Enumerating Rights: More is Not Always Better

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Abstract

Contemporary political and policy debate rhetoric increasingly employs the language of 'rights': how they are assigned and what entitlements individuals in a society are due. While the obvious constitution design issues surround how rights enumeration affects the relationship between a government and its citizens, we instead analyze how rights enumeration affects how citizens interact with each other. We design and implement a novel experiment to test whether social cooperation depends on the assignment of citizens' rights, by framing the right of subjects to take a particular action either positively or negatively. We find that when rights are framed positively, there exists an 'entitlement effect' that reduces social cooperation levels and crowds-out the tendency of individuals to act pro-socially.

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1. Introduction

A major insight from modern economics is that political institutions matter for economic outcomes. In particular, constitutions – as templates for establishing the "rules of the game" – impact a variety of economic variables through both private actions and public policy (Persson and Tabellini, 2005; Voigt, 2011). The issue of which rights to enshrine constitutionally has traditionally centered on the benefits of protecting individuals from coercion by the state (e.g. Buchanan and Tullock, 1962). It has long been argued that a relatively small set of fundamental rights (for example to speech, liberty and security of the person) should be codified to protect citizens from government abuse (see Locke, 1689; Rawls, 1970). Similarly, property rights can prevent capricious governments from expropriating private property, thus incentivizing investment, trade, and economic growth (Besley, 1995; Besley and Ghatak, 2010; Moral-Benito, 2012).

In this study, rather than focusing on interactions between individuals and government, we explore how the explicit enumeration of rights affects cooperation *between* citizens. We present results of a novel laboratory experiment designed to test how rights-enumeration affects outcomes of situations where citizens must work together to achieve efficient outcomes. **The design of our experiment allows us to explore the dynamics of behavior between individuals who are paired with one another continuously over many periods. Our results show that enumerating more rights does not necessarily encourage cooperation between citizens and may even reduce it. However, we also find that *the paths* by which individuals achieve cooperation depend significantly on how rights are assigned. For example, we find that enumerating more**

rights leads to less pro-social behavior of a specific form, i.e. that in which paired individuals sacrifice their own immediate payoffs to benefit their partner. On the other hand, we show that coordination is 'stickier' when rights are formally enumerated: paired individuals are more likely to coordinate if they have done so in the recent past but less likely to coordinate if they have not.

Our specific interest lies in exploring how rights-enumeration impacts the back-and-forth that takes place between citizens in settings where formally-defined rights are not *ipso facto* necessary for citizens to cooperate. In our experimental design, we frame the "right" of individuals to take a particular action either positively or negatively, while keeping the structure of game-play the same across treatments. Every pattern of play that may occur when rights are framed positively for matched individuals can also occur when they are not, implying that differences in observed outcomes are due to differences in behavior induced by the institutional environment (Basu, 2018). This approach also allows us to investigated differences in patterns of play across treatments which lead (or not) to social coordination.

Our study is motivated by both theoretical and empirical ambiguity surrounding the value of rights within civil society. Evidence suggests that enumerated rights that govern interactions between citizens may have positive, detrimental, neutral or ambiguous effects. Of these, the rights-as beneficial arguments are the most familiar. Coase's (1960) canonical case of an externality-producing activity takes an intentionally agnostic view on how rights should be assigned: economically efficient outcomes can be contracted when rights are given to at least one affected party, and the particular *distribution* of such rights is unimportant. Well-specified and well enforced property rights also help ensure that firms and individuals have an incentive to engage in economic activities. For example, well-written patent protections, the right to sole benefit from one's innovation for a set period, may encourage firms to innovate (Mansfield 1986; Moser 2013). The ability to enforce rights granted by a contract in reasonable time and cost facilitates business activity, and is a sensible government role since it facilitates economic growth (Knack and Keffer, 1995; Norton 1998).

On the other hand, suggestive evidence exists which supports the rights-as-detrimental hypothesis. Bjornskov and Mchangama (2019) use a cross-country panel dataset to investigate the consequences of including certain positive[1] economic and social rights in constitutions. They find that the inclusion of constitutional "rights to" education, health and social security in a constitution has either a negligible or even negative medium-term impact on real measures of these outcomes. The authors hypothesize that including formal rights to (for example) education might reduce the level of provision by increasing social demand for education while also distorting the incentives of education providers. A downside of stronger rights might also emerge from their impact on grassroots interactions within civil society. Sunstein (1993) opposed expanding including positive social rights in constitutions on the grounds that they encouraged a sense of entitlement among citizens. This view is supported by Frey (1997), who argues that more explicit regulation can crowd out social virtue and diminish social trust,

[1] Positive rights are often differentiated from negative rights as implying a "right to" some claim rather than a "right from" interference. See Cross (2001).

resulting in less social cooperation, a result later verified experimentally (Bohnet, Frey and Huck, 2001). Elsewhere, empirical work demonstrated that measures of social trust are negatively correlated with the length of national constitutions (Bjornskov and Voigt, 2014).

A third possibility is that some formal rights may in fact be redundant or ineffective. The framers of the United States Constitution, for example, used the 9th Amendment to indicate that rights did not necessarily depend on their formal inclusion in the constitution.[2] More recently, work using a cross-country panel dataset demonstrated that the nominal inclusion of many individual rights in a constitution had little or no effect on the real level of rights-protection (Chilton and Versteeg, 2015). On the other hand, social norms of behavior might endure even when rights are formally assigned, implying that formal assignment is of secondary importance to social conventions. A classic study demonstrates that California cattle ranchers and landowners settled land disputes informally, even when well-defined land rights and regulations existed on the books (Ellickson, 1986).

We investigate the question of rights enumeration and its implications for constitutional design using experimental methods at the level of two person interactions: does the assignment of rights among citizens, or the *lack of assignment* of rights to citizens, affect levels of coordination and/or the *paths* by which coordination outcomes are reached? Our experimental task is a two stage repeated version of the battle of the

[2] The full text of the Ninth Amendment reads "The enumeration in the Constitution, of certain rights, shall not be construed to deny or disparage others retained by the people."

sexes (BOS) game, however, unlike experiments that examine cheap talk communication (Cooper et al., 1989; Crawford, 1998) in our game the first stage action concerns the order of play in the second stage and is binding. The BOS is an abstract representation of some key elements of society in that it features both cooperative and competitive tensions with two coordination equilibria, each of which is favored by a particular player. That not all cooperative equilibria are judged equally by each player introduces a competitive element: cooperation is necessary to preserve efficiency, but each player should prefer cooperation on their own terms.

