Behind the Screens: Does the Coase Theorem Hold Online? *

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Abstract
Rapid technological diffusion has led to a heavy reliance on digital communication media, fundamentally changing how people transact and coordinate. This paper studies how adopting digital communication impacts efficiency and welfare distributions in a Coasian bargaining experiment. After replicating seminal face-to-face bargaining experiments, we find that conducting the same experiments in a digital environment leads to a nearly fourfold increase in self-regarding behavior and a 22.9 percent decrease in efficiency. As many firms and institutions continue to wrestle with the migration of face-to-face activities to an online setting, our results offer insight into some potential effects of this transition.

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1 Introduction

Coase (1960) revolutionized how economists think about externalities with his proposal that adversaries can bargain to obtain an efficient outcome so long as property rights are well defined (Hoffman and Spitzer 1982; Medema and Zerbe 2000). However, Coase's proposal sparked some controversy by assuming away transaction costs and ignoring distributional considerations. Further, the proposal proved difficult to test using observational data because of data and measurement problems (Bertrand 2019).

Elizabeth Hoffman and Mathew Spitzer circumvented the limitations of observational data to test the predictions of Coase (1960) using carefully crafted experimental protocols (Hoffman and Spitzer 1982; 1985; 1986, HS hereafter). This series of seminal works offered support to the validity of Coase's efficiency proposal by demonstrating that bargainers routinely achieved Pareto efficiency, yet they also found that bargainers routinely violated game theoretic predictions regarding the allocation decisions of profit-maximizing agents. HS then demonstrated which factors of the bargaining environment caused violations, which provided incredible insight into how behavioral considerations affect resource allocation. Many of HS's insights lie at the core of modern behavioral economics. However, despite their obvious importance, these early seminal works have not yet been replicated.

Since the publication of this seminal work, technological diffusion and innovation have increased our dependence on telecommunication to conduct transactions, coordinate behavior, and achieve cooperative outcomes. People often negotiate digitally while interacting in secondary markets like Craigslist, eBay, or Facebook Marketplace. Telecommunication allows work groups to bargain over task allocation and coordinate remotely, allows students and academics to learn and work collaboratively from around the globe, and facilitates contract negotiation, dispute resolution, and other tasks such as counseling.

Given this, we have two goals in writing this manuscript. First, we replicate the seminal Coasian bargaining protocols implemented in HS (1982; 1985). In these experiments, we vary the strength of property rights (strong or weak) and whether subjects bargain repeatedly (one-shot or two-shot). Second, we extend the replication by providing new evidence on how efficiency and allocations change when negotiations migrate from a face-to-face to a digital setting. This yields a 2x2x2, between-subjects design wherein subjects in each treatment make ten bargaining decisions. Though others have studied how face-to-face and digital communication yield different outcomes, we are the first to implement carefully designed protocols that allow for a strict test of the Coase theorem (Coase 1960) in both communication settings to understand how the transition from a face-to-face to a digital bargaining environment impacts efficiency and allocations.

Consistent with HS (1982; 1985), we show face-to-face negotiation frequently yields Pareto efficiency (more than 80 percent of the time) with equal or near-equal allocations. This is true in both one- and two-shot bargaining settings and with both strong and weak property rights. Transitioning to a digital bargaining environment yields a 22.9 percent decrease in efficiency, constituting a significant efficiency cost. Additionally, we observe that subjects with property rights (Controllers), who can make unilateral decisions, are nearly 4 times more self-regarding in the digital environment. These Controllers consistently deny propositions that would result in higher efficiency but require them to bargain to earn payoffs at least as great as their unilateral payoff maximizing decision. Efficiency also increases as subjects gain experience in both settings. Though nearly 100 percent of bargains yield Pareto efficient allocations in the later rounds of face-to-face negotiation sessions, this number tops out at about 80 percent for digital negotiation sessions. This increase in efficiency corresponds to a decrease in the amount of money Controllers sacrifice relative to the unilateral maximum.

Our findings indicate that efficiency and fairness concerns are sensitive to migrating negotiations to a less interpersonal setting that moderates the efficacy of communication and increases social distance. Importantly, our bargaining setting preserves features of many others in which fluid communication is necessary to complete a transaction, settle a dispute, or assign responsibilities and tasks. Because of this, our findings provide insight into how changes in communication media influence coordination and pie-cutting behavior in many general negotiation settings.

2 Literature Review

2.1 Bargaining and Fairness

HS (1982; 1985) designed an experimental environment that implements the assumptions of the Coase theorem as closely as possible. We discuss their designs in detail, as they are the basis for our study. HS (1982) set up a simple bargaining problem, where groups of two (or three) subjects are randomly assigned to the roles of player A or player B, and negotiate over seven possible outcomes, with sample payoffs shown in Table 1.[1] Each row of the payoff table is numbered (0-6) and consists of specific payoffs to player A and player B. Notice that payoff number 1 is Pareto efficient, with payoffs totaling $14. The pair negotiates face-to-face over which number to choose.

Table 1. Sample Payoffs from HS (1982).

Decision Table		
Number	*Payoff to Player A*	*Payoff to Player B*
0	0.00	12.00
1	4.00	10.00
2	6.00	6.00
3	8.00	4.00
4	9.00	2.00
5	10.00	1.00
6	11.00	0.00

One member of the pair is chosen as the "Controller," and that person has "property rights" in the decision. The Controller may simply choose an outcome, and then the experiment comes to an end. But the other party can "attempt to influence the Controller to reach a mutually acceptable joint decision; the other participant may offer to pay part or all of his or her earnings to the Controller" (HS 1982, p. 83). Suppose the Controller is Player A. Notice that the Controller can choose to equalize payments on his or her own by selecting number 2 or can maximize his or her payoff by choosing number 6 (each at a

[1] The three person games involved one controller and two non-controllers but were otherwise the same. We did not replicate the three-person games, and do not discuss these treatments further.

significant sacrifice in efficiency). Alternatively, the Controller can achieve a higher total payoff for both parties by choosing number 1 and accepting a side payment from the other player. If a joint agreement is reached, both parties sign a written document stating the agreement.

HS (1982) explores several different treatments in a between-subjects design that varies two factors and assigns property rights randomly in all treatments. The first factor is the number of periods (1 or 2), which tests whether the prospect of repeated interaction enhances equal division. This behavior was of interest due to the predominance of equal division outcomes in the prior bargaining literature (see Roth 1995 for a review). The second factor is information: both players either have full information about payoffs or asymmetric information, where each player only knows his or her payoffs under the latter.

The results show strong support for the efficiency prediction of Coase (1960), with 95 percent of pairs choosing the joint-payoff-maximizing number. Repeated interaction led to a higher frequency of equal divisions (90 percent of repeated bargains compared to 33 percent of one-shot bargains).[2] Finally, some Controllers sacrificed their earnings to achieve a more equal division, accepting less than they could have guaranteed themselves on their own. This result prompted HS to further investigate the roles of entitlement and fairness in determining payoff distributions in a second study (HS 1985). HS hypothesized that randomly assigned property rights failed to create a moral basis for self-regarding behavior. The second study introduced two methods of reinforcing property rights, to give controllers a greater sense of entitlement: competition, where the role of Controller is determined by a game; and entitlement framing, where the Controllers are told they "earned" their role. The study is a 2x2, between-subjects design, with full information about payoffs. The results are shown in Table 2.

As in the previous study, HS observed high rates of efficiency: 91 percent of pairs across treatments (78 of 86) selected the payoff-maximizing number. There were no significant differences across cells in the degree of efficiency, showing that efficiency was robust to both competition and entitlement priming. The combined effect of the game plus entitlement language substantially impacted equal divisions, and their measure of inequality of payoffs (the "greed index") shows greater inequality, with both entitlement-enhancing methods leading to higher levels of the index.

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[2] Most bargaining outcomes in the repeated interaction setting involved an equal splitting of money.

