



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 framing impacts how citizens interact with each other. We design and implement a novel experiment to test whether social cooperation depends on the enumeration and positive or negative framing of the right of subjects to take a particular action. 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.

Keywords Constitutional design · Coase theorem · Framing · Preferences · Rights · Battle of the sexes

JEL Classification D71 · P48

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 & 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 & Tullock, 1962). It has long been argued that a relatively

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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, 1971). Similarly, environments in which individuals “have (property) rights” can prevent capricious governments from expropriating private property, thus incentivizing investment, trade, and economic growth (Besley, 1995; Besley & Ghatak, 2010; Moral-Benito, 2012).

In this study, rather than focusing on interactions between individuals and government, we explore via an experiment how the enumeration and framing of rights affects cooperation *between citizens* when they must work together to achieve desirable outcomes.¹ We compare a baseline environment where a specific action is allowed but not enumerated with two others where rights are enumerated using either a positive or negative frame. We hypothesize that the enumeration and framing of rights may affect not only the interaction between citizens and government but may also affect behavioral social norms and thus interactions between citizens. For example, the US Constitution does not codify an explicit right to privacy, although legal scholars have argued that there is an implied right to privacy (Douglas, 1964). A constitutional change establishing an enumerated privacy right could change existing social norms, causing citizens to insist on a higher level of privacy than that which seems acceptable in the current environment. Conversely, the US Constitution codifies citizens’ free speech rights, potentially creating a social norm where individuals feel that it is acceptable to make statements that hurt others, and which they might keep to themselves if these rights were unenumerated.²

Our experimental design allows an exploration of the dynamics of behavior between individuals who are paired with one another continuously over many periods. A novel element of this research is that, across treatments, decision-makers essentially have the same decision-making space – the key treatment differences result from whether an action is called a “right,” and if so whether it is positively or negatively framed. Our results show that a frame of “having rights” does not necessarily encourage cooperation between citizens and may even reduce it. Moreover, and importantly, we find that *the paths* by which individuals achieve cooperation depend significantly on how rights-frames are assigned. For example, we find that the frame of “more rights” leads to less pro-social behavior. On the other hand, we show that coordination is ‘stickier’ when rights are framed positively (i.e. individuals “have rights”): paired individuals are more likely to coordinate on an action if they have done so immediately before, but less likely to coordinate if they have not. Note that in our experiment 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 investigate differences in patterns of play across treatments which lead (or not) to social coordination.

This work is motivated by both theoretical and empirical ambiguity surrounding the value of rights within civil society. We next summarize research showing that 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

¹ That the rhetoric of individuals “having rights” plays a central role in the wearing of masks in the COVID-19 pandemic continues to be promoted extensively in certain media.

² Consider, for example, the incident involving Chris Rock and Will Smith on 3/27/22 at the 94th Academy Awards.

rights are given to at least one affected party, and the *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 & Keffer, 1995; Norton, 1998).

On the other hand, suggestive evidence exists that supports the rights-as-detrimental hypothesis. Bjornskov and Mchangama (2019) use a cross-country panel dataset to investigate the consequences of including certain positive³ 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 including positive social rights in constitutions on the grounds that they encouraged a sense of entitlement among citizens, a view consistent with the spirit of our experimental findings. This view is also supported by Frey (1997), who argues that more explicit regulation can crowd out social virtue and diminish social trust, resulting in less social cooperation, a result later verified experimentally (Bohnet et al., 2001). Elsewhere, empirical work demonstrates that measures of social trust are negatively correlated with the length of national constitutions (Bjornskov & 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 Ninth Amendment to indicate that rights did not necessarily depend on their formal inclusion in the constitution, thus highlighting the existence of unenumerated rights.⁴ More recently, analysis 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 & 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 enumerating rights without changing decision-makers' sets of allowable actions 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 sexes (BOS) game, where every play of the first stage game involves an opportunity to make a statement about whether one wishes to be first mover in the second stage BOS game. However, unlike experiments that examine cheap talk communication (Cooper et al., 1989; Crawford, 1998) in our game the

³ Positive rights are often differentiated from negative rights as implying a "right to" some claim rather than a "right from" interference. See Cross (2001).