In our design, we mimic real-world situations where there are repeated interactions in the same institutional environment by having fixed pairs of subjects interact for multiple periods. Each period has two stages of decision-making. In the first stage subjects must simultaneously announce whether they wish to choose first or second in the second stage. In the second stage subjects play the BOS in the manner determined in the first stage: simultaneously if both subjects chose to play first or second, or sequentially if one subject chose to play first and the other chose to play second. Unlike environments with pre-play communication of the cheap talk variety (e.g. Cooper et al., 1989), in our environment the first stage interaction determines the sequence of actions subjects can take in the second stage.

To explore the effect of rights on decision making we introduce treatments which alter the implied distribution of rights among subjects by *framing* the first-stage environment differently across three treatments. A typology of valence frames is discussed in Levin et al. (1998) who distinguish between risky choice, attribute and goal framing; our focus is on attribute frames. In our first (baseline) treatment, no rights are enumerated in the first stage: both subjects are simply told that they can decide whether they want to choose first or second in the second stage of a period. In the second treatment, both subjects are told that they have the right to choose first in the second stage of a period but can choose *not* to exercise that right. The third treatment places subjects in an asymmetric environment: one is told s/he has the right to choose first but can waive it, whereas the other is told s/he does not have the right to choose first but can claim it. Although the game-theoretic structure is identical across treatments, such that conventional analyses would predict no between-treatment differences, we find that different rights-distributions induced by the treatments do in fact have real effects.

In particular, we demonstrate the existence of a small but significant "entitlement effect" that mitigates cooperation among paired subjects: when both individuals are framed as being endowed with a first-mover right, they cooperate significantly less than in the other treatments over time. We also find evidence of a specific form of pro-social behavior, in which first-moving players manipulate the second-stage game so as to advantage their partner. We find differences in this behavior *between* treatments. These differences suggest that the framing of rights affects not only the frequency of coordination, but also impacts the path of game play which leads to that coordination. Examining not just outcomes but also the path of game play is important as it elucidates any behavioral deviations from otherwise standard hypotheses on outcomes. That is to say, while we think that in terms of outcomes there ought to be no or minimal treatment differences, in terms of the path of play we can observe any differences in pro-social behavior across treatments.

It is also possible to observe a number of dynamic behavioral patterns in our data. For example, subjects often share the proceeds of cooperating in the BOS over time by choosing one another's preferred coordination outcomes in alternate rounds. Such 'turn-taking' behavior (conjectured by Luce and Raiffa, 1957; formalized by Lau and Mui, 2008; and observed experimentally by Duffy, Lai and Lim, 2017) exists in all treatments, but to varying extents *across* treatments (standard game theory would predict no treatment differences due to frames, what we term "no stickiness" in our hypotheses below).

We structure the paper as follows. In section 2 we discuss previous experimental research on 'rights'; section 3 describes the design and experimental procedures. We then discuss results in terms of both final outcomes and the path of play in section 4. We conclude in section 5 with our interpretation of the data from our unique design: societies codifying a large number of rights in constitutions may face the unintended consequence of reduced pro-social behavior.

2. Literature Review

A literature addresses the assignment of rights in one shot games using experimental methods. Hoffman and Spitzer (1982) test whether agents would voluntarily strike Pareto-optimal bargains in the absence of transaction costs in an externality setting, and under various informational regimes. They find that in approximately 90% of cases, Pareto-optimal outcomes were obtained through bargaining, although such bargains were not typically mutually advantageous: the holder of a 'right' settled for a smaller share of a larger pie than could have been obtained without cooperation. In an extension of this work, rights were assigned based on the winner of a pre-negotiation game and, in a second treatment, winning the game is framed so that winners are told they have 'earned' their assigned rights (Hoffman and Spitzer, 1985). While efficiency results continued to hold across experimental versions (i.e. maximized joint payoffs), payoff divisions were affected both by whether a right had been earned or not, and by the 'earned the right' frame. Similar results were found when using a dictator game (Oxoby and Spraggon, 2008). Harrison and McKee (1985) extended Hoffman and Spitzer (1982, 1985) and exogenously varied the rights regimes. They concluded that the division of surplus from bargaining is indeed sensitive to the assignment of rights, but that the efficiency result is largely invariant to this assignment.

These studies examined efficiency in a one shot, competitive bargaining environment where the main task was to 'split the pie', and found more egalitarian behavior than was predicted by economic theory. By contrast, we use a cooperative environment in which the magnitude of social surplus varies depending on the play of subjects in a repeated interaction. By allowing repeated interaction between subjects, we can examine how cooperation develops over time depending on the framing of rights, with the result that the manner in which subjects' action space is framed affects their tendency to cooperate over time.[3] That the efficiency of outcomes is sensitive to whether 'rights' are framed in a positive or negative manner gives us a different experimental take on behavior. While our results do not overturn Coase's (1960) intuition, they do suggest

[3] See Levin et al (1998) for a typology of valence frames that affect behavior.

that the efficiency-rights invariance result found there may be more sensitive than previously established.

Other studies have used experimental methods to explore how variation in legal rules can impact behavior. Croson and Johnston (2000) varied property rights regimes across treatments to investigate how bargaining is impacted by rights-assignment. They found that the decision to enter into consensual exchange of an object (versus non-consensual taking) is strongly affected by the entitlements implied by property rights. Oxoby (2013) found evidence that giving experimental subjects the ability to constrain the choice sets of others (i.e. limit others' rights) led to greater efficiency in a public-goods contribution game.

Other work has explored how the strength of contract enforcement affects subjects' willingness to enter into contracts (Bohnet, Frey and Huck, 2001). Using a multi-round design with fixed pairings, results demonstrate that contracting does not increase monotonically in enforcement strength. That is, weaker enforcement elicited greater trustworthiness among subjects, while trustworthiness was crowded-out for medium-enforcement levels.

Finally, our results complement Frechette et al (2003, 2005) who investigate proposal power in the bargaining model of Baron and Ferejohn (1989). While there is no direct frame in this work, there is a power/entitlement effect when the odds of proposer power are altered vs. when odds are equal. Insofar as our 'rights' are similar to proposer power, we find complementary results to these analyses.

3. Experimental Design, Hypotheses and Procedure

3.1 The Battle of the Sexes

The BOS game offers an ideal vehicle for our exploration of rights frames because it captures both competitive and cooperative tensions. In the one-shot (simultaneous) version of the game with two players, each player chooses to attend either the Opera or a Fight (boxing match) that evening. Each player has a (different) favorite event: all else equal, Player I prefers to attend the Opera and Player II prefers to attend the Fight. The best option for each player is to attend their favorite event in the company of their partner, but both would rather attend the other event in the company of their partner rather than attend their favorite event alone.

