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. In this paper, we study how adopting digital communication impacts allocative efficiency and welfare distributions in a Coasian bargaining experiment. Compared to a baseline where subjects bargain face-to-face, we find that digital bargaining induces a 22.5 percentage point drop in efficient decision making and a more than quadrupling in self-regarding behavior. The effects are persistent as bargainers gain experience and are also larger when bargaining is a one-shot game and when we increase the strength of property rights.

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1 Introduction

Coase (1960) revolutionized the externality problem with his proposal that well-defined property rights can facilitate allocative efficiency. While impactful, the proposal incited controversy by ignoring distributional considerations and assuming no information asymmetries or transaction costs (Hoffman and Spitzer 1982; Medema and Zerbe 2000). The proposal also proved difficult to test empirically due to data limitations and measurement problems inherent to bargaining in the field (Bertrand 2019). A series of experiments by Hoffman and Spitzer (1982; 1985; 1986, HS hereafter) addressed these issues, providing experimental support for Coase's efficiency proposal, while revealing new insight into how structural features of the bargaining setting – repeated interactions or the mechanism for assigning property rights, for example – can lead to non-core allocations that violate the basic predictions of cooperative game theory.

In the intervening decades, technological diffusion and innovation drastically increased our reliance on digitized communication to conduct transactions, coordinate behavior, and achieve cooperative outcomes.[1,2] Although digitized communication introduces many benefits relative to face-to-face interaction (e.g., speed, convenience, and supply) it also introduces new behavioral considerations such as increased social distance and anonymity, which in turn can affect the quality of communications and perhaps the allocative efficiency and welfare distributions arising from bargaining.[3]

Our goals in this paper are twofold. First, we seek to test whether Coase's efficiency prediction holds when bargaining transpires via a digital medium rather than face-to-face. Second, we seek to study how the digital transition affects welfare distribution. To do this, we adopt and modify the experimental

[1] Facebook Marketplace saw 18 million new listings during May 2017 and estimates this new market activity led to growth of 77 percent in users (Cohen 2017). Similarly, just two years after its launch in 2015, Letgo saw 75 million downloads of its online marketplace application, 200 million listings, and 3 billion messages (Lowe 2017).

[2] From 2018 through 2022, online collaboration platforms Microsoft Teams and Slack saw their daily active userbase grow from 8 million to 270 million and 18 million, respectively (Curry 2023).

[3] A large literature has studied the role of social distance and anonymity in various settings. Examples include: Roth and Malouf (1979); Hoffman et al. (1994); Hoffman et al. (1996); Laury et al. (1995); Eckel and Grossman (1996); Bohnet and Frey (1999); Valley et al. (2002); Naquin and Paulson (2003); Dufwenberg and Muren (2005); and Charness and Gneezy (2008).

protocols used in Hoffman and Spitzer (1982; 1985; 1986, HS hereafter), which test the predictions of Coase's theorem and how the strength of property rights and repeated bargaining affect allocations.

This yields a 2x2x2, between-subjects experiment that varies the bargaining environment (face-to-face or digital chat), the strength of property rights (strong or weak), and whether bargaining is repeated (one-shot or two-shot).[4] Subjects in our experiment bargain with full information and make a total of 10 bargaining decisions, which enables us to study the evolution and persistence of bargaining behavior. Though others have studied differences in face-to-face and digital communication, we are the first to implement carefully designed experimental protocols that allow for a strict test of how this change in the communication medium impacts efficiency and allocations in Coasian bargaining.

We have four main results. First, we show that transitioning to a digital bargaining environment is costly. Compared to face-to-face bargaining, digital bargaining reduces efficient decision-making by 22.5 percent.

Second, we observe that subjects with property rights (Controllers), who can make unilateral decisions, are over four times as self-regarding when bargaining digitally.

Third, we find the drop in efficiency is driven by the increase in self-regarding behavior. Digital Controllers consistently deny proposals from Bargainers (i.e., subjects without property rights) that would result in higher efficiency but require them to sacrifice earnings or bargain to achieve their unilateral maximum payoff.

