ESSAYS IN EXPERIMENTAL ECONOMICS AND CORPORATE FINANCE

by

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The first chapter experimentally examine repeated partnerships with imperfect monitoring, where participants can unilaterally sever partnerships at any time. The experiment examines effects from changes in the value of an outside-the-partnership option. We find four main results where partners have access to the same outside option: i) the presence of a dissolution option increases cooperation; ii) the use of dissolution is dictated by individual rationality; iii) where dissolution is used as a punishment, subjects increases lenience, but are still forgiving; iv) overall efficiency is non-monotone in the outside option. An extension examines asymmetric outside options finding: advantages to terminating first-movers creates highly inefficient outcomes; a last-mover advantage is less inefficient but reduces forgiveness; while an arbitrator-mechanism assigning higher payoffs to "more-deserving" parties increases efficiency.

The second chapter experimentally investigates the effects from adding a simple and intuitive stage before the start of a repeated partnership, where agents communicate about strategies they intend to play. Varying the bindingness of the message sent in the preplay communication, I examine the efficiency gain of adding these two communication institutions and behavior of senders and receivers in each of them. I find that adding both forms

of preplay communication increases cooperation and efficiency in the ensuing repeated partnership. In particular, when the communication is binding, i.e. senders formulate specify strategies as contracts, the efficiency of the repeated partnership is highest. Moreover, I find that more cooperative and lenient strategies are sent and receivers are more cooperative in partnerships governed by contracts.

The third chapter analyzes the sales method for a sample of 575 M&A deals announced between 1998 and 2012 and find that targets use auctions to increase the probability of finding bidders that can relax their financial constraints rather than to create operational synergies. Auctions, compared to negotiated deals, are associated with significantly higher target announcement returns, especially for relatively small targets. Bidder returns are positively related to auctions for bidders acquiring relatively small targets, not for the full sample. Taking into account size differences, we find that auctions, decrease target gains and increase bidder gains expressed in dollars.

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Preface

This work would not have been possible without the help and support by many people. I owe my gratitude to all of my commitee members, Dr. Alistair Wilson, Dr. John Duffy, Dr. Lise Vesterlund and Dr. Frederik Schlingemann, to make this happen.

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This long and lonely journey would not have been possible without the support and love of my mother, Wei Chen, father, Minghua Wu and my girlfriend, Xinyang He. Their faith in me has kept me striving for completing the dissertation.

1.0 DISSOLUTION OF PARTNERSHIPS IN INFINITELY REPEATED GAMES

1.1 INTRODUCTION

Dissolving a relationship is a familiar, easy-to-understand dynamic response, and can be readily incorporated as a future punishment threat to support cooperation today. It is clearly a force in many repeated scenarios of interest to economists: Workers quit firms that treat them badly, and are fired by firms that find them unproductive. Couples petition for divorce if their marriages become unhappy. Consumers stop patronizing businesses where they have had bad experiences, while firms refuse to deal with problem customer (schools expelling students, insurers denying renewals). But in these examples participants also have access to, and make use of, in-relationship punishments: Workers strike and conduct slow-downs, firms demote workers, cut back hours, or withhold bonuses. Couples argue, and atone for things they did not do. Businesses win back customers with steep discounts after bad service, while consumers can retain access to firms by paying more (a donation to the alumni fund, paying higher premiums). In environments where mistakes or bad outcomes are inevitable, despite the best efforts of all parties, in-relationship punishments allow for the possibility of forgiveness, and a return to cooperation, where leaving the relationship does not.

Whether or not dissolution is preferable to in-relationship punishment for the individidual depends on several factors: What is the in-relationship punishment's expected value, can it recoordinate on an efficient outcome? How severe are the dissolution frictions (legal costs, losses on the sale of illiquid jointly held asset, reputation shocks, etc.)? What is the expected value from starting afresh in a new relationship (given equilibrium selection, and accounting for the population available to rematch)? Are outside options fixed, and commonly available, or do they depend on the way the relationship ends? Such environments have many moving parts, and the values to both remaining in the relationship and dissolving it are endogenous, leading to many possible equilibrium outcomes. To get a handle on such a complex problem, our paper uses a laboratory study to examine selected outcomes as we *exogenously* vary the outside-option value with exogenous, stationary rematching. Using a fixed in-relationship stage game—a prisoner's dilemma (PD) game with imperfect public monitoring—our experimental treatments alter the possibility of/payoffs attained by walking away.

In the static PD game, and in finitely repeated play, we have an inefficient equilibrium outcome: both players defect, and the outcome is Pareto dominated by joint cooperation. However, where interactions are repeated indefinitely, more-efficient equilibria become possible. Folk theorems tell us that with patient enough participants, all individually rational payoffs can be supported in equilibrium Fudenberg and Maskin (1986)—each individual can condition their future behavior, and so any deviations from the intended path today will be punished tomorrow. The ensuing multiplicity in prediction has been addressed by a recent and growing body of experimental work analyzing equilibrium selection, finding strong support for conditional cooperation when players are patient enough. Moreover, in experimental implementations with imperfect monitoring—where actions in the stage game are unobserved, with outcomes providing an imperfect signal—results indicate subjects have an affinity for lenient and forgiving strategies Fudenberg et al. (2010). Subjects require multiple bad outcomes to enter a punishment phase (so they are lenient), while punishment durations are short followed by a return to cooperation (so they are forgiving).

Because signals are imperfect, entering a punishment phase eventually is unavoidable, even with both parties cooperating. The selection of forgiving punishments therefore serves to increase efficiency, as the relationship will not become mired in an inefficient punishment. To these games, our paper adds an intuitive, empirically relevant punishment device, ending the relationship, where terminating a relationship precludes forgiveness.

Whether or not dissolution is preferable to in-relationship punishment depends on several factors: What is the in-relationship punishment's value (and can it recoordinate on efficient outcomes)? How severe are any dissolution frictions (legal costs, losses from illiquid markets for jointly held assets, reputation shocks, etc.)? What is the expected value from starting afresh in a new relationship (given the selected strategies, accounting for the population available to rematch)? Such environments have many moving parts, and the values to both remaining in the relationship and dissolving it are endogenous, leading to many possible outcomes. To get a handle on such a complex problem, our paper uses a laboratory study to examine selected outcomes as we *exogenously* vary the value of terminating with exogenous, stationary rematching. Using a fixed in-relationship stage game—a PD with imperfect public monitoring—our experimental treatments alter the possibility of/payoffs attained by walking away.