Table 2. Efficiency and Sharing Outcomes from HS (1985).

	Neutral Language		Entitlement Language	
Random Entitlement	*N*	22	*N*	20
	Efficient	20 (.91)	*Efficient*	19 (.95)
	Equal division	10 (.5)	*Equal division*	9 (.47)
Game Entitlement	*N*	22	*N*	22
	Efficient	18 (.82)	*Efficient*	21 (.95)
	Equal division	9 (.5)	*Equal division*	4 (.18)

Notes: The leftmost column indicates the property rights assignment mechanism used. The top row of each property rights assignment indicates the total number of decisions made in each property rights-language treatment cell. Below the number of decisions made in each treatment cell, we present the total number of efficient decisions made, the total number of decisions where an equal division of the available surplus was realized, and in parentheses the fraction of the total number of decisions that these outcomes constitute.

Many bargaining experiments extended this work. HS (1986) considered groups of 4, 10, and 20 participants. They show that more than 90 percent of groups achieve efficiency in full- and limited-information settings for all group sizes. Cherry and Shogren (2005) further reinforce the importance of property rights; they study how transaction costs affect bargaining in settings with secure and insecure property rights and found that bargaining efficiency is inversely related to the security of property rights.[3]

2.2 Communication and Social Distance

Face-to-face communication leads to more efficient outcomes in a variety of game settings. For example, in public goods games, open face-to-face communication leads to efficient levels of cooperation and provision of public goods (e.g., Ostrom and Walker 1991; Ledyard 1995; Ahn et al. 2003; Cardenas et al. 2004; Volland and Ostrom 2010). Moreover, face-to-face interaction also makes others' payoffs more salient, leading to more other-regarding behavior in the form of equalizing payoffs (e.g., Bohnet and Frey 1999). Social distance makes communication more difficult, but it also tends to make the preferences and outcomes of others less salient. Thus, increased social distance should reduce both efficiency and other-regarding behavior.

[3] Hoffman and Spitzer's early work was part of the inspiration for many subsequent studies that explored entitlement and fairness in bargaining and in markets (Kahneman et al. 1986, 1990; Thaler 1988; Güth and Tietz 1990; Cherry et al. 2002). Hoffman et al. (1994) explored property rights and fairness in ultimatum and dictator games.

Many papers have examined the role of anonymity in bargaining games, the simplest of which is the dictator game, where one player determines the allocation of resources between themselves and another person. In effect, the "dictator" is like the Controller in the HS games. Early bargaining studies showed high levels of cooperative behavior, and the results tended to contradict simple game-theoretic models that assume payoff-maximizing agents: subjects were much too kind to each other. Hoffman et al. (1994; 1996) argued that the lack of anonymity in bargaining games might be an important factor in producing these cooperative outcomes. They developed a procedure to ensure that the dictator-game giving was anonymous and blind to the experimenter. The effect of this double-blind procedure was to substantially reduce other-regarding behavior.

Bohnet and Frey (1999) explored the role of social distance in dictator games and found that the dictators were more other-regarding when they knew more personal information about recipients.[4] Charness and Gneezy (2008) examined how behavior changes in dictator games with varying degrees of anonymity and social distance, finding that revealing some information about recipients to dictators, such as family names, caused more generosity. Thunström et al. (2016) show that dictators often prefer to reduce social distance by determining how deserving recipients are and acting on that frame by giving more to deserving recipients. Eckel and Petrie (2011) allow subjects to purchase access to a partner's photo before making a decision in a trust game and find that trust is higher when photos are purchased; both senders and responders send more money when a photo is observable and when it is purchased.

2.3 Digital vs. In-Person

Psychologists and ergonomic researchers studying digital versus in-person interactions have shown there is less reliance on social cues and more equal participation when communicating digitally (Keisler et al. 1984; Rice 1987; Adrianson and Hjelmquist, 1991; Dubrovsky et al. 1991; Hiltz et al. 1986; Weisband et al. 1995), that agreements routinely take longer online since communication is not synchronous and negotiators employ different tactics (Hiltz et al. 1986; Keisler and Sproull 1992; Valacich et al. 1993; Galin et al. 2007), and that online negotiators report feeling less satisfied with their

[4] Many subsequent papers have explored other-regarding behavior from a theoretical and experimental perspective. See Cooper and Kagel (2009) for a survey.

outcomes, less trusting of their partner, and having less desire for future interaction with the same partner (Naquin and Paulson 2003).[5]

Economists have focused on when and how communication media influence coordination, cooperation, trust, and reciprocity, and the evidence is mixed. Some studies find that digitizing communication (without a video image) reduces cooperation, coordination, and efficiency. Frohlich and Oppenheimer (1998) study prisoner's dilemma games across email and face-to-face environments and find that electronic communication is less helpful than face-to-face communication for cooperation, particularly when the nature of the decision and the content and information needing to be communicated are complex. Brosig et al. (2003) study a cooperation game using face-to-face, video, and audio communication and show that visual cues conveyed face-to-face and in video settings are a crucial component of cooperation. Bicchieri and Lev-On (2007) study a social dilemma game and find that cooperation is more difficult to establish and maintain in a computer-based setting, which is not as effective as a face-to-face setting at inducing preferences and expectations conducive to cooperation. Diermeier et al. (2008) study coalition formation and find that groups negotiating face-to-face were significantly more efficient than those using a computer (70 percent versus 11 percent). Rocco and Warglien (1996) find increases in opportunistic behavior and communication breakdown in social dilemma games in a computer-mediated setting. Online negotiation settings are also conducive to cheating (Conrads and Lotz 2015; Cohn et al. 2022) and poorer promise-making (but not promise-keeping) behavior (Conrads and Reggiani 2016).

In contrast, other researchers find that digital communication has no deleterious impact on interactions. Croson (1999) studies negotiation behavior in integrative (i.e., win-win) games and finds no losses in efficiency across the two environments and that computerized agreements are significantly more equal than face-to-face agreements. Abatayo et al. (2018) find that young adults are equally adept at achieving and sustaining cooperative agreements when communicating within an online Facebook group chat as they are in person. Galeotti et al. (2019) study how subjects trade off efficiency for equality in

[5] Bordia (1997) provides a review of early experimental studies of face-to-face versus computer-mediated communication, and Geiger (2020) provides a review of theoretical vantage points on communication media and negotiation, and summaries of empirical findings from papers over the last six decades.

online bargaining and find that subjects prefer efficiency over equality. Bochet et al. (2006) find high levels of cooperation and efficiency in voluntary contribution experiments in treatments where subjects communicate through a computer chat room and face-to-face, but not in the treatment where communication was limited to numerical signals.

People opt into negotiations more often in digital than face-to-face settings because online settings reduce confrontation costs (Gago 2019), which have been shown to lead to worse outcomes (Brooks and Schweitzer 2011). Although agents may use online chat for screening and that signaling content embedded in chat dialogues has value (Babin 2018) the literature suggests that face-to-face communication may sometimes be more effective. For instance, when the information needing conveyance has deep substance or complexity, when there is a need to establish what both individual and group interests dictate, when subtler cues are needed to engender a cooperative atmosphere, and when fairness is a concern.

3 Experimental Design & Lab Procedures

Our study replicates key elements of HS (1982; 1985) and extends their work to a computer-mediated setting. We use a 2x2x2, between-subjects design with three factors: property rights assignment, repeated bargaining, and bargaining environment. First, rather than replicate all four treatment combinations in HS (1985), we focus on the two extremes and consider two types of property rights: strong property rights (competing for rights and entitlement priming) and weak property rights (randomizing rights and no entitlement priming). Second, following HS (1982), we have subjects engage in either one-shot or two-shot bargaining. Third, we have subjects bargain either face-to-face or anonymously on a computer. Additionally, we expand the number of bargaining decisions that subjects face to increase the amount of data per subject. Subjects made a total of 10 bargaining decisions each. Subjects in one-shot sessions bargained 10 times with a total of 10 partners, and those in two-shot sessions bargained 10 times with a total of 5 partners (two periods each). The payoff table changed each bargaining period, but the structure of the payoffs was the same as in Table 1. Payoffs to both players in each period were always common knowledge (see Appendix C for a list of payoff tables used). Subjects also completed a short demographic survey after completing all bargaining periods.

