Information-Signaling in Pre-Trial Bargaining: An Experiment

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Abstract

This paper provides experimental evidence on the effectiveness of self-penalizing commitments as signaling mechanisms in pre-trial bargaining under asymmetric information. Using an online experiment (N=2,041), we design a novel two-person signaling game with asymmetric information, where one party (the seller, analogous to a defendant) possesses private information regarding the strength of its case, and proposes a take-it-or-leave-it settlement offer to another party (the buyer, analogous to a plaintiff). In the baseline scenario, the buyer must decide whether to accept the offer or reject it and proceed to a costly trial. In our novel signaling treatments, the seller can credibly commit ex ante to a costly monetary penalty contingent upon losing at trial. Consistent with theoretical predictions, our results show that self-penalizing commitments significantly reduce trial rates: sellers with strong cases utilize commitment mechanisms more frequently, achieving settlements at lower offers. However, we also document some departures from theory. When the signal is available but omitted, buyers penalize otherwise generous offers by rejecting them—an effect we term "signaling by omission." Our findings highlight the dual function of commitment-based mechanisms: they can enhance bargaining efficiency by reducing informational asymmetries, but may also backfire when their absence is misinterpreted.

Keywords: Pre-trial bargaining; Signaling games; Asymmetric information; Experimental economics; Litigation; Settlement; Commitment
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1 Introduction

Bargaining under asymmetric information is a major source of costly conflicts, inefficiencies, and distributive distortions in litigation and dispute resolution contexts (Bebchuk, 1984; Reinganum and Wilde, 1986; Pecorino and Van Boening, 2018; Schaller and Skaperdas, 2020). The strategic nature of negotiations, combined with privately held information regarding the strength of a party's case, often leads to bargaining failures and costly trials. As a result, identifying institutional and strategic mechanisms capable of reducing informational asymmetries and fostering settlements has long been a core focus of economic research on bargaining and litigation (see, e.g., Schelling, 1956; Lavie and Tabbach, 2020, 2023; Chen et al., 2024, and the references therein).

Among the mechanisms proposed by the theoretical literature to credibly convey private information, self-penalizing commitments—where parties voluntarily commit ex-ante to suffer additional penalties contingent on trial outcomes—have attracted significant attention (e.g., Ellingsen and Miettinen, 2008; Lavie and Tabbach, 2023). More generally, costly signals are theoretically appealing because they enable informed parties to distinguish themselves credibly, thereby reducing inefficient bargaining breakdowns (Schelling, 2006; Choné and Laurent, 2010; Hoffmann et al., 2015). However, empirical validation of these mechanisms in controlled settings remains scarce. Our study addresses this gap by providing the first experimental evidence on the effectiveness and interpretability of self-penalizing commitments in pre-trial bargaining scenarios.

Specifically, we implement a two-person signaling game with asymmetric information in a laboratory environment, building upon the framework proposed by Pecorino and Van Boening (2018). In our experiment, one party (the Seller, analogous to a defendant) possesses private information regarding the strength of its case (either strong or weak), and proposes a take-it-or-leave-it (TIOLI) settlement offer to another party (the Buyer, analogous to a plaintiff). In the baseline scenario, the Buyer must decide whether to accept the offer or reject it and proceed to a costly trial. In our novel signaling treatments, Sellers can credibly commit to a costly monetary penalty (donation to a neutral third party) conditional on losing at trial, thereby potentially signaling their case strength to the Buyer.

We find strong empirical support for the theoretical prediction that self-penalizing commitments can function as credible informative signals. Offers accompanied by these commitments are significantly more likely to be accepted, reducing the overall dispute rate. In line with theory, Sellers with strong cases disproportionately utilize these signals, although we also observe some degree of signaling by weaker-case Sellers, suggesting deviations from strictly rational signaling strategies. Notably, the introduction of commitment options also reshapes the informational baseline in bargaining interactions: even generous offers are penalized by rejection if Buyers perceive the deliberate omission of available signals as indicative of weaker cases. This effect, which we refer to as "signal by omission," reveals the nuanced nature of strategic signaling in bargaining environments.

Additionally, our study provides a methodological innovation by introducing interactions between human participants and pre-programmed algorithmic bargaining partners (the "Robots"). By employing robot-generated proposals and responses, we isolate recipient behavior from confounding factors typically present in human interactions, such as fairness concerns, reciprocity, and strategic anticipation (Arruñada and Casari, 2016; Chugunova and Sele, 2022). Results indicate that participants interacting with robotic agents behave in closer alignment with equilibrium predictions, including a remarkable willingness to accept equilibrium-consistent, zero-surplus offers—rarely observed in human-to-human ultimatum games (e.g., Pecorino and Van Boening, 2010). These insights highlight the methodological advantage of using artificial agents for isolating strategic motivations in bargaining research.

Our contributions are threefold. First, we empirically validate judgment-contingent commitments as effective signaling mechanisms under asymmetric information. Second, we identify the strategic interpretability of signaling omissions, demonstrating how silence itself can function as an informative cue within signaling environments. Third, by incorporating robotic counterparts, we advance experimental methodologies in bargaining research, providing clearer identification of equilibrium behaviors and fostering future explorations of human strategic responses in automated negotiation contexts.

The remainder of the paper is organized as follows. Section 2 describes the experiment and our sample. Section 3 presents the theoretical equilibria and the hypotheses. Section 4 reports the main results, and Section 5 shows additional analyses, including learning effects. Section 6 discusses the findings and concludes. Additional materials, including experiment instructions and robustness checks, are available in the Online Supplementary Material (OSM).

1.1 Related Literature

Our study contributes to several strands of literature on bargaining under asymmetric information, specifically focusing on how strategic signaling and commitment mechanisms influence efficiency in pre-trial negotiations.

First, our paper builds directly upon the theoretical literature on pre-trial bargaining under asymmetric information, initiated by seminal contributions from Bebchuk (1984) and Reinganum and Wilde (1986). Subsequent theoretical developments have explored various costly signaling mechanisms—such as preliminary injunctions, costly disclosures, and judgment-contingent penalties—

as means to reduce bargaining inefficiencies (Choné and Laurent, 2010; Jeitschko and Kim, 2012; Hubbard, 2017; Lavie and Tabbach, 2020). Particularly relevant to our study is the theoretical framework by Lavie and Tabbach (2023), who formalize how judgment-contingent commitments can credibly convey private information and consequently reduce litigation. Our paper empirically evaluates these theoretical predictions, providing novel experimental evidence on their behavioral validity.

Second, we extend the growing but still relatively scarce empirical and experimental literature on pre-trial bargaining under asymmetric information. Experimental studies, notably by Pecorino and Van Boening (2018, 2019a,b) and Deck et al. (2024), have documented cognitive and strategic challenges inherent in signaling and screening models of litigation. Among these studies, the most relevant to our project is the influential experimental work by Pecorino and Van Boening (2018). In their study, Pecorino and Van Boening experimentally examine pre-trial bargaining using two canonical negotiation structures: the screening game (where the uninformed party proposes a TIOLI offer) and the signaling game (where the informed party proposes the offer). The results show an imperfect alignment between theoretical predictions and observed laboratory behavior. For example, they find that litigants holding the power to make TIOLI offers fail to extract the entire available surplus from settlement, thereby diminishing the anticipated distributional effects when transitioning from a screening to a signaling game.

Their results from the signaling game, in particular, provide valuable insights for our research. Pecorino and Van Boening emphasize that the signaling game "is cognitively much more challenging for both the sender ... and the recipient," primarily due to the sender's strategic decision regarding whether and how to bluff (Pecorino and Van Boening, 2018, pp. 237-238). Nevertheless, they find that participant "behavior conforms reasonably well" to theoretical predictions about semi-pooling equilibria, although some anomalous offers, sometimes anomalously accepted, also emerge (Pecorino and Van Boening, 2018, pp. 237-8). Additionally, Pecorino and Van Boening observe meaningful learning effects, documenting that participant behavior aligns increasingly closer to theoretical predictions as the game progresses (Pecorino and Van Boening, 2018, pp. 229–30, 239). Further experimental research has explored related aspects such as two-type signaling in both standard and negative-expected-value litigation contexts (Deck et al., 2024), costly voluntary disclosures (Pecorino and Van Boening, 2019a), and the use of costly discovery mechanisms by uninformed parties in signaling and screening games (Pecorino and Van Boening, 2019b).

Our paper significantly contributes to this growing experimental literature. We introduce a novel experimental design explicitly testing the role of self-penalizing commitments as credible signals in pre-trial negotiations, thereby filling an important gap in existing empirical knowledge. We provide evidence of how participants strategically employ costly signaling opportunities, and how the availability of signaling options fundamentally reshapes both offer interpretation and acceptance behaviors. Moreover, to our knowledge, we are the first experimental study to investigate pre-trial bargaining interactions involving pre-programmed algorithmic counterparts (the "Robot").

Third, we connect our findings to broader experimental studies examining signaling and commitment in bargaining more generally. Research by Hoffmann et al. (2015) and Chen et al. (2024) highlights that pre-commitment mechanisms significantly alter bargaining outcomes—for example in ultimatum games and prisoner's dilemma settings—primarily by influencing perceptions of credibility and trustworthiness. Our results reinforce these insights, while also revealing nuanced behaviors—particularly what we refer to as "signaling by omission"—that have not been captured in prior contributions.

Fourth, we methodologically contribute with the emerging experimental literature on human–robot interactions within economic contexts. Despite early foundational work on human-computer interactions (Reeves and Nass, 1996; Nass and Moon, 2000), the experimental literature specifically addressing economic interactions between humans and robots remains at an early stage. A growing body of empirical evidence has begun documenting behavioral shifts when individuals negotiate or cooperate with automated agents, indicating that human participants often exhibit greater rationality and less emotional involvement when interacting with robots compared to human counterparts (Arruñada and Casari, 2016; Chugunova and Sele, 2022; Maggioni and Rossignoli, 2023).

Chugunova and Sele (2022) provide an extensive interdisciplinary literature review of experimental studies on human–robot interactions, emphasizing consistent findings that interactions with automated agents tend to elicit more rational decision-making. Similarly, Maggioni and Rossignoli (2023) show that in strategic games such as the Prisoner's Dilemma, individuals are generally less inclined to cooperate with robots unless the robots exhibit human-like communicative features. Existing studies primarily focus on strategic games, auctions, and managerial decision-making scenarios (Astor et al., 2013; Ratan, 2015), leaving unexplored how humans negotiate specifically in high-stakes legal or litigation-related bargaining contexts when confronted with robotic counterparts—precisely the gap we address in this study.

In this regard, our experiment provides evidence for an imperfect yet notable alignment between theoretical predictions and observed laboratory behaviors in pre-trial bargaining scenarios. Participants in our study demonstrated a reasonable capacity to converge towards a semi-pooling equilibrium (centered approximately around the midpoint of the joint surplus) and displayed a surprisingly high willingness to accept offers leaving them zero surplus—a pattern that diverges markedly from typical observations in standard human-to-human ultimatum games (see, e.g., Pecorino and Van Boening, 2010). By employing robotic counterparts, we eliminate typical complications encountered in previous human-to-human experiments, such as fairness concerns, reciprocity motives, and anticipatory strategic reasoning.

Finally, our study contributes substantively and methodologically to the broader research agenda on conflict and efficiency in bargaining. By providing novel empirical evidence on the efficacy and interpretability of costly signaling mechanisms, and advancing experimental methodologies through human–robot interactions, our study responds to recent calls for multi-method research that combines theoretical modeling with experimental evidence to understand how institutions and strategic behaviors jointly influence bargaining outcomes under private information (Aguiar and Silveira, 2019; Karagözoğlu and Kocher, 2019; Karagözoğlu and Rachmilevitch, 2021; Miettinen and Vanberg, 2023).