⁴ 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".

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 the two players each prefer a different equilibrium introduces a competitive element: cooperation is necessary to preserve efficiency, but each player prefers 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 enumeration and framing 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. Importantly, our three treatments do not change subjects' ability to announce whether they wish to choose first or second in the second stage of each round, instead varying the instructions surrounding these announcements. In our first (baseline) treatment, rights are not mentioned, allowing later treatments with rights to be compared to one where the same actions are allowed but those actions are not referred to as rights. Here, in the first stage of a period both subjects are told that they can decide whether they want to choose first or second in the second stage of a period. In the second treatment rights are enumerated by telling both subjects 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 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. 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 several 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 periods. Such ‘turn-taking’ behavior

(conjectured by Luce & Raiffa, 1957; formalized by Lau & Mui, 2008; and observed experimentally by Duffy et al, 2017, and also Arifovic & Ledyard, 2018) is not predicted by standard theory, yet we find it in all treatments to varying degrees.

We structure the paper as follows. In Sect. 2 we discuss previous experimental research on ‘rights’; Sect. 3 describes the design and experimental procedures. We then discuss results in terms of both final outcomes and the path of play in Sect. 4. We conclude in Sect. 5 with our interpretation of the data from our unique design: societies considering codifying a large number of rights in constitutions should consider 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 & Spitzer, 1985) – it is this last treatment that is closest to our experiments in that it does not affect whether the rights exist or not, but only how they are described to the experiment’s participants. 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 & Spraggon, 2008; Cappelin et al., 2013; Dreber et al., 2013). 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. We note that these entitlement effects might not occur in all environments (Demiral & Mollerstrom, 2020).

The above 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 conventional 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.⁵ 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

⁵ See Levin et al. (1998) for a typology of valence frames that affect behavior.

results do not overturn Coase's (1960) intuition, they do suggest 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 et al., 2001). Using a multi-period 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, Frechette et al. (2003, 2005) 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 frames' 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 as used in our experimental implementation, see Table 1):

Table 1 The BOS game

	Player II (Column)	
	Opera	Fight
<i>Player I (Row)</i>		
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 next, subjects play a two-stage BOS game repeatedly across periods 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.⁶

3.2 Design

To introduce framing (per Levin et al., 1998) we add a first stage to the BOS game in which players’ decisions in the first stage of a period where players resolve the order of play by making an announcement about whether they intend to move first in the second stage BOS for that period game. While the BOS game does not change between our three treatments, experimental treatments for the first stage within a period concern players’ rights to determine whether play in the second stage of the same period will be simultaneous or sequential. 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 periods with the same partner, there is no private information and instructions are common knowledge.

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. All players know this.

In the Have Rights (hereafter, HR) treatment, rights are positively framed. 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. All players know this.

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

⁶ It is for this reason that our insights are obtained via repeated interaction vs. examining data from the first period only.

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. All players know this. The AR treatment allows us to comment on how outcomes and play are affected by an unequal rights frame. 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 their choices result 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, within a period. 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 within a period.

The thrust of our hypotheses are straightforward: conventional game theory would suggest no treatment effects. 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 when compared across treatments. However, higher levels of stage one coordination could be observed 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 versus 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 the frame of 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, the frame of 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 & 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 periods 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 due to a lack of a rights-frame.

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

Although there are several avenues by which dyads can achieve coordination, we expect that there will be no difference 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 period). This is not to say, for example, that we do not expect turn taking (see Lau & Mui, 2008). Our null hypothesis is that *across treatments* we do not expect to see different patterns of turn taking. We also expect that there will be no differences across treatments when dyads play the simultaneous BOS game; i.e. they fail to coordinate at the first stage.

Hypothesis 5: No treatment effects on the persistence of play across treatments.

We expect that there will be no differences in the “stickiness” in the patterns of play described above, *across treatments*. We define “stickiness” as: to what extent does the type of outcome in a period depend on the same outcome having occurred in the period immediately preceding it? Again, we do not conjecture that there will be no stickiness/persistence, but rather that there will be no differences in stickiness *across treatments*.