We recruited undergraduate students from Texas A&M University using ORSEE (Greiner 2004) and conducted 16 sessions (2 per treatment) with 12 subjects per session (24 subjects in each treatment) between December 2016 and September 2017. Power calculations indicate that our sample sizes are sufficiently large to detect effects similar to those from HS's original results at conventional levels of significance with 80% power.

In each session, we randomly selected two bargaining decisions for payment. For two-shot sessions, we paid subjects for both bargaining decisions made with a single partner. For one-shot sessions, we paid subjects for two bargaining decisions made with two different partners.[6]

3.1 Lab Procedures for Face-to-Face Bargaining Sessions

We implement complete stranger matching for both one- and two-shot bargaining sessions, which means a bargaining pair never matched more than once in a session (see Appendix A.1 for a full description of our face-to-face matching protocols). We arranged the laboratory to maximize the distance between bargaining stations to allow privacy between bargaining pairs.[7] The same moderator read instructions aloud for each session, and we also provided paper instructions for reference.[8] We concluded instructions with a comprehension quiz that we checked individually before proceeding. During bargaining, Controllers always had the unilateral ability to choose a payoff allocation for both players, and the opportunity to entertain offers from the Bargainer to select a different allocation and or make a transfer of money between one another. Payoffs to both players were common knowledge each period. Once a pair finished bargaining and filled out and signed the contract in a period, they signaled an experimenter who collected payoff tables and the contract and instructed subjects to wait quietly until all pairs finished bargaining before continuing to the next period.

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[6] In the nine sessions that took place between December 2016 and May 2017, we paid subjects a $5 show-up fee. In the seven sessions conducted in September 2017, we paid subjects a $10 show-up fee due to a change in lab policy.

[7] About 15 feet of distance separated each bargaining station. This helped prevent bargaining parties from overhearing one another and adopting one another's bargaining strategies and provided privacy from experimenter scrutiny.

[8] See Appendix A.2 and Appendix A.3 for the instructions used in all face-to-face sessions, and Appendix A.4 for the agreement form that bargaining pairs filled out and signed after finishing each decision.

Face-to-Face Property Rights:

We allocated *weak property rights* randomly via coin flip at the pair level. If the result was heads, the subject with the lower identification number in each pair was told they were *designated* as the Controller for that period (we assigned each subject a unique identification number between 1 and 12 during check-in). We allocated *strong property rights* by having subjects play a deterministic hash mark game (see Appendix A.2), and the winner was told they had *earned the right* to be the Controller for that period.[9]

3.2 Lab Procedures for Digital Bargaining Sessions

We used the same laboratory as in the face-to-face sessions and left an empty computer station between subjects. The same moderator read instructions aloud for each session, and we also provided paper instructions for reference.[10] We concluded instructions with a comprehension quiz that we checked individually before proceeding.

We conducted all digital bargaining sessions with a digital interface programmed using ZTree (Urs Fischbacher, 2007). This program used previously generated complete-stranger matches for each period for one-shot sessions and every two periods for two-shot sessions. Bargaining in the digital environment flowed identically to bargaining in face-to-face bargaining. After Controllers selected a unilateral decision for implementation in cases of bargaining failures, Bargainers learned of this decision. Next, subjects used a chat box to bargain with one another. If subjects agreed to a mutual decision, both players could indicate this with a button provided on the chat screen. If both subjects clicked this button, then the Bargainer completed a contract and forwarded it to the Controller for approval. Controllers could refuse a contract for any reason. If a Controller refused a contract or did not engage in bargaining, the program implemented the Controller's unilateral decision and the period ended. If the Controller

[9] We asked subjects to record a strategy for this game. There is no evidence that any subject solved the game.

[10] See Appendices B.1 through B.4 for the instructions used in all digital sessions, which include screenshots of the bargaining interface at all stages.

approved the contract, then the program implemented payoffs according to the terms of the contract and the period ended.

Digital Property Rights:

We allocated *weak* property rights at the pair level via random number generation. We allocated *strong* property rights by having subjects compete in a simple addition task for time. Though this competition task is different from the one employed during face-to-face bargaining, we saw little difference in the frequency of role switching as a result. We chose a programmable task that we thought best replicated the deterministic, competitive properties of the hash mark game described above. We further discuss in the results section why we believe that this difference had no impact on behaviors across environments.

4 Results

4.1 Replication Results

In the face-to-face treatment, we first replicate the research protocols employed in the two-person, full-information bargaining treatments from HS (1982). We consider our replication successful if we obtain a significant result in the same direction as the result of interest in the original study, which is the most rigorous replication standard (as measured by relative replication rates) used in Camerer et al. (2016). HS (1982) focused on two things: testing the predictive power of Coase's theorem and understanding how strategic considerations might alter bargaining outcomes in repeated interactions. Table 3 reports the numbers and percentages (in parentheses) of efficient and 'sharing' allocations for both one- and two-shot bargaining within and across each study. Following HS, we define *sharing* as any allocation where Controller and Bargainer payoffs are within $1 of equality. For example, if the joint payoff of an allocation is $14, then ($7, $7) and ($8, $6) are sharing allocations but ($9, $5) is not.

We use Fisher's exact tests to test for statistical differences in the proportions of efficient and sharing allocations both within and across studies.[11] We observe an equivalently high proportion of

[11] Fisher's exact test is a proportions test that is designed for use within small samples. HS find no statistically significant difference in the number of efficient decisions in their one- and -two shot bargaining environments, but they do find a difference in the number of sharing decisions.

efficient decisions ($p > 0.10$, Fisher's exact test). Comparing across the two studies, Fisher's exact test indicates there are no statistically significant differences in the proportions of efficiency achieved in one-shot ($p = 0.64$) and two-shot ($p = 0.11$) bargaining. Thus, we replicate HS's efficiency results. Regarding payoff distributions, we replicate the finding that Controllers in two-shot bargaining are other-regarding. However, we observe no statistical difference in the proportion of sharing decisions between our one- and two-shot bargaining treatments ($p > 0.10$, Fisher's exact test). Thus, we fail to replicate HS's finding that repeated interaction increases other-regarding behavior.[12]

Table 3. Baseline Results for Repeated Bargaining and Comparing to HS (1982; 1985).

		HS Data	***Our Data***	***Fisher's Exact (HS vs. Us)***
1-Shot Bargaining	*N*	12	24	
	Efficient	11 (.92)	20 (.83)	$p = 0.11$
	Sharing	5 (.42)	18 (.75)	$p = 0.48$
2-Shot Bargaining	*N*	34	24	
	Efficient	32 (.94)	19 (.79)	$p = 0.11$
	Sharing	26 (.76)	19 (.79)	$p = 1.00$
Fisher's Exact (1-Shot vs. 2-Shot)	*Efficient*	$p = 1.00$	$p = 0.34$	
Fisher's Exact (1-Shot vs. 2-Shot)	*Sharing*	$p = 0.04$	$p = 0.36$	

Notes: This table presents the number of Pareto efficient and sharing decisions (proportion of total decisions in parentheses) by session type in both our and HS's experiments. We report the *p*-values for Fisher's exact test. Of the 34 observations in the 'HS Data' two-shot bargaining panel, 12 are from the two-shot full-information (coin flip) sessions in HS (1982), and 22 from the two-shot no-entitlement (coin flip) sessions in HS (1985). The 'HS Data' one-shot bargaining panel includes 12 observations, all of which come from their one-shot full-information (coin flip) sessions in HS (1982) since one-shot bargaining was not used in HS (1985). Results reported in the 'Our Data' columns (or panels) include data from only the first two periods of the face-to-face, coin flip (weak property rights) no entitlement priming sessions.