2 The Experiment

2.1 Design

The baseline setup of our experiment follows Pecorino and Van Boening (2018), in that we design a similar two-person signaling game with asymmetric information. The players are a Seller (defendant) and a Buyer (plaintiff), engaged in a legal dispute over a defective cellphone. The existence of the defect is common knowledge; however, the value of the defect is private information known only to the Seller. This value can be either high (150 points) or low (50 points), defining two types of Sellers: H-Seller and L-Seller, respectively. The Buyer holds a prior belief about the Seller's type, with a $2/3$ probability of facing an H-Seller and a $1/3$ probability of facing an L-Seller. This is known to the Seller.

Each round of the game proceeds as follows. The Seller (the informed party) makes a TIOLI settlement offer $S \in [0, 150]$ to the Buyer (the uninformed party).[1] If the Buyer accepts S, the Seller pays S and the game ends. If the Buyer rejects S, the dispute proceeds to trial.

Each party incurs litigation costs of 30 points,[2] and the Seller must pay the full defect value (150 or 50 points, depending on her type) to the Buyer. We use this baseline setup as our control condition.

The key innovation in our design is the introduction of an optional signaling device: a self-penalizing commitment mechanism, available in specific treatment groups. We adopt a 3 (Roles: Buyer, H-Seller, L-Seller) $\times$ 2 (Commitment Availability: Not Available vs. Available) between-

[1] This is different from Pecorino and Van Boening (2018), where, in their signaling setup, the informed proposing party expects to receive a payment from the uninformed responding party. In our setup, instead, the informed proposing party is the one liable to pay the uninformed responding party.

[2] As in Pecorino and Van Boening (2018), we adopt the American rule: each side bears its own legal expenses.

subjects design, for a total of six experimental conditions (Table 1).

INSERT TABLE 1 HERE

In the *Commitment Available* treatments, Sellers are given the option to attach a commitment to their settlement offer S. This commitment is a self-imposed penalty: if the offer is rejected, and a trial ensues, the Seller transfers an amount equal to the defect value to a third party (a research fund unrelated to the dispute), in addition to paying litigation costs and compensating the Buyer. That is, an H-Seller who commits and is rejected pays 30 (litigation) + 150 (to Buyer) + 150 (to research fund). L-Sellers pay analogous amounts (30 + 50 + 50). The Buyer pays 30 in trial costs and receives only the defect value (150 or 50), gaining no monetary benefit from the commitment.[3]

If instead the Seller decides not to use the self-penalizing commitment, the same payment rules defined for the *No Commitment* control condition apply. Crucially, the Buyer is always informed (before making her accept/reject decision) whether (i) the commitment mechanism is available to the Seller and, if so, (ii) whether the Seller decided to use it in that round.

Subjects are randomly assigned to one of the six between-subject conditions. In four of the six groups a human subject takes the role of the Seller: HS and HS_C (H-Seller, No Commitment and Commitment Available), and similarly, LS and LS_C (L-Seller, No Commitment and Commitment Available). In the other two groups, a human subject takes the role of the Buyer, B and B_C (No Commitment and Commitment Available). The opposite role is played by a pre-programmed algorithm (the "Robot")—our key methodological innovation. This design choice allowed us to derive clear behavioral predictions, as the Robot is pre-programmed to behave as a fully rational agent: it always selects the payoff-maximizing action according to economic theory, has no memory of past interactions, hence does not learn from experience. This feature was common knowledge among participants through clear instructions provided at the beginning of the experiment.[4] The algorithm governing the Robot's behavior is detailed in Section 3.3.

The game is repeated for 12 identical rounds, preceded by three training rounds. Final payment is determined by randomly selecting one of the 12 game rounds. To ensure approximately equal expected payoffs across conditions and avoid negative earnings, we (i) set conversion rates specific to each treatment and (ii) provided subjects with initial treatment-specific endowments sufficient to cover the maximum possible loss (reported in the OSM, Additional Tables, "Endowment Points").

[3]This design ensures that the commitment serves purely as a costly signal, without altering the Buyer's trial payoff.

[4]Participants were explicitly informed from the very beginning that they would be matched and interacting with a Robot that: (i) always makes rational choices, (ii) has no memory of past rounds, and (iii) does not learn from experience (see the experiment instructions in the OSM). They were not, of course, provided with the precise response rules of the Robot, i.e., no information about the specific decision patterns it would follow during the game.

Between rounds, participants received immediate feedback. In the Seller treatments, participants were informed whether their offer had been accepted or rejected by the Robot Buyer, followed by their payoff for that round (i.e., initial endowment minus the settlement offer if accepted, or minus the defect value and trial costs if rejected; and similarly, if committed, minus the amount committed). In the Buyer treatments, participants were informed of their own payoff after making the accept/reject decision, also broken down by components (i.e., initial endowment plus the settlement offer if accepted, or plus the defect value minus trial costs if rejected).

After the training phase and before proceeding to the main game, participants were required to correctly answer a set of control questions. In addition, following a growing practice for online experiments (Oppenheimer et al., 2009; Peer et al., 2014), we included two set of Instruction Manipulation Checks—one before and one after the game (for details, see the experiment instructions in the OSM). At the end of the experiment, participants were asked to complete a post-experiment demographic questionnaire.

2.2 Procedures

We obtained ethics approval from the University of Bologna and informed consent from every subject prior to participation. The experiment was preregistered on OSF: osf.io/8pdms.

To determine the appropriate sample size, we conducted an ex-ante power analysis using the software STATA 17. Power calculations were based on the concept of minimum detectable effect (e.g., Bloom, 1995). Specifically, we aimed to achieve a statistical power of 0.95 to detect a medium effect size of at least 0.05, with a significance level set at $\alpha = 0.01$. Considering 12 rounds per subject, this yields approximately 296 observations per treatment group, hence 1,776 subjects overall. To account for potential attrition in online experiments (Albert and Smilek, 2023), we increased the target sample by approximately 15%. Accordingly, a total of 2,041 participants were recruited and randomly assigned to one of the six treatment conditions, with group sizes ranging from a minimum of 319 in treatment HS_C to a maximum of 353 in treatment B.

Participants were recruited on August 20$^{\text{th}}$, 2024, through Prolific (`https://www.prolific.co/`, last access: March 21, 2025; Palan and Schitter, 2018), using the platform's *representative* sampling tool. This feature allowed us to obtain a UK-representative sample, stratified by age, sex, and ethnicity based on national census data.[5]

[5]The platform uses census data from the UK Office of National Statistics to match the distribution of the national population. See `https://researcher-help.prolific.com/en/article/95c345` (last access: March 21, 2025). It is worth noting that, at the time of data collection, the Prolific platform charged additional fees—up to three times higher than standard recruitment—for the use of representative sampling, due to the stratification procedures and enhanced quality controls.

The eligible participants received a unique single sign-on link that redirected them to a Qualtrics-based survey. Upon accessing the survey, they were required to read and accept an informed consent form, which included detailed information about the structure and purpose of the study, the anonymity of responses, voluntary participation, and the compensation mechanism (see the experiment instructions in the OSM). After providing consent, participants were randomly assigned to one of the six experimental treatments.

On average, participants took 13 minutes to complete the study, with a mean payment of £1.62 (including a participation fee of £1.18). This compensation level aligns with the average for similar behavioral tasks on Prolific, and with current ethical standards for online experiments (Peer et al., 2022; Albert and Smilek, 2023).

2.3 Sample

The pooled sample consists of 2,041 participants, approximately evenly distributed across the six treatment groups. Overall, 52% of participants identified as female, with a mean age of 46.5 years. Regarding educational background, 4.8% reported training in economics, while 2.7% in law. Around 48% reported having completed High School (GCSE), and the largest share of participants (20.4%) indicated an income between £35,000 and £50,000. Roughly 39% of subjects reported having no prior experience with similar experimental studies.

Detailed descriptive statistics are reported in Appendix A Table A2; the full set of the variables used in the analyses, along with their descriptions, are provided in Appendix A Table A1. The sample is balanced across treatment groups (balance tests—conducted following Chiapello (2018)—are available in the OSM, Additional Tables, "BalanceTab").

3 Theoretical Considerations

We begin by presenting the baseline no-commitment case (Section 3.1), and then extend the model by introducing the possibility to commit (Section 3.2). We subsequently define the Robot Response Function (Section 3.3), which specifies how the algorithm is pre-programmed to interact with human participants. In presenting the theory, we will use the parameter values from our experiment to generate the relevant predictions.

3.1 No-Commitment Equilibria

In the no-commitment case, there are two possible types of equilibria: a fully-revealing equilibrium, or multiple semi-pooling equilibria (e.g., Pecorino and Van Boening, 2018, pp. 221–223). In the fully-revealing equilibrium, the two Seller types, H and L, propose an offer that corresponds their type. The L-Seller proposes the lowest acceptable offer, 20 (i.e., the payoff the Buyer could get from going to trial against an L-Seller, net of trial costs, $50-30$), while the H-Seller offers the highest rational offer she could make, 120 (i.e., the highest payoff that the Buyer could get from trial, $150-30$). High settlement offers, 120, are always accepted by the buyer (indeed, the buyer cannot do better at trial). Low offers, 20, are rejected by the buyer at a sufficient rate, such that H-Sellers are just indifferent between *revealing* (offering 120) and *mimicking* an L-Seller (offering 20).

A semi-pooling equilibrium occurs when the L-Seller proposes a settlement offer S higher than 20, and the H-Seller "pools" with some probability on the same offer. As these offers increase, the Buyer accepts them at a higher rate, and as a consequence, the probability that an H-Seller mimics an L-type also increases. In equilibrium, the H-Seller is again just indifferent between mimicking (and pooling with L-Sellers to S) and revealing (offering 120). The probability of mimicking by H-Sellers leaves the buyer just indifferent between accepting and rejecting (and going to trial).

The rate at which the Buyer should accept S offers, denoted $p(S)$, could be derived by the following indifference function of H-Sellers:

$$H-c_B = p(S)S+(1-p(S))(H+c_S),$$

where H and L are the H- and L-Sellers' expected liability at trial, and c_B and c_S are the Buyer's and Seller's trial costs. The left-hand side represents revealing (and settling), and the right-hand side mimicking and proposing S (and risking a trial with probability $1-p(S)$). As $C=c_B=c_S=30$, and the expected liabilities are 50 and 150, the predicted acceptance rate is

$$p(S)=\frac{2C}{H+C-S}=\frac{2\times 30}{150+30-S}. \tag{3.1}$$

A fully-revealing equilibrium occurs where $S=20$, hence, $p(20)=37.5$. Semi-pooling offers can be any offers in the range $20<S\leq 80$, where $p(S)$ is increasing in S. Offers in the range $80<S<120$ should be rejected with certainty. An offer above 80 implies that the proposer is an H-Seller, because it would be irrational for an L-Seller to offer an amount above the total cost from going to trial (in case of trial, an L-Seller would end up paying $50+30$, with trial expenses). Accordingly, the Buyer would be better off rejecting this offer since she would receive a higher amount of 120 $(150-30)$ from trial.

We can similarly define the rate at which H-Sellers mimic a given offer S, denoted by q. Assuming that the proportions of L- and H-Sellers in the population are α and $1-\alpha$, respectively, the Buyer should be indifferent between accepting offer S, or going to trial against a mixed pool of L-Sellers and mimicking H-Sellers. Formally:

$$S = \frac{\alpha}{\alpha + q(1-\alpha)}(L - c_B) + \frac{q(1-\alpha)}{\alpha + q(1-\alpha)}(H - c_B). \tag{3.2}$$

In our numerical example, $q = (S-20)/[2(120-S)]$; note that, if $S = 20$, $q = 0$ and there is indeed no mimicking.