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

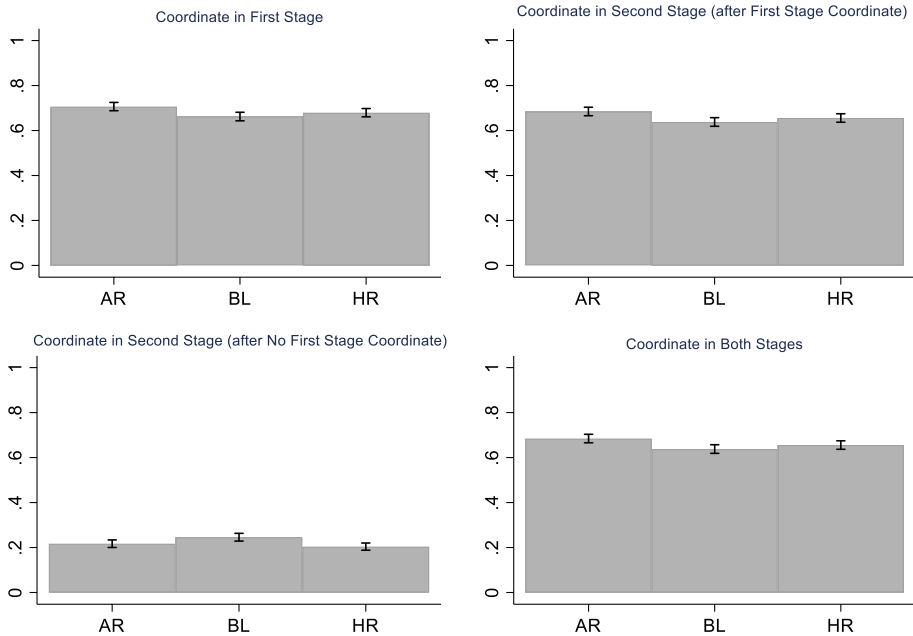


Fig. 1 Coordination across treatments (with 95% C.I., averages across dyads)

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 periods. Participants simultaneously decided about claiming or waiving rights in the first stage of a period, 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 period at its end, and the history of play was available to participants.

4 Results

We evaluate the data by looking at how framing of rights affects both outcomes and patterns of behavior across treatments. Of particular interest are (i) first stage decisions about *how* to play the BOS game, (ii) earnings and second-stage coordination in the BOS game, (iii) observed patterns in play, such as ‘pro-social’ behavior, ‘turn-taking’, and consecutive coordination without taking turns, and (iv) the persistence of coordination and specific patterns of play 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 ($FirstStage_{it} = F_{it} = 1$ or 0). A dyad is said to *coordinate (or not) in the second stage of period t* if one of the two

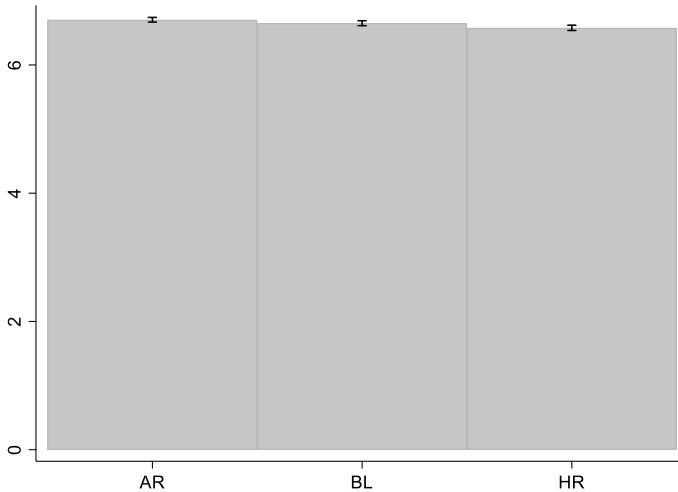


Fig. 2 Average earnings across treatments (with 95% C.I.)

pure strategy equilibria to the BOS game are chosen ($SecondStage_{it} = S_{it} = 1$ or 0). A dyad *coordinates in both stages* of period t if they coordinate in both stages ($BothStages_{it} = B_{it} = 1$ iff $F_{it} = 1$ and $S_{it} = 1$); otherwise $B_{it} = 0$. Earnings ($Earnings_{it} = E_{it}$) are measured as a continuous variable in experimental dollars.