The game is captured in the following matrix (with values used in our experimental implementation):

Table 1. The BOS Game

		Player II (Column)	
		Opera	Fight
Player I (Row)	Opera	(4,3)	(2,2)
	Fight	(2,2)	(3,4)

The pure-strategy Nash equilibria are (Opera, Opera) and (Fight, Fight), and both are Pareto-efficient outcomes. The game also admits a mixed strategy equilibrium in which each player randomizes over the two actions, selecting their favorite activity two-thirds of the time and their least-favorite activity one third of the time. This mixed strategy equilibrium is Pareto-inefficient, since players will mis-coordinate with probability 5/9.

In the sequential version of the BOS game, players move in a predetermined order. In the unique subgame-perfect equilibrium, the player who moves first chooses his favorite activity, and the other player best-responds to this choice by matching the chosen activity. Thus, coordination on the preferred activity of the first-mover is the predicted outcome in the sequential version.

In our experiment, described in the next subsection, subjects play a two-stage BOS game repeatedly with the same partner. This feature is important for our investigation of 'rights frames' as it allows the dynamic tension between competition and cooperation to fully develop in each pair of subjects. While repeatedly playing any equilibrium of the single stage BOS game is an equilibrium in the repeated game, the continued interaction of individuals also admits a vast set of potential dynamic behavior that could depend on the history of play within a pair.[4]

3.2 Design

To introduce framing (per Levin et al., 1998) we add a first stage to the BOS game in which players' decisions determine whether the second stage will be conducted as a simultaneous or sequential version of the BOS. While the BOS game itself does not change between our three treatments, the first stage assigns 'framed rights' to determine play in the second stage. This design allows us to explore how these treatments affect coordination and efficiency in the BOS compared to other treatments and to a standard one-stage game. In all treatments, subjects play the two-stage BOS for 60 rounds with the same partner, there is no private information and instructions are common knowledge. In the first stage of the game players resolve the order of play by making an announcement about whether they intend to move first in the second stage BOS game.

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[4] It is for this reason that our insights are obtained via repeated interaction vs. examining data from the first period only.

In the Baseline (hereafter, BL) treatment no specific rights are enumerated: both subjects are told: "The first decision is whether you want to choose first or second in the second decision." If the first (second) player chooses to move first and the second (first) does not, then the game is played sequentially with the first (second) player moving first. If both players choose to move first, or neither chooses to move first, then the game is played simultaneously.

In the Have Rights (hereafter, HR) treatment, rights are specifically enumerated. Both players were told: "You and your counterpart each have the right to choose first in the second decision." In this case, if the first (second) player waives the right and the second (first) does not then the game is played sequentially with the second (first player) moving first. If both players waive the right, or neither player waives the right, then the game is played simultaneously.

In the final Asymmetric Rights (hereafter, AR) treatment, rights are enumerated differently for different players. The column player was told: "Your counterpart has been randomly chosen to have the right to move first in the second decision. However, your counterpart can waive that right and you can claim it." The row player is given the opposite instructions - that they have the right to move first but can waive it, just as in the HR treatment. In the AR treatment, if the row player does not waive the right to move first, and the column player does not claim it, then the BOS game is played as a sequential game with the row player moving first. If the row player waives their right and the column player claims it then the BOS game is played as a sequential game with the column player moving first. If row does not waive and column claims, or row waives and column does not claim, then the BOS game is played simultaneously. The AR treatment allows us to comment on how outcomes and play are affected by an unequal rights assignment. Note, however, that all of the BL, HR, and AR treatments are strategically identical, differing only by the frame. Within each treatment, the enumeration of rights (or lack thereof) is common knowledge: each player knows the rights enumerated to the other player, and knows that the other player has this information, and so on.

3.3 Hypotheses

In our BOS game, earnings are low when subjects fail to coordinate and are high when they succeed. Since coordination in BOS games is more likely if players move sequentially, much of our analysis focuses on levels of coordination. Across treatments, a pair of subjects, or dyad, is said to *coordinate in the first stage* if it results in the second stage of the game being played sequentially. This means that exactly one participant in a pair expresses an interest to move first in the subsequent BOS game. A dyad is said to *coordinate in the second stage* if, regardless of whether the BOS is played simultaneously or sequentially, one of the two pure strategy equilibria identified in the single shot game is chosen. A dyad *coordinates in both stages* if they coordinate in the first stage *and* they coordinate in the second stage.

The thrust of our hypotheses are straightforward: conventional game theory would suggest no treatment effects on any dimension. Insofar as there are "entitlement effects" given rights frames, alternate hypotheses investigate the possibility of rights, frames and entitlements affecting coordination levels and/or paths to coordination.

Our null hypothesis is that framing does not matter and should have no effect on coordination or resulting dyadic payoffs in any stage when compared across treatments. We expect, however, to observe higher levels of stage one coordination in our BL treatment (in which subjects were not told they had a 'right to move first' but could claim it) versus our HR treatment (in which subjects were told they did have 'right to move first' but could waive it).

Hypothesis 1: Higher levels of stage one coordination in the BL vs HR treatment

This would occur because people are less likely to give up something they have than to claim something they do not have. While this result is not suggested by conventional game theory, extant research on the disparity between willingness to accept and willingness to pay suggests that people's reservation for selling an item exceeds their reservation value for buying an item (Coursey et al., 1987). If having a right changes the value of that right in a way similar to setting a reservation price for selling in the study of reservation values, we should expect subjects in the HR treatment to be more resistant to allowing their counterpart to go first in stage 2. Alternatively, having rights may make subjects feel 'entitled' thereby affecting cooperation levels in both stages, a notion that we explore in the sections below (see Hoffman and Spitzer, 1985).

Hypothesis 2: Higher levels of stage two coordination and greater earnings in the BL vs HR treatment

This is a natural consequence of the first hypothesis. While stage two coordination is unlikely to vary in rounds when players fail to coordinate in stage one, we expect to see overall stage two cooperation to increase. The earnings component of the hypothesis is a consequence of increased coordination.

Hypothesis 3: More pro-social behavior in the BL treatment.

One form that pro-social behavior might take is "I go first but we coordinate on your preferred outcome." This behavior could occur in any treatment, so a null hypothesis is that pro-social behavior is equal across treatments. If it is the case that participants are sensitive to entitlements, however, then more pro-social behavior will be observed in the BL treatment where no player feels they have an entitlement.

Hypothesis 4: No treatment effects **on the patterns of play leading to coordination.**

We expect that there will be no "stickiness" in patterns of play, *across treatments*, irrespective of whether a dyad plays a BOS sequentially or simultaneously (itself a result of stage one interaction within a round). This is not to say that we do not expect turn taking (see Lau and Mui, 2008). Our null hypothesis is that *across treatments* we do not expect to see different patterns of turn taking; this is what we term "stickiness" (across treatments and irrespective of whether play is sequential or simultaneous).