Among the multiple equilibria, one can verify that the L-Seller minimizes her liability where $S = 20$, that is, the fully-revealing equilibrium.[6] More generally, all semi-pooling equilibria could be refined by using the D1 refinement concept (e.g., Pecorino and Van Boening, 2018, p.221). In that case, the theoretical prediction is a revealing offer of 20 (120) by L-Sellers (H-Sellers). However, previous literature (e.g., Pecorino and Van Boening, 2018) and our own experiment suggest that the fully-revealing equilibrium does not hold in bargaining experiments.

3.2 Commitment Equilibria

As in the no-commitment case, in the commitment case there may exist a fully revealing equilibrium as well as multiple semi-pooling equilibria. In the revealing equilibrium, the H-Seller again offers 120, without committing (indeed, these offers are expected to be accepted with certainty, making the commitment strategically unnecessary). The L-Seller offers 20 but adds the self-penalizing commitment (we will show below that the L-seller is indeed better off with the commitment).

Accordingly, we can structure semi-pooling equilibria in which the L-Seller raises her offer S above 20 and adds a self-penalizing commitment, and an H-Seller mimics with some probability. We can again utilize the indifference function of H-Sellers to derive the equilibrium acceptance rate of offers S that are coupled with a self-penalizing commitment, denoted $p_c(S)$:

$$H - c_B = p_c(S)\,S + (1 - p_c(S))(2H + c_S).$$

Note that a mimicking H-Seller now faces the risk of paying an additional H—triggered by the self-penalizing commitment if the offer is rejected. This increased cost of mimicking allows

[6]The L-Seller liability is $p(S)S + (1-p(S))(L+c_S)$; with our parameters, $L(S) = 20(480-S)/(180-S)$, which indeed increases with S.

the Buyer to accept a larger share of settlement offers compared to Eq. (3.1). Indeed, under our numerical example,

$$p_c(S) = \frac{2C+H}{H+C+H-S} = \frac{2\times 30+150}{150+30+150-S}. \tag{3.3}$$

As before, p_c increases with S, although the marginal increase is lower than in the no-commitment case. Importantly, $p_c(S)$ is higher than $p(S)$ for every S in the relevant range. To illustrate, in the fully-revealing equilibrium, $p_c(20) \approx 67.7$, compared to $p(20) = 37.5$, without commitment.

The mimicking rate q in semi-pooling equilibria with the possibility to self-penalize remains identical to the previous setup. Recall that q is determined by the Buyer's indifference condition. Since the addition of a self-penalizing commitment does not alter the Buyer's payoffs, her indifference curve remains as defined in Eq. (3.2).

Finally, we ask whether an L-Seller benefits from committing to self-penalize at trial. We showed that, in the baseline case an L-Seller is best off by proposing the lowest possible offer, $S = 20$. One can verify that committing to self-penalize when offering $S = 20$ minimizes the L-Seller's expected liability. Thus, by refining all semi-pooling equilibria, we expect L-Sellers to offer 20 and commit. However, as S increases within the semi-pooling range, the value of commitment declines. Intuitively, the commitment is more valuable when the acceptance rate is low; instead, when S is already likely to be accepted, the commitment adds little signaling value while still imposing a potential cost if the offer is rejected. In fact, within the range $S \in [31, 80]$, a self-penalizing commitment is no longer beneficial for L-Sellers.

3.3 Robot Response Function

3.3.1 Robot as a Buyer

When the robot acts as a Buyer, i.e., in groups HS and LS (no commitment) and HS_C and LS_C (commitment available), its response depends on the settlement offer S and whether the Human Seller selects the self-penalizing commitment option. Specifically, the Robot Buyer follows the equilibrium acceptance functions, as shown in Fig. 1.

INSERT FIGURE 1 HERE

In the No-Commitment case (dashed line in Fig. 1):

- If $S < 20$, the offer is always rejected, as the Buyer's minimum expected trial payoff is 20.
- For $20 \leq S \leq 80$, the acceptance probability follows $p(S)$ as defined in Eq. (3.1).

- If $80 < S < 120$, the Robot always rejects, as these offers are believed to be proposed by H-Sellers.

- If $S \geq 120$, the Robot always accepts, as the offer at least weakly dominates the expected trial payoff.

In the Commitment case (solid line in Fig. 1), the Robot Response Function is identical outside the $[20, 80]$ interval. Within this range, the acceptance probability follows $p_c(S)$, as defined in Eq. (3.3).

3.3.2 Robot as a Seller

When the robot acts as a Seller, i.e., in groups B (no commitment) and B_C (commitment available), its response is defined according to the fully revealing equilibrium. Specifically, the Robot H-Seller—encountered with probability $2/3$ by the Human Buyer—always proposes a settlement offer of $S = 120$ and never includes a commitment. The Robot L-Seller always offers $S = 20$, which is coupled with a self-penalizing commitment in group B_C.

3.4 Hypotheses

Based on the theoretical framework discussed above and our experimental design, we formally test two pre-registered hypotheses:

- **H1 (Commitment Choice)**: The rate of selecting the commitment option is higher in LS_C (L-Seller) than in HS_C (H-Seller), as L-Sellers can use the signal to credibly convey their strength.

- **H2 (Buyer Acceptance of Low Offers)**: Human Buyers in B_C are more likely to accept low settlement offers compared to those in B, as low offers are automatically coupled with a commitment in B_C.

As exploratory analyses, we also examine whether the availability of commitment influences Sellers' offer levels. Specifically, we explore whether Sellers in the commitment treatments (HS_C and LS_C) propose different settlement offers than their no-commitment counterparts (HS and LS). As the commitment is particularly beneficial for LS_C L-Sellers making low offers in the range [20, 30], L-Sellers may desire to decrease their offer.

Furthermore, we investigate whether Human Buyers in B_C are less likely to accept high settlement offers compared to those in B, as the (automatic) absence of commitment—despite being available—may affect the tendency of human participants to accept. In particular, we suspect that human participants may interpret the absence of a signal negatively.

4 Results

We structure the analysis around three core dimensions of participant behavior: offer decisions (Section 4.1), use of the commitment option (Section 4.2), and acceptance of settlement proposals (Section 4.3). We report robustness checks in Section 4.4.

4.1 Offer Behavior

We first examine offer behavior in the control conditions without commitment (*HS* and *LS*). As shown in Fig. 2, H-Sellers (white bars) displayed a multi-modal pattern: the median and mode offer was 120—consistent with full revelation—yet a substantial share of offers deviated from theoretical predictions.

INSERT FIGURE 2 HERE

Following the approach of Pecorino and Van Boening (2018), we categorize offers into five intervals: (i) offers below 20 (always rejected), (ii) bluffing offers in the semi-pooling range (20–80],[7] (iii) anomalous mid-range offers (81–119), which are expected to be rejected with certainty, (iv) revealing offers (exactly 120), and (v) over-generous offers (>120). About 57% of H-Seller offers were consistent with equilibrium play: 33% were bluffing offers in the semi-pooling range (mean 58, median 55, mode 50), and 24% were revealing offers of 120. However, 27% were overly generous and 16% fell in the anomalous mid-range.[8]

Turning to L-Sellers, the offer pattern is markedly different. As shown in Fig. 3 (white bars), the vast majority (around 80%) of offers fell within the semi-pooling range (20–80], with average 56, median 55, and mode 50. Very few offers were anomalous, and almost none exceeded 120. This aligns with theory, which predicts semi-pooling behavior by strong types.[9]

INSERT FIGURE 3 HERE

[7] Offers of 20 were rare (fewer than 2%). For this reason, we do not report them.

[8] These proportions differ notably from Pecorino and Van Boening (2018), where only 25% of offers by weak types (analogous to H-Sellers in our setup) were anomalous and 63% were fully revealing. In their signaling treatment, low-damage plaintiffs most frequently offered their true value, while bluffing was rare.

[9] These findings are broadly in line with those in Pecorino and Van Boening (2018), where 90% of offers by strong types were within the semi-pooling range.

Interestingly, the bluffing offers made by H-Sellers and the semi-pooling offers made by L-Sellers largely converged on the same focal point. Conditional on being in the 20–80 interval, the median offer was 55 and mode was 50 in both groups. This coordination on an intermediate value close to the expected result at trial (50) suggests a form of behavioral equilibrium that mirrors the mid-point of the surplus.[10]

We now consider the commitment treatments (HS_C and LS_C), the gray bars in Figures 2 and 3, respectively. For H-Sellers, the presence of the commitment option had negligible impact on offer behavior. As shown in Fig. 2, the median offer remains at 120, and the share of anomalous offers is similar to the control condition. This aligns with the theoretical expectation that weak types do not benefit from signaling and thus maintain their baseline strategy. Notably, a small subset of H-Sellers still chose to commit, but this did not significantly alter offer levels.[11]

In contrast, L-Sellers in the commitment condition reduced their offers. The average settlement decreased from 63 to 58, and the median dropped from 60 to 50. Among those who chose to commit, the average offer was even lower, 52, compared to 60 for those who did not. We can speculate that the strong types compensated their commitment with a lower offer, or believed that the H-Sellers would have likewise mimicked a lower offer.

Comparing Sellers between treatments, the data show no statistically significant difference among H-Sellers ($p = 0.4213$; one-sided t-test), while we observe a statistically significant difference among L-Sellers, who proposed a lower amount when commitment was available ($p = 0.0031$; one-sided t-test). This result is confirmed in OLS regression analyses (Table 2): average settlement offers by L-Sellers were almost 7 points lower in the Commitment Available treatment ($p < 0.01$; Table 2, Models 3-4); instead, the commitment availability had no significant effect on H-Seller offer levels (Table 2, Models 1-2).

INSERT TABLE 2 HERE

4.2 Commitment Choice

H1 posits that L-Sellers (strong type) are more inclined to use the commitment option than H-Sellers (weak type), as the signal is more valuable for them. We test this by comparing commitment rates in the two treatment groups HS_C and LS_C, where Sellers could choose whether or not to attach

[10]This convergence contrasts with findings in Pecorino and Van Boening (2018), where strong types tended to offer the mid-point settlement, but the weak-types' bluffing offer was substantially lower, that is, more generous.

[11]Committing H-Sellers slightly lowered their offers, but the effect was not statistically significant. We return to this point in Section 4.2.

a costly self-penalizing commitment to their offer. Fig. 4 displays the average commitment rates across these conditions.

INSERT FIGURE 4 HERE

We find that 31.8% of L-Sellers (gray bars) opted to commit, compared to 24.0% of H-Sellers (white bars)—a statistically significant difference ($p = 0.0013$; one-sided t-test). OLS regressions confirm this result: H-Sellers were almost 11% significantly less likely to use the commitment device than L-Sellers ($p < 0.001$; Table 3, Model 2).

INSERT TABLE 3 HERE

This pattern is consistent with theoretical predictions: strong types are more likely to commit. However, two deviations from the model are worth noting. First, only about one-third of L-Sellers committed, despite theory suggesting that they should use this signal whenever proposing low offers; relatedly, we see a substantial proportion of commitment coupled with intermediate offers, again, against theoretical predictions. Second, a non-trivial share of H-Sellers also chose to commit—although they can do (at least weakly) better by simply offering 120.

4.3 Acceptance Behavior

In this section we investigate how the possibility to signal private information through a self-penalizing commitment affects acceptance decisions in pre-trial bargaining. We focus on Human Buyers' behavior when facing TIOLI offers from Robot Sellers, distinguishing across treatments with and without the commitment option (B_C and B, respectively). The use of Robot Sellers allows us to isolate how Human Buyers responded to fully-revealing proposals. Our aim is to assess whether signaling mechanisms increase the credibility of low offers and influence the interpretation of high offers.

We test H2, which predicts that Human Buyers in B_C are more likely to accept *low* settlement offers compared to those in B, as these offers are automatically coupled with a commitment. As an exploratory analysis we also explore whether Human Buyers in B_C are less likely to accept *high* settlement offers compared to those in B. These offers are not accompanied by a commitment, despite its availability, and we posit that this omission may be interpreted as a negative signal.