Fourth, we find that efficiency increases in both bargaining environments as subjects gain experience. 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.

[4] We use chat to match typical bargaining interactions in online secondary markets like eBay, Craigslist, Facebook Marketplace, OfferUp, and LetGo. Although some online secondary markets, such as Facebook Marketplace, offer video conference calling as a complementary communication mechanism, the use of videos during negotiations is infrequent. In fact, in 2005, eBay purchased Skype in an attempt to enhance communication among users via video. However, in 2009 it abandoned this idea and began to divest, noting it overestimated consumer demand for the service (eBay Inc. 2014). A recent report from the IMF notes that dealers in over the counter (OTC) securities markets also increasingly rely on text messages to facilitate bilateral negotiations (Dodd 2023).

The increase in efficiency corresponds to a decrease in the amount of money Controllers sacrifice relative to their unilateral maximum. This implies that the experiential efficiency gains achieved in the digital environment primarily result from a change in non-Controller bargaining behavior.

Our findings suggest a tradeoff exists between efficiency and the form of individual rationality predicted by cooperative game theory. On one hand, Coase's efficiency predictions hold when bargaining occurs face-to-face, but Controllers consistently give up earnings. On the other hand, migrating bargaining to a digital setting significantly reduces efficiency while largely restoring Controllers' individual rationality. This is true in all treatments except with both repeated interactions and weak property rights. Why?

Removing the strategic considerations of repeated bargaining or using strong property rights compels digital Controllers to behave in a strongly self-regarding manner. This finding aligns with the notion that Controllers in these treatments, regardless of environment, prefer self-regarding behavior but abstain when bargaining face-to-face, potentially to avoid interpersonal conflict (Gago 2019) or other nonpecuniary costs associated with initiating a negotiation (Jindal and Newberry 2018). If instead the high level of equitability we observe in face-to-face bargaining was truly driven by other-regarding preferences, we would not see a drastic shift in payoff distributions between bargaining environments.

This is supported by the two main changes in bargaining behavior we observe between bargaining environments. First, digital Controllers are significantly more likely to deny Bargainer proposals and instead form unilateral decisions. Second, as digital Bargainers gain experience, they propose increasingly less equitable allocations that favor Controllers, which leads to an increase in efficiency over time in our digital bargaining sessions.

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 own 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.[5] 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 randomly 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 own payoff by choosing number 6 (each at a 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.

[5] 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.

HS (1982) explored 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 enables a test of whether 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 have either full or asymmetric information about payoffs, where each player only knows his or her own 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).[6] Finally, some Controllers sacrificed their own earnings to achieve a more equal division, accepting less than they could have guaranteed themselves by making a unilateral decision. 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.

[6] 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, and found that more than 90 percent of groups achieve efficiency in full- and limited-information settings for all group sizes. Cherry and Shogren (2005) further reinforced the importance of property rights; they studied 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.[7]

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, 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

[7] 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 (e.g., 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.

outcomes of others less salient. Thus, increased social distance should reduce both efficiency and other-regarding behavior. A closely related paper to ours is Valley et al. (2002), which shows that face-to-face communication can increase instances of trade in a double-auction experiment with private information relative to written communication and no-communication. Though similar, our paper differs from theirs in that we explore how communication media impacts both efficiency and allocations in a Coasian bargaining environment with clearly defined property rights, common information, and unilaterally imposed outside options. Further, our experiment can show how various methods of communication interact with behavioral features of the bargaining environment, including how we assign property rights and whether interactions are one-shot or repeated.

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 player. 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.[8] 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) showed that dictators often prefer to reduce social distance by determining how deserving recipients are and acting on that frame by giving more to deserving

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

recipients. Eckel and Petrie (2011) allowed subjects to purchase access to a partner's photo before making a decision in a trust game and found 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 ergonomics researchers have studied digital versus in-person interactions for decades, with numerous findings that are relevant to our study.[9] For example, 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). Similarly, agreements routinely have been shown to 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). Lastly, online negotiators report feeling less satisfied with their outcomes, less trusting of their partner, and having less desire for future interaction with the same partner (Naquin and Paulson 2003).