3.4 Procedure

Each treatment involved about 40 pairs of participants seated randomly at computer terminals in the CBEES laboratory at the University of Texas at Dallas or the Economics Research Lab at Virginia Tech. Prior to the start of data collection the research procedure was reviewed and approved by each university's Institutional Review Board that monitors research on human subjects. All subjects provided informed consent prior to participating. The experiment was computerized, participants read through self-paced instructions, and decisions were made using the computer keyboard (using z-tree; Fischbacher, 2007). The instructions explained the game as well as the process used to decide the order of moves.

Participants' earnings were summed and, along with a $5 show-up fee, were paid privately, in cash, at the end of the experiment. Average earnings were $26 including the show-up fee. The experiments lasted approximately one hour. A minimum detectable effect calculation at 80% power suggested each treatment should have approximately 25 pairs of subjects. There were 39 pairs in the BL treatment, 41 in the HR treatment and 41 pairs in the AR treatment. The instructions included a quiz to measure understanding on the part of the participants, and all participants correctly responded before the experiment began.

In all treatments, roles and anonymous partners were randomly assigned at the beginning of the experiment, and remained constant across the 60 rounds. Participants simultaneously decided about claiming or waiving rights in the first stage of a round, these decisions were then matched and the game was played sequentially or simultaneously as appropriate in the second stage. Subjects learned the outcome of each round at its end, and the history of play was available to participants.

4. Results

We evaluate the data by looking at how framing affects first stage decisions about how to play the BOS game, earnings and coordination in the BOS game, and observed pro-social behavior and its' persistence over time. In our analysis, dyads are indexed by i, periods by t and we define a number of variables that describe types of coordination and prosocial behavior. A dyad is said to *coordinate (or not) in the first stage* of period t if players' decisions in this stage result in the second stage BOS game being played sequentially ($First\ Stage_{it} = F_{it} = 1\ or\ 0$). A dyad is said to *coordinate (or not) in the second stage* of period t if one of the two pure strategy equilibria to the BOS game are chosen ($Second\ Stage_{it} = S_{it} = 1\ or\ 0$). A dyad *coordinates in both stages* of period t if they coordinate in both stages ($Both\ Stages_{it} = B_{it} = 1\ if\ F_{it} = 1\ and\ S_{it} = 1$) otherwise $B_{it} = 0$. Earnings ($Earnings_{it} = E_{it}$) are measured as a continuous variable in experimental dollars.

4.1 Overall Coordination and Earnings across Treatments

Our data takes a panel structure in each treatment (dyads observed through time), and we conducted two sets of formal tests of any treatment differences in first, second or both stage coordination rates, in addition to earnings.

First we averaged across dyads for each period and conducted two sided Wilcoxon non-parametric tests on three coordination variables across treatments (see Figure 1). With respect to coordination in the first stage, we find a significant difference across the AR and HR treatments (z = 2.292, p-value of 0.0219) and the AR and BL treatments (z = 2.929, p-value of 0.0034) but not the BL and HR treatments (z = -1.572, p-value of 0.1159). With respect to coordination in the second stage we found a difference across the AR and HR treatments (z = 2.683, p-value of 0.0073). Finally, with respect to coordination in both stages we found differences between the AR and HR treatments (z = 2.303, p-value of 0.0213) and the AR and BL treatments (z = 3.034, p-value of 0.0024).

We next conducted non-parametric tests across the three treatments for the three variables *without* averaging across dyads. While the pattern of results with respect to coordination in the first stage was similar to the results with dyad-averaged data, with respect to coordination in the second stage we found significant differences. Specifically, in comparing the BL and HR treatments, coordination in the second stage was significantly different (z = 2.500, p-value of 0.0124). The same held when comparing the AR and HR treatments (z = 4.474, p-value of 0.000) and the AR and BL treatments (z = 1.996, p-value of 0.0459). Coordination in both stages using non-averaged data followed the same pattern as that exhibited by the dyad-averaged data described above.

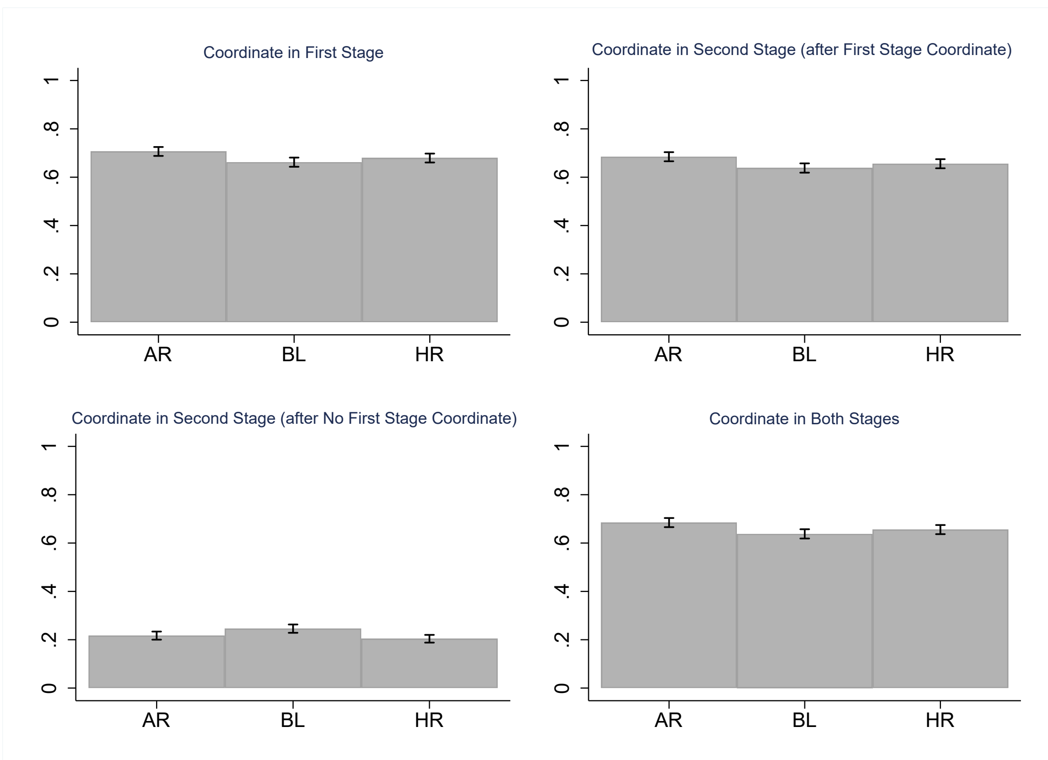

Figure 1. Coordination across Treatments (with 95% C.I., averages across dyads)

With respect to earnings (see Figure 2), when employing data averaged across dyads within a period, we observe a significant difference only across the AR and HR treatments (z = 2.683, p-value of 0.0073). Using data on earnings that are *not* averaged across dyads, however, results in significant differences across the BL and HR treatments (z = 2.500, p-value of 0.0124), the AR and HR treatments (z = 4.474, p-value of 0.0000) and also the AR and BL treatments (z = 1.996, p-value of 0.0459).