INSERT FIGURE 5 HERE

Fig. 5 shows the average acceptance rates of high (left panel) and low (right panel) offers across treatment groups. In the control group (B, white bars), the robot H-Seller always offers $S = 120$, which corresponds to the Buyer's expected value from going to trial and should be accepted with certainty in equilibrium. Indeed, the observed acceptance rate is approximately 92% (white bar, left panel), confirming the theoretical prediction. This result is particularly interesting in light of the experimental literature on ultimatum games, where offers leaving zero surplus are often rejected.[12] In our case, Buyers routinely accepted such zero-surplus offers, suggesting that acceptance decisions were guided less by fairness concerns and more by rational evaluation of expected payoffs. The fact that these offers came from a robot may have further reduced the emotional salience of rejecting an unequal split.

Still in the control condition, the robot L-Seller offers $S = 20$. These offers, although theoretically optimal when made by a strong L-type and absent mimicry incentives, were accepted in only 13.9% of cases (white bar, right panel). This is considerably lower than the theoretical benchmark of roughly 38%, and aligns with prior findings of systematic over-rejection of low settlement offers in signaling contexts (see Pecorino and Van Boening, 2018). Overall, the control condition sets a clear baseline: high offers are accepted nearly always, and low offers are overwhelmingly rejected.

In the treatment group (B_C, gray bars), the Robot Seller retains the same offer strategy, but now has the option to attach a self-penalizing commitment. By design, the Robot H-Sellers continue to offer $S = 120$ without committing, while the Robot L-Sellers offer $S = 20$ and always commit. The acceptance rate for committed low offers rises to 19.4% (gray bar, right panel)—a statistically significant increase compared to the 13.9% observed in the control group ($p = 0.0134$; one-sided t-test). OLS regressions confirm a statistically significant 4% increase in the acceptance rate of low offers in the commitment treatment, where such offers were always coupled with a self-penalizing commitment ($p < 0.05$; Table 4, Models 1–2). This result supports the prediction that a costly commitment enhances the credibility of low offers, increasing their likelihood of acceptance.[13]

INSERT TABLE 4 HERE

As to acceptance of offers from H-Sellers, we find that the rate of acceptance dropped from 92% in the control condition to 86% in the treatment group (gray bar, left panel)—a statistically significant difference ($p = 0.0001$; one-sided t-test). This effect is confirmed by OLS regressions, which show a 5–6% decrease in the acceptance rate of high offers when the commitment option is available but, by design, unused ($p < 0.001$; Models 3–4, Table 4).

[12] Participants in ultimatum games experiments "typically offer one-third to one-half of the pie to the recipient," and likewise, less generous offers are accordingly often rejected (Pecorino and Van Boening, 2010, p. 263). In the context of litigation-style games, the surplus left is smaller, around 1/6 (Pecorino and Van Boening, 2010).

[13] However, the acceptance rate remained substantially below the theoretical benchmark of 68%.

This finding highlights an important behavioral asymmetry: even though the offer itself remains unchanged, its interpretation varies depending on the strategic environment. When Buyers know that the commitment device is available but not used, its absence may be treated as an implicit signal—specifically, as a deliberate omission. Buyers appear to penalize high offers not accompanied by commitment, potentially inferring that Sellers deliberately withheld a costly signal that could have enhanced credibility. This effect—which we term "signal-by-omission"—implies that the presence of a signaling device alters not only the interpretation of committed offers but also the baseline tendency to accept no-signal offers.

Taken together, these findings support H2 and our exploratory prediction about acceptance of high offers. Buyers interpreted commitment as an informative signal: they were more likely to accept low offers when a commitment was present, and more likely to reject high offers when no commitment was provided in a context where signaling was possible. These results illustrate how the mere possibility of signaling can alter behavior, not only through direct use but also through expectations and inferences in its absence.

4.4 Robustness Checks

The results withstand various robustness checks, reported in the OSM ("Robustness Checks"). First, we replicate all offer-related regressions excluding irrational proposals—defined as settlement offers exceeding 120 for H-Sellers or 80 for L-Sellers. Second, we estimate probit and fractional response models (fractional probit) for our three dependent variables bounded between 0 and 1: (i) commitment rates among sellers (dummy variable); (ii) buyer average acceptance rates of low settlement offers (continuous variable); and (iii) buyer average acceptance rates of high settlement offers (continuous variable). Third, we replicate all main regressions after excluding participants who failed at least one comprehension question and at least one attention check (a subsample of 998 observations, approximately 49% of the full sample). All robustness checks validate the findings of our study.

5 Additional Results

As additional analyses, we assess whether the introduction of the commitment option affected overall trial rates (Section 5.1), the distribution of participants' payoffs (Section 5.2), and learning dynamics (Section 5.3, and more in Appendix B).

5.1 Trial Rates

Among L-Sellers, we find a significant reduction in dispute rates when commitment was available: 45% in LS_C vs. 54% in LS ($p < 0.001$; one-sided t-test). This effect is driven by higher acceptance rates. Among H-Sellers, the decline is slightly smaller, yet statistically significant, 26% in HS_C vs. 30% in HS ($p = 0.0121$; one-sided t-test)—reflecting the minority of H-types who committed. These results are supported by regression analyses, as reported in Table 5 where the dependent variable is the Robot Average Acceptance Rate of offers from L-Sellers (Models 1-2-3) or H-Sellers (Models 4-5-6).

INSERT TABLE 5 HERE

In the Buyer role (B vs. B_C), the treatment effect is more nuanced (as discussed in Section 4.3). On the one hand, low offers were accepted more frequently when paired with commitment—19.4% vs. 13.9% in the control group ($p = 0.0134$; one-sided t-test). On the other, high offers were less likely to be accepted if not accompanied by commitment—86% vs. 92% in the control group ($p = 0.0001$; one-sided t-test). These results are supported by previous regression analyses (Table 4 in Section 4.3).

Overall, the results support the theoretical prediction that commitment mechanisms reduce trial rates in pre-trial bargaining under asymmetric information. The availability of a self-penalizing commitment significantly decreased dispute rates, particularly among L-Sellers, where signaling incentives are strongest. These effects are attributable to the increased credibility of offers (reflected in higher acceptance rates). Even among H-Sellers—where incentives to signal are weaker—the commitment option induced a modest but statistically significant reduction in trial rates. On the receiver side, buyers responded predictably to the presence of commitment: accepting low offers more frequently when accompanied by commitment. However, human buyers penalized high offers when the signal was omitted.

5.2 Distributional Effects

We now consider whether the introduction of the commitment option affected participants' payoffs—that is, whether participants were better or worse off under the commitment treatments. We examine this separately by role.

Participants assigned to the role of H-Seller earned significantly lower average payoffs in the commitment treatment (HS_C) than in the control group (HS)—219 vs. 224 points on average, respectively ($p < 0.001$; one-sided t-test). This result is somewhat counterintuitive, as H-Sellers were not expected to benefit from commitment and should have theoretically refrained from using it. The observed earnings loss is mostly driven by the H-Sellers who did choose to commit and offer 81-119, thereby exposing themselves to higher potential losses in case of rejection. In theory, they would have been better off revealing their type through a 120-points offer without commitment. Further research is needed to investigate the underlying motives behind this behavior.

Participants assigned to the L-Seller role did not earn significantly different payoffs in the commitment treatment (LS_C) compared to the control group (LS): both groups obtained an average of 85 points, with the difference being statistically insignificant ($p = 0.1091$; one-sided t-test).

The absence of a significant improvement in earnings can be attributed to suboptimal strategy combinations. Specifically, many L-Sellers chose to commit while simultaneously making intermediate offers (e.g., $S = 50$). This behavior increased their exposure in the event of trial—due to the added cost of the commitment—without sufficiently benefiting from the signal's credibility. In theory, L-Sellers could have maximized payoffs either by pairing commitment with low offers, $S = [20-30)$, thereby signaling strength, or by avoiding commitment altogether and offering moderately.

On the other hand, the L-Sellers have seized an indirect benefit from the commitment. Those who chose to commit offered a lower settlement, and accordingly improved their payoff (recall that for L-Sellers liability decreases with S). These two effects—committing with intermediate (rather than low) offers, and reducing the offer size—apparently have canceled out each other.

For Buyers, payoffs are invariant across treatments. The Robot Seller always proposes equilibrium-consistent offers ($S = 120$ for H-types and $S = 20$ for L-types) and the Buyer's expected value from going to trial is identical in both scenarios (i.e., 120 or 20 points, net of litigation costs). Therefore, in equilibrium, Buyers should be indifferent in expectation between accepting or rejecting the offer. The data broadly confirmed this prediction. Average earnings for Buyers remain stable across conditions: 116 points in B vs. 117 in B_C.

All these findings are confirmed in the GLS fixed-effects regressions reported in Table 6.

INSERT TABLE 6 HERE

5.3 Learning Effects

Prior studies on signaling games have shown that participants tend to adjust their behavior over time, exhibiting learning effects across repeated rounds (e.g., Pecorino and Van Boening, 2018). In our setting, participants were exposed to feedback between rounds—such as whether their offer was accepted and the payoff received—which could support experience-based learning.

To explore this possibility, we analyze behavioral evolution over time along three key dimensions: (i) the settlement offers; (ii) the use of commitment; and (iii) the offers acceptance. For brevity reasons, we report the complete analysis in Appendix B.

Overall, we find some adaptive behavior over time, particularly in H-Seller offer choices and Buyer responses to low offers, which shifted in directions consistent with theoretical predictions. In contrast, other behavioral outcomes remain largely stable, and the availability of the commitment option did not significantly influence these temporal patterns. Taken together, the results point to partial and role-specific learning.

6 Discussion and Conclusion

This paper provides experimental evidence on the role of self-penalizing commitments as a signaling mechanism in pre-trial bargaining under asymmetric information. By embedding these signals in a two-person signaling game with asymmetric information—where one has private information and the other must decide whether to accept or reject a TIOLI offer—we tested core theoretical predictions from the literature on judgment-contingent settlements (Lavie and Tabbach, 2020, 2023).

We highlight two main contributions of our study. First, we provide clear evidence that self-penalizing commitments can serve as credible signals in pre-trial bargaining. Second, we show the methodological value of using pre-programmed algorithmic agents to isolate recipient behavior in bargaining games.

Our main findings strongly support the hypothesis that commitment mechanisms function as credible signals in settlement bargaining. L-Sellers (strong types) were significantly more likely to utilize the commitment option than H-Sellers (weak types), offering substantially lower settlements when committing. Offers coupled with credible commitment were, accordingly, more likely to be accepted by Buyers. These results indicate that participants were able, at least partially, to interpret the commitment as a signal of case strength, thus validating its informative value in practice.

Importantly, our analysis also reveals a novel signaling dynamic: not only did signaling affect the interpretation of committed offers, but also of non-committed ones. In particular, high offers of $S = 120$, which were almost always accepted in the absence of commitment, became significantly less likely to be accepted when commitment was available but unused. This suggests that the omission of a costly signal in a signaling-rich environment is itself interpreted as a signal—albeit a negative one. In other words, the mere availability of a commitment changes the informational baseline: even generous offers can be penalized if Buyers infer that the Seller deliberately withheld a signal that could have enhanced credibility. This "signal-by-omission" effect highlights a subtle yet powerful aspect of strategic communication: when signaling is expected but does not occur, the sender's silence acquires informational weight and can alter the recipient's behavior in unexpected ways. Hence, not only do recipients draw inferences from silence, but they may also react "irrationally"—rejecting offers that would otherwise leave them with zero.