We now turn to HS (1985), which tests the role of entitlement to property rights on bargaining behavior. We present the efficiency and sharing results across property rights assignment treatments in Table 4. We find equivalently high levels of efficiency in our strong and weak property rights treatments

[12] Note that when using an equal split definition of sharing, our results do not change across one-shot and two-shot environments. However, the statistical difference in the proportion of sharing between one- and -two shot bargaining in HS disappears as there are four fewer sharing decisions in their two-shot bargaining sessions under this definition.

($p > 0.10$, Fisher's exact test). We do not find a statistically significant difference between the proportions of sharing allocations in our weak and strong property rights sessions ($p > 0.10$, Fisher's exact test). Hence, we also replicate HS's finding that the strength of property rights does not moderate efficiency but fail to replicate their finding that strengthening property rights reduces the proportion of sharing allocations obtained in face-to-face bargaining.

Table 4. Baseline Results for Property Rights Assignment and Comparing to HS (1985).

		HS Data	Our Data	*Fisher's Exact (HS vs. Us)*
Strong Property Rights / 2-Shot	*N*	22	24	
	Efficient	21 (.95)	15 (.63)	$p < 0.001$
	Sharing	7 (.32)	12 (.50)	$p = 0.245$
Weak Property Rights / 2-Shot	*N*	22	24	
	Efficient	20 (.91)	19 (.79)	$p = 0.418$
	Sharing	14 (.64)	18 (.75)	$p = 0.525$
Fisher's Exact (Strong vs. Weak)	*Efficient*	$p = 1.00$	$p = 0.34$	
Fisher's Exact (Strong vs. Weak)	*Sharing*	$p = 0.069$	$p = 0.14$	

Notes: This table presents the number of Pareto efficient and sharing decisions (proportion of total decisions in parentheses) by session type (strong vs. weak property rights) in both our and HS's experiments. All 22 observations for each panel in the HS column come from the weak property rights sessions (random entitlement plus no entitlement priming) and strong property rights sessions (game entitlement plus entitlement priming) in HS (1985). Results reported in the 'Our Data' column include data from only the first two periods in each respective set of two-shot treatment sessions.

We provide additional results from HS (1985) alongside our own in Table 5, which reports an Average Greed Index (AGI), a measure of self-regarding behavior introduced by HS (1985).[13] The AGI measures how much more a Controller earns for a given bargaining outcome than what he or she would have earned from an equal-split payoff. Thus, an AGI > 0 indicates an unequal payoff favoring the Controller, an AGI = 0 indicates an equal split, and an AGI < 0 indicates an unequal payoff favoring the Bargainer. Table 5 shows that similar to HS (1985), we find that strong property rights produce more self-

[13] We use the two-shot bargaining data from the first two periods of our face-to-face, weak-property-rights sessions and from our face-to-face, strong property rights sessions to compare and replicate HS (1985).

regarding behavior than weak property rights (testing $AGI_{strong} > AGI_{weak}$ yields $p = .094$). Thus, we find results consistent with those of HS that strong property rights induce more self-regarding behavior than weak property rights, as measured by the AGI.

Table 5. Impact of Entitlement and Fairness on Payoff Distributions.

	Strong Property Rights		**Weak Property Rights**	
	HS Data	***Our Data***	***HS Data***	***Our Data***
Average Greed Index	\$4.52	\$1.10	\$1.00	\$0.23

Notes: AGI is the average of the difference between a Controller's final payoff and what he or she would have earned from choosing an equal split of the total payoff for that realized decision outcome. We use 22 observations to calculate AGI for each HS panel, which come from the strong property rights sessions (game entitlement plus entitlement priming) and weak property rights sessions (random entitlement plus no entitlement priming) in HS (1985). Results reported in the 'Our Data' columns include data from only the first two periods (24 observations) in each set of two-shot treatment sessions.

To summarize, we replicate the finding that subjects negotiate efficient allocations, that efficiency is equally high in one-shot and repeated bargaining, and that efficiency is invariant to the strength of property rights. Additionally, we replicate the finding that strong property rights produce a higher AGI but fail to replicate the finding they produce a different proportion of sharing allocations than weak property rights. However, unlike HS, we do not find that one-shot bargaining produces more self-regarding behavior than does two-shot bargaining.

4.2 Extension Results: Face-to-Face vs. Digital Environment

We now turn to an analysis of the full data from our experiment and compare the two bargaining environments concerning efficiency and other-regarding behavior.

4.2.1 Efficiency

Table 6 and Figure 1 summarize Pareto efficiency in each of our four treatment types in face-to-face and digital environments. First, we note that the communication environment itself leads to stark differences in efficiency; 90 percent of the bargaining outcomes in the face-to-face treatments are Pareto efficient whereas only 67 percent are efficient in the digital treatments. Further, this difference in efficiency is present regardless of the strength of property rights or whether bargaining is repeated. To assess whether these differences are statistically significant, we use a series of Probit and Linear Probability regression models. We report the results of these regressions in Table 7.

Table 6. Efficient Outcomes

	Face-to-Face		Digital	
Strong / 1-Shot	*N*	120	*N*	120
	Efficient	112 (.93)	*Efficient*	78 (.65)
Strong / 2-Shot	*N*	120	*N*	120
	Efficient	109 (.91)	*Efficient*	84 (.70)
Weak / 1-Shot	*N*	120	*N*	120
	Efficient	106 (.88)	*Efficient*	70 (.58)
Weak / 2-Shot	*N*	120	*N*	120
	Efficient	106 (.88)	*Efficient*	91 (.75)
Total	*N*	480	*N*	480
	Efficient	433 (.90)	*Efficient*	323 (.67)

Notes: This table presents the number of Pareto efficient bargaining outcomes (proportions of total decisions in parentheses) for each of our eight treatments.

Figure 1: Pareto efficient Bargaining Outcomes

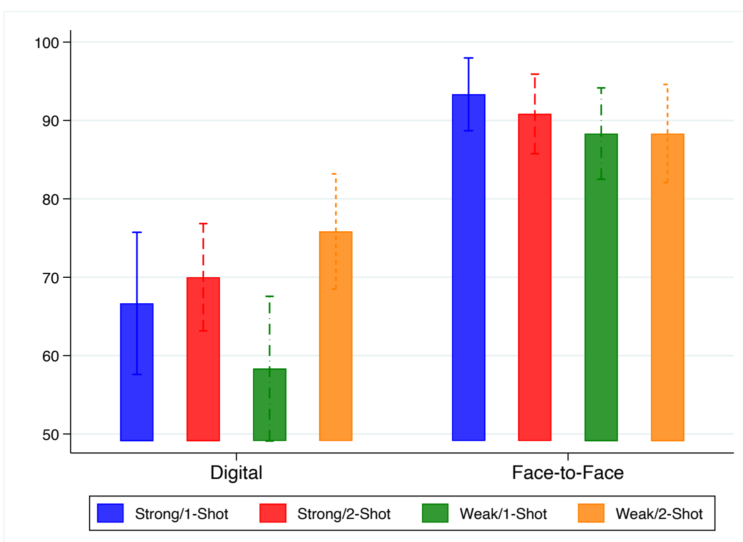

Figure 1: This table presents the percentage of Pareto efficient bargaining outcomes for each of our eight treatments, grouped by bargaining environment. We include 95% confidence intervals constructed using Wild bootstrapped standard errors.