The paper addresses two main questions: i) How does the presence of an option to walk away affect efficiency, and how does it change as we alter the outside option's value? ii) What types of punishment are used to support cooperation at each outside-option value; what triggers their use, and how does the presence of dissolution crowd-out the selection of potentially more-forgiving in-relationship punishments?

A first set of experiments examines dissolution with symmetric payoffs to the participants, where each has access to the same in-relationship actions/payoffs and the same fixed outside option. Here the fundamental tension is the interplay between incentive compatibility and efficiency of inside and outside the relationship punishments. A slight strengthening of the folk theorem (weakly undominated, individually rational payoffs) implies termination should never be used when its value is lower than the *expected* value of the in-relationship minmax (in our game, joint defection). Though its presence might alter equilibrium selection, we should not see relationship dissolve along the path. In contrast, once the dissolution payoff exceeds the expected in-relationship minmax, the effect is to alter the individually rational point. Dissolved relationship are now expected to be observed along the path. Our experimental results are mostly in line with this hypothesis. When outside options are far below the in-relationship minmax (in which region, we can think of termination as a form of costly punishment) we see very low rates of dissolution. When very high, we see much more substantial rates of dissolution. However, in contrast to the prediction, we observe large increases in termination use once outside options exceed the minimum *realization* attainable from the in-relationship minmax, rather than its expectation.

In terms of the types of strategy used we find two main results: First, the *presence* of termination increases the selection of initially cooperative strategies, even where termination is weakly dominated and is an unused action by most subjects. Second, as mentioned above, once the outside option exceeds the minmax realization, strategies using termination become the focal form of punishments. Subjects utilizing termination punishments are more lenient in entering punishment phase, but just as forgiving as those subjects in treatments without termination or with much-lower outside options. This somewhat counter-intuitive result is explained by the frequent selection of a compound-punishment device we refer to as "probation" strategies. Here, the first part of the punishment is the probation period, an in-relationship punishment (defection in the PD). Successful outcomes in probation lead back to cooperation, so the punishment is potentially forgiving. On the other hand, continued bad outcomes in the probation phase lead to the second part of the punishment: dissolution. However, despite selecting forgiving strategies, the rate at which intially cooperative strategies are used is mostly unchanged from other treatments. Though observed cooperation rates in ongoing relationships do increase slightly with higher outside options, this is an effect of attrition, as relationships with uncooperative players are dissolved more frequently.

In terms of overall efficiency, our experimental results point to a non-monotonicity over the outside-option value. Through an increased selection of cooperative strategies, the presence of termination with a low payoff provides an initial boost to efficiency. From the lowest outside option levels, efficiency increases with the outside option, until the dissolution payoffs exceeds the minimal payoff from the in-relationship minmax. Ending a relationship becomes a more plausible threat as the value increases, but its actually use is infrequent. However, increasing the outside option beyond this point leads to much higher rates of termination. Because using dissolution is fairly inefficient ex post, and we do not observe increased cooperation, this leads to a large welfare drops. Increasing outside options still further increase welfare again. The selection of cooperative strategies and the usage of dissolution remain flat, while the ex post inefficiency on termination decreases proportionally.

The symmetric outside-option treatments addressed the *size* of the pie on dissolution, and provide a window into the effect of *ex post* frictions on *ex ante* outcomes in relationships. Our second set of treatments examines effects from changes to the relative *distribution* on dissolution. Here we are motivated by relational contracts specifying how assets/costs are divided when relationships are dissolved. Examples range from prenuptial agreements in marriages to LLP's incorporation documents, consumer mortgages and residential leases to joint-venture by firms; severance payments for fired workers and non-compete clauses for ship-jumping employees. In these examples, contracting parties retain rights to unilaterally void the contract, subject to the early termination costs/benefits specified. Our second treatment set examines three plausible asymmetric divisions on dissolution: i) an environment where the party terminating receives the larger amount; ii) an environment where the party who is being terminated gets the larger amount; and iii) an environment where an independent arbitrator/judge diagnoses "blame" for the partnership's dissolution, assigning the larger amount to the more-cooperative party.

Our findings in these asymmetric environments are stark. Rewarding the party ending the relationship leads to a strong selection of termination from round one, despite expected outside options being Pareto dominated by the in-relationship minmax. Relationships with this type of division rarely seem to get off square one in our experiments. In contrast, in what seems to be a more common arrangement in leases and labor contracts, an asymmetric division rewarding the party being terminated substantially reduces termination rates, though we do find reduced cooperation and fewer subjects using forgiving strategies relative to comparable symmetric-payoff treatments. Finally, our simulated-arbitrator treatment produces very high cooperation rates, leading to the most-efficient outcomes across the studied environments. Subjects select lenient strategies, with high initial cooperation rates (where punishments exhibit similar forgiveness levels to other treatments). Because subjects are highly cooperative, and lenient towards bad outcomes, the selected strategies lead to lower dissolution rates than the symmetric treatments where terminating strategies are selected. Despite dissolution payoffs with ex post inefficiency comparable to our worst symmetric treatment, the asymmetric assignment produces the right marriage between plausibility of the punishment's use by cooperators, and the punishment power to discourage deviations.

Below we briefly discuss the connection between our paper and the current literature. Section 2 describes our experimental design and explores some of the theoretical predictions in each of our seven treatments. Section 3 reports the results from the experiments, while Section 4 discusses the results and concludes.

1.1.1 Literature

The folk theorem for repeated games with discounting is articulated in Fudenberg and Maskin (1986), and shows that any feasible, individually rational payoff can be sustained in equilibrium when players are sufficiently patient.[1] The theorem is constructive and shows that cooperation can be supported by punishing deviations by minimizing the maximum amount deviators can obtain, over a number of punishment round. Repeated games with *imperfect* public monitoring were initially studied with respect to cartel behavior in dynamic Cournot-type competition. Firms receive imperfect signals of the other cartel members' quantity decisions from the market via prices (Porter, 1983), citepgreen1984noncooperative, but monopoly quantities can be implicitly supported by the cartel with high prices with market flooding punishments (Cournot quantity choices) whenever the price gets too low. Further theory for imperfect monitoring is developed in Abreu et al. (1986, 1990), where the authors demonstrate the simple structures that can support optimal cooperation.