These findings resonate broadly with real-world legal environments, where mechanisms such as preliminary injunctions, costly pre-trial disclosures, or judgment-contingent penalties serve as de facto signals of privately held information (Choné and Laurent, 2010; Jeitschko and Kim, 2012; Hubbard, 2017). Our experiment provides clean evidence that such signaling devices are interpretable and influential even in a controlled, stylized setting.

From a methodological perspective, our design contributes by relying on robot-generated and robot-accepted offers to isolate recipient behavior. This allows us to clearly identify signaling effects on acceptance decisions, free from potential confounds typically associated with human interactions, such as social preferences, fairness concerns, or strategic anticipation. Indeed, our results demonstrate that the use of robotic agents may elicit more "rational" play, and thus represents a valuable tool for testing theoretical mechanisms in bargaining.

Our design raises an important avenue for future research: understanding how the dynamics we identify—particularly those concerning strategic signaling and its interpretation—might differ in human-human interactions. In our experiment, robot agents provided neutrality and consistency in offer behavior, and eliminated confounding social complexities such as emotional engagement or fairness concerns. Future research should thus systematically compare these results against purely human bargaining scenarios, explicitly investigating how elements such as fairness, reciprocity, and trust may modulate or amplify the observed signaling effects.

Connecting our results explicitly to the existing literature, we find substantial alignment as well as notable differences. Consistent with the experimental findings of Pecorino and Van Boening (2018), participants in our signaling games exhibited significant but imperfect convergence toward theoretical equilibria. Similar to Hoffmann et al. (2015) and Chen et al. (2024), we find empirical support for the effectiveness of pre-commitment mechanisms in enhancing bargaining efficiency, while also uncovering nuanced behavioral patterns—e.g., signaling by omission—that prior contributions did not capture. Additionally, our results echo broader insights from the literature on human-robot interactions, reinforcing previous findings that robotic counterparts often induce more rational and less emotionally-driven decision-making (Arruñada and Casari, 2016; Chugunova and Sele, 2022; Maggioni and Rossignoli, 2023).

In sum, our findings provide novel evidence that judgment-contingent commitments can reduce trial rates by credibly signaling private information, while simultaneously uncovering subtle strategic dynamics associated with signaling omission. This dynamic, where the availability of a costly commitment reshapes the interpretation of silence, may be particularly relevant in legal and organizational settings. Our contribution thus bridges signaling theory and behavioral economics, offering novel perspectives and practical tools for designing more efficient conflict resolution mechanisms under asymmetric information.

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Declarations

- Conflict of interest: The authors have no competing interests to declare.
- Funding: This research was supported by Tel Aviv University, Israel.

- Ethics Approval: This study was approved by the Bioethics Committee of the University of Bologna prior to being conducted (prot. N. 0208843 of July 26, 2023).
- Preregistration: Key aspects of this study, including measures, hypotheses, and study design, were preregistered on the Open Science Framework (OSF) Registration platform on August 16, 2024, prior to data collection. The pre-registration is available on https://osf.io/8pdms.
- Data availability: Data are available upon request.
- Author contribution: The authors contributed equally to all aspects of the research.

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Tables

Table 1: Experimental Treatments Overview (3 × 2 Between-Subjects Design)

		Commitment Option	
		Not Available (Control Group)	Available (Treatment Group)
Roles	H-Seller (vs. Robot Buyer)	HS	HS_C
	L-Seller (vs. Robot Buyer)	LS	LS_C
	Buyer (vs. Robot H- or L-Seller)	B	B_C

Notes: The two Commitment Option conditions are referred to throughout the paper as *No Commitment* (control group) and *Commitment Available* (treatment group), depending on whether a self-penalizing commitment device is available to the seller.

Table 2: OLS Regressions on Settlement Offers by H- and L-Seller

DV: Average Settlement Offer	by H-Seller		by L-Seller	
	(1)	(2)	(3)	(4)
Commitment Available	-0.471	-1.823	-4.984**	-6.623**
	(2.376)	(2.474)	(1.741)	(2.110)
Constant	105.859***	96.876***	63.035***	57.943***
	(1.609)	(7.964)	(1.267)	(5.941)
Controls	No	Yes	No	Yes
Observations	656	611	684	648
Adjusted R^2	0.0001	0.0636	0.0109	0.0556
AIC	6342.053	5900.051	6233.442	5874.178
BIC	6351.026	5957.447	6242.501	5932.339

Notes: OLS regressions with *Average Settlement Offer* made by H-Sellers (Columns 1 and 2) and by L-Sellers (Columns 3 and 4) as dependent variable, defined as the average of settlement offers submitted by each seller in the negotiation phase. Columns (2) and (4) include a full set of control variables, as described in Table A1. For the full estimates with controls, see Appendix A, Table A3.
Robust standard errors in parentheses.
$^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 3: OLS Regressions on Average Commitment Rate

DV: Average Commitment Rate	(1)	(2)
H-Seller	-0.078**	-0.105***
	(0.026)	(0.029)
Constant	0.318***	0.319**
	(0.018)	(0.101)
Controls	No	Yes
Observations	664	619
Adjusted R^2	0.0136	0.0626
AIC	413.379	382.9016
BIC	422.376	440.4669

Notes: OLS regressions with *Commitment Rate* as dependent variable, defined as the proportion of rounds in which sellers chose to use the commitment device. *H-Seller* is a dummy variable equal to 1 for participants in the HS_C treatment and 0 for those in the LS_C treatment. Column (2) includes a full set of control variables, as described in Table A1. For the full estimates with controls, see Appendix A, Table A4.
Robust standard errors in parentheses.
$^+$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4: OLS Regressions on Average Acceptance Rate of Low and High Offers

DV: Average Acceptance Rate	Low Offers		High Offers	
	(1)	(2)	(3)	(4)
Commitment Available	0.045*	0.042*	-0.063***	-0.055**
	(0.020)	(0.021)	(0.017)	(0.018)
Constant	0.135***	0.103	0.920***	0.900***
	(0.013)	(0.079)	(0.009)	(0.059)
Controls	No	Yes	No	Yes
Observations	697	663	700	666
Adjusted R^2	0.0070	0.0145	0.0196	0.0312
AIC	151.9837	169.4832	-99.9307	-81.94247
BIC	161.07735	227.9413	-90.82854	-23.42571

Notes: OLS regressions with *Average Acceptance Rate* of Low Offers (Columns 1–2) and High Offers (Columns 3–4) as dependent variable, defined respectively as the proportion of low or high settlement offers accepted by the buyer across all negotiation rounds. Columns (2) and (4) include a full set of control variables, as described in Table A1. For the full estimates with controls, see Appendix A, Table A5.
Robust standard errors in parentheses.
$^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 5: OLS Regressions on Robot Average Acceptance Rate of Offers from Low- and High-type Sellers

DV: Robot Average Acceptance Rate	L-Seller Offers			H-Seller Offers		
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment Available	0.065***	0.058***	0.073***	0.037*	0.030+	0.039**
	(0.013)	(0.017)	(0.016)	(0.016)	(0.017)	(0.013)
Average Settlement Offer			0.002***			0.005***
			(0.000)			(0.000)
Constant	0.464***	0.482***	0.356***	0.699***	0.638***	0.195***
	(0.009)	(0.052)	(0.051)	(0.012)	(0.058)	(0.048)
Controls	No	Yes	Yes	No	Yes	Yes
Observations	684	647	647	656	612	612
Adjusted R^2	0.0348	0.0485	0.1207	0.0077	0.0641	0.4769
AIC	-434.9392	-400.3399	-449.4814	-173.6888	-178.6899	-532.1547
BIC	-425.8804	-342.1794	-386.8469	-164.7165	-121.2936	-470.3433

Notes: OLS regressions with *Robot Average Acceptance Rate* as dependent variable. Models (1) to (3) refer to average acceptance rate of offers made by L-Sellers, and by H-Sellers in Models (4) to (6). Models (2), (3), (5), and (6) include a full set of control variables, as decribed in Table A1. Models (3) and (6) control for the average settlement offer made by the L- or H-Seller. For the full estimates with controls, see Appendix A, Table A6.
Robust standard errors in parentheses.
$^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 6: GLS Fixed-Effects Regressions on Average Payoff

DV: Average Payoff	H-Seller		L-Seller		Buyer	
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment Available	-5.272***	-4.820***	0.718	1.939	0.790	0.887
	(1.035)	(0.981)	(0.975)	(1.111)	(0.979)	(0.969)
Constant	224.258***	225.775***	84.846***	88.517***	116.407***	122.304***
	(0.503)	(3.255)	(0.491)	(1.933)	(0.485)	(2.878)
Controls	No	Yes	No	Yes	No	Yes
Observations	7872	7344	8220	7776	8388	8388
σ_u	4.286	4.412	1.655	1.681	2.346	2.427
σ_ε	51.560	51.576	26.403	26.065	47.079	47.101
ρ	0.007	0.007	0.004	0.004	0.002	0.003

Notes: GLS Fixed-Effects regressions with *Average Payoff* as dependent variable. Models (1)-(3) refer to average payoff of H-Sellers, Models (3)-(4) of L-Sellers, and Models (5)-(6) of Buyers. Models (2), (4), and (6) include a full set of control variables, as decribed in Table A1. For the full estimates with controls, see Appendix A, Table A7.
Robust standard errors in parentheses.
$^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Figures