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) studied prisoner's dilemma games across email and face-to-face environments and found 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) studied behavior in a cooperation game using face-to-face, video, and audio communication and showed that visual cues conveyed face-to-face and in video settings are a crucial component of cooperation. Bicchieri and Lev-On (2007) studied behavior in a social dilemma game and found that cooperation is more difficult to establish and maintain in a computer-based setting, which is not

[9] 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.

as effective as a face-to-face setting at inducing preferences and expectations conducive to cooperation. Diermeier et al. (2008) studied coalition formation and found that groups negotiating face-to-face were significantly more efficient than those using a computer (70 percent versus 11 percent). Rocco and Warglien (1996) found increases in opportunistic behavior and communication breakdown in social dilemma games in a computer-mediated setting. Online negotiation settings have also been shown to be 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 have found that digital communication has no deleterious impact on interactions. Croson (1999) studied negotiation behavior in integrative (i.e., win-win) games and found 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) found 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) studied how subjects trade off efficiency for equality in online bargaining and found that subjects prefer efficiency over equality. Bochet et al. (2006) found high levels of cooperation and efficiency in voluntary contribution experiments in treatments where subjects communicate through a computer chatroom and face-to-face, but not in the treatment where communication was limited to numerical signals.

Lastly, previous research shows that people opt into negotiations more often in digital than face-to-face settings because digital settings reduce confrontation costs (Gago 2019), which can lead to worse negotiation outcomes (Brooks and Schweitzer 2011). Although agents may use online chat for screening purposes and 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

We use a 2x2x2, between-subjects design and three factors: bargaining environment, property rights assignment, and repeated bargaining. This design enables us to study the effects of a change in the communication medium, but also to develop a richer understanding of how the digital environment might interact with structural features that distort other-regarding behavior. To do this, our experiment borrows several key elements of the full-information protocols used in HS (1982; 1985). First, rather than adopting 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.

Additionally, each subject in our experiment makes a total of 10 bargaining decisions, which enables us to study how experience affects allocative efficiency and welfare distributions in both bargaining environments. 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. Bargainers had full information during negotiations, meaning the 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.

All treatments were randomized at the session level. 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.[10]

3.1 Lab Procedures for Face-to-Face Bargaining Sessions

We implemented 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 experimental stations to allow privacy between bargaining pairs.[11] The same moderator read instructions aloud for each session, and we also provided paper instructions for reference.[12] 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. 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.

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.[13]

[10] 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.
[11] About 15 feet of distance separated each station. This helped prevent bargaining parties from overhearing one another and adopting one another's bargaining strategies and provided privacy from experimenter scrutiny.
[12] 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.
[13] We asked subjects to record a strategy for this game. There is no evidence that any subject solved the game.

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 the subjects. The same moderator read instructions aloud for each session, and we also provided paper instructions for reference.[14] We concluded instructions with a comprehension quiz that we checked individually before proceeding.

We conducted all digital bargaining sessions with a computer interface programmed in ZTree (Urs Fischbacher, 2007). Our 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 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, and the winner was told they had *earned the right* to be the Controller for that period. Though this competition task is different than the one employed during face-to-face bargaining, we observe no 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.

[14] 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.

4 Results

4.1 Baseline Results

We focus first on the replication of the two-person, full-information bargaining treatments HS (1982). We say we replicate an original finding if we obtain a significant result in the same direction as the original study, which is the most rigorous standard used in Camerer et al. (2016). 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.

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, with *p*-values for Fisher's exact tests. 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 the one-shot full-information (coin flip) sessions in HS (1982) since one-shot bargaining was not used in HS (1985). Results in the 'Our Data' column include data from only the first two periods of the face-to-face, coin flip (weak property rights) no entitlement priming sessions.

We use Fisher's exact tests to test for statistical differences in the proportions of efficient and sharing allocations both within and across studies.[15] The results comparing our baseline face-to-face sessions to those of HS are as follows:

Result 1: We fully replicate HS's efficiency results. We observe an equivalently high proportion of 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.

Result 2: 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.[16]

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. Following HS, results reported in the 'Our Data' column include data from only the first two periods in each respective set of two-shot treatment sessions.