Table 7 reports results from a series of regressions wherein we project a binary variable indicating whether a bargaining outcome was Pareto efficient onto indicator variables describing the bargaining environment. Columns (1) through (3) report results from a Probit model while columns (4) through (6) report results from a Linear Probability model. For each model type, we include a set of period fixed effects and construct standard errors in three different ways. First, columns (1) and (4) report robust standard errors. This approach assumes observations are independent, which is likely not valid in our case. Thus, we consider two approaches to address the likely serial correlation present in bargaining outcomes across periods. Columns (2) and (4) report robust standard errors clustered at the session level, which allow for correlation among bargaining outcomes within a session. Though this addresses the issue of correlated bargaining outcomes, it introduces a potential concern about our limited number of clusters (16 sessions total). Thus, columns (3) and (6) report Wild bootstrapped errors.

Table 7: Pareto Efficiency.

	(1)	(2)	(3)	(4)	(5)	(6)
		Probit Model			**Linear Probability Model**	
Face-to-Face	0.225***	0.225***	0.225***	0.225***	0.225***	0.225***
	(0.0225)	(0.0391)	(0.0234)	(0.0240)	(0.0416)	(0.0234)
Random	-0.0273	-0.0273	-0.0273	-0.0250	-0.0250	-0.0250
	(0.0234)	(0.0409)	(0.0241)	(0.0240)	(0.0416)	(0.0247)
Sequential	0.0421*	0.0421	0.0421*	0.0458*	0.0458	0.0458*
	(0.0233)	(0.0401)	(0.0242)	(0.0240)	(0.0416)	(0.0239)
Period FEs	Yes	Yes	Yes	Yes	Yes	Yes
Errors	Robust	Robust, Clustered	Wild Bootstrap	Robust	Robust, Clustered	Wild Bootstrap
N	960	960	960	960	960	960

Notes: This table reports the results of a series of regression results using Probit (columns 1 - 3) and Linear Probability models (columns 4-6). Note that we report average marginal effects from our Probit regressions. For each regression, we project a variable indicating that a bargaining outcome was Pareto efficient onto a set of indicator variables that describe the bargaining environment. Columns (1) and (4) report robust standard errors, (2) and (4) report robust errors clustered at the session level, and (3) and (6) report errors using Wild bootstrapping with 2000 repetitions. We denote statistical significance as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Regardless of the model we use or how we construct our standard errors, we see that bargaining F2F leads to a highly statistically significant increase of 22.5% in the probability of achieving an efficient bargaining outcome. However, the method of property rights assignment (random or competitive) has no effect on efficiency under any specification. Finally, we provide some evidence that repeated bargaining increases the probability of efficiency. However, whether this effect is statistically significant depends on how we specify our standard errors.

In Figure 2, we present the proportion of efficient decisions made in each treatment by period. A difference in efficiency rates across communication environments persists throughout all 10 periods in each of the one-shot treatments, and 9 of 10 periods in the strong, two-shot treatment. In the weak, two-shot treatment, differences in the percentage of efficient decisions disappear entirely by the 5th period.

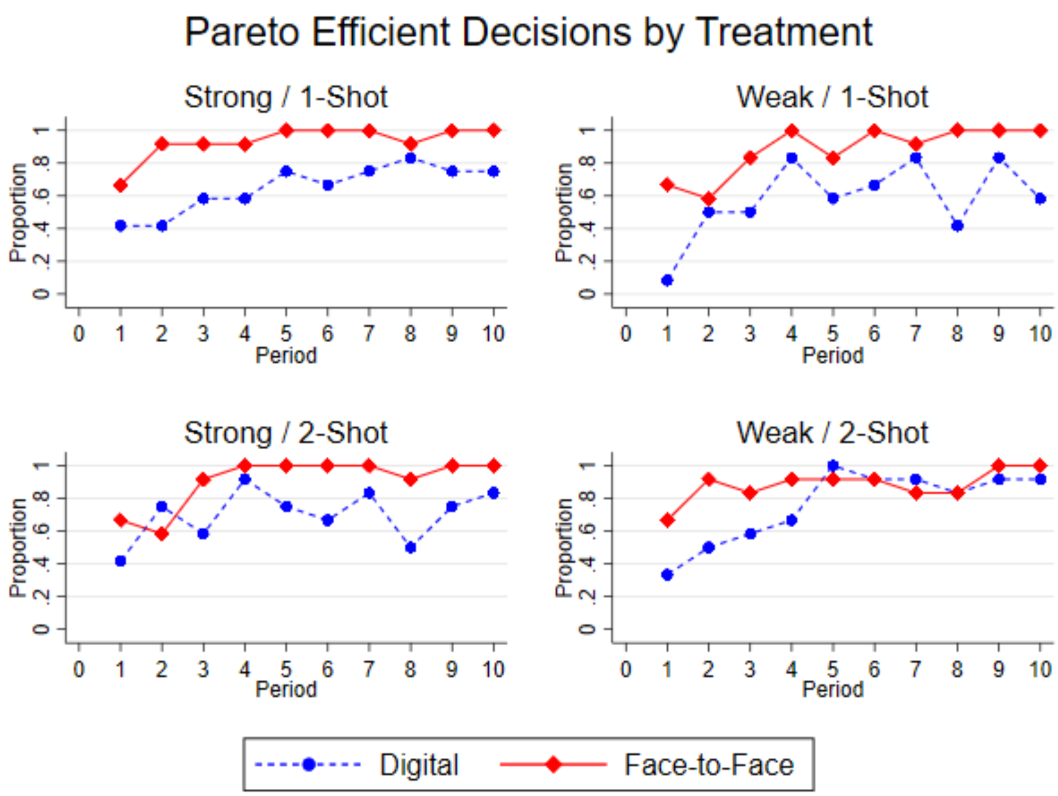

Figure 2: Each panel shows the percentage of efficient decisions made in both face-to-face and digital bargaining environments for each of our four treatment types. Each period comprises 12 decisions made by 24 subjects for each of the face-to-face and digital environments.

In Figure 3, we present the aggregate proportion of efficient decisions made in each period and environment. It shows that the gap in efficiency rates between the two environments closes over time but that learning subsides about halfway through the digital sessions and a clear difference in efficiency persists. Though subjects participating in digital bargaining can learn through experience to achieve a higher rate of efficient outcomes, on average they are unable to converge to complete efficiency as do subjects participating in face-to-face bargaining.

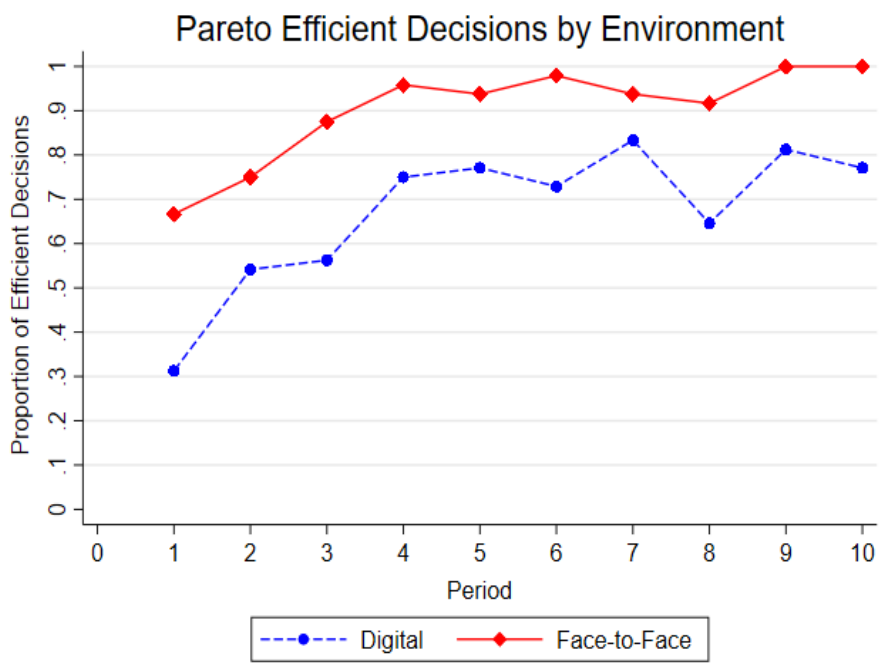

Figure 3: 96 subjects made a total of 48 decisions in each environment for each period. Subjects make significant improvements as they gain experience in early periods but learning levels out around period four. Subjects learned at about the same rate in each environment but subjects in the digital environment failed to converge to complete efficiency as did subjects bargaining in the face-to-face environment.