4.1 Average coordination and earnings results

Our first set of results (Fig. 1) pertain to tests of any treatment differences in first, second or both stage coordination rates (using proportions tests), in addition to earnings (using an OLS regression). With respect to coordination in the first stage messages, we find significantly more coordination in the AR than HR treatment ($z=2.037$, $p=0.042$) and more coordination in the AR than BL treatment ($z=3.282$, $p=0.001$) but no difference in coordination between the BL and HR treatments ($z=-1.276$, $p=0.203$) having corrected for clustering across groups. Next, with respect to coordination on an equilibrium in the second stage, we observe weakly less coordination in the BL than HR treatment ($z=2.500$, $p=0.012$), significantly more coordination in AR than HR treatment ($z=4.474$, $p<0.001$) and significantly more coordination in AR than BL treatment ($z=1.997$, $p=0.046$) having again corrected for clustering across groups. With respect to coordination in both stages we find significantly more coordination in the AR than HR treatment ($z=2.130$, $p=0.033$), significantly more coordination in the AR than BL treatment ($z=3.396$, $p=0.001$) and significantly more coordination in BL than HR treatment ($z=-1.292$, $p=0.195$). Since earnings is a continuous variable we also estimate an OLS cluster corrected regression of earnings on dummies for the HR and AR treatments and a time trend. Only the time trend is significant ($\beta=0.008$, $p<0.001$) (Fig. 2).

Our conclusions from this overall analysis of testing the group-period data are as follows.

Result 1: There is no difference in coordination in the first stage between BL and HR. AR, however, leads to significantly more coordination in the first stage than either BL or HR.

Table 2 “Pro-social” ($Pro - social_{it}$) outcomes

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

Standard deviations in parentheses

Each dyad represents 60 observations (periods 1–60)

Result 2: Framing of rights in HR and AR leads to less coordination in second-stage play than BL, but the result is only weakly significant between the BL and HR treatments.

4.2 Types of second-stage coordination

The previous section outlined our results on average coordination levels; however, it misses some interesting characteristics of feasible outcomes in BOS games since the two players have different preferences over the two equilibria. For example, a player can repeatedly choose their preferred equilibrium outcome, forcing the second player to always settle for their less preferred outcome, or they can be more egalitarian and alternate between the two equilibria. One notable type of coordination that we observe in our data involves sequential play (within a period) where the first mover defers to the second mover in the second stage and takes the action that allows the second mover to have her preferred outcome in the BOS game. We call such outcomes “pro-social”.⁷ Thus, we say that 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 ($ProSocial_{it} = P_{it} = 1or0$). 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 even though she prefers the (Fight, Fight) outcome, and then the Row player chooses Opera (see Table 1). 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 4.⁸

Table 2 presents statistics on pro-social outcomes, including the overall frequency across treatments, the average number of such outcomes by dyad for each treatment, and the average spell length of pro-social play across dyads.⁹

⁷ We use the term “pro-social” to capture the implicit sacrifice of payoff by the first-mover to the second when play is sequential in a particular period. By playing “pro-socially” in a specific period, the first-mover rules out the possibility that he will obtain his preferred outcome in that period while giving his partner an opportunity to have hers instead. In this sense our definition of pro-social is a static one. We recognize that other forms of repetitive behavior (such as the “turn-taking” of preferred outcomes) could also be seen as pro-social in a dynamic context. We thank an unnamed referee for pointing out this distinction.

⁸ There are also a small fraction of observed non-coordination outcomes which may be described as “anti-social”, in which play is sequential in the second-stage but the second-mover induces an off-diagonal outcome from Table 1. An example would be, “row moves first, column moves second, row chooses Opera, column chooses Fight”. These outcomes comprise approximately 2% of the total with no significant differences across treatments.

⁹ 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.