Result 3: We find equivalently high levels of efficiency in our strong and weak property rights treatments ($p > 0.10$, 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 replicate the finding in HS (1985) 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.

[15] 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.

[16] 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.

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 right sessions (game entitlement plus entitlement priming) in HS (1985). Results in the 'Our Data' column include data from the first two periods in each respective set of two-shot treatment sessions.

HS (1985) introduced the Average Greed Index (AGI) as an additional measure to explore the effect of property rights strength on self-regarding behavior. 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 presents the results from HS (1985) alongside our own.[17]

Result 4: When self-regarding behavior is measured by the AGI, we find evidence consistent with HS (1985) that strong property rights induce more self-regarding behavior than do weak property rights (testing $AGI_{strong} > AGI_{weak}$ yields $p = 0.094$).

[17] 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 to HS (1985).

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 decision outcome. We use 22 observations to calculate AGI for each HS panel, which come from the strong property right sessions (game entitlement plus entitlement priming) and weak property rights sessions (random entitlement plus no entitlement priming) in HS (1985). Results in the 'Our Data' column include data from the first two periods in each respective set of two-shot treatment sessions.

To summarize our baseline face-to-face results, 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 Face-to-Face vs. Digital Bargaining

We now turn to an analysis of the full dataset from our experiment to compare bargaining behavior across face-to-face and digital communication environments. We first explore the effects on efficiency and then on other-regarding behavior.

4.2.1 Efficiency

Table 6 demonstrates the stark differences in efficiency we observe across the two bargaining environments; 90 percent of bargains 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.

Result 5: We observe that bargaining face-to-face leads to a large increase in the probability of achieving an efficient bargaining outcome.

Table 6. Pareto Efficient Bargaining 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.

To assess whether these differences are statistically significant, Table 7 reports the results from a series of Probit and Linear Probability models 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 Probit models while columns (4) through (6) report results from Linear Probability models. For each model type, we include a set of period fixed effects and construct standard errors in three different ways. Columns (1) and (4) report estimates from models using 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 (5) report estimates from models using 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 estimates from models with Wild bootstrapped errors, a common method used in settings with as few as five clusters (Cameron et al. 2008).

Regardless of the model we use or how we construct our standard errors, we see that bargaining face-to-face leads to a highly statistically significant increase of 22.5% ($p < 0.01$) 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 ($p > 0.10$). Finally, we provide some evidence that repeated bargaining increases the probability of efficiency ($p < 0.10$), although whether this effect is statistically significant depends on how we specify our standard errors ($p > 0.10$ for specifications with robust standard errors clustered at the session level).

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 through 3, average marginal effects) and Linear Probability models (columns 4 through 6). 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.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We next explore the persistence of disparities in efficiency across bargaining environments. In Figure 2, we present the aggregate proportion of efficient decisions made in each period by bargaining environment. It shows that the gap in efficiency between the two environments closes over time, but learning subsides about halfway through the digital sessions and a clear difference in efficiency persists.

Though experiential learning occurs among subjects participating in digital bargaining, on average they are unable to converge to complete efficiency as do subjects participating in face-to-face bargaining.

In Figure 3, 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 in 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.

Result 6: Differences in efficiency persist across digital and face-to-face bargaining environments, even as subjects gain experience (Figure 2). This result holds for all treatments except the weak property rights, two-shot bargaining treatment, where we see efficiency rates in the digital environment converge to those in the face-to-face environment (Figure 3).