4.2.2 Payoff Distributions

We now turn our focus to payoff distributions. To start, we compare the AGI across bargaining environments in Table 8 and across environments by treatment and period in Figures 4a and 4b. Panel 1 of Table 8 reports the average AGI of all decisions, including equal splits, across all periods of each treatment for each bargaining environment. Panel 2 reports the same but only includes decisions that were not equal splits. Finally, panel 3 reports the proportion of decisions that were not equal splits for all eight treatments.

Table 8. Average Greed Index

Panel 1: AGI Including Equal Splits

	All Treatments	***Weak / 1-Shot***	***Weak / 2-Shot***	***Strong / 1-Shot***	***Strong / 2-Shot***
Face-to-Face	.73	.35	.26	.97	1.25
Digital	2.96	3.04	.71	4.23	3.87

Panel 2: AGI Without Equal Splits

	All Treatments	***Weak / 1-Shot***	***Weak / 2-Shot***	***Strong / 1-Shot***	***Strong / 2-Shot***
Face-to-Face	1.95	1.75	1.78	1.48	2.79
Digital	3.95	3.73	2.38	4.34	4.26

Panel 3: Proportion of Non-Equal Splits

	All Treatments	***Weak / 1-Shot***	***Weak / 2-Shot***	***Strong / 1-Shot***	***Strong / 2-Shot***
Face-to-Face	.375	.2	.2	.65	.45
Digital	.75	.82	.3	.98	.91
Observations	960	240	240	240	240

We see from column one of Panel 1 in Table 8, which includes equal split decisions, that moving from a face-to-face to a digital bargaining environment more than quadruples the AGI from 0.73 to 2.96 ($p < 0.001$ based on regression results in Table 10). Further, we see that AGI increases when moving to the face-to-face environment regardless of how we assign property rights or whether bargaining was repeated. This suggests that Controllers are more likely to behave in an individually rational way and are most self-regarding in the digital environment.

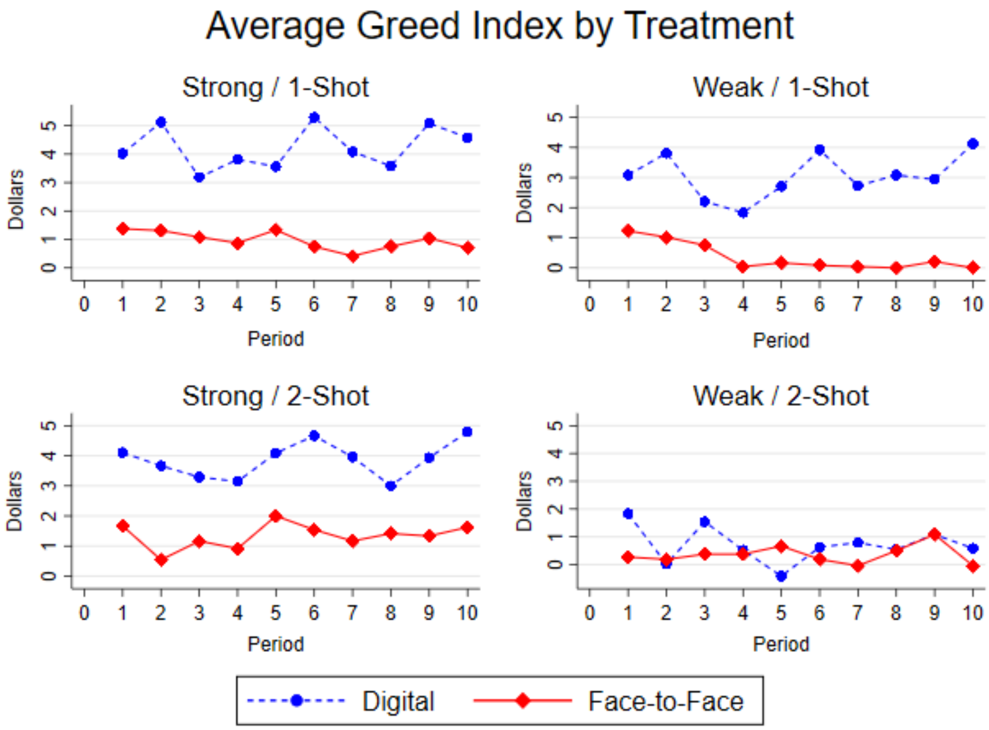

Figure 4a: Average Greed Index for each of our eight treatments.

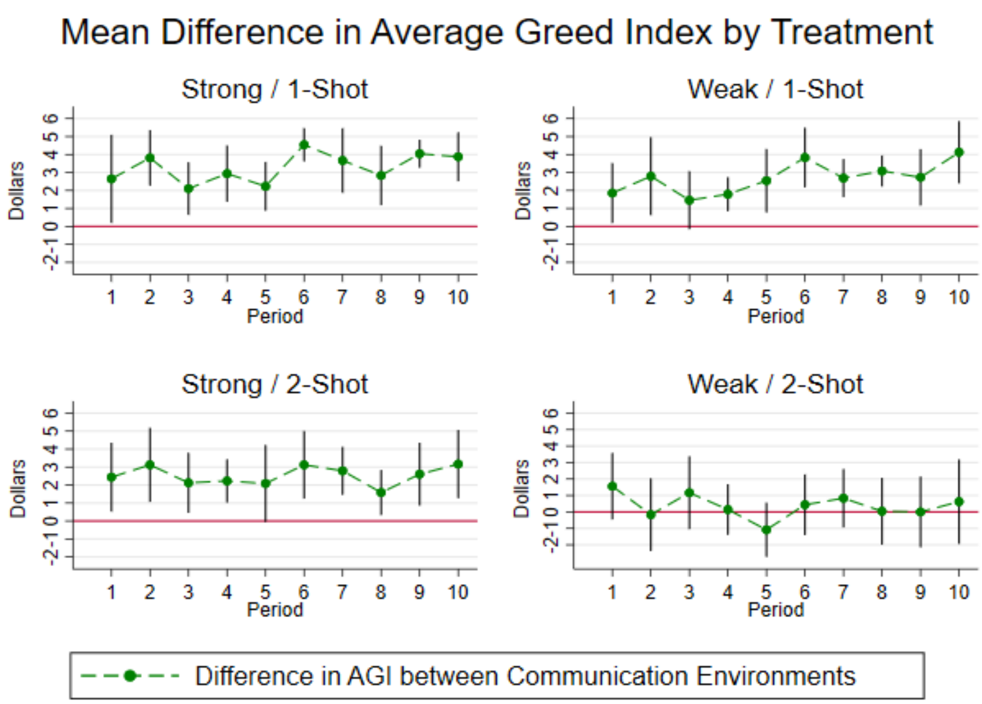

Figure 4b: The mean difference in AGI between digital and face-to-face bargaining by period and treatment. We include 95% confidence intervals constructed using Wild bootstrapped errors.

We now turn our attention to Figure 5. The four panels in this figure show the proportion of efficient decisions and corresponding payoff distributions (in terms of proportions) for each of our eight treatments. Notice that behavior is most similar across environments in our weak property rights, two-shot bargaining sessions. This is true of both efficiency and payoff distributions. In fact, Controllers and Bargainers' average per-period average earnings across environments in this treatment are statistically indistinguishable (see Table XX in Apendix XX).