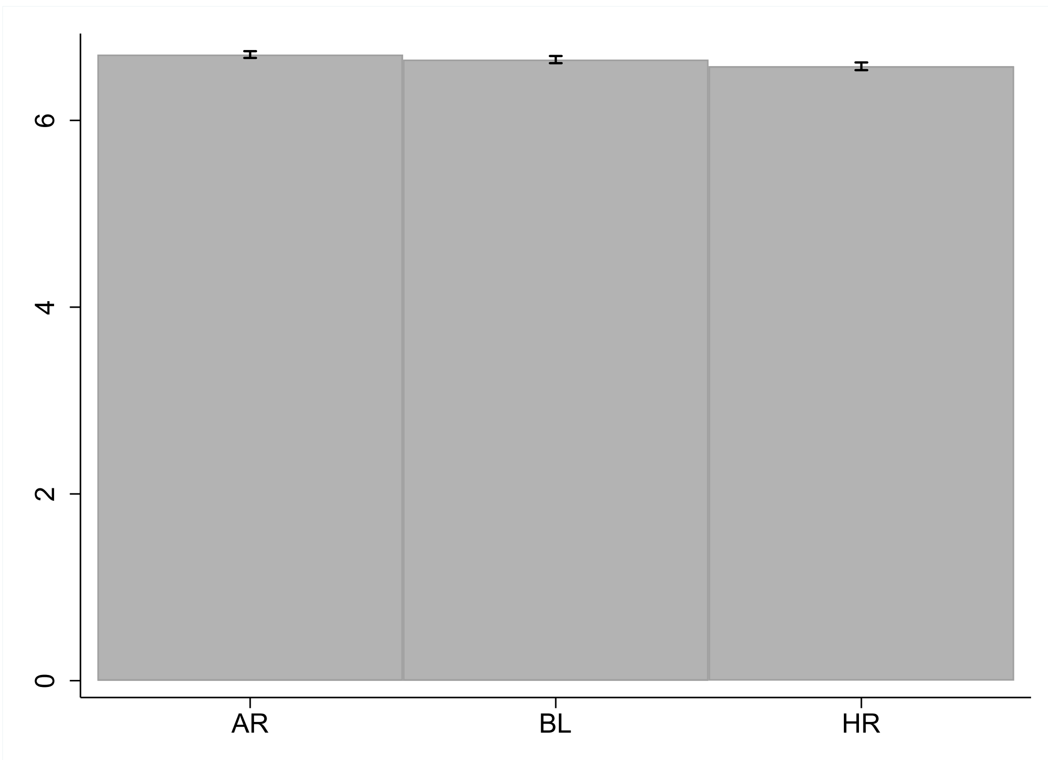

Figure 2. Average Earnings across Treatments (with 95% C.I.)

Next, we investigate treatment differences for outcomes with OLS regressions. Each column of Table 2 corresponds to a dependent variable regressed on dummy variables controlling for treatment, with the omitted category being the BL treatment.[5] Table 2 reports two sets of regressions, the first set (columns 2-5, labeled "Regressions across Groups and Time") employ all data across groups (dyads) and time (periods). Here, we find that the HR (AR) treatments decrease (increase) coordination and earnings. The second set of regressions in Table 2 (columns 6-9, labeled "Regressions across Time") employ data averaged across groups within a period. Doing so results in a time series for each treatment in which coordination in the first, second and both stages are proportions (used in OLS estimation).[6]

Table 2. Regression Results

	Regressions across Groups and Time				Regressions across Time			
Dep. Var.	Coord. in the First Stage	Coord. in the Second Stage	Coord. In Both Stages	Earnings	Coord. in the First Stage	Coord. in the Second Stage	Coord. In Both Stages	Earnings
HR	0.0783	-0.219**	0.078	-0.072***	0.017	-0.024**	0.018	-0.072**
	-0.0613	-0.087	-0.06	-0.028	-0.012	-0.001	-0.011	-0.029
AR	0.207***	0.191**	0.210***	0.054**	0.044***	0.018**	0.047***	0.054**
	-0.0628	-0.095	-0.062	-0.027	-0.012	-0.001	-0.012	-0.027
t	0.00930***	0.0248***	0.010***	0.007***	0.002***	0.002***	0.002***	0.007***
	-0.00149	-0.00226	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
Constant	0.393***	1.342***	0.259***	6.422***	0.601***	0.808***	0.568***	6.423***
	-0.0613	-0.0843	-0.0603	-0.03	-0.013	-0.011	-0.013	-0.032
LogL/R^2	-4475.01	-2545.79	-4588.45	0.021	0.284	0.46	0.348	0.46

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors below coefficient estimates.

We observe from Table 2 that coordination in the second stage is reduced in the HR treatment relative to the BL treatment, while increased in the AR treatment relative to the BL treatment.

The main results from the above non-parametric tests and regressions are as follows:

[5] We estimated the regression specifications using panel techniques as well. The log-likelihoods (and R^2) of the regressions suggested a better fit with pooled data.

[6] In addition to the specifications reported in Table 2, we estimated variations with no qualitative change in results. For example, the inclusion of a nonlinear time trend (e.g. t^2) did not change the results significantly.

Result 1: No difference in coordination in the first stage between BL and HR. AR, however, leads to significantly more coordination than BL and HR.

Result 2: Framing of rights leads to less coordination in second-stage play and lower earnings within a dyad when rights are *positively* enumerated (HR) than when they are unenumerated (BL).

4.2 Forms of Second-stage Coordination [This whole section is new]

The previous section outlined our results on overall coordination levels but did not examine the specific forms of coordination (or not) across the various treatments. In this section we define a number of behavioral patterns of play that can occur within a dyad, and present results on the frequencies of such play across treatments.

One notable type of outcome involves sequential play when the first mover gives up his own earnings to allow the second mover to earn more; that is, he defers to the second mover and takes the action that allows her to have her preferred outcome in the BOS game. We call such outcomes "pro-social."[7] Thus, a dyad is pro-social in period t if the outcome of the first period choices results in sequential play, and the second stage outcome is the non-subgame perfect equilibrium to the BOS ($ProSocial_{it} = P_{it} = 1 \; or \; 0$). Using the example from Table 1 to illustrate, suppose that, based on decisions made in the first stage, the Column player moves first and the Row player moves second in the second stage. This dyad behaves pro-socially if the Column player chooses Opera

[7] We use the term pro-social to capture the sacrifice of payoff by the first-mover in the simultaneous sense, although we recognize that other forms of behavior (such as "turn-taking" of preferred outcomes) may be seen as pro-social in a dynamic context. We thank an unnamed referee for pointing out the distinction in the nomenclature.

even though she prefers the (fight, fight) outcome, and *then* the Row player chooses Opera. Given the payoffs used in our experiment, this leads to an outcome where the first-mover (Column) earns only 3 while the second-mover (Row) earns 4.