The closest theory paper to our environment is Radner et al. (1986), who study partnerships game with imperfect monitoring and positive discount rates.[2] Their finding is that the supergame equilibria are bounded away from full efficiency, uniformly over the discount. Fudenberg et al. (1994) subsequently extends the folk theorems to infinitely repeated games with imperfect public monitoring, articulating a condition (pairwise identification) on the monitoring technology for the folk theorem.[3] The partnership game in Radner et al. (1986), and our own stage game, fails this condition, and so points on the feasible-payoff frontier are not attainable, even for $\delta \to 1$.

Given the endogenous endpoints (choosing to end the relationship) in our experiments, our environment is more technically a dynamic or stochastic game Dutta (1995). That is, we require an additional state variable that determines the particular stage game (here whether or not a player has terminated before the current period), and this state variable

[1]For earlier work see references within Fudenberg and Maskin (1986), in particular Friedman (1971).

[2]In this version of the partnership game, monitoring is two-sided imperfect. In contrast, in the principal-agent game in Radner (1985), imperfect monitoring is one-sided. See also Cole and Kocherlakota (2005) for characterization of symmetric public-perfect equilibria attainable with finite memory strategies.

[3]Essentially the condition requires that players' deviations can be statistically distinguished, so that punishment can be optimally used.

is endogenous, determined by players' actions. In many dynamic games the focus is on Markov perfect equilibrium Maskin and Tirole (2001), an equilibrium refinement where strategies are conditioned only on the state variable. Given the simple binary nature of the state variable in our experiments, and the complete lack of agency in one state (inactive partnerships), our focus will instead be on a larger set of equilibria for our game, public-perfect equilibria (cf. Fudenberg and Tirole (1991)).

Theoretical models for dividing surplus on termination have focused on ex post division of the partnership's assets Cramton et al. (1987); Preston McAfee (1992).[4] Comino et al. (2010) posit that firms might strategically leave out explicit termination clauses to ensure costly litigation on early termination, which is related to findings with low termination values.[5]

Early experiments on infinitely repeated games showed that cooperation is greater when it can be supported in equilibrium, but that subjects fail to make the most of the opportunity to cooperate (see Roth and Murnighan (1978); Murnighan and Roth (1983); Palfrey and Rosenthal (1994)). More recent experiments (Dal Bó (2005); Aoyagi and Fréchette (2009); Duffy and Ochs (2009)) have provided more positive results on subject's ability to support cooperation in infinitely repeated games. The theory on infinitely repeated games listed above does not generically provide sharp predictions—the folk theorems predict no specific payoffs within the individually rational set, without further refinement (for example, symmetry and efficiency). Recent experimental evidence has made a contribution to these equilibrium-selection questions. Dal Bó and Fréchette (2011) study the evolution of cooperation in infinitely repeated PD games, but they introduce a methodology for estimating the strategies used. They find that in treatments where cooperation *cannot* be supported in equilibrium, the level of cooperative strategies decreases with experience and eventually converges to a fairly low level. When cooperation *can* be supported in equi-

[4]For an experimental examination of ex post dissolution see Brooks et al. (2010).
[5]See also Li and Wolfstetter (2010).

librium, subjects fail to cooperate as much as they can, with *Tit-for-Tat*-like strategies becoming more prevalent at higher discount rates, where the gains from cooperation are highest.

Experiments have also studied noise/imperfect public monitoring in repeated games. Fudenberg et al. (2010) study repeated PD games with noise (implemented as a probability that a selected *cooperate* choice is implemented by the computer as *defect*, and vice versa) and find that successful strategies are "lenient" in not retaliating after a single deviation, and that many use "forgiving" strategies in order to return to cooperation after a punishment phase. In a different setting, Aoyagi and Fréchette (2009) examine imperfect public monitoring by experimentally varying the quality of the public signal. Their main finding is a rise in cooperation levels with increased signal quality. Embrey et al. (2011) examine an environment with the most similar signal structure to our own paper, where their experiments manipulate the game through the addition/subtraction of a third intermediate action between the standard cooperate and defect, and the relative temptations. Their paper's aim is to examine the empirical validity of alternative equilibrium concepts (in particular renegotiation proofness). Though they do not find much support for the renegotiation predictions, they do find that it helps predict the selection of more-forgiving strategies.

Experiments have also introduced punishments into the repeated PD game. Dreber et al. (2008) study the option for temporary and costly punishment, where they find that the existence of a punishment option substantially increases cooperation but not the average payoff of the group. In our treatments with symmetric but low outside options, termination is similar to costly punishment, though termination commits to an irreversible path and cannot be used to realign non-cooperative agents. However, for higher outside options, termination takes on a different role and adds equilibrium-selection questions.

Perhaps closest in motivation to our paper is Rand et al. (2011). They study cooperation in a structured network, where they examine the effects on the ability to change partners. Their paper examines termination and re-partnering as a punishment/selection device, but where the value of terminating is endogenously determined. They find that when subjects update their network connections frequently, cooperation is maintained at a much higher level through endogenous selection. Our own paper seeks to map out the effects of exogenous changes to the outside-option/punishment on cooperation, where we do not allow new links to form.

Another related experimental paper is Hyndman and Honhon (2014), where they examines preferences for flexibility over termination options (though here in a coordination game). They find that subjects show a slight preference for flexibility in ending the relationship. However, subjects over-use the termination option, with too much sensitivity to short-term noisy outcomes. Our own paper indicates much more forgiveness, with subjects more willing to delay termination after a bad outcome.

