked equilibria.

[7] 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. While we are agnostic about the specific form of intrinsic motivation, see for a survey of the theories and the empirical evidence on social preferences, Fehr and Schmidt (2001).

[8] See Camerer (2003) who identifies "what game people think they are playing" as one of the "top ten open research questions".

[9] Higher cooperation rates are typically found for two-way, face-to-face communication rather than for one-way or anonymous communication in prisoner's dilemma games (see e.g. Bohnet and Frey 1999 or Frohlich and Oppenheimer 1998). In coordination games, two-way communication increases coordination rates in stag hunt games while one-way communication works better in the battle of the sexes game (see e.g. Charness 1998, Cooper et al. 1992 and for a survey, Camerer 2003).

Van Huyck et al. 1992, Wilson and Rhodes 1997). Experimental evidence suggests that such information can serve as an equilibrium selection principle, provided that the assignment does not compete with another focal principle.

Assignments guiding the players to the Pareto-dominant equilibrium have been found to be particularly successful (Van Huyck et al. 1992). A penalty on the strategy leading to the Pareto-inferior equilibrium can guide people to the strategy leading to the Pareto-superior equilibrium if people want to avoid guilt and losses. "Loss and guilt-avoidance" could be used as focal principles to exclude equilibria where, compared to the reference point, one may lose money and incur psychological cost (Cachon and Camerer 1996).

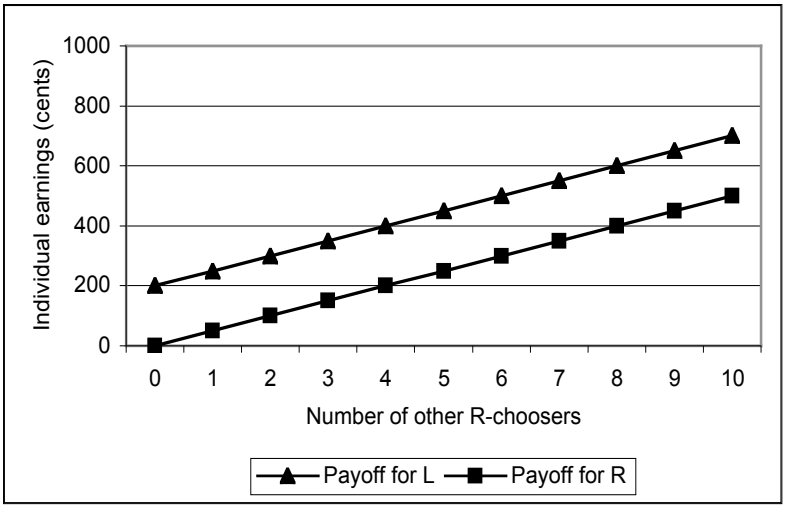
Based on the evidence on framing and coordination discussed, we conclude that a penalty will affect behavior more strongly in games with multiple equilibria than in games with a unique Nash equilibrium. We hypothesize that a penalty will serve as an equilibrium selection principle in our coordination games but will not affect preferences in the prisoner's dilemma or crowding games.

II. 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. We used behavior in the control version to see whether people's perception of the game accord with objective payoffs or not. The second treatment was the experimental group where we introduced a small expected penalty of 20 cents for choosing L (sanction treatment). In all games, the marginal cost of choosing L rather than R 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.[10]

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



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. Second, the following sentences were added to the instructions:

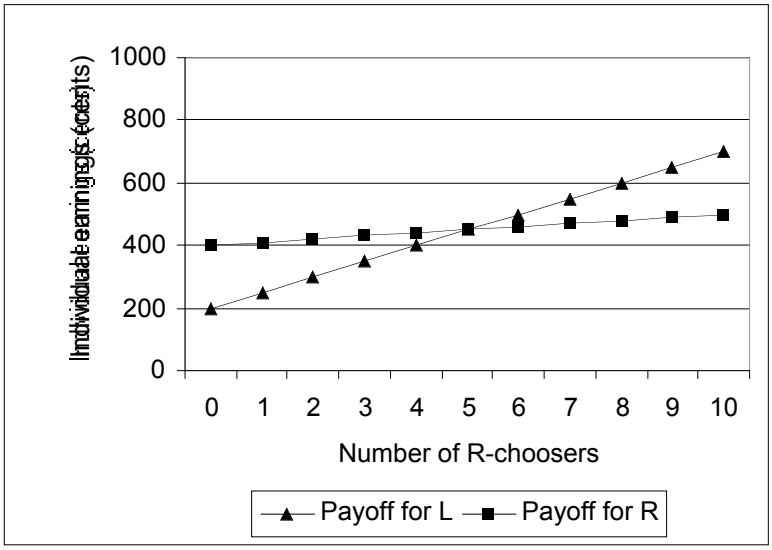
"Note: Choosing L will be punished. The penalty for choosing L instead of R is 200 cents. The penalty will be enforced with a probability of 0.1. After you have made your choice, we will determine whether the penalty will be enforced. There is a deck of 9 black and 1 red cards. One of the participants 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."