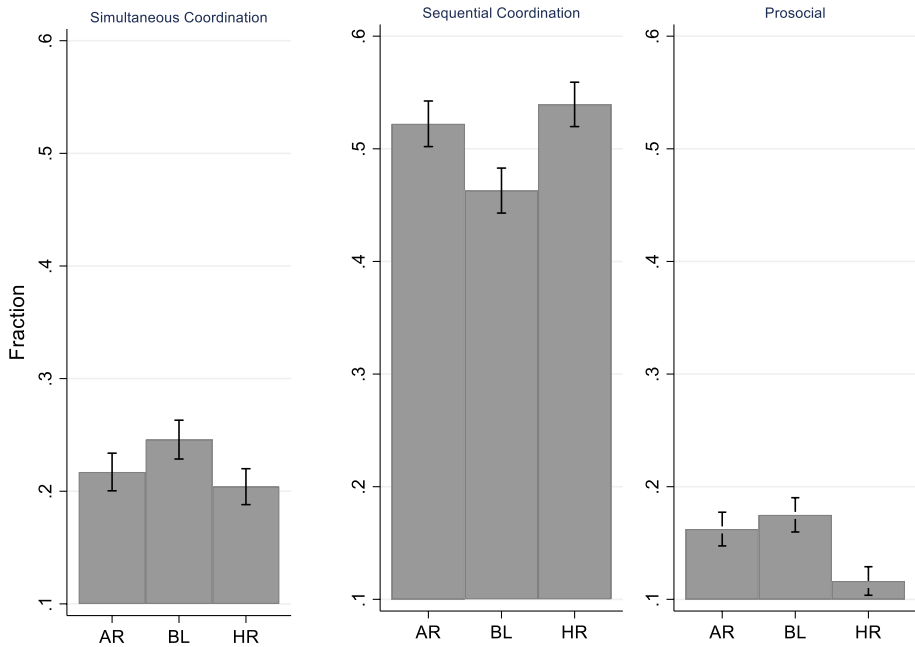


Fig. 3 Paths of play to second-stage coordination across treatments (with 95% C.I.)

Cluster correction proportions tests across treatments find prosocial behavior differed across treatments. We observe more prosocial coordination in the BL than HR treatments ($z=5.810$, $p<0.001$), more prosocial coordination in the AR than HR treatments ($z=4.621$, $p<0.001$) but no differences between the AR and BL treatments ($z=1.158$, $p=0.247$).

The pro-social outcomes described above represent the minority of second-stage coordination outcomes which are arrived at sequentially (i.e. conditional on first-stage coordination), in each treatment. 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.

The first panel of Fig. 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. 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). We find more sequential coordination in BL than HR ($z=3.488$, $p=0.001$), no difference between AR and HR ($z=1.107$, $p=0.268$) and more in BL than AR ($z=-2.345$, $p=0.019$). The second panel of Fig. 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. Here there is more sequential coordination in HR than BL ($z=-5.334$, $p<0.001$), no difference between AR and HR ($z=-1.194$, $p=0.232$), and more coordination in AR than BL ($z=4.083$, $p<0.001$). The third panel of the Figure shows the frequency of pro-social outcomes by treatment. We find more pro-social outcomes in BL than HR ($z=5.810$, $p<0.001$), more in AR

Table 3 Turn-taking (Narrow measure)

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

Standard deviations in parentheses

Each dyad represents 59 observations (periods 2–60)

Table 4 Turn-taking (Broad measure)

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

Standard deviations in parentheses

Each dyad represents 59 observations (periods 2–60)

than HR ($z=4.622$, $p<0.001$), and no difference between AR and BL ($z=-1.159$, $p=0.247$).

We can in fact say more about the ‘standard’ form of sequential coordination illustrated in the middle panel of Fig. 3 by defining other patterns of play which could be observed. 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 $TakeTurnN_{it} = TTN_{it}$, equals 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 sub-game perfect in each period ($TTN_{it} = 0$ in all other cases). Broad turn-taking, defined by the variable $TakeTurnB_{it} = TTB_{it}$, equals 1 if coordination was on Row’s (resp. Column’s) choice this period and Column’s (reps. Row’s) choice last period, regardless of whether play in each period was sub-game perfect or not ($TTN_{it} = 0$ in all other cases). The broad measure therefore captures narrowly defined turn-taking *plus* those cases where dyads take turns with pro-social outcomes in one or both adjacent periods.¹⁰ Tables 3 and 4 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.