1.2 EXPERIMENTAL DESIGN AND THEORETICAL PREDICTIONS

1.2.1 Repeated Partnership Game

Our experimental game has two players engaged in a repeated joint-production task with imperfect public monitoring, similar to that in Radner et al. (1986). In every round t of their interaction, each partner i simultaneously chooses a private action $a_i^t \in \{C, D\}$. Given the resulting action profile $a^t = (a_1^t, a_2^t)$, a public signal $Y^t \in \{\text{Success}, \text{Failure}\}$ is realized and observed by both players, alongside their round payoff $r_i(a_i^t, Y^t)$. The probability of a *Success* signal each round is a function of the selected actions, and the expected round payoff to partner i given the action profile (a_1, a_2) is

$$u_i(a_1, a_2) = \Pr\{\mathrm{S}\,|(a_1, a_2)\} \cdot r_i(a_i, S) + (1 - \Pr\{\mathrm{S}\,|(a_1, a_2)\}) \cdot r_i(a_i, F).$$

The partnership continues indefinitely under an exponential discount rate δ, so that the discounted-average expected payoff for the partnership is

$$W_i\left(\left\{a_1^t, a_2^t\right\}_{t=1}^{\infty}\right) = (1-\delta)\sum_{t=1}^{\infty}\delta^{t-1}u_i(a_1^t, a_2^t). \tag{1.1}$$

Table 2.1 indicates the payoff realizations $r_i(a_i, Y)$ and the conditional success rates $\Pr\{S\ |a\}$ chosen for our experiment, where the resulting expected payoffs make clear that the stage-game is a PD in expectation.[6] The game can be thought of as two partners making choices over their individual effort levels into a jointly held venture (with C being high effort). The two partners equally split a \$5 firm revenue on a success and \$2 revenue on a failure. If either chooses to put in high effort they individually incur an additional \$1 cost. Higher efforts increases the likelihood of a successful outcome: if both players expend effort there is a 98 percent chance the outcome is success; if both players put in low effort the probability of success is just 10 percent; if one exerts high effort and the other free rides the success probability is 50 percent.

Our main experimental treatments modify the repeated game above by adding a third action to the game, which allows either partner to unilaterally dissolve the partnership. If either party chooses this third action, T(ermination), no further action choices are made by either partner in subsequent rounds,and both parties receive a fixed payoff. Our experiments will focus on two types of termination: i) symmetric termination payoffs to each partner (π_T, π_T); and ii) asymmetric payoffs on termination $(\overline{\pi}, \underline{\pi})$, with the assignment of the the higher payment $(\overline{\pi} > \underline{\pi})$ dictated by players' actions.

[6]The game is therefore a prisoner's dilemma where outcomes are simple lotteries. Under constant relative risk-aversion there are *no* risk parameters that would reverse the PD ordering on the stage-game payoffs. Under constant absolute risk aversion, the risk aversion coefficient would need to be between 1.5 and 2.4 (for payoffs in cents) to induce a different ordinal game; consistent preferences at this level would require experimental subjects to take 50¢for sure over an even gamble between zero and a million dollars.

Table 1.1: Stage-game payoffs

		Payoff, $r_i(a_i, y)$		$\Pr\{\text{Success} \mid (a_i, a_j)\}$		Expectation, $u_i(a_i, a_j)$	
		y:		a_j:		a_j :	
		Success	Failure	C	D	C	D
a_i:	C	150	0	0.98	0.5	147	75
	D	250	100	0.5	0.1	175	115

1.2.2 Experimental Specifics

In Appendix B.1 we include more detailed instructions and screenshots of the interface used, but we here summarize the experimental design choices. The design is between subject: students are recruited for general economic experiments, and placed in sessions with exogenous and fixed assignment of treatment, the particular payoff on termination. Subjects participate in sessions of 12–16 subjects, at the start of which they are provided with instructions (a representative example of which is included in Appendix B.1) which were read aloud.

As asking subjects to provide infinite choice sequences is infeasible, the experiment mirrors an infinitely-repeated game with exponential payoff discounting through an exogenous, stochastically determined end point. That is instead of scaling down the payoffs receive the next period and onward by a factor $\delta = \frac{4}{5}$, we scale down the probability of obtaining *any* additional amount in the next and subsequent rounds, and retain the same stakes. After every round of the game where the partnership is still accumulating payment, there is a $\frac{1}{5}$ probability that payment for the supergame will end, and $\frac{4}{5}$ that it continues. The agents expected *discounted-average* payoff from the supergame is given by (1.1), as the probability of getting to round $t \in \{1, 2, \ldots\}$ is given by δ^{t-1}. Subjects are paid the sum of their realized round payoffs $r_i(a_i^t, Y^t)$ or (after a dissolution) their round termination payoffs π_T, up to point where the partnership payment exogenously stops.

This method for implementing repeated games (exogenous stochastic termination) with no fixed horizon goes back to Roth and Murnighan (1978) and has been used extensively in experimental studies of dynamic behavior. One drawback to using a stochastic endpoint is that observed relationships can be very short, where such short interactions offer limited power to assess the strategies being used. To increase the length of the observed partnerships in the experiment, we use a block design Fréchette and Yuksel (2013). Subjects are only informed on when/whether the partnership payment has ended after every block of five rounds. That is, at the end of every round the computer rolls a 100-sided die, common to all subjects in a session. The first round where the die-roll exceeds 80 is the last round for which we pay subjects. However subjects only observe the outcomes from these rolls after rounds 5, 10, 15, etc.. If all five rolls are less than or equal to 80 the game continues to another block of five rounds, otherwise the partnership ends and payment is made on all rounds up to the first die-roll over 80.

We will refer to each experimental supergame, a repeated partnership with another fixed individual, as a "cycle." At the end of each cycle, subjects are randomly and anonymously rematched, and they begin a new cycle. Sessions continue for at least an hour (excluding the time taken to read instructions). The first cycle to end after an hour is the end of the session. Two cycles are randomly chosen for payment. By design, each cycle in the experiment has the same duration for all participants, whether the partnership is dissolved or not. Subjects cannot influence their time in the laboratory, nor can they increase their payoffs by playing more cycles. However, one potential concern we had was that subjects might not use the termination option as they have no actions to take if they do. To mitigate this, our experimental design has each subject participating in two cycles concurrently (this method is also used in Hauk and Nagel (2001)).[7] To facilitate these two concurrent partnership, the matching protocol in each cycle randomly and anonymously forms subjects into a circle. The subjects' two cycle partners are the session participants clockwise and counterclockwise from their position in the randomly formed circle. In this way, we minimize the ability a subject has to affect their clockwise partner through actions they take with their counterclockwise partner. In addition, all elements of the design are held constant across treatments except the treatment variable, the availability of termination, and its payoff when chosen.