Table X presents statistics on prosocial outcomes, including the overall frequency across treatments, the average number of such outcomes by dyad for each treatment, and the average spell[8] length of pro-social play across dyads.

Table X. "Pro-social" outcomes (Generous first-mover)

Treatment	Overall Frequency	Av. #/dyad	Av. Spell length/dyad
BL	17.50%	10.50	1.66
		(15.43)	(4.77)
HR	11.63%	6.98	0.95
		(12.26)	(2.11)
AR	16.24%	9.74	0.813
		(12.26)	(0.714)

Note: Standard deviations in parentheses. Each dyad represents 60 observations (periods 1-60).

Wilcoxon non-parametric test results across treatments found that prosocial behavior differed across treatments. Using data averaged across dyads for each round, we find significant differences across BL and HR ($z = 7.236$, p-value of 0.0000) and AR and HR ($z = 4.834$, p-value of 0.0000). Using data directly (without averaging dyads within a round) we find differences across the BL and HR treatments ($z = 5.810$, p-value of 0.0000), across the AR and HR treatments ($z = 4.621$, p-value of 0.0000) but not across the AR and BL treatments ($z = 1.158$, p-value of 0.2467).

[8] A spell is defined by consecutive periods in which $P_{it} = 1$. Since some dyads report zero episodes of pro-social play, the average spell length may be less than one.

The pro-social outcomes described above only comprise a fraction of second-stage coordination outcomes which are arrived at sequentially (i.e. conditional on first-stage coordination). The remainder involve the (subgame-perfect) stage outcomes which are favorable to the first mover. Figure 3 below depicts the observed frequencies of three different paths by which participants reached second stage coordination outcomes grouped by treatment.

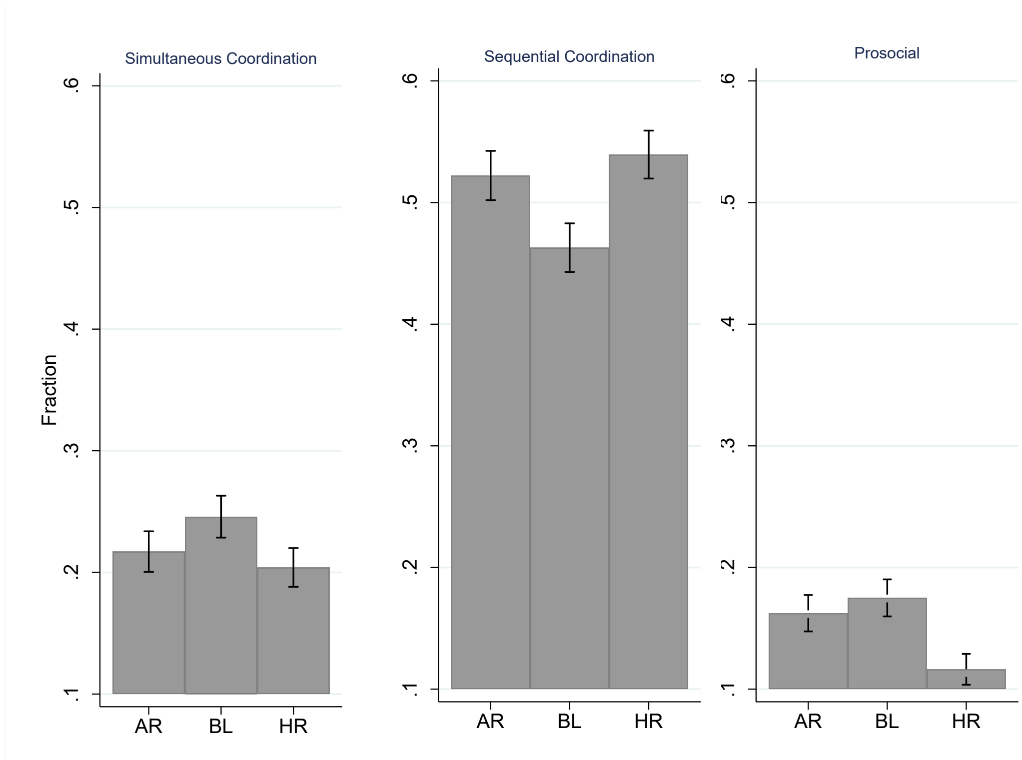

Figure 3. Paths of Play to Second-Stage Coordination across Treatments (with 95% C.I.)

From left to right, the first panel of Figure 3 represents the frequency of simultaneous coordination outcomes (by treatment); that is, those cases where dyads achieve coordination when they chose simultaneously in the second stage. The frequency of simultaneous coordination did not differ across treatments. **[IS THIS TRUE? LOOK AT BL**

AND HR] As noted above, sequential coordination can occur on either the first-mover's preferred outcome (as predicted by standard game theory) or the second-mover's preferred outcome (the pro-social case). The second panel of Figure 3 illustrates the frequency of sequential coordination outcomes, *net* of pro-social outcomes; that is, those cases where play is sequential and the dyad coordinates on the first-mover's preferred outcome. This subset of sequential coordination was lowest in the BL treatment (middle panel of Figure 3). The third panel of the Figure shows the frequency of pro-social outcomes by treatment.

We can in fact say more about the 'standard' form of sequential coordination illustrated in the middle panel of Figure 3 by defining additional patterns of play which are commonly observed in our data. Under 'turn taking', dyads alternate between coordination on the preferred choice of each player, given sequential play (1st stage coordination). We derive two measures of this behavior, one narrow and one broad. In narrow turn-taking, the variable $TakeTurnN_{it} = TTN_{it} = 1$ if coordination was on Row's (resp. Column's) choice this period and Column's (reps. Row's) choice last period and play was subgame perfect in each period, and $TTN_{it} = 0$ in all other cases. Broad turn-taking, defined by the variable $TakeTurnB_{it} = TTB_{it} = 1$ under the 'narrow' criteria above, plus any pro-social outcomes in which preferred outcomes of Row and Column switch between periods (pro-socially). Figures X and X below give the frequencies for these calculated variables. In each case only periods 2-60 are included for each dyad since turn taking involves behavior in adjacent periods: **[NEED TO CALCULATE PROPORTION TESTS**

HERE AS WELL. Hypothesis: less narrow turn taking under BL than HR. Comparable under broad measure.]