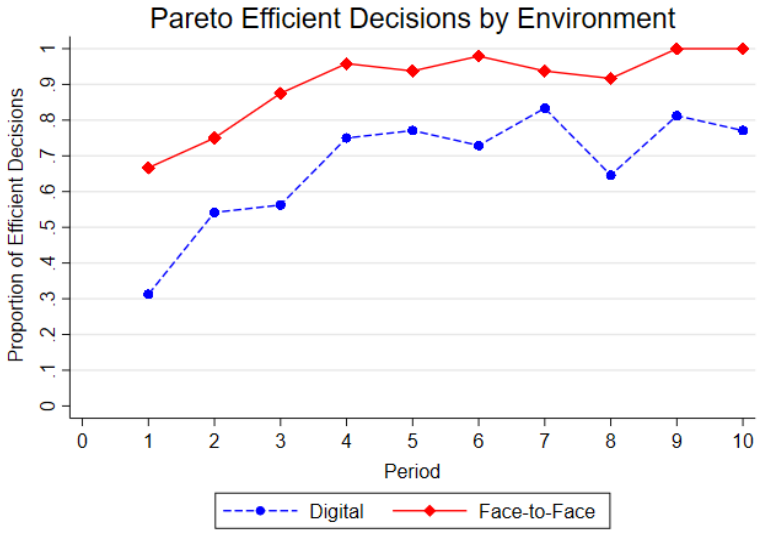

Figure 2: 96 subjects made a total of 48 decisions in each environment for each period. Subjects make significant improvements as they gain experience in early periods but learning levels out around period four. Subjects learned at about the same rate in each environment but subjects in the digital environment failed to converge to complete efficiency as did subjects bargaining in the face-to-face environment.

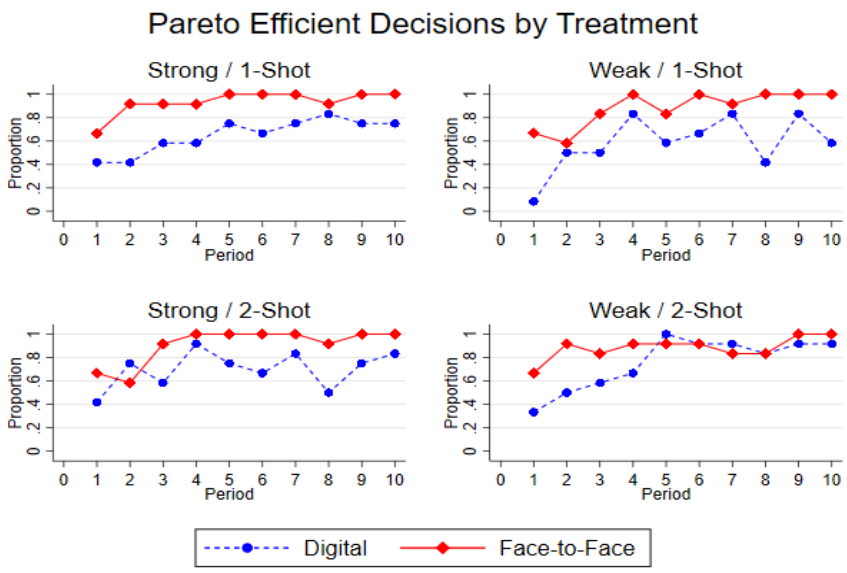

Figure 3: Each panel shows the percentage of efficient decisions made in both face-to-face and digital bargaining environments for each of our four treatment types. Each period comprises 12 decisions made by 24 subjects for each of the face-to-face and digital environments.

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 Figure 4. 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.

Result 7: We observe that Controllers are more self-regarding and individually rational in the digital environment than in the face-to-face environment.

Moving from a face-to-face to a digital bargaining environment more than quadruples the AGI from 0.73 to 2.96, which we show in column 1 of Panel 1 in Table 8 ($p < 0.001$, based on regression results in columns 3 and 4 of Table 11). Further, we find that the AGI increases when moving from face-to-face to the digital environment. This result holds for all treatments except the weak property rights, two-shot bargaining session, where we see no statistically significant difference in AGI between the face-to-face and digital bargaining environments (Figure 4). This suggests that Controllers are more likely to behave in an

individually rational way and are most self-regarding in the digital environment when property rights are clearly defined and/or repeated interaction is not a concern.