1.2.3 Termination Institutions

Theoretically, endogenous dissolution introduces a state variable into the partnership, that can be either *Active* or *Inactive*, where the transitions between states are determined by participants' choices. Our experimental game is therefore a stochastic game with imperfect information. However, because the *Inactive* state is absorbing (once a partner terminates the game never returns to the *Active* state) and degenerate (no players have any available choices over their actions here) this particular stochastic game is fairly simple. We will focus on examining choices in the *Active* state, and we will use standard infinitely repeated game concepts for our analysis.[8]

1.2.3.1 Symmetric Termination Payoffs Our first set of treatments consider an institution where the two partners receive the same payoff π_T if the partnership is dissolved. Interpretations for this are a partnership with assets having a net value of $2 \cdot \pi_T$ on dis-

[7] Referred to as *Partnership Red* and *Partnership Blue* within the experiment, both partnerships are affected by session-wide realizations for the exogenous end of payment. However, the probability of Success and Failure in each partnership depends only on the partnership-specific actions chosen, and are independent of all other outcomes in the experiment.

[8] N.B. The only pure-strategy Markov perfect equilibria for this dynamic game involves always defecting or always terminating, depending on the termination payoff π_T.

solution, where the partners equally split the proceeds, or that each partner anonymously re-enters some stationary matching market for new partnerships with an expected outcome net rematching costs of π_T. The effects from the addition of the termination option to the partnership game is to change the sets of feasible and individually rational (IR) payoffs. Generically, the feasible payoffs are the convex hull of the expected stage-game outcomes in the *Active* state and the termination payoff vector (π_T, π_T).

In terms of individual rationality, the addition of termination complicates things. Because termination can be unilaterally imposed there are weak Nash equilibria of the game where both parties terminate in round one, regardless of the value π_T.[9] Because of this, our focus here will be on a refinement: individually rational outcomes of the game attainable in weakly undominated strategies.

Given the weakly undominated restriction we will examine two cases: when $\pi_T < u_i(D, D)$ and $\pi_T \geq u_i(D, D)$, where the extra-relationship payoff is below and above the in-relationship minmax. In the first case, any strategy involving termination is weakly dominated by the same strategy with D-forever after replacing any termination action. In the second case, because termination can be unilaterally imposed any discounted-average payoff less than π_T is not individually rational. So, weakly undominated individual rationality yields the following testable hypotheses:

Hypothesis 1. *When $\pi_T < u_i(D, D)$, termination is not used and the expected discounted-average payoff vector satisfies $(W_1, W_2) \geq (u_1(D, D), u_2(D, D))$*

Hypothesis 2. *When $\pi_T \geq u_i(D, D)$, the expected discounted-average payoff vector satisfies $(W_1, W_2) \geq (\pi_T, \pi_T)$*

Hypothesis 1 is slightly more amenable to experimental tests than Hypothesis 2, as it precludes termination as an observed action choice, whereas the defect action can still be chosen along the path when $\pi_T \geq u_i(D, D)$ (but only in transition to playing C again, or where the other player uses termination). Hypothesis 2 does have a complementary

[9] Note, this does require a fixed division of the termination payoff. If, for example, the person who initiates dissolution gets a lower value $\underline{\pi}$, the (T, T) equilibrium will not hold.

prescription, that *both* players cannot play *Always Defect* after any history.[10]

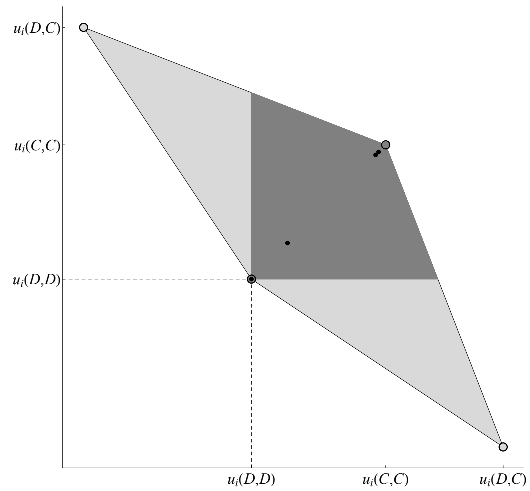

Figure 1.1: Feasible, Weakly-Undominated–Individually-Rational and Equilibrium payoffs-No Termination

[10]N.B. For all values of $\pi_T > u_1(D, D)$ the asymmetric strategy pairing (*Always Terminate*, *Always Defect*) constitutes a weakly undominated equilibrium outcome.

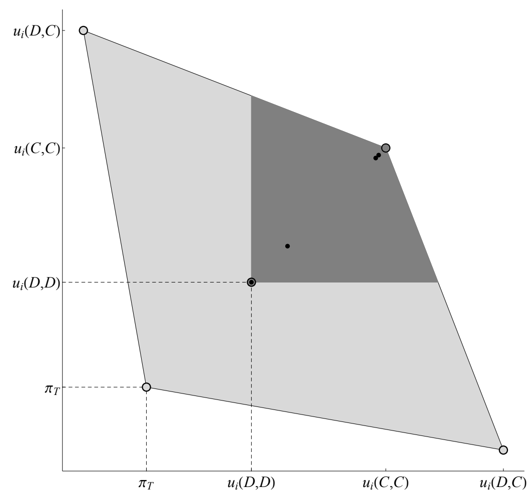

Figure 1.2: Feasible, Weakly-Undominated–Individually-Rational and Equilibrium payoffs-Termination lower than joint defection

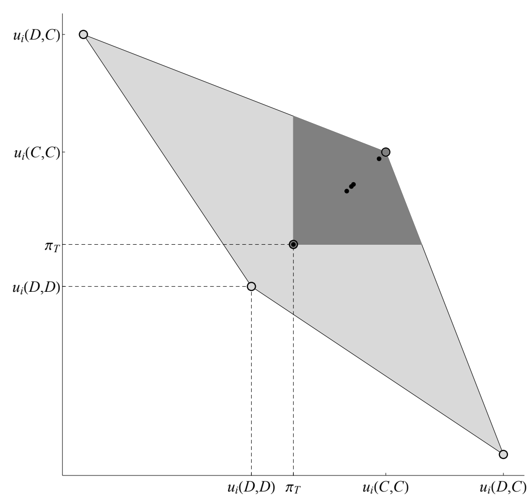

Figure 1.3: Feasible, Weakly-Undominated–Individually-Rational and Equilibrium payoffs-Termination higher than joint defection

These two hypothesis are illustrated graphically in Figure 1.1, Figure 1.2 and Figure

1.3. In each figure the lighter gray polygon represents the set of feasible discounted-average expected payoffs, while the darker-gray region is the set of weakly undominated, individually rational expected payoffs. The first figure (1.1) illustrates the partnership game without a termination option, figure (1.2) the game with a termination payoff of $\pi_T < u_i(D,D)$ (in particular $\pi_T = \$0.75$), while figure (1.3) illustrates the case where $\pi_T > u_i(D,D)$ (in particular $\pi_T = \$1.25$). In addition to the IR payoffs, Figure 1.1-Figure 1.3 also indicates the expected payoffs possible from all symmetric equilibria using 3-state machines (with further details provided in section 1.2.4 below).