We again perform cluster corrected tests to examine whether proportions of turn-taking differ between treatments. First, we consider the narrow measure of rights-trading. We find less turn taking in BL than HR ($z=-4.518$, $p<0.001$), less in AR than HR ($z=-2.45$, $p=0.014$), and less in BL than AR ($z=2.04$, $p=0.042$). For the broad measure, there are significant differences between AR and HR ($z=3.04$, $p=0.002$), but not AR and BL ($z=1.56$, $p=0.119$) nor BL and HR ($z=1.48$, $p=0.138$). We observe that the narrow

¹⁰ For example, in period $t-1$, a dyad might play “Row moves first, Column moves second, Coordinate on Row”, which is the subgame-perfect stage outcome. Suppose in period t the dyad plays “Row moves first, Column moves second, Coordinate on Column” (a pro-social stage outcome). This pattern of play would be defined as turn-taking in period t under the broad measure but not the narrow measure.

Table 5 No turn-taking, but repeated sequential coordination, narrow measure (*NTN*)

Treatment	Overall frequency (%)	Av. #/dyad	Av. spell length/dyad
BL	6.27	3.70 (8.88)	1.79 (3.89)
HR	12.65	7.46 (13.91)	1.91 (3.62)
AR	7.21	4.26 (9.28)	1.30 (2.81)

Standard deviations in parentheses

Each dyad represents 59 observations (periods 2–60)

Table 6 No turn-taking, but repeated sequential coordination, Broad Measure (*NTB*)

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

Standard deviations in parentheses

Each dyad represents 59 observations (periods 2–60)

version of turn taking is most pronounced in the HR treatment, but the least under the broad version. This latter result is likely due to the relative reduction in pro-social play under HR compared to the other treatments.

We also observe cases where sequential second-stage coordination occurs in adjacent periods but without taking turns: i.e. either Row-Row or Column-Column. Again we can define a broad measure which includes pro-social play and a narrow measure that does not. We define the broad measure of ‘no turn-taking, but repeated sequential coordination’ or $NTB_{it} = 1$ 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 sub-game perfect or pro-social in either period ($NTB_{it} = 0$ in all other cases). The narrow measure, NTN_{it} , equals 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 sub-game perfect in each period. Tables 5 and 6 gives statistics for these variables.

Cluster corrected tests indicate differences in repeated coordination without turn-taking across treatments. Using the narrow measure, we find less turn taking in BL than HR ($z = -7.51, p < 0.001$), and less in AR than HR ($z = -6.22, p < 0.001$), but no difference between AR and BL ($z = 1.28, p = 0.199$). Using the broad measure, BL has less turn taking than HR ($z = -7.33, p < 0.001$), AR has less than HR ($z = -3.78, p < 0.001$), and BL has less than AR ($z = 3.56, p < 0.001$).

Finally, we turn our attention to coordination in simultaneous play. As discussed in the previous section, there are no significant differences in first-stage coordination frequencies across treatments. However, there may exist differences in coordination across treatments *conditional* on simultaneous play; such differences would indicate that dyads play the simultaneous BOS game differently in different treatments. Indeed, we find evidence that this is the case: conditional on simultaneous play BL dyads coordinate 73% of the time, under AR 74% of the time but under HR only 63.5% of the time. To explore this discrepancy further, we construct a sub sample of those dyad-period observations in which play is simultaneous ($F_{it} = 0$) and perform a random effects logistic regression with second-stage

Table 7 Coordination conditional on simultaneous play

Dep. var.:	Coord _t
HR	−0.750** (0.383)
AR	0.051 (0.397)
<i>t</i>	0.025*** (0.009)
<i>t</i> ×HR	0.004 (0.014)
<i>t</i> ×AR	−0.006 (0.013)
Constant	0.514* (0.285)
ln(σ_v^2)	2.953*** (0.203)
LogL	−1429.28
# of dyads	114

Clustered standard errors (by dyad) in parentheses

The estimate of ln(σ_v^2) measures the variance attributable to individual variation in a panel

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

coordination as the dependent variable, using clustered standard errors by dyad.¹¹ The results of this regression are presented in Table 7.

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. We can summarize the results of this section as follows:

Result 3: Less pro-social play under HR compared to BL and AR.