Table X. Turn-Taking (Narrow Measure)

Treatment	Overall Frequency	Av. #/dyad	Av. Spell length/dyad
BL	19.03%	11.23	6.46
		(19.64)	(13.57)
HR	24.43%	14.41	7.99
		(21.18)	(15.22)
AR	21.43%	12.64	6.94
		(19.92)	(15.17)

Note: Standard deviations in parentheses. Each dyad represents 59 observations (periods 2-60).

Table X. Turn-Taking (Broad Measure)

Treatment	Overall Frequency	Av. #/dyad	Av. Spell length/dyad
BL	43.47%	25.65	12.69
		(23.97)	(15.91)
HR	41.34%	24.39	11.92
		(23.69)	(17.89)
AR	45.76%	27.00	14.35
		(22.70)	(19.59)

Note: Standard deviations in parentheses. Each dyad represents 59 observations (periods 2-60).

An alternative variable accounts for adjacent periods in which sequential second-stage coordination occurs but turn-taking does not; we define ‘no turn-taking, but coordination’ or $NTTBC_{it} = 1$ if coordination was on Row’s (resp. Column’s) choice this period and also Row’s (resp. Column’s) choice last period, and play was either subgame perfect or pro-social in each period (and $TTN_{it} = 0$ in all other cases). Table X gives statistics for this variable. **[AGAIN NEED TO CALCULATE PROPORTION TESTS HERE AS WELL. Hypothesis: less of this under BL than HR.]**

Table X. Outcomes without turn-taking but sequential second-stage coordination (*NTTBC*)

Treatment	Overall Frequency	Av. #/dyad	Av. Spell length/dyad
BL	7.12%	4.20	1.90
		(8.83)	(3.81)
HR	13.60%	8.02	1.94
		(14.13)	(3.61)
AR	10.04%	5.92	1.58
		(9.80)	(2.68)

Note: Standard deviations in parentheses. Each dyad represents 59 observations (periods 2-60).

Finally, we turn our attention to coordination in simultaneous play. As discussed in the previous section, there are few significant differences in of first-stage coordination frequencies across treatments. However, as illustrated in figure 3, there may exist differences in *coordination* across treatments given simultaneous play. To explore this possibility, we construct a sub sample of those time-dyad pairs for which play is simultaneous ($F_{it} = 0$) and perform a random effects logistic regressions with second-stage coordination as the dependent variable, using clustered standard errors by dyad.[9] The results of this regression are presented in Table X.

Table X. Coordination Conditional on Simultaneous Play

Dep. Var.:	Coordination_t
HR	-0.750**
	(0.383)
AR	0.051
	(0.397)
t	0.025***
	(0.009)
t×HR	0.004

[9] The sample comprises 114 of 120 dyads. In 3 BL dyads and 3 AR dyads there were no episodes of simultaneous play. In total there are 2,287 observations, roughly a third of the larger data set.

	(0.014)
t×AR	-0.006
	(0.013)
Constant	0.514*
	(0.285)
$\ln(\sigma_v^2)$	2.953***
	(0.203)
LogL	-1429.28
# of obs	2,287
# of dyads	114

Note: Clustered standard errors (by dyad) in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The estimate of $\ln(\sigma_v^2)$ measures the variance attributable to individual variation in a panel.

Here we make two observations. First, conditional on playing sequentially, dyads tend to coordinate better over time, although this effect does not differ significantly by treatment. Second, coordination is significantly lower under the HR treatment compared to BL . **[Margins for logit? OLS?]**

We can summarize the results of this section as follows: **[TIDY UP DESCRIPTION]**

Result X: Less prosocial play under HR (sort of like what is now Hypothesis 3 (previously 4)).

Result Y: More "narrow" turn taking under HR. More sequential coordination without turn taking under HR.

Result Z: Conditional on sequential play, less coordination under HR (but overall similar amount of sequential play).

4.3 Persistence in Play

We now turn to results on **Hypothesis 4** concerning "stickiness" of play across treatments. We exploit the fixed-pairings, repeated-play feature of our experimental design by estimating a series of dynamic random effects logistic regressions which incorporate a lagged dependent variable in order to examine any persistence, or "stickiness", in behavior across treatments.[10] **In each case our estimates include standard errors clustered by dyad to account for the natural assumption that error terms will be correlated within groups on account of the repeated interaction of individuals within a pair. [Reference?]**

We first explore the relationship of second-stage coordination in a period t to whether or not coordination occurred in the previous period, t-1. This is the most straightforward measure of persistence since it captures to what extent any form of previous coordination – i.e. whether simultaneous or sequential – influences current coordination. Table X presents results, where the variable Coord_t is analogous to the previously defined S_{it}. To account for the lagged dependent variable, we omit period 1 from the sample. The benchmark for comparison, captured by the constant is "BL treatment, no coordination last period". We note that coordination in the current period is significantly and positively influenced by coordination last period, which likely captures the effect of dynamic strategies such as turn-taking in all treatments. The only significant

[10] Let C_{it} be the dependent variable of interest, where $C_{it} \in \{0,1\}$, i indicates group, and t indicates period. Specifically, we regress C_{it} on C_{it-1} and a set of other explanatory variables as,

$$C_{it} = \alpha + \beta C_{it-1} + \gamma X_{it} + \varepsilon_{it}$$

where the random effects assumption is captured by

$$\varepsilon_{it} = \lambda_i + \delta_t + \xi_{it}, \qquad \xi_{it} \sim f(0, \sigma^2)$$

treatment effects arise from HR: the aforementioned influence of previous coordination is significantly *stronger* for HR (at the 5% level), but the raw treatment effect is negative. In other words, compared to the BL treatment, under the HR treatment the likelihood of coordination is stronger when coordination of any kind occurred in the previous period, and weaker when it did not. No such effects are significant for AR.

Table X. Persistence of second-stage coordination (with or without first-stage coordination)

Dep. Var.:	$Coord_t$
$Coord_{t-1}$	0.966***
	(0.286)
$Coord_{t-1} \times$ HR	0.755**
	(0.359)
$Coord_{t-1} \times$ AR	0.474
	(0.396)
HR	-0.994**
	(0.469)
AR	--0.480
	(0.470)
t	0.023***
	(0.008)
$t\times$HR	0.004
	(0.010)
$t\times$AR	0.005
	(0.011)
Constant	1.352***
	(0.349)
$\ln(\sigma_v^2)$	0.533***
	(0.165)
Log L	-1939.91
# of obs	7,080
# of dyads	120

Note: Clustered standard errors (by dyad) in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The estimate of $\ln(\sigma_v^2)$ measures the variance attributable to individual variation in a panel. Estimates include 59 periods (2-60) to account for the presence of the lagged variables.

Result Y: Under HR, compared to BL, previous coordination encourages more coordination; previous non-coordination encourages non-coordination.