Given that the expected discounted-average payoff from joint defection is \$1.15 in our stage game, our main symmetric treatments focus on termination values of $\pi_T = \$0.75$ and $\pi_T = \$1.25$, which we label as *S-75* and *S-125* respectively, where we run three independent experimental sessions for each. To contrast the behavior when termination is available we also conduct three sessions where the termination action is not available (a treatment we will refer to as *No T*). In addition, to understand substitution between termination and defection as a punishment we run one session each for $\pi_T \in \{\$0.85, \$0.95, \$1.05, \$1.15, \$1.35\}$, (with the treatment labels S-$100 \cdot \pi$). In Section 1.2.4 we specify further restrictions over strategies, however, we first introduce our asymmetric treatments.

1.2.3.2 Asymmetric Termination Payoffs Our second set of treatments examine the same repeated partnership game with a dissolution option, however the two partners here receive different payoffs on termination. One partner receives a high payoff amount $\overline{\pi}$, while the other gets the low payoff, $\underline{\pi}$ .Which partner gets which payoff is decided by the actions preceding termination. All of our treatments with ties on the assignment-rule break them with a fair coin, so the expected payoff on a tie is $\hat{\pi} = \frac{1}{2}\overline{\pi} + \frac{1}{2}\underline{\pi}$ for both players.

In general, the asymmetric division could depend on the entire available history $\left\{a_1^t, a_2^t, y^t\right\}_{t=1}^T$ up to the point of termination. There is subsequently a huge constellation of asymmetric division rules. Our focus will be on the three rules and parametrizations, which to our minds, are both strategically interesting *and* policy relevant:[11]

Asymmetric First: this treatment assigns a higher payoff to the party ending the relationship, and the lower payment to the party being terminated, where we label this treatment *A-First*. The treatment is motivated by partnerships where parties that exit first are best prepared, or where last-movers are left stuck with large debts/costs. There are recent examples of law and accountancy firms, incorporated as partnerships, which upon hitting hard times saw large-scale partner defections to rival firms (one would assume bringing with them many of their previous firms' clients).[12] The treatment mirrors this tension, a strong belief that the other will leave the partnership makes leaving yourself a best response.

Unlike the symmetric cases, joint-termination cannot be removed by appealing to weak dominance for any $\overline{\pi} > \underline{\pi}$. Each partner strictly prefers to end the relationship if they believe the other is going to too. In particular, we will examine the case where the discounted-average value of joint termination, $\hat{\pi} = \frac{1}{2}\overline{\pi} + \frac{1}{2}\underline{\pi}$, is lower than \$1.15. The treatment therefore alters the weakly undominated individually rational point to $(\hat{\pi}, \hat{\pi})$, an equilibrium outcome that is Pareto-dominated by the in-relationship minmax.

The addition of this inefficient equilibrium might not matter if subjects are able to coordinate on different equilibrium strategies. However, asymmetric division with a first-mover advantage has another negative effect compared with comparable partnership games with symmetric outside options. Per the above, the set of weakly undominated IR payoffs in the symmetric game is $\{\text{Feasible}\} \cap \{(u', u'') \,|u', u'' \geq \max(\hat{\pi}, u_i(D, D))\}$. However, because of the asymmetric division and absorbing nature of dissolution, any feasible payoff outside

[11] An interesting institution which we have not pursued is asymmetric fixed division, where player one gets $\overline{\pi}$ with certainty on dissolution. Theoretically, this is not too different from the symmetric division discussion above, but can allow for 'abusive' equilibria where player one uses the threat of termination to produce asymmetric outcomes such as *(D,C)*.

[12] For instances: Arthur Andersen LLP, saw huge partner defections after the Enron accounting scandal broke; Howrey LLP, a global law firm dissolved itself in 2011, but witnessed extensive partner defections to competing firms before this point.

of $\{(\hat{\pi},\hat{\pi}),(\underline{\pi},\overline{\pi}),(\overline{\pi},\underline{\pi},)\}$ involves some positive probability of both players *not* terminating. As such, any expected payoffs to the player in $(\hat{\pi},\overline{\pi})$ cannot be enforced, as terminating in the first round would produce a strictly better outcome. Where $\overline{\pi} > u_i(D,D)$, the asymmetric division has the effect of removing any equilibria which rely on punishment phases with continuations in $(u_i(D,D),\overline{\pi})$, any pure-strategy equilibria of the game using termination must do so jointly.

Given the above, our parametrization for this treatment chooses dissolution payoffs of $\overline{\pi} = \$1.25$ and $\underline{\pi} = \$0.75$. Joint-termination in round one is Pareto dominated by *Always Defect*, but $\overline{\pi} > u_i(D,D)$, so the high termination payoff creates a meaningful restriction on the equilibrium set as *Always Defect* cannot be an equilibrium punishment. However, the chosen parametrization does allow for equilibria with initial cooperation supported by joint-termination on failure.

Hypothesis 3. *In the* A-First *treatment the discounted-average payoff vector* either *satisfies* $(W_1,W_2) > (\overline{\pi},\overline{\pi})$ or *involves joint termination in round 1 and* $(W_1,W_2) = (\hat{\pi},\hat{\pi})$.

Asymmetric Last: Our second asymmetric treatment *A-Last* is the mirror of *A-First*, assigning the higher payoff to the party who has been terminated, and the lower payoff to the partner choosing to terminate. Individual rationality is now dictated by the point $\max\{\underline{\pi},\min\{\overline{\pi},u_i(D,D)\}\}$. Each player can guarantee themselves the higher of: i) the lower termination payoff $\underline{\pi}$ if they choose to end the game themselves, or ii) if they switch to defect forever, the other partner can force them to take the minimum of the joint-defection payoff or the high termination value $\overline{\pi}$.