Game 2 is the crowding game with negative complementarities and a unique equilibrium. Starting with 0 R-choosers, choosing R pays more than choosing L, but

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

increasing the number of R-choosers produces negative externalities of 40 cents for all other R-choosers. Negative externalities accumulate as more players choose R until the sixth potential R-chooser is indifferent between choosing R or L. Subjects thus confront a payoff table in which choosing R pays when most people choose L, whereas choosing L pays when most people choose R, and a stable interior equilibrium occurs when 6 players choose R (see figure 2, and payoff table 2 in appendix A).[12]

Figure 2: The 11-person crowding game



In the sanction treatment of game 2, the same changes were made as in the prisoner's dilemma: the paragraph explaining the penalty was added and the payoffs for choosing L were adjusted to hold the marginal cost of choosing one strategy instead of the other.

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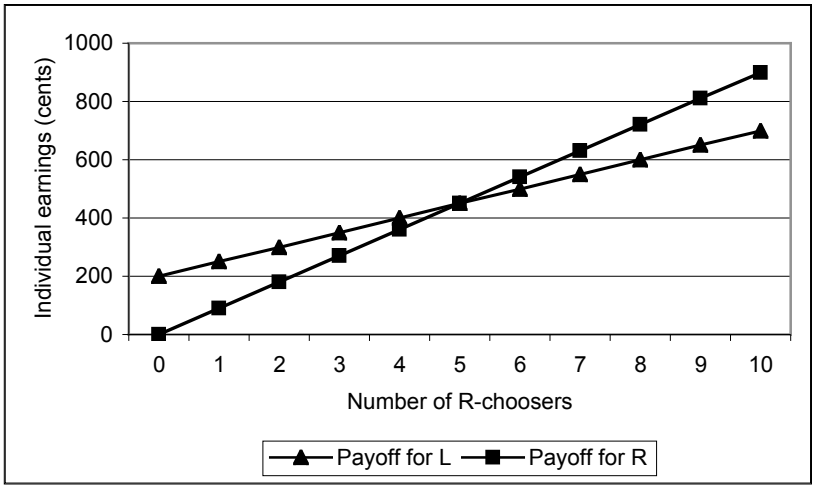
[11] Figure 1 describes the payoffs as they present themselves to the marginal 11th player.

[12] 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.

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. To contrast it with the crowding game, we call it an "affiliation game". 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 (see figure 3, and payoff table 3 in appendix A).[13] The sanction treatment was implemented as in the prisoner's dilemma and the crowding game.

Figure 3: The 11-person affiliation game



We designed the affiliation game so that the tipping point occurs with 6 R-choosers, regardless of the total number of players in the game. Consequently, the proportion of R-choosers required to tip behavior to the Pareto-dominant equilibrium increases as group size decreases. To illustrate by actual group sizes in our

[13] 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.

experiments, the tipping point requires 55 percent R-choices in a group of 11 players, 60 percent in a group of 10, 67 percent in a group of 9, 75 percent in a group of 8, and 86 percent in a group of 7. With this design, our prior belief was that coordinating on the Pareto-dominant equilibrium would be easier as group size increased. We had no priors about the particular percentage of R-choosers required to tip behavior to the Pareto-dominant equilibrium. We chose a range of group sizes in the expectation of identifying situations where players were stuck at the Pareto-inferior equilibrium in the game's control version, so we could see whether they would tip to the Pareto-dominant equilibrium in the experimental treatment.

Table 4 summarizes the experimental design and each cell indicates the number of subjects, whose overall total equals 454 individuals.

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

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

			=27	=24	=21
Game 3: Coordination Control Sanction	=33	=30	=27 =27	=24 =24	=28 =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 without the help of an additional equilibrium selection principle. Consequently, we focused our experiments on groups that coordinated imperfectly on the payoff-dominant equilibrium in the control groups with 9, 8 and 7 participants.

Each game was repeated five 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. The experiments were run double-blind, with neither the experimenter nor other subjects being able to identify individual decisions.[14] 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 greater Boston area.[15] They received a show-up fee of $5

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

[15] 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.

and earned approximately $10 in the experiment. Their earnings in two randomly chosen rounds determined payment. The experiment took 45 minutes.