Result 4: More episodes of “turn taking” (defined narrowly) under HR compared to BL and AR. Fewer episodes of turn-taking under HR when pro-social outcomes are included in the measure of turn-taking.

Result 5: More episodes of “no turn taking, but repeated sequential coordination” under HR compared to BL and AR, whether or not pro-social outcomes are included in the measure of turn-taking.

Result 6: Conditional on simultaneous play, there is less coordination under HR than the other treatments. That is, when the BOS game is played simultaneously in a stage, dyads are less likely to coordinate under HR.

4.3 Persistence in play

We now turn to results on our fifth hypothesis 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 to examine any persistence, or “stickiness”, in behavior across

¹¹ 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.

Table 8 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} × HR	0.755** (0.359)
Coord _{t-1} × AR	0.474 (0.396)
HR	-0.994** (0.469)
AR	-0.480 (0.470)
<i>t</i>	0.023*** (0.008)
<i>t</i> × HR	0.004 (0.010)
<i>t</i> × AR	0.005 (0.011)
Constant	1.352*** (0.349)
ln(σ _v ²)	0.533*** (0.165)
Log L	-1939.91
# of obs	7,080
# of dyads	120

Clustered standard errors (by dyad) in parentheses

The estimate of ln(σ_v²) 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

*** *p* < 0.01, ** *p* < 0.05, * *p* < 0.1

treatments.¹² In each case our estimates again 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.

We first explore the relationship of second-stage coordination in a period *t* to whether 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 8 presents results. 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 treatment effects arise from HR: the 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.

¹² Let *C_{it}* be the dependent variable of interest, where *C_{it}* ∈ {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.

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

Table 9 Persistence of turn-taking behavior

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 (0.848)	0.465 (0.727)
AR	0.659 (0.787)	0.256 (0.671)
<i>t</i>	0.027*** (0.010)	0.025*** (0.008)
<i>t</i> × HR	0.007 (0.014)	- 0.004 (0.011)
<i>t</i> × AR	- 0.004 (0.013)	0.007 (0.001)
Constant	- 6.647*** (0.762)	- 3.112*** (0.481)
ln (σ _v ²)	2.778*** (0.289)	2.072*** (0.276)
LogL	- 932.28	- 1619.18
# of obs	6,960	6,960
# of dyads	120	120

Clustered standard errors (by dyad) in parentheses

The estimate of ln(σ_v²) 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

*** *p* < 0.01, ** *p* < 0.05, * *p* < 0.1

Result 7: Compared to BL, coordination and non-coordination are more persistent under HR.

Persistence in turn-taking can also be explored. Table 9 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.¹³ As a result, we only consider periods 3–60 in the regression. The benchmark for comparison, in the regression is “BL treatment, no turn-taking last period”.

We observe that turn taking in a period is dependent on whether turn taking has occurred in the preceding period, but we find no differences in this stickiness across treatments under the narrow measure. Under the broad measure, which includes turn-taking episodes which feature pro-social play in one or more period, we find a marginally significant result that there is less stickiness under HR (significant at the 10% level). This implies that there might be a tendency for dyads in HR to take turns in a period when they have *not* done so immediately prior.

Result 8: No treatment differences in turn-taking stickiness across treatments using the narrow measure. Less stickiness under HR using the broad measure.

¹³ For example, the sequential subgame perfect in each period pattern “Coordinate on Row in period 3, Coordinate on Column in period 2, Coordinate on Row in period 1”) would imply TT₃=1 and TT₂=1 using both measures.

Table 10 Persistence of sequential second-stage coordination without turn-taking

Dep. var	NTN _t	NTB _t
NTN _{t-1}	3.603*** (0.433)	
NTN _{t-1} × HR	-2.465** (1.120)	
NTN _{t-1} × AR	-0.745 (0.628)	
NTB _{t-1}		3.050*** (0.449)
NTB _{t-1} × HR		-2.110** (1.035)
NTB _{t-1} × AR		-0.986 (0.627)
HR	0.343 (0.661)	0.161 (0.587)
AR	-0.621 (0.720)	0.022 (0.582)
<i>t</i>	-0.027** (0.010)	-0.024** (0.010)
<i>t</i> × HR	0.037** (0.015)	0.032** (0.014)
<i>t</i> × AR	0.031** (0.015)	0.024* (0.013)
Constant	-4.408*** (0.499)	-3.909*** (0.421)
Ln(σ_v^2)	1.531*** (0.275)	2.778*** (0.289)
LogL	-1023.62	-1304.38
# of obs	6,960	6,960
# of dyads	120	120