This treatment is designed to mirror contract clauses that specify the party initiating early termination incur additional costs. Many employment contracts allow for severance payments ("golden parachutes" for executives) if the firm voids the relationship. If the contract is voided by the employee this severance payment is not made (in the other direction, employees who walk away might have to adhere to non-compete agreements, reducing their outside options). This might induce employees in faltering relationships to seek termination by the other party, rather than quitting themselves.[13]

Unlike the *A-First* treatment, *A-Last* does not have joint-defection as an equilibrium outcome, as each partner does strictly better by defecting if they believe the other will terminate. Cooperation cannot be supported by joint termination, and so equilibrium punishments must use some defection by at least one player. When $\underline{\pi} < U_i(D, D)$, the weakly undominated IR set is identical to the symmetric termination games with $\pi_T < U_i(D, D)$. In order to focus on the tension between ending the relationship oneself vs having the other player terminate, our treatments use the parametrization $\underline{\pi} = \$1.25$ and $\overline{\pi} = \$1.35$. Given this, unilaterally dissolving the partnership is better than joint-defection forever, but each partner strictly prefers that the other party is the one terminating. Individual rationality is therefore defined by the low-termination payoff of \$1.25, where the weakly undominated IR set is the same as the *S-125* treatment. However, the equilibrium sets do differ, which we discuss in more detail below.

Hypothesis 4. *In the* A-Last *treatment the discounted-average payoff vector satisfies* $(W_1, W_2) \geq (\underline{\pi}, \underline{\pi})$

Asymmetric-Judge: Our final asymmetric treatment is motivated by arbitration-hearings after a relationship ends. A judge/arbitrator (through some perfect, possibly costly, forensic process) obtains access to the complete history $\left\{a_1^k, a_2^k, y^k\right\}_{k=1}^t$, not just the public history. She assigns the higher dissolution payoff to the party that cooperated most. That is, once one player chooses termination in round t, the judge examines the action sequence $\left\{a_1^k, a_2^k\right\}_{k=1}^t$, and assigns the higher termination payoff to player i and the lower payoff to player j if

$$\sum_{k=1}^{t} \left(\mathbf{1}\left\{a_i^k = C\right\} - \mathbf{1}\left\{a_j^k = C\right\} \right) > 0.$$

[13] An alternative non-equilibrium interpretation is that the party firing/quitting/jilting the other does so publicly. This public signal could plausibly affect their ability to recruit new partners/gain subsequent employment when rematching, so forcing the other party to leave is preferable.

We will call this our *A-Judge* treatment, and it is intended to mirror an institution where a third party chooses a division of assets, taking into account the partners' behavior. Examples of this institution are: divorce settlements, where judges might take into account the behavior of each party when dividing assets and custody of children, and labor arbitration hearings, where firms and workers abide by the third-party's decision.

The institution has the intuitive effect of increasing the outside option for cooperating players, and decreasing it for deviators. Termination punishments therefore make deviations more costly, while entering the punishment is more palatable to cooperators. However, in-relationship punishment becomes less useful, as resorting to it can be detrimental, as those being punished can cooperate, and then and seek arbitration.

Technically, this treatment induces a somewhat complex stochastic game, with an imperfectly observed, endogenous state, the cooperation-difference $\omega_t = \sum_{k=1}^{t-1} \mathbf{1}\left\{a_1^k = C\right\} - \mathbf{1}\left\{a_2^k = C\right\}$. Each agent observes their private history $\{a_i^k, Y^k\}_{k=1}^{t-1}$, from which they update their beliefs about ω_t, which influences their expected payoff from taking the action $a_i^t = T$. For a patient enough player, individual rationality can be shown to be arbitrarily close to $\overline{\pi}$.[14] However, it is beyond the scope of the present work to examine the game for arbitrary δ. Our focus will be on the strategies human subjects use in this environment, and whether they can be rationalized. We think this environment is well motivated both by observed institutions, and through the highly cooperative play we will document within it. However, as we mention below, none the simple set of strategies we will examine (nor non-trivial extensions) are equilibria of the *A-Judge* game.

Hypothesis 5. *In the* A-Judge *treatment the discounted-average payoff vector satisfies* $(W_1, W_2) \geq (\overline{\pi}, \overline{\pi})$.[15]

[14] Each player can specify a strategy that cooperates initially. After every period it calculates the probability of the observed sequence of outcomes under the null that the other play is cooperating. The strategy terminates if the probability of the observed sequence drops below some pre-specified confidence level $\alpha^\star$. So long as $\alpha^\star$ is small enough and δ large enough, ex ante this strategy guarantees an amount close to $\overline{\pi}$, regardless of the other player's choices.

[15] N.B. We present here the hypothesis for $\delta \to 1$. When $\delta = \frac{4}{5}$ the individual rational payoff is

1.2.4 Equilibrium Strategies

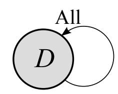

Figure 1.4: Simple three-state machines with Termination-Always Defect

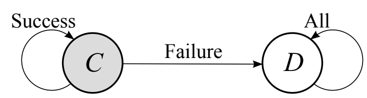

Figure 1.5: Simple three-state machines with Termination-Grim Trigger

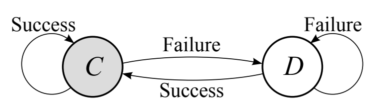

Figure 1.6: Simple three-state machines with Termination-Mono

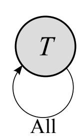

Figure 1.7: Simple three-state machines with Termination-Always Terminate

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strictly lower, between [101.7,115.0]. Given arbitrary strategies σ_i and σ_j for the two players, and expected discounted-average payoff of $\hat{W}_i(\sigma_i, \sigma_j)$. The two bounds on $\min_{\sigma_i} \max_{\sigma_i} \hat{W}(\sigma_i, \sigma_j)$ come from: $101.7 = \min_{\sigma_j} W_i(\hat{\sigma}_i, \sigma_j)$ for the player strategy $\hat{\sigma}_i$ which plays two periods of initial cooperation then terminates after the first failure; and $115.0 = \max_{\sigma_i} W_i(\sigma_i, \hat{\sigma}_j)$ for the strategy $\hat{\sigma}_j$ of *Always Defect*.