III. RESULTS

We present out main findings in this section.

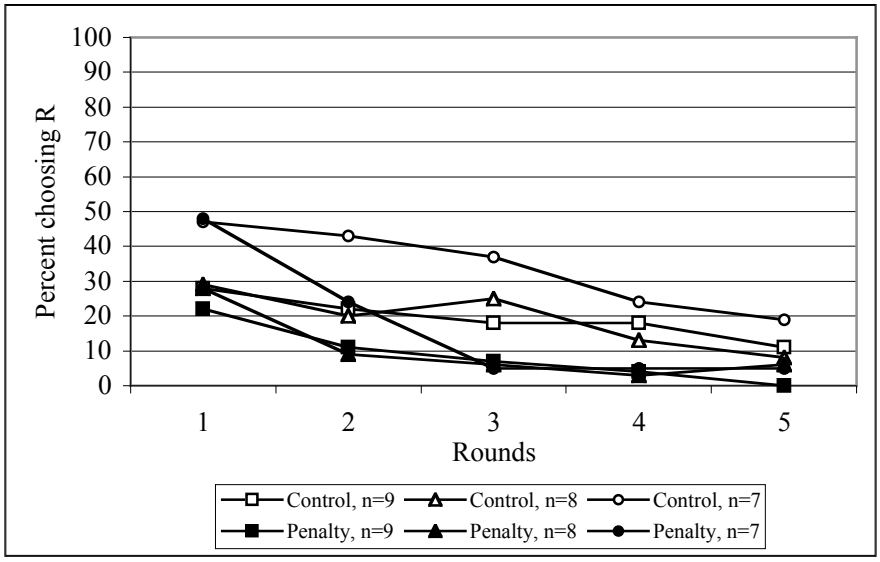
Observation 1: There is no evidence for a change in preferences due to the announcement of a small penalty.

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.[16] The only exception is round 3 in groups with 7 participants where significantly more subjects cooperated in the control than in the sanction 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.[17] Our data looks very similar to standard one-shot repeated prisoner's dilemma games, with a decline over time to cooperation rates close to the equilibrium prediction (see Ledyard 1995, Camerer 2003). The results for all group sizes are presented in figure 4.

[16] 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.

[17] While the differences are small, they are in line with the "crowding out" of voluntary cooperation induced by small fines found by Bohnet et al. (2001), Fehr and Gaechter (2000), Gneezy and Rustichini (2000), and more generally by Frey and Oberholzer-Gee (1997).

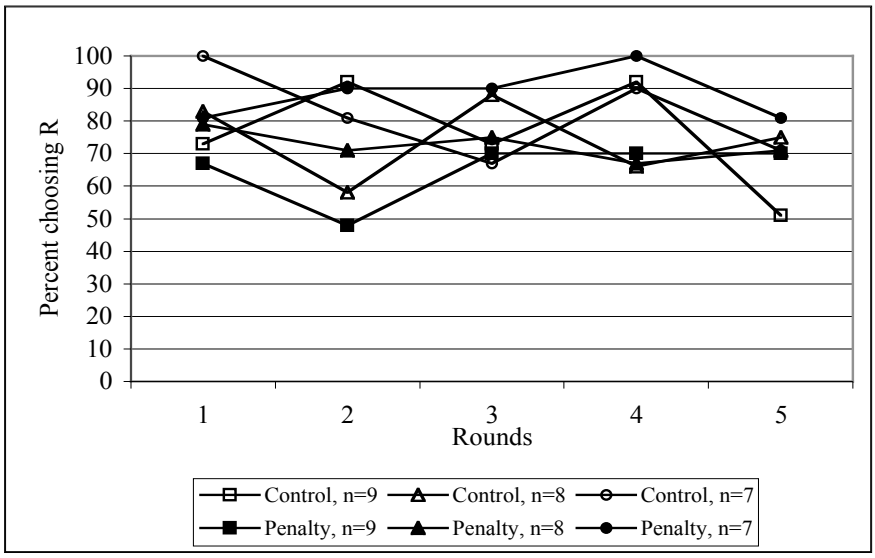
Figure 4: Percentage of R-choices in the prisoner's dilemma games



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.[18] No time trend can be observed. Figure 5 presents the results graphically.

[18] 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.