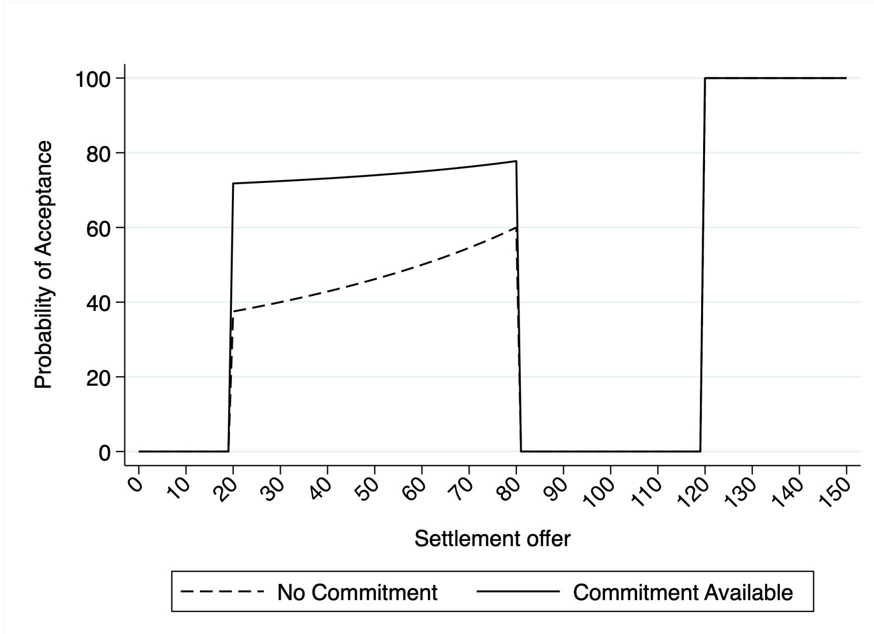


Figure 1: Probability of Acceptance by Robot Buyer

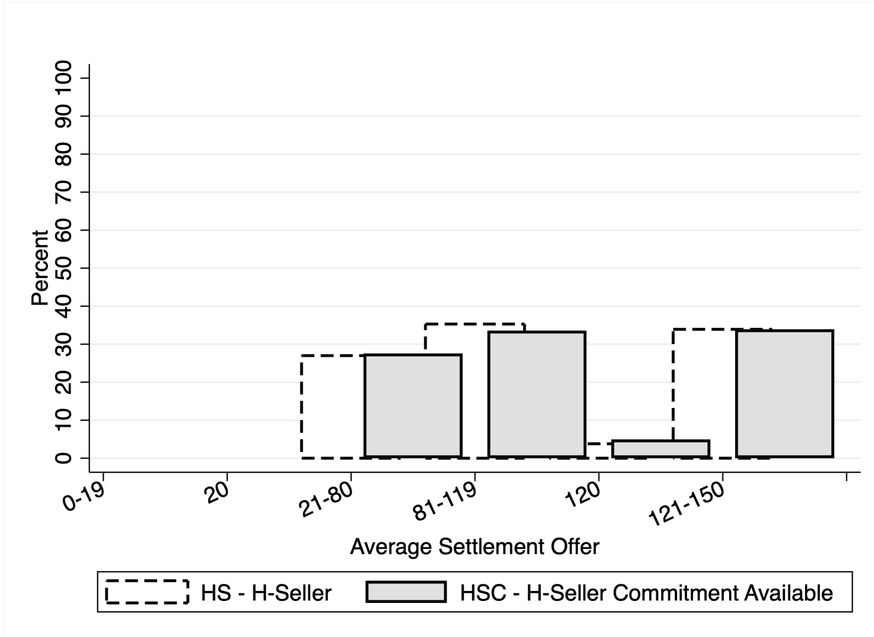


Figure 2: Distribution of Settlement Offers by H-Sellers

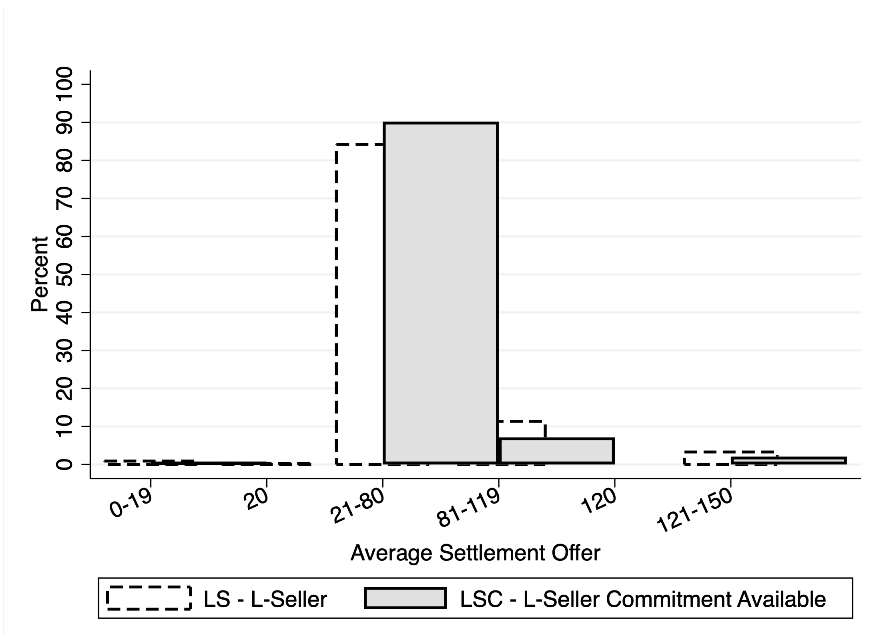


Figure 3: Distribution of Settlement Offers by L-Sellers

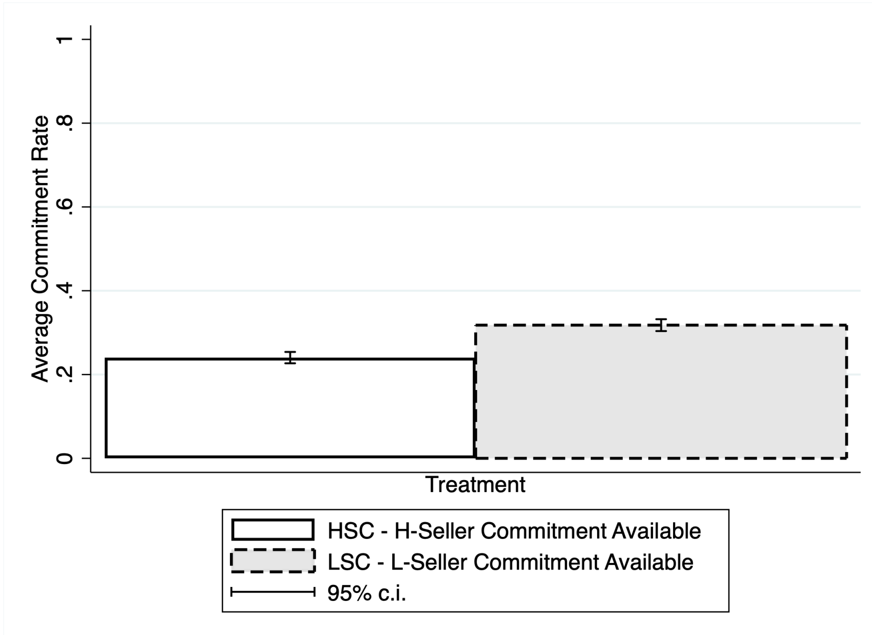


Figure 4: Commitment Rate by Seller Type and Treatment

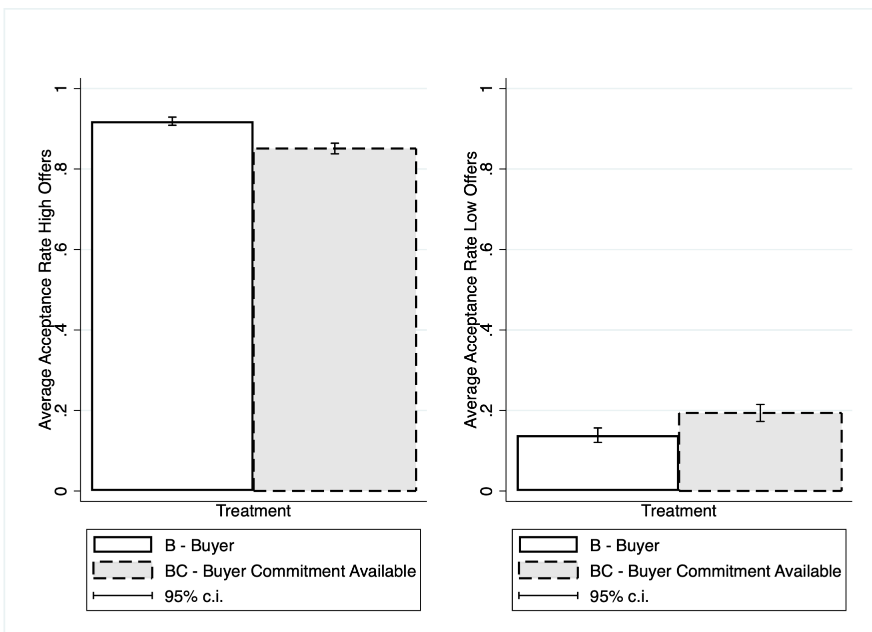


Figure 5: Acceptance Rates of High and Low Offers by Human Buyers

Appendix A Additional Tables

Table A1: Variables Description

Variable Name	Description	Values
Female	Gender	0 = Male, 1 = Female
Student	Answer to the question: *“Are you currently a student?”*	0 = No, 1 = Yes
Age	Self-reported age	Integer
Education	Highest completed level of education, based on the question: *“What is the highest grade or year of school you completed?”*	Categorical
Income	Answer to the question: *“What is your annual household income from all sources?”*	Categorical
Similar Experiment Experience	Answer to the question: *“Have you ever participated in experiments similar to this one?"*	0 = No, 1 = Yes
Econ Experiment Experience	Whether the participant has prior experience in economics experiments	0 = No, 1 = Yes
Econ Background	Main education background is Economics	0 = No, 1 = Yes
Law Background	Main education background is Law	0 = No, 1 = Yes
Risk Attitude	Self-reported risk propensity on a 0–100 scale. Answer to the question: *“Generally speaking, how often, as a percentage of time, are you willing to take risks?”*	Integer (0–100)
Time	Total time (in minutes) taken to complete the entire experimental session, recorded automatically by the Qualtrics platform	Continuous
Commitment Available	Treatment indicator: whether the commitment option was available	0 = No, 1 = Yes
H-Seller	Seller role type, randomly assigned	0= L-Seller, 1 = H-Seller
Average Settlement Offer	Average value of settlement offers proposed by the Seller	Continuous (0–150)
Average Commitment Rate	Share of rounds in which the Seller selected the commitment option, conditional on it being available	Continuous (0–1)
Acceptance Rate of Offers	Share of settlement offers accepted by the Buyer	Continuous (0–1)

Table A2: Pooled Sample Descriptive Statistics

Variable	**Mean**	**SD**	**Min**	**Max**
Female	0.52	0.50	0	1
Student	0.05	0.22	0	1
Age	46.52	15.49	18	87
Similar Experiment Experience	0.72	0.45	0	1
Econ Experiment Experience	0.38	0.49	0	1
Time (Minutes)	16.24	66.01	5.27	1520.22
Econ Background	0.05	0.21	0	1
Law Background	0.03	0.16	0	1
Risk Attitude	44.70	22.58	0	100
Education Level	**Freq.**	**%**	**Cum %**	
Refuse to answer	7	0.34	0.34	
Never attended school	2	0.10	0.44	
Primary School (Year 1–6)	1	0.05	0.49	
Secondary School (Year 7–9)	22	1.08	1.57	
High School (GCSE)	982	48.11	49.68	
A Levels	344	16.85	66.54	
Undergraduate/Vocational	90	4.41	70.95	
Postgraduate	535	26.21	97.16	
PhD	58	2.84	100	
Income Level	**Freq.**	**%**	**Cum %**	
Don't know / Refuse	113	5.54	5.54	
< £10,000	92	4.51	10.04	
< £15,000	107	5.24	15.29	
< £20,000	132	6.47	21.75	
< £25,000	202	9.90	31.65	
< £35,000	334	16.36	48.02	
< £50,000	416	20.38	68.40	
< £75,000	356	17.44	85.84	
< £100,000	178	8.72	94.56	
< £150,000	83	4.07	98.63	
< £200,000	15	0.73	99.36	
£200,000+	13	0.64	100	
Total Observations		2041		

Table A3: OLS Regressions on Settlement Offers by H- and L-Seller (full estimates)

DV: Average Settlement Offer	by H-Seller		by L-Seller	
	(1)	(2)	(3)	(4)
Commitment Available	-0.471	-1.823	-4.984**	-6.623**
	(2.376)	(2.474)	(1.741)	(2.110)
Female		1.467		4.902**
		(2.540)		(1.802)
Student		-1.243		-0.915
		(6.805)		(3.733)
Age		0.157+		0.131*
		(0.085)		(0.064)
Education		2.233*		1.084
		(0.865)		(0.819)
Income		-0.557		-0.861+
		(0.650)		(0.470)
Similar Experiment Experience		2.764		-3.971+
		(2.987)		(2.050)
Econ Experiment Experience		0.225		-2.907
		(2.589)		(1.833)
Time		0.283		-0.020**
		(0.218)		(0.006)
Econ Background		3.666		8.669
		(4.812)		(5.415)
Law Background		0.067		0.357
		(6.365)		(5.610)
Risk Attitude		-0.245***		0.017
		(0.058)		(0.043)
Constant	105.859***	96.876***	63.035***	57.943***
	(1.609)	(7.964)	(1.267)	(5.941)
Observations	656	611	684	648
Adjusted R^2	0.0001	0.0636	0.0109	0.0556
AIC	6342.053	5900.051	6233.442	5874.178
BIC	6351.026	5957.447	6242.501	5932.339

Notes: OLS regressions with *Average Settlement Offer* made by H-Sellers (Columns 1 and 2) and by L-Sellers (Columns 3 and 4) as dependent variable. For variable definitions, see Table A1.
Robust standard errors in parentheses.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A4: OLS Regressions on Average Commitment Rate (full estimates)