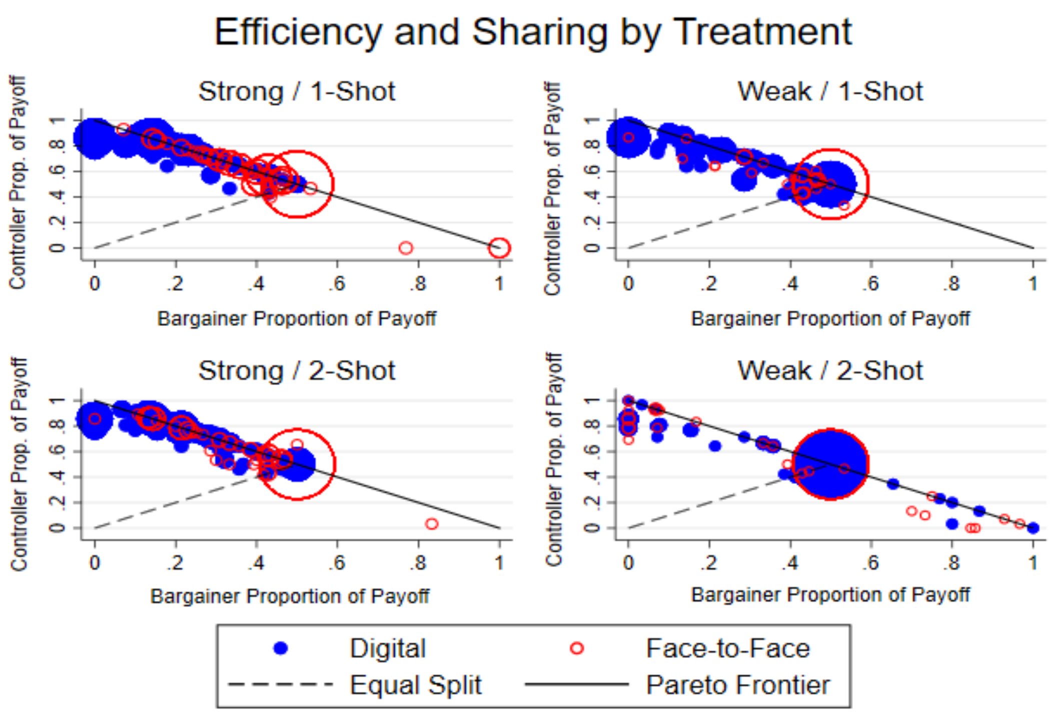

Figure 5: The proportion of efficient decisions and corresponding payoff distributions (in terms of proportions of total payoff) for each of our eight treatments. The data used in this figure include all 960 observations across all 10 periods of bargaining from both communication environments.

Removing the strategic considerations of repeated bargaining or using strong property rights both cause a large and highly significant reduction in average Bargainer earnings in digital sessions but have a relatively small and weakly significant impact in the face-to-face environment. Each change compels Controllers in digital sessions to behave in a strongly self-regarding manner. This finding aligns with the notion that Controllers in these treatments, regardless of environment, may desire to behave in a self-regarding manner, but do not do so in a face-to-face setting as this desire is dampened by a concern for the other player's payoff, or by desires to avoid uncomfortable interpersonal interaction in the face-to-face setting.

If, for example, the high level of other-regarding behavior observed in the face-to-face setting was truly driven by other-regarding preferences, then we would not expect to see such a drastic shift in payoff distributions as a result of migrating our bargaining experiment to a digital environment. We see then in the weak property rights, two-shot bargaining sessions, where both strategic concerns and moral ambiguity persist, that other-regarding behavior is invariant to the differences in our two environments.

Table 9 summarizes sharing allocations in each of our four treatment types in face-to-face and digital environments. We find that the communication environment itself impacts sharing, with the proportion of sharing allocations sharing being lower in the digital environment than in the face-to-face environment. Overall, 68 percent of outcomes were sharing allocations in our face-to-face treatments compared to only 29 percent in our digital treatments.

Table 9. Sharing Outcomes.

	Face-to-Face		**Digital**	
Strong / 1-Shot	*N*	120	*N*	120
	Sharing	54 (.45)	*Sharing*	5 (.04)
Strong / 2-Shot	*N*	120	*N*	120
	Sharing	70 (.60)	*Sharing*	12 (.10)
Weak / 1-Shot	*N*	120	*N*	120
	Sharing	103 (.87)	*Sharing*	36 (.30)
Weak / 2-Shot	*N*	120	*N*	120
	Sharing	97 (.81)	*Sharing*	88 (.73)
Total	*N*	480	*N*	480
	Sharing	324 (.68)	*Sharing*	141 (.29)

Notes: The leftmost column indicates the property rights assignment mechanism used. The top row of each property rights assignment panel indicates the total number of decisions made across face-to-face and digital sessions. Below the number of decisions made, we present the total number of decisions where a sharing division of the available surplus was realized and the proportion of the total number of decisions that this constitutes in parentheses.

Table 10 reports results from a series of regressions exploring how the bargaining environment affects allocations. Columns (1) and (2) report regression results from Probit models while columns (3) through (6) report pooled OLS regression results. For each outcome of interest, we report both robust standard errors clustered at the session level (odd number columns) and Wild bootstrapped errors (even numbered columns). Results indicated that moving from the face-to-face to the digital environment causes a decline in the probability that the Controller and Bargainer share an outcome equally (Equal Split), an increase in the amount of money a Controller earns in excess of an equal split (Greed Index), and a decline in the amount of earnings a Controller gives up relative to the unilateral maximum (Sacrifice).

Table 10: Allocations

	(1)	(2)	(3)	(4)	(5)	(6)
	Equal Split		**Greed Index**		**Sacrifice**	
Face-to-Face	0.347***	0.347***	-2.232***	-2.232***	0.170***	0.170***
	(0.0501)	(0.0218)	(0.370)	(0.139)	(0.0301)	(0.010)
Random	0.338***	0.338***	-1.464***	-1.464***	0.127***	0.127***
	(0.0511)	(0.0191)	(0.370)	(0.146)	(0.0301)	(0.011)
Sequential	0.186***	0.186***	-0.601	-0.601***	0.0469	0.0469***
	(0.061)	(0.026)	(0.370)	(0.142)	(0.0301)	(0.011)
Constant			4.346***	4.346***	.103***	.103***
			(0.356)	(0.260)	(0.028)	(0.021)
Period FEs	Yes	Yes	Yes	Yes	Yes	Yes
Errors	Robust, Clustered	Wild Bootstrap	Robust, Clustered	Wild Bootstrap	Robust, Clustered	Wild Bootstrap
N	960	960	960	960	960	960

Notes: Regression results from Probit models in columns (1) and (2) and pooled OLS in columns (3) through (6). Column titles correspond to the dependent variable. For each dependent variable, we report two types of standard errors: robust errors clustered at the session level and Wild bootstrapped errors. We denote statistical significance as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Similarly, requiring Bargainers to earn property rights via competition (rather than receive them by random assignment) decreases the probability of Bargainers and Controllers splitting an outcome equally, increases the amount of money a Controller earns in excess of the Bargainer, and decreases the amount of money a Controller is willing to sacrifice relative to the unilateral maximum outcome. Finally, we also provide some evidence that participating in one-shot bargaining decreases the likelihood of an equal split, increases the amount of money a Controller earns relative to the bargainer, and decreases the amount of money a Controller is willing to sacrifice relative to the unilateral maximum.

Table 11: Difference in Controller and Bargainer Earnings

	(1)	(2)	(3)	(4)
	Strong/1-Shot	**Strong/2-Shot**	**Weak/1-Shot**	**Weak/2-Shot**
Face-to-Face	-6.538***	-5.222***	-5.387***	-0.713
	(0.476)	(0.530)	(0.479)	(0.540)
Constant	8.467***	7.730***	6.088***	1.425***
	(0.283)	(0.366)	(0.467)	(0.398)
N	240	240	240	240

Notes: This table reports results from a series of pooled OLS regressions wherein we project the difference between the Controller's and the Bargainer's payoff onto an indicator variable for whether or not bargaining was face-to-face. Each column denotes results for the bargaining environment described by the column header. A negative coefficient indicates that the average difference between Controller and Bargainer earnings was lower in the Face-to-Face setting. We use the following to denote statistical significance: * p<.1, ** p<.05, *** p<.01

Introducing social distance and ambiguity while removing the threat of interpersonal conflict reveals to us that what HS and Harrison and McKee (1985) identified as other-regarding behavior is perhaps instead a sort of self-regarding behavior motivated by a desire to avoid interpersonal conflict, including psychological and confrontation costs such as awkwardness, embarrassment, or guilt (Jindal and Newberry 2018; Gago 2019).