Figure 5: Percentage of R-choices in crowding games



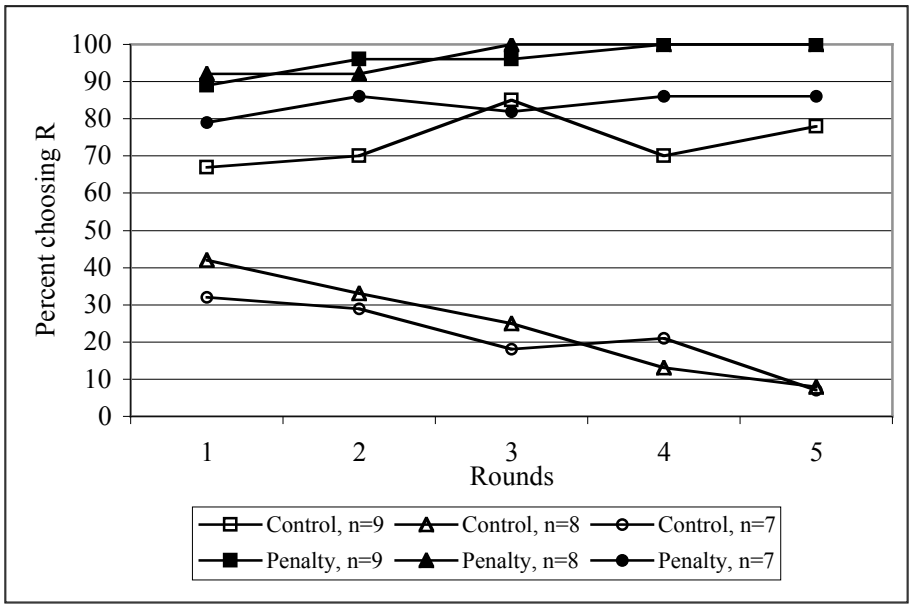
Observation 2: There is evidence for a change in beliefs due to the announcement of a small penalty.

The likelihood of choosing R is higher in the sanction treatments than in the control sessions in the coordination game. 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.

When group size increases to 9, the percentage of subjects required to tip behavior to the Pareto-dominant equilibrium falls to 67%, so subjects are likely to

choose R in the control group. With N=9, the differences between control and experimental treatments are only marginally significant (r.1: $p=0.05$, r.2: $p=0.10$, r.3: $p=0.16$, r.4: $p<0.01$, r.5: $p<0.01$). With N=9, however, the groups in the sanction treatment coordinate perfectly on the Pareto-dominant equilibrium by round 4, whereas the groups in the control treatment coordinate imperfectly even in rounds 4 and 5.

Figure 6: Percentage of R-choices in coordination games



Finally, we ask, "Why do players sometimes coordinate merely by observing the payoffs, without a penalty or other communication?" When multiple equilibrium points are Pareto-ranked, people may look for payoff-dominance to resolve the strategic uncertainty (Harsanyi and Selten 1988, Schelling 1960). Experimenters have tested whether a uniquely Pareto-dominant equilibrium provides a sufficient focal point to coordinate behavior in a set of multiple equilibria. The evidence suggests that people can coordinate on the payoff-dominant equilibrium in small groups. In larger groups (six or more members), however, coordination typically fails.[19] Our experiments extend these conclusions to the percentage of players required for tipping.

Observation 3: The smaller the percentage of subjects required to tip behavior to the Pareto-dominant equilibrium is, the more likely coordination succeeds.

In our experiments, coordination successes decrease when the relative share of R-choosers required to tip behavior to the efficient equilibrium increases. Round-by-round comparisons in the control treatments reveal significant differences depending on whether or not the equilibrium point is above 67%. Groups with tipping points of 67% or below (n=9, n=10 or n=11) coordinate on the Pareto-dominant equilibrium, and groups with tipping points above 67% (n=7 or n=8) collapse to the Pareto-inferior equilibrium. By round 5, aggregate behavior has almost completely converged to one of the stable equilibria for all group sizes but n=9.

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[19] See for the empirical relevance of various selection criteria in coordination games, see Cooper (1990) and Haruvy and Stahl (1998). 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 that when groups are started small and additional players are added slowly enough, even large groups with 12 members can avoid coordination failures.

Table 5 summarizes the results.

Table 5: Probability of choosing R in the coordination game (control groups)

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%

Our result that coordination failure increase in groups where the tipping point requires a large percentage of R-choosers is consistent with the results in weak-link coordination games (Van Huyck et al. 1990, Cachon and Camerer 1996, Knez and Camerer 1994, Weber 1998). However, in our experiments increasing group size makes coordination easier, whereas in weak-link experiments increasing group size makes coordination harder. Specifically, in weak-link studies one "non-cooperator" will tip the group to the Pareto-inferior equilibrium, regardless of group size. If intrinsic motivation is randomly distributed among players, the probability that a group includes a non-cooperator increases with its size. Players in these games correctly regard cooperating as a riskier strategy when the group's size increases. In general, if the number of non-cooperators required to destroy the efficient outcome in a game is independent of the group's size, then cooperating is a riskier strategy for a player when the group's size increases.