Persistence in turn-taking can also be explored using our method. Table X gives regression results for turn taking (both narrow and broad measures). The lagged variable in each measure describes whether the behavior in the previous period also involved turn taking, meaning that data from three adjacent periods is included in this analysis.[11] As a result, we only consider periods 3-60 in the regression.

Table X. Persistence of Sequential-period Coordination, with trading on opposite outcomes

Dep. Var.:	$TakeTurnN_t$ (Narrow)	$TakeTurnB_t$ (Broad)
$TakeTurnN_{t-1}$	3.837***	
	(0.376)	
$TakeTurnN_{t-1}$ × HR	-1.920	
	(1.269)	
$TakeTurnN_{t-1}$ × AR	-0.166	
	(0.497)	
$TakeTurnB_{t-1}$		3.322***
		(0.293)
$TakeTurnB_{t-1}$ × HR		-1.410*
		(0.855)
$TakeTurnB_{t-1}$ × AR		-0.407
		(0.529)
HR	0.883	0.465
	(0.848)	(0.727)
AR	0.659	0.256
	(0.787)	(0.671)
t	0.027***	0.025***
	(0.010)	(0.008)
t×HR	0.007	-0.004
	(0.014)	(0.011)
t×AR	-0.004	0.007
	(0.013)	(0.001)
Constant	-6.647***	-3.112***

[11] For example, the pattern t=3 Row t=2 Column t=1 Row would imply a value of TTi3= 1 and TTi2=1.

	(0.762)	(0.481)
$\ln(\sigma_v^2)$	2.778***	2.072***
	(0.289)	(0.276)
LogL	-932.28	-1619.18
# of obs	6,960	6,960
# of dyads	120	120

Note: Clustered standard errors (by dyad) in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The estimate of $\ln(\sigma_v^2)$ measures the variance attributable to individual variation in a panel. Estimates include 58 periods (3-60) to account for the presence of the lagged variables.

Result X: Broadly-defined turn-taking is less sticky under HR. i.e. turn taking is less dependent on whether it occurred immediately before.

Table X. Persistence of sequential second-stage coordination without turn-taking (*NTTBC*)

Dep. Var.:	$NTTBC_t$
$NTTBC_{t-1}$	3.050***
	(0.449)
$NTTBC_{t-1} \times$ HR	-2.110**
	(1.035)
$NTTBC_{t-1} \times$ AR	-0.986
	(0.627)
HR	0.161
	(0.587)
AR	0.022
	(0.582)
t	-0.024**
	(0.010)
$t\times$HR	0.032**
	(0.014)
$t\times$AR	0.024*
	(0.013)
Constant	-3.909***
	(0.421)
$\ln(\sigma_v^2)$	2.778***
	(0.289)
LogL	-1304.38

# of obs	6,960
# of dyads	120

Note: Clustered standard errors (by dyad) in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The estimate of $\ln(\sigma_v^2)$ measures the variance attributable to individual variation in a panel. Estimates include 58 periods (3-60) to account for the presence of the lagged variables.

Result X: Sequential coordination with no turn-taking is also less sticky under HR. i.e. coordinating in adjacent periods, but not turn taking, is less dependent on whether it occurred immediately before.

Having examined treatment differences in persistence across sequential and simultaneous first stage coordination as part of our first hypothesis, we now turn towards observed persistence in pro-social outcomes which obtain as a subset of sequential play (Table 6).

Table 6 (NEW). Persistence in Pro-Social Outcomes

Dep. Var.:	Pro-Social$_t$
Pro-Social$_{t-1}$	-4.465***
	(0.612)
Pro-Social$_{t-1}$ × HR	2.510*
	(1.311)
Pro-Social$_{t-1}$ × AR	1.313
	(1.125)
HR	-1.034
	(0.936)
AR	0.079
	(0.880)
t	0.015
	(0.013)
t×HR	-0.024
	(0.020)
t×AR	-0.003
	(0.015)
Constant	-4.107***
	(0.737)

$\ln(\sigma_v^2)$	2.953***
	(0.203)
LogL	-1429.28
# of obs	7,080
# of dyads	120

Note: Clustered standard errors (by dyad) in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The estimate of $\ln(\sigma_v^2)$ measures the variance attributable to individual variation in a panel.

Table 6 above presents dynamic logistic panel regressions to investigate whether pro-social outcomes precede one another sequentially. The benchmark case is "BL Treatment, no pro-social outcome last period." Here we see that a pro-social outcome in the period preceding is a negative predictor of a pro-social outcome this period. This (negative) effect is significantly weaker for both the HR treatment relative to BL. Overall, pro-social outcomes decline over time. That is, given the data described in Figure 1 and the subsequent tests and regressions, we find that under positively enumerated rights (the HR treatment) there are fewer episodes of pro-social behavior, but those episodes are more persistent than in other treatments (Table 6). Overall, we find:

Result 4: {redo this} One thing to note: the **only significant treatment effects are for HR. So there is something special about the assignment of rights to both versus just one (AR). It doesn't necessarily **reduce** coordination so much as change how we get there. Much less pro-social play, but seems to allow and maybe even encourage reciprocity in actions.** Dyad behavior is relatively more persistent when rights are positively enumerated (HR), in terms of simultaneous play, sequential play, and pro-sociality.

We note, however, that positive enumeration is only desirable if the starting point behavior of a dyad is pro-social to begin with, so this is a mixed result with regard to constitution design.

5. Conclusion

The issue of enumeration of rights in a society periodically resurfaces in policy debates, particularly in emerging nation-states (Selassie, 2003; Gluck and Ballou, 2014). We develop an experimental Battle of the Sexes environment to explore how enumerating rights affects advantageous coordination and pro-social behavior. Our design implements "framed" rights that give rise to potential entitlement effects. We find evidence that actively specifying rights tends to reduce coordination levels and that pro-social behavior is influenced by such "psychological rights frames." These effects hold even though pure game-theoretic considerations predict no differences across our experimental treatments.

Our results can help to inform the problem of constitutional design by demonstrating how the distribution of assigned rights affects efficiency in two person interactions. Constitutions can be thought of as coordinating devices which help guide society to certain desirable equilibria (Ordeshook, 2002). A central theme in our findings is that differential framing of rights can affect the ways in which coordination occurs or does not. In particular, we observe that equal and positively enumerated rights are associated with less pro-social behavior, as characterized by individuals seeking to coordinate to the benefit of others. The unintended consequence of positively specifying rights may therefore be to "crowd out" citizens' tendency to be pro-social, an outcome supportive of Frey’s (1997) concept of civic virtue. Given our finding that pro-social behavior is linked with greater earnings and efficiency, this observation implies that more enumerated rights may not always be better.

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