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

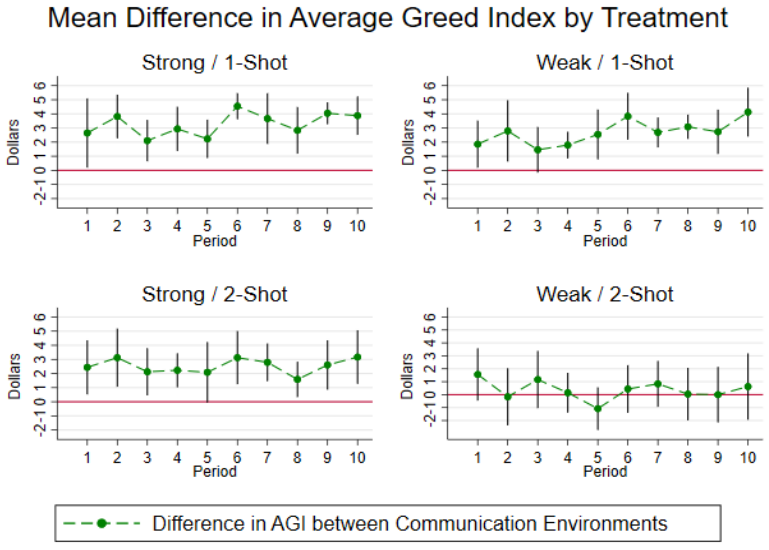

Figure 4: The mean difference in AGI between digital and face-to-face bargaining by period and treatment. We include 95% confidence intervals constructed using Wild bootstrapped errors.

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' per-period average earnings across environments in this treatment are statistically indistinguishable (Table 9).

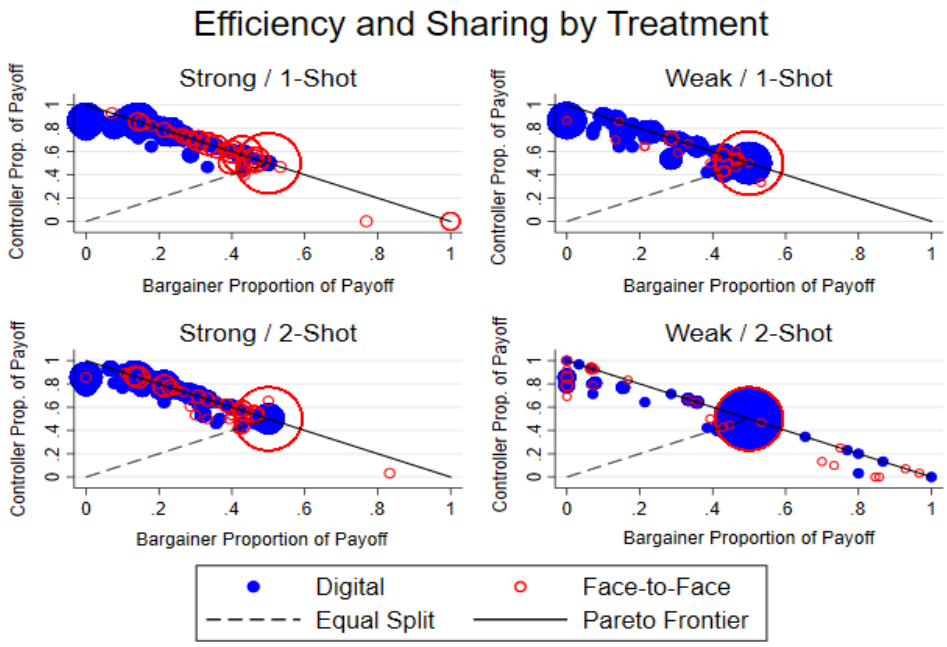

Figure 5: The proportion of efficient decisions and corresponding payoff distributions (in terms of proportions of total payoff), using data from all 10 periods in each treatment (240 observations per treatment).

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 out of concern for the other player's payoff, or to avoid uncomfortable interpersonal interaction in the face-to-face setting.

If 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 a drastic shift in payoff distributions when

migrating our bargaining experiment to a digital environment. However, we find that migrating to a digital bargaining environment shifts payoffs significantly in favor of the Controller in all but the weak, two-shot treatment (Table 9). This indicates that other-regarding preferences cannot fully explain the equitable outcomes observed in the face-to-face setting.