DV: Average Commitment Rate	(1)	(2)
H-Seller	-0.078**	-0.105***
	(0.026)	(0.029)
Female		0.022
		(0.027)
Student		-0.010
		(0.065)
Age		0.001
		(0.001)
Education		-0.020+
		(0.011)
Income		-0.007
		(0.006)
Similar Experiment Experience		-0.025
		(0.033)
Econ Experiment Experience		-0.070*
		(0.028)
Time		0.003
		(0.002)
Econ Background		-0.046
		(0.049)
Law Background		-0.024
		(0.072)
Risk Attitude		0.002***
		(0.001)
Constant	0.318***	0.319**
	(0.018)	(0.101)
Observations	664	619
Adjusted R^2	0.0136	0.0626
AIC	413.379	382.9016
BIC	422.376	440.4669

Notes: OLS regressions with *Commitment Rate* as dependent variable. For variable definitions, see Table A1.
Robust standard errors in parentheses.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A5: OLS Regressions on Average Acceptance Rate of Low and High Offers (full estimates)

DV: Average Acceptance Rate	Low Offers		High Offers	
	(1)	(2)	(3)	(4)
Commitment Available	0.045*	0.042*	-0.063***	-0.055**
	(0.020)	(0.021)	(0.017)	(0.018)
Female		-0.006		-0.012
		(0.022)		(0.018)
Student		0.045		0.044
		(0.064)		(0.035)
Age		0.001		0.001
		(0.001)		(0.001)
Education		0.003		-0.007
		(0.009)		(0.007)
Income		0.003		0.001
		(0.006)		(0.004)
Similar Experiment Experience		-0.036		0.003
		(0.025)		(0.019)
Econ Experiment Experience		-0.032		0.027
		(0.024)		(0.019)
Time		-0.000		0.000***
		(0.000)		(0.000)
Econ Background		0.047		0.021
		(0.051)		(0.034)
Law Background		0.044		-0.116
		(0.082)		(0.073)
Risk Attitude		0.000		0.000
		(0.001)		(0.000)
Constant	0.135***	0.103	0.920***	0.900***
	(0.013)	(0.079)	(0.009)	(0.059)
Observations	697	663	700	666
Adjusted R^2	0.0070	0.0145	0.0196	0.0312
AIC	151.9837	169.4832	-99.9307	-81.94247
BIC	161.07735	227.9413	-90.82854	-23.42571

Notes: OLS regressions with *Average Acceptance Rate* of Low Offers (Columns 1–2) and High Offers (Columns 3–4) as dependent variable. For variable definitions, see Table A1.
Robust standard errors in parentheses.
$^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table A6: OLS Regressions on Robot Average Acceptance Rate of Offers from Low- and High-type Sellers (full estimates)

DV: Robot Average Acceptance Rate	L-Seller Offers			H-Seller Offers		
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment Available	0.065***	0.058***	0.073***	0.037*	0.030+	0.039**
	(0.013)	(0.017)	(0.016)	(0.016)	(0.017)	(0.013)
Average Settlement Offer			0.002***			0.005***
			(0.000)			(0.000)
Female		0.002	-0.009		-0.005	-0.011
		(0.014)	(0.014)		(0.018)	(0.014)
Student		-0.026	-0.024		-0.001	0.005
		(0.030)	(0.029)		(0.053)	(0.046)
Age		0.000	-0.000		0.002**	0.001+
		(0.001)	(0.001)		(0.001)	(0.000)
Education		0.003	0.000		0.010	-0.000
		(0.007)	(0.006)		(0.006)	(0.004)
Income		-0.005	-0.003		-0.002	0.000
		(0.003)	(0.003)		(0.004)	(0.003)
Similar Experiment Experience		-0.020	-0.011		-0.008	-0.021
		(0.016)	(0.015)		(0.020)	(0.015)
Econ Experiment Experience		-0.016	-0.010		0.001	-0.000
		(0.015)	(0.014)		(0.018)	(0.014)
Time		-0.000***	-0.000***		0.002	0.001
		(0.000)	(0.000)		(0.002)	(0.001)
Econ Background		0.023	0.004		0.032	0.015
		(0.034)	(0.031)		(0.032)	(0.020)
Law Background		-0.056	-0.057		0.027	0.026
		(0.040)	(0.038)		(0.050)	(0.041)
Risk Attitude		0.000	0.000		-0.002***	-0.001
		(0.000)	(0.000)		(0.000)	(0.000)
Constant	0.464***	0.482***	0.356***	0.699***	0.638***	0.195***
	(0.009)	(0.052)	(0.051)	(0.012)	(0.058)	(0.048)
Observations	684	647	647	656	612	612
Adjusted R^2	0.0348	0.0485	0.1207	0.0077	0.0641	0.4769
AIC	-434.9392	-400.3399	-449.4814	-173.6888	-178.6899	-532.1547
BIC	-425.8804	-342.1794	-386.8469	-164.7165	-121.2936	-470.3433

Notes: OLS regressions with *Robot Average Acceptance Rate* as dependent variable. Models (1) to (3) refer to average acceptance rate of offers made by L-Sellers, and by H-Sellers in Models (4) to (6). For variable definitions, see Table A1.
Robust standard errors in parentheses.
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A7: GLS Fixed-Effects Regressions on Average Payoff (full estimates)

DV: Average Payoff	H-Seller		L-Seller		Buyer	
	(1)	(2)	(3)	(4)	(5)	(6)
Commitment Available	-5.272***	-4.820***	0.718	1.939	0.790	0.887
	(1.035)	(0.981)	(0.975)	(1.111)	(0.979)	(0.969)
Female		-3.667*		-2.113**		0.075
		(1.418)		(0.539)		(0.846)
Student		-0.004		-0.612		-7.416+
		(3.366)		(1.423)		(3.641)
Age		0.062		-0.046*		0.002
		(0.038)		(0.019)		(0.033)
Education		-0.394		-0.667*		-0.641+
		(0.302)		(0.216)		(0.346)
Income		0.481		0.384+		-0.121
		(0.333)		(0.206)		(0.173)
Similar Experiment Experience		-2.622		1.561+		0.622
		(1.525)		(0.803)		(1.301)
Econ Experiment Experience		-0.195		1.915**		0.018
		(1.042)		(0.539)		(1.500)
Time		-0.107		-0.251***		-0.167+
		(0.116)		(0.029)		(1.306)
Econ Background		0.169		-6.057***		-4.992
		(3.166)		(1.159)		(0.000)
Law Background		1.090		-0.870		3.335
		(2.089)		(0.924)		(0.081)
Risk Attitude		-0.027		-0.005		-0.012
		(0.021)		(0.006)		(0.020)
Constant	224.258***	225.775***	84.846***	88.517***	116.407***	122.304***
	(0.503)	(3.255)	(0.491)	(1.933)	(0.485)	(2.878)
Observations	7872	7344	8220	7776	8388	8388
σ_u	4.286	4.412	1.655	1.681	2.346	2.427
σ_ε	51.560	51.576	26.403	26.065	47.079	47.101
ρ	0.007	0.007	0.004	0.004	0.002	0.003

Notes: GLS Fixed-Effects regressions with *Average Payoff* as dependent variable. Models (1)-(3) refer to average payoff of H-Sellers, Models (3)-(4) of L-Sellers, and Models (5)-(6) of Buyers. For variable definitions, see Table A1.
Robust standard errors in parentheses.
+ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Appendix B Learning Effects

B.1 Offer Behavior

We begin with offer patterns over rounds (Fig. B1). Fig. B1a shows the evolution of average offers across rounds for H-Sellers in the control (HS, dashed line) and treatment (HS_C, solid line) conditions. Among H-Sellers, we observe a clear trend toward more rational behavior over time, with average offers gradually converging toward 120. By round 4, both the median and mode stabilize at this level, suggesting that participants increasingly recognize that offering the full trial value is optimal when acting as a high-type. These learning effects are evident in both the commitment and no-commitment conditions.

In contrast, learning among L-Sellers appears much more limited (Fig. B1b). As shown in Section 4.1, the majority of offers were already concentrated in the semi-pooling range ($20 < S \leq 80$) from the outset. This pattern remains remarkably stable across rounds: the average offer fluctuates between 50 and 75, the mode consistently stays at 50, and the median shows minimal variation.[14]

INSERT FIGURE B1 HERE

We further investigate whether Sellers learn to avoid making irrational offers over time. We define an offer as irrational if it exceeds 120 for H-Sellers or 80 for L-Sellers. For H-Sellers, 120 corresponds to the Buyer's expected trial payoff; offering more than this amount reduces the Seller's expected earnings. Similarly, for L-Sellers, 80 is the maximum total payment they would incur at trial—including litigation costs—so offering above this threshold is also suboptimal.

Fig. B2 illustrates the dynamics of irrational offers over time. As shown in Panel B2a, H-Sellers make irrational offers more frequently than L-Sellers (Panel B2b), but their rate of such offers declines slightly across rounds, suggesting some learning. In contrast, the incidence of irrational offers among L-Sellers remains relatively stable. These effects are similar in both the commitment and no-commitment conditions (solid and dashed lines, respectively).

INSERT FIGURE B2 HERE

B.2 Use of Commitment

We now examine whether participants adjusted their use of the commitment mechanism over time. If behavior aligns with rational incentive-responsiveness, we would expect a declining trend in commitment among H-Sellers—who have limited strategic benefit from signaling—and an increasing trend among L-Sellers, for whom commitment can credibly convey private information.

Fig. B3 plots the average commitment rate by round for both seller types. For both H- and L-Sellers, commitment rates remain largely stable over time, suggesting limited evidence of learning in this domain.

INSERT FIGURE B3 HERE

[14]This limited adjustment is somewhat surprising, given the availability of round-by-round feedback. It may indicate that L-Sellers quickly anchored on a salient focal point (e.g., 50) and saw little, if no reason to deviate.

B.3 Acceptance Behavior

Finally, we assess whether participants in the Buyer role adjusted their behavior across rounds in response to the Robot Seller's offers. We separately examine the acceptance of high offers from H-type robots and low offers from L-type robots.

Fig. B4a displays the acceptance rates over time for high offers ($S = 120$) made by robot H-Sellers, under both the control (B, dashed line) and treatment (B_C, solid line) conditions. Acceptance remains consistently high across all rounds, indicating stable behavior in response to generous offers. A similar time stability emerges in Fig. B4b, which shows the acceptance rates of low offers ($S = 20$) made by robot L-Sellers. Here, acceptance rates remain low and stable.

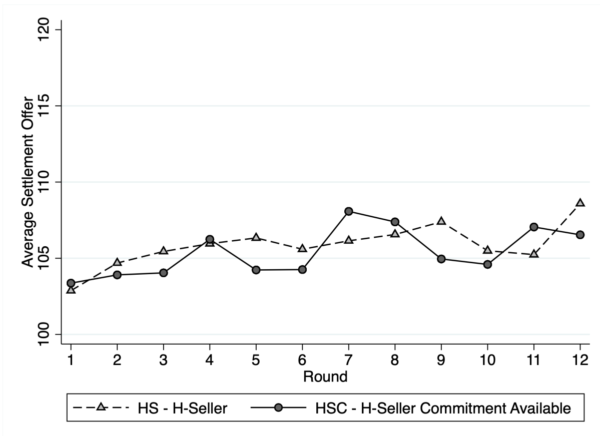
INSERT FIGURE B4 HERE

B.4 Regression Results

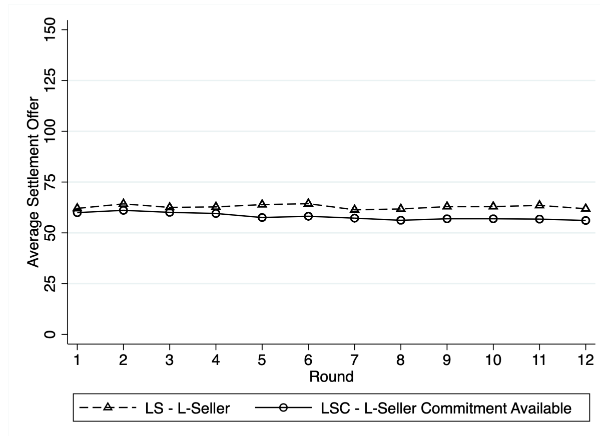
Table B1 reports GLS random-effects regressions assessing behavioral dynamics across rounds. Overall, the time trends described in the previous sections are confirmed. Specifically, average settlement offers by H-Sellers (Model 1) exhibit a clear and statistically significant upward trend over rounds, consistent with increasing recognition that offering the full trial value ($S = 120$) is payoff-maximizing. This pattern is not moderated by the Commitment Available treatment, as indicated by the non-significant Round × Treatment interaction terms. A second significant trend emerges in the acceptance of low offers (Model 4), which increases over time, particularly in the later rounds (e.g., Rounds 10–11). This pattern is, again, not moderated by the Commitment Available treatment.