In our experiments, however, unravelling requires an increasing proportion of non-cooperators as the group's size increases. Players in our experiments correctly

regard cooperating as a safer strategy when the group's size increases. In general, the determinant of coordination success or failure in groups whose members cannot communicate with each other is the proportion of people required for tipping, not group size.[20]

IV. CONCLUSIONS

Predictions based on multiple equilibria are difficult to falsify, and this fact makes coordination effects difficult to investigate in the field. Laboratory experiments provide first insights about the relevance of law's coordination effects. Our design allows us to separate the effects of changes in beliefs and changes in preferences. We compare games with multiple equilibria where introducing a sanction could serve as an equilibrium selection principle and games with unique Nash equilibria where a sanction can only affect behavior by influencing preferences. We find that the introduction of a penalty too small to deter wrong-doing helps actors to coordinate on the Pareto-dominant equilibrium in the coordination game, but that it does not measurably affect preferences in the prisoner's dilemma and the crowding game.[21]

Lawmakers often have to decide whether or not to make a law whose enforcement is too difficult to deter wrongdoers by fear. 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? Our experimental results suggest that enacting a law with non-deterring sanctions is most likely to change

[20] There is a possible caveat to our interpretation: in our design, the payoff for 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 for the Pareto-inferior equilibrium of $2.00, coordination becomes comparatively more attractive in larger groups. 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 Huyck 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.

behavior when the underlying normative system has positive complementarities that cause multiple equilibria.

Positive complementarities seem more prevalent than one might initially think. Empirical evidence on tax compliance indicates that variables other than expected punishment are more helpful in accounting for observed differences in tax morale.[22] Several studies found that the perceived likelihood of others' tax paying significantly affects individual compliance.[23] Coordination effects may account for the large differences in tax compliance from place to place. Similarly, police seldom enforce laws against smoking in public buildings or against littering in public places, yet most people obey these laws in some countries and not in others. Field studies suggest that people are less likely to litter in a clean environment where most others do not litter, again suggesting a coordination game (Cialdini et al. 1990). Similar arguments may apply to large differences in compliance with laws governing speeding on the highway, jay-walking, shop-lifting, or riding public transportation without paying.

Based on the General Social Survey data from 1972-1994, Glaeser and Glendon (1998) find that strategic complementarity is one of the key causes of gun ownership. Field evidence on gun ownership suggests that coordinating through law requires credible lawmakers and common knowledge of law. Thus researchers in the

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[21] This result is corroborated by a recent study by Tyran and Feld (2002) who introduce small sanctions in a public goods game and find no increase in cooperation if the sanction is exogenously imposed.
[22] 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.
[23] 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)

"Boston Gun Project" found that violence and gang membership among youth depended on fear and the need for self-defence, i.e. on the likelihood of others being violent. Accordingly, they suggested, and successfully implemented, a strategy consisting not only of law enforcement, but also of public information on law enforcement, thus creating common knowledge and a new equilibrium. (Piehl et al. 2000)

Legal scholars disagree about the extent to which lawmakers can cause social change. Some argue that the law has an "expressive function" that can change behavior.[24] For example, laws imposing desegregation in the southern states of the U.S. may have changed social norms concerning racial discrimination. Others are skeptical about the courts' ability to influence such phenomena as racial discrimination.[25] Our research suggests that expressing legal commitments is more likely to change behavior through coordination than changing preferences.

The expressive use of law to coordinate behavior depends on lawmakers' credibility. Lawmakers establish credibility by enacting laws that cause people to change their 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 the payoffs people face. Tax administrators, for example, should be concerned about whether people perceive taxation as a public goods game with a dominant strategy to defect or as a

[24] See e.g. Cooter 1998, Ellickson 1996, McAdams 2000, Posner 2000a, Sunstein 1996.
[25] Rosenberg (1993) argues that, e.g. *Brown v. Board of Education,* failed to integrate southern schools. For other skeptical perspectives, see Adler (2000) and Anderson and Pildes (2000).

coordination game with multiple equilibria. In general the effect of small sanctions depends on the game people play.

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 five rounds. Participants are only identified by "code numbers". In order to guarantee privacy and anonymity, do not show anyone your code number! 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: Choosing L will be punished. 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 four rounds:
The exact same procedure as in Round 1 will be repeated in the following four rounds. 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.

At the end of the study, we will 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 by presenting your code number form. Your earnings will be in a sealed envelope marked with your code number.

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

We thank you for participating in the study.

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