Table 9: 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	960	960	960	960

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

Table 10 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 being lower in the digital than 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 10. 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 11 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 numbered columns) and Wild bootstrapped errors (even numbered columns). The results indicate that moving from face-to-face to the digital environment induces a 34.7% decline in the probability that the Controller and Bargainer share an outcome equally (columns 1 and 2), a more than doubling in the amount of money a Controller earns in excess of an equal split ($4.35 for digital Controllers vs. $2.11 for face-to-face Controllers; columns 3 and 4), and a decline in the amount of earnings a Controller gives up relative to the unilateral maximum (10% for digital Controllers vs. 17% for face-to-face Controllers; columns 5 and 6).

Table 11: 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)	(0.0103)
Random	0.338***	0.338***	-1.464***	-1.464***	0.127***	0.127***
	(0.0511)	(0.0191)	(0.370)	(0.146)	(0.0301)	(0.0106)
Sequential	0.186***	0.186***	-0.601	-0.601***	0.0469	0.0469***
	(0.061)	(0.026)	(0.370)	(0.142)	(0.0301)	(0.0109)
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.

To summarize, we find that introducing social distance and ambiguity 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 preferences to avoid interpersonal conflict, including psychological and

confrontation costs such as awkwardness, embarrassment, or guilt (Jindal and Newberry 2018; Gago 2019), which are present when bargaining face-to-face.

4.2.3 Experience

In Table 12, we report results showing the share of Controllers making unilateral profit-maximizing decisions by treatment for all periods. In all digital treatments, we observe a higher incidence of unilateral decision-making by Controllers, but that this tendency is most pronounced in the treatment with strong property rights with one-shot bargaining, and weakest in the treatment with weak property rights and two-shot bargaining. In Table 13, we report per-period results showing requested Controller sacrifice rates, actual Controller sacrifice rates, and the number of unilateral Controller decisions.

Result 8: The observed increases in efficiency in later rounds of our digital bargaining treatments are driven primarily by Bargainers learning to ask for and accept less equitable allocations that are Pareto improving relative to the unilateral maximum allocation.

Though we find that Controllers in the digital setting on average are more likely to engage in unilateral decisions (Table 12), this tendency appears to subside in later periods (Table 13). 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 13). We interpret 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 12: 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 13: 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 Controller earnings that result from unilateral maximization that he or she would sacrifice by accepting the proposal.

5 Discussion

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 several 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 9 through 13). This places the onus on the Bargainer 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 text messages.[18]

Second, our results 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 might consider working to reduce social distance among team members whenever teams do not work face-to-face. This may help with task allocation, productivity, and intra-team dispute resolution. 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.

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) and is implicit in heuristic thinking. For example, in the eBay bargaining setting, Backus et al. (2019) find that buyers commonly use round-number offers, which invite aggressive behavior from experienced sellers that creates more successful bargains. This suggests that signaling

[18] 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.

remains important in bargaining but is an element requiring a degree of sophistication that likely varies across communication media.

Finally, other research has shown that bargainers learn how to use communication in online bargaining, and that the messaging strategies of experienced sellers are correlated with successful bargaining (Backus et al. 2021). However, we see that the efficiency gap in our experiment does not close in most settings. This potentially occurs because Bargainers do not sufficiently update their bargaining beliefs or expectations about Controller allocation decisions as observed in Backus et al. (2019), or possibly due to a limited degree of experience accruing in our setting which does not allow behavioral norms to develop (Backus et al. 2020). Some research also 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

In this paper, we build on the seminal work of HS (1982; 1985; 1986) to study how adopting digital communication impacts allocative efficiency and welfare distributions in a Coasian bargaining experiment. In addition to varying whether subjects bargain digitally or face-to-face, we vary the strength of property rights assignment and whether subjects engaged in repeated bargaining, which enables us to develop a richer characterization of how the digital environment interacts with structural features known to distort other-regarding behavior. Additionally, each subject in our experiment made 10 bargaining decisions, which enables us to study how experience affects behavior and outcomes in both bargaining environments.

Our baseline face-to-face 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. 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.[19]

In the digital setting, we find 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 and social distance, and loss of interpersonal connection and 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.

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

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