In contrast, no temporal dynamics are observed in other behavioral outcomes. Offers by L-Sellers (Model 2), average commitment rates (Model 3), and acceptance rates of high offers (Model 5) remain stable over time, with round coefficients that are not statistically significant. Moreover, across all models, interaction effects with the treatment condition are generally not significant.

INSERT TABLE B1 HERE

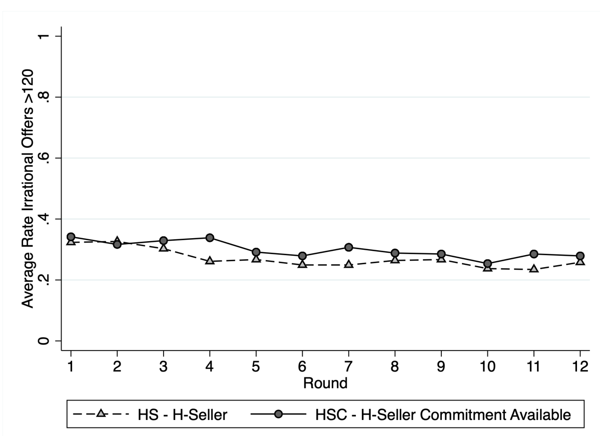


(a) H-Seller

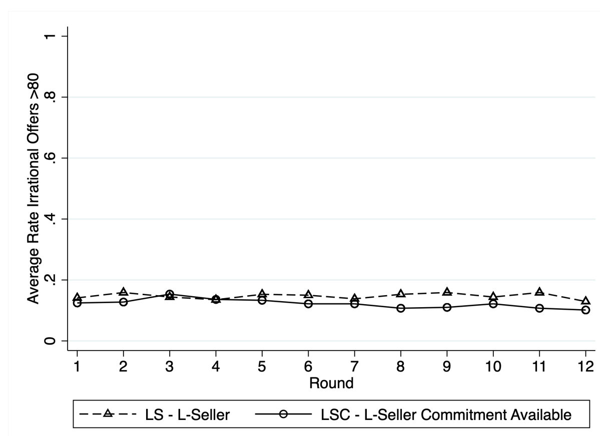


(b) L-Seller

Figure B1: Evolution of Offers Over Rounds



(a) H-Seller



(b) L-Seller

Figure B2: Evolution of Irrational Offers Over Rounds

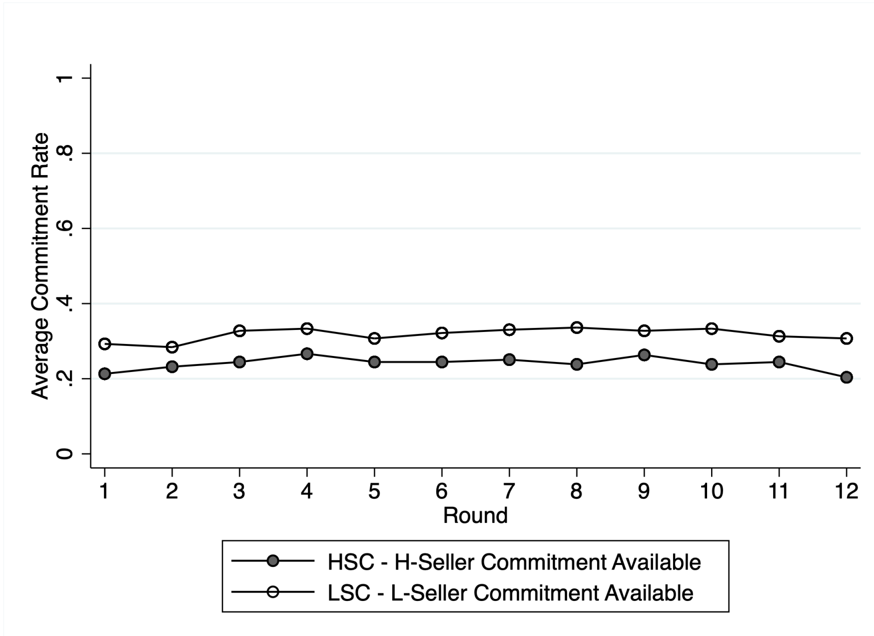
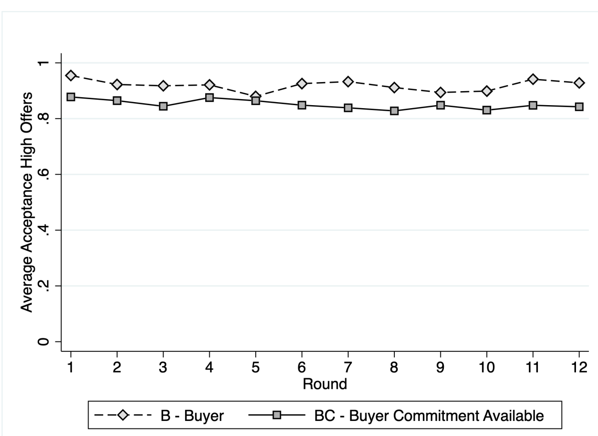
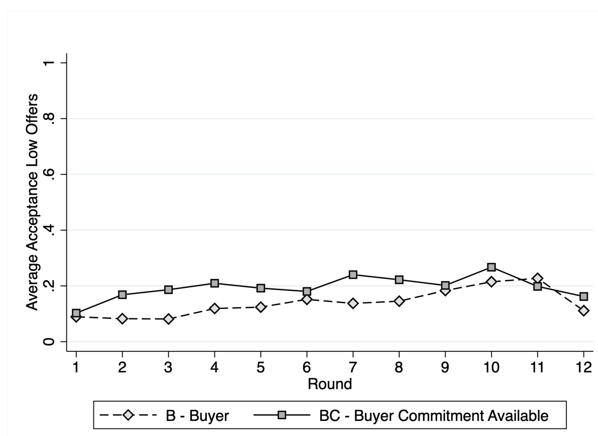


Figure B3: Commitment Usage Over Rounds



(a) High Offers



(b) Low Offers

Figure B4: Acceptance Rate Over Rounds (Buyers)

Table B1: GLS Random-Effects Regressions

	Average Settlement Offer by H-Seller	Average Settlement Offer by L-Seller	Average Commitment Rate	Average Acceptance Rate Low Offers	Average Acceptance Rate High Offers
	(1)	(2)	(3)	(4)	(5)
Round 2	1.816	2.218^{+}	-0.009	0.014	-0.016
	(1.524)	(1.206)	(0.023)	(0.030)	(0.015)
Round 3	2.573^{+}	0.474	0.035	0.008	-0.024
	(1.517)	(1.186)	(0.025)	(0.029)	(0.016)
Round 4	3.086^{+}	0.735	0.041	0.033	-0.030
	(1.592)	(1.181)	(0.027)	(0.032)	(0.020)
Round 5	3.460*	1.853	0.014	0.053	-0.069**
	(1.698)	(1.349)	(0.027)	(0.034)	(0.022)
Round 6	2.721	2.318^{+}	0.029	0.066^{+}	-0.018
	(1.731)	(1.320)	(0.028)	(0.037)	(0.018)
Round 7	3.273^{+}	-0.685	0.038	0.061^{+}	-0.024
	(1.815)	(1.340)	(0.028)	(0.035)	(0.018)
Round 8	3.688*	-0.294	0.043	0.081*	-0.039^{+}
	(1.829)	(1.356)	(0.028)	(0.035)	(0.020)
Round 9	4.519**	0.871	0.035	0.091*	-0.053*
	(1.753)	(1.274)	(0.027)	(0.038)	(0.021)
Round 10	2.620	0.876	0.041	0.140***	-0.051*
	(1.868)	(1.416)	(0.027)	(0.039)	(0.020)
Round 11	2.368	1.465	0.020	0.129***	-0.002
	(1.812)	(1.386)	(0.027)	(0.036)	(0.018)
Round 12	5.715**	-0.162	0.014	0.053	-0.020
	(1.913)	(1.310)	(0.028)	(0.035)	(0.021)
Treatment[a]	0.494	-2.082	-0.080*	0.038	-0.054*
	(2.977)	(2.032)	(0.034)	(0.037)	(0.024)
Round 2 × Treatment	-1.274	-1.079	0.028	0.026	-0.008
	(2.362)	(1.644)	(0.031)	(0.053)	(0.026)
Round 3 × Treatment	-1.899	-0.334	-0.003	0.060	0.000
	(2.425)	(1.715)	(0.035)	(0.048)	(0.028)
Round 4 × Treatment	-0.208	-1.158	0.013	0.040	0.004
	(2.490)	(1.767)	(0.037)	(0.051)	(0.030)
Round 5 × Treatment	-2.595	-4.259*	0.017	-0.005	0.032
	(2.552)	(1.899)	(0.036)	(0.053)	(0.033)
Round 6 × Treatment	-1.831	-4.106*	0.002	-0.011	-0.027
	(2.523)	(1.945)	(0.037)	(0.053)	(0.031)
Round 7 × Treatment	1.439	-2.042	-0.000	0.040	-0.032
	(2.668)	(1.907)	(0.038)	(0.059)	(0.031)
Round 8 × Treatment	0.330	-3.480^{+}	-0.018	0.021	-0.026
	(2.657)	(1.926)	(0.037)	(0.061)	(0.033)
Round 9 × Treatment	-2.933	-3.894*	0.015	-0.011	0.003
	(2.573)	(1.890)	(0.037)	(0.059)	(0.033)
Round 10 × Treatment	-1.391	-3.897*	-0.016	0.007	-0.014
	(2.663)	(1.962)	(0.037)	(0.059)	(0.033)
Round 11 × Treatment	1.315	-4.656*	0.011	-0.026	-0.035
	(2.767)	(2.017)	(0.037)	(0.056)	(0.033)
Round 12 × Treatment	-2.543	-3.702^{+}	-0.024	-0.003	-0.023
	(2.682)	(2.004)	(0.037)	(0.058)	(0.034)
Constant	102.872***	62.044***	0.293***	0.077**	0.949***
	(2.050)	(1.479)	(0.025)	(0.024)	(0.013)
Observations	7872	8220	7968	2790	5610
σ_u	22.358	29.799	0.317	0.212	0.208
σ_ε	16.566	20.294	0.317	0.296	0.237
ρ	0.646	0.683	0.500	0.339	0.435

Notes: GLS Random-Effects regressions with *Average Settlement Offer* (Models 1-2), *Average Commitment Rate* (Model 3), and *Average Acceptance Rate* (Models 4-5) as dependent variable. For variable definitions, see Table A1.

[a] *Treatment* is *Commitment Available* in Models (1), (2), (4), and (5); and *H-Seller* in Model (3).

Robust standard errors clustered at individual level in parentheses.

$^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.