4.2.3 Experience

Controllers in our digital setting are more likely to engage in unilateral decisions (Table 11), and we also see that Bargainers in the digital setting initially expect Controllers to agree to equitable allocations but modify this as they gain bargaining experience (Table 12). We take this as suggestive evidence that the increased efficiency in later bargaining periods in our digital setting results primarily from a change in Bargainer behavior. Because Controllers in this environment do not face the same interpersonal pressure during negotiations faced by those in the face-to-face environment, they more often deny disadvantageous deals they may have otherwise accepted if bargaining face-to-face. Though the effect of anonymity on self-regarding behavior is well documented, this would be the first time, to our knowledge, that an experiment has documented the impact of anonymity on efficiency in this sort of bargaining environment.

Table 11: Instances of Unilateral Maximization.

Treatment	Digital	Face-to-Face
Strong / 1-Shot	67.5%	6.7%
Strong / 2-Shot	59%	19.2%
Weak / 1-Shot	42.5%	1.7%
Weak / 2-Shot	12.5%	9.2%

Notes: Instances of Unilateral Maximization. This table reports the percentage of bargaining interactions where Controllers unilaterally maximize earnings. Differences in proportions are all highly significant across environments ($p < .001$) except for the weak property rights, two-shot bargaining treatments ($p \approx .41$).

Table 12: Requested vs. Actual Sacrifice Rates

Period	1	2	3	4	5	6	7	8	9	10
Requested Average Sacrifice	.62	.47	.38	.39	.40	.37	.26	.30	.24	.24
Actual Average Sacrifice	.21	.21	.18	.18	.22	.16	.12	.11	.16	.17
Unilateral Decisions	16	12	9	9	7	11	5	13	8	10

Notes: Sacrifice Rates. Let S be Sacrifice, U be the unilateral maximum amount available to a Controller, and B be the payoff to the Controller conditional on accepting a Bargainer's proposal. Then we define the following measure, $S = \frac{U - B}{U}$, which represents the percentage of her earnings that result from unilateral maximization she would sacrifice by accepting the proposal.

5 Practical Implications

Despite evidence of learning, we find persistent differences in the ability of subjects to find gains from trade when completing a simple negotiation task in digital and face-to-face settings. We also show that subjects take advantage of minimal guilt or social-norm repercussions during digital negotiations and distribute surplus less equally than in a face-to-face setting. We believe these findings have important practical implications for settings in which the ability to complete coordinated tasks is a function of skilled communication and in markets where impersonal negotiations increasingly occur.

First, the transition to digital bargaining emboldens Controllers to be more rigid in their bargaining positions, more often denying propositions that involve high sacrifice rates and engaging in unilateral payoff maximization (Tables 10 through 12). This places the onus on Bargainers to either fully internalize the Controller's property rights and make precise propositions or receive no payoff. We believe these results may be applicable outside simple negotiation settings. For example, DellaVigna et al. (2012) shows that individuals prefer not to give to charitable causes, but dislike saying no. Coupled with our findings, this suggests that charitable campaigns might be more successful if they avoid impersonal outreach media like email or texts.[14]

Second, our results also suggest that face-to-face interactions may lead to more successful dispute resolution. For example, this finding might relate to settings of legal arbitration like divorce where parties negotiate over resource allocation and child custody. The sudden increase in the role of telecommunications in this process may lead to an increase in failure rates and in outcomes that more heavily favor the party who has perceived bargaining power. Reducing interpersonal interaction could also increase the frequency of bargaining delays and lead to costly litigation (Fenn and Rickman 1999; Hubbard 2018). Similarly, firms should work to reduce social distance among team members whenever

[14] One potential exception is that some people may prefer the veil of the screen in certain negotiation settings. For example, evidence from Leibbrandt and List (2015) show that in a setting with minimal social interaction between employers and job applicants, women are just as likely as men to apply and enter wage negotiations when there is an explicit mention that the "wage is negotiable" in the application description. Further, digital negotiations may appeal to individuals who are more text savvy, adept at social judgment, and effective at screening conversations, which Babin (2018) suggests women are best at.

teams do not work face-to-face. This may help with task allocation, productivity, and intra-team dispute resolution.[15]

Third, digital bargaining may dampen information flow thereby increasing the difficulty of 'type detection', which is the ability to assess the counterpart's disposition (i.e., cooperative vs. non-cooperative, friendly vs. not friendly, etc.). This ability to type detect is a primary driver in cooperative decision-making in social dilemmas (He et al. 2017).

Finally, we see that the efficiency gap in our experiment does not close in most settings. This likely occurs because Bargainers do not sufficiently adjust their approach to bargaining or expectations about allocations. Some research shows that using mediators or negotiation assistants can improve bargaining outcomes in situations where simple bargaining heuristics tend to fail (Nunamaker et al. 1991; Babcock and Loewenstein 1997; Rangaswamy and Shell 1997; Larsen et al. 2021). Thus, using mediators or negotiation assistants might improve outcomes in real-world settings that mimic our digital one-shot and strong-property-rights bargaining settings.

6 Conclusion

This paper replicates and extends the seminal work of HS testing Coase (1960) in the experimental laboratory using a 2x2x2, between-subjects design that varied the method of assigning property rights, whether subjects engaged in repeated bargaining, and whether subjects bargained face-to-face or in a digital environment. As did HS, we either reinforced property rights with entitlement priming (strong property rights) or instead used neutral language (weak property rights) to further sharpen the notion of property rights in our strong property rights treatments.

Our results are consistent with several key findings from the early work of HS: subjects often choose the efficient allocation when bargaining, efficiency is equivalently high for one- and two-shot bargaining, and efficiency is invariant to the strength of property rights. We also find that weak property rights produce equitable allocations whereas strong property rights produce self-regarding behavior.

[15] For example, Greiner et al. (2014) find that cooperativeness in Ultimatum Game experiments is as high in Second Life (a virtual world setting) as it is in a laboratory setting featuring pre-decision, face-to-face communication.

However, when using comparable data, we do not replicate the finding from HS (1982) that one-shot bargaining produces more self-regarding behavior than does two-shot bargaining.[16]

We find in the digital setting that subjects choose efficient allocations significantly less often than do subjects who bargain face-to-face, conditional on subjects bargaining with strong property rights and/or in one-shot bargaining treatments. Subjects engaging in two-shot bargaining with weak property rights converge to similar behaviors (in terms of efficiency and payoff distributions) in both environments. Additionally, we find that subjects greatly improve their ability to achieve efficient bargaining outcomes with practice in both environments. This learning occurs at about the same rate in both environments and tapers out at about the same time in both environments, which suggests digital negotiation may be okay in settings where there is a repeated relationship with symmetric bargaining positions. However, if negotiations stray from this along either dimension then it might be better to interact in person or at least use communication media that foster more personal interaction.

Differences in both allocations and efficiency that arise between the face-to-face and digital settings are likely due to increased anonymity, social distance, loss of interpersonal connection, and loss of social cues in the digital setting. Given that efficiency and other-regarding behavior are not invariant to the negotiating environment, these results suggest that Coase's theorem may require additional behavioral considerations; in particular, the theorem may lack predictive power whenever negotiations occur under the veil of anonymity.

[16] We cannot rule out that this is driven by a tit-for-tat strategy.

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