Clustered standard errors (by dyad) in parentheses

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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Similar to the above, persistence can also be examined in behavior where dyads do not take turns but do coordinate sequentially in repeated periods. Table 10 below gives regression results for the “no turn-taking” outcomes defined in the previous section: the benchmark for comparison is “BL treatment, no ‘coordination without turn-taking’ occurred last period”. Again, previous non-turn-taking (but coordination) is a good predictor of current non-turn-taking. We find *less* stickiness under the HR treatment in our results, using either the broad or narrow measure, the interpretation being that dyads under HR are more likely to coordinate without turn taking if they have also *not* done so immediately prior.

Result 9: Less stickiness in sequential coordination without turn-taking under HR using either narrow or broad measures.

Finally we turn to observed persistence in pro-social outcomes which obtain as a subset of sequential play (Table 11).

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. There is a marginally significant (negative) effect for the HR treatment relative to BL. That is, given the data described in Fig. 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 11).

Table 11 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)
<i>t</i>	0.015 (0.013)
<i>t</i> × HR	-0.024 (0.020)
<i>t</i> × AR	-0.003 (0.015)
Constant	-4.107*** (0.737)
ln(σ_v^2)	2.953*** (0.203)
LogL	-1429.28
# of obs	7,080
# of dyads	120

Clustered standard errors (by dyad) in parentheses

The estimate of ln(σ_v^2) measures the variance attributable to individual variation in a panel

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Result 10: More stickiness in pro-social behavior under HR, but less pro-social behavior overall under HR.

To recap our empirical results, there are a number of treatment effects that exist both in the patterns of coordination that occur as well as the persistence (i.e. “stickiness”) of these patterns across treatments. We note the dissimilarity of HR (where rights are positively framed to both individuals in a pair) to those where only one individual (AR) or neither (BL) individual receives a positive attribute based frame.

5 Conclusion

The issue of enumeration of rights in a society periodically resurfaces in policy debates, particularly in emerging nation-states (Gluck & Ballou, 2014; Selassie, 2003). We develop an experimental Battle of the Sexes environment to explore how enumerating and framing rights affects advantageous coordination and pro-social behavior between individuals who interact repeatedly. Note that our work differs from most previous experiments on “rights” in that our treatments do not affect an individual’s action space. We find evidence that enumerating and framing rights influences outcomes in terms of coordination levels and an analysis of the path of play suggests that pro-social behavior is also affected. These effects hold even though pure game-theoretic considerations predict absolutely no differences across our experimental treatments. We conjecture that rights enumeration and framing affects outcomes by creating entitlements thus changing the social norms affecting interactions.

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 framed 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 framing rights may therefore be to “crowd out” citizens’ tendency to be pro-social, perhaps by changing behavioral social norms, an outcome supportive of Frey’s (1997) concept of civic virtue.

This work also contributes to the literature on repeated play in BOS games, where, to our knowledge, ours is the first work to look at repeated two-stage BOS games. That pairs follow a pattern of alternating between strategies is not a new result (Rapoport et al., 1978; McKelvey & Palfrey, 2002; Sonsino & Sirota, 2003). In fact, Arifovic and Ledyard (2018) apply the Individual Evolutionary Learning model to repeated BOS games and show that the model explains the tendency to alternate between the two Nash Equilibria, even when participants do not know their opponent’s payoffs. The two-stage feature of our experimental design expands on this literature by giving matched pairs additional scope to achieve coordination in the repeated BOS, namely by allowing them to avoid the simultaneous BOS entirely by choosing playing sequentially. This design has allowed us to extend the notion of turn-taking to alternating the order of moves in repeated sequential play (i.e., “who goes first”) in addition to alternating the coordination outcomes themselves.

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Declarations

Competing interests The authors declare that there are no financial, general or institutional competing interests.

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