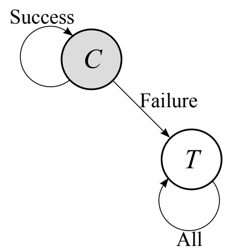


Figure 1.8: Simple three-state machines with Termination-1 Strike

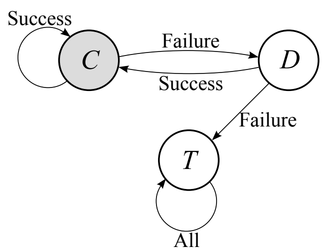


Figure 1.9: Simple three-state machines with Termination-Probation

The above outlines fairly broad hypotheses using just weak dominance and individual rationality to produce lower-bound predictions on the payoffs possible in any equilibrium of the game, for arbitrary δ. We now look more constructively for simple equilibria of these games under $\delta = \frac{4}{5}$. Our focus will be on the following restricted, but cognitively simple set of equilibria: weakly undominated, symmetric, stationary, public-perfect equilibria (wSSPPE), which can be represented as finite machines. That is, we will look for strategies of the game that depend only on the public signal in the last round Y^{t-1} and a finite number of internal states. The machine's chosen action at each point in time is dictated by its internal state, and the transition between these states depends only on public signal (S or F) and the internal state last round. For simplicity, we here examine only those machines with three internal states C, D and T, where the three states are connected to the actions cooperate, defect, and terminate, respectively. By discussing only the strategies that fall into this small category we hope to illustrate the broad effects on the types of equilibria possible from changes to the outside option. In our data analysis, we will consider a larger constellation of machines, and asymmetric pairings between them.

Because termination is an absorbing state by construction, the set of 3-state machines in our setting has 81 distinct entries.[16]

In terms of theoretical prediction, from those 81 possible machines, just three form wSSPPEs when the symmetric outside option π_T is less than \$1.15 (or when dissolution is not present). These three machines are depicted in the Figure 1.4- Figure 1.6, and correspond to the strategies (1.4) *Always Defect*, (1.5) the *Grim Trigger*, and (1.6) the *Mono(tone)* strategy, where we have highlighted the most-efficient starting state in gray.[17] When the value of termination is strictly greater than \$1.15 the set of equilibria changes, *Always Defect* is now replaced by the strategy (1.7) *Always Terminate* as an equilibrium, while the *Grim Trigger* is replaced by its termination complement (1.8), which we will refer to as *1-strike*. However, the forgiving *Mono* machine remains a symmetric equilibrium so long as the value of termination is not too much greater than \$1.15. As the termination value increases past \$1.24, *Mono* stops being an equilibrium—the continuation value in the D state falls below the dissolution option π_T, so the punishment is no longer sub-game perfect. As the value of termination increases further still (beyond \$1.31) the *1-strike* strategy drops from the equilibrium set too, as defecting in the C state becomes profitable as the dissolution punishment lacks power.[18],[19]

[16] The C and D states each have two edges, with three possible destinations, so there are $3^4 = 81$ possible machines, where the machines can start in any state

[17] If we look at the set of all two-state machines without termination (allowing for asymmetric matchings between different machines) there are four equilibria (unique up to state relabelings): symmetric *Always Defect*, symmetric *Mono* starting in either the *Cooperate* or *Defect* state, and symmetric *Grim Trigger*.

[18] Moreover, once the value of termination crosses \$1.32, the *only* perfect equilibrium of any form for $\delta = \frac{4}{5}$ is *Always Terminate*.

[19] In addition to the existence of differing equilibria, when termination increases in value past \$1.15, the comparable risk-dominance orderings of the equilibria change. Introduced in Blonski and Spagnolo (2004) and applied by Dal Bó and Fréchette (2011), risk dominance has been a useful measure to predict subjects'

Other than *Mono*, of the 81 machines we consider there are no other forgiving wSSPPEs for any termination value π_T—strategies capable of returning to cooperation after entering a punishment phase.[20] The only forgiving machine with incentive-compatible cooperation using both dissolution and the D action in the punishment path is the *Probation* strategy illustrated in Figure 1.9.[21] However, despite the punishment supporting cooperation, the strategy is not a wSSPPE as the punishment phase is not incentive compatible. Best-responding agents will deviate to play C (or T if π_T is large enough) where the strategy specifies the D action. This is because of a much higher probability of returning to the high-payoff cooperation state if they deviate to C (a 50 percent chance) as opposed to D (a 10 percent chance). Other simple forgiving strategies such as *Win-stay-lose-shift* (WSLS) and the unconditional one-round-punishment *(T11)* lack incentive compatibility for cooperation, as continuation values in the in-relationship punishment are too high.[22]

The asymmetric-division treatments have similarly stark predictions. For *A-First* just two of the three-state machines are wSSPPE: *Always Terminate* and *1-Strike*. In the *A-Last* treatment, none of these 81 machines are symmetric equilibria, though asymmetric combinations such as *Always Defect/Always Terminate and Grim/1-Strike* are w(no S)SPPEs. Finally, in the *A-Judge* treatment, none of the 81 machines, nor any asymmetric pairing are PPEs of the game. The *A-Judge* game requires much more sophisticated asymmetric strategies to form an equilibrium outcome (in particular mixed strategies with

response in perfect monitoring environments. Among the three equilibrium strategies for $\pi_T < \$1.15$ *(Grim, Mono* and *Always Defect*), we find that *Always Defect* risk dominates *Grim*, which in turn risk dominates *Mono*. Where $\pi_T \geq \$1.15$, *Mono* and *1-strike* both risk dominate *Always Terminate*, which dominates *Always Defect*.

[20] No "lenient" strategies are possible without allowing for additional states that play the C action.

[21] A variant of the Probation strategy which exchanges the *Success* and *Failure* arrows in the D-state is also not an equilibrium, though here because cooperation is not incentive compatible when the termination value is weakly greater than \$1.15, and for lower values the termination action is weakly dominated.

[22] There are many asymmetric equilibria when $\pi = \$1.15$, involving combinations of *Defection* and *Termination*. Once the termination value increases to \$1.25 *all* conditionally cooperative asymmetric equilibria combining the 81 machines involve at least one player using a variant of *1-strike*. For example, one player use the *Suspicious 1-strike* that starts at *Defect*, moves to *Terminate* on a failure, and to the standard 1-Strike *Cooperation* state on a success; and the other player can use *Suspicious-Grim* (replace the T action in *Suspicious 1-strike* with *Always D*).

a corresponding belief update rule over the game state ω_t).

1.3 RESULTS

We have conducted 20 sessions at the Pittsburgh Experimental Economics Laboratory. A total of 291 subjects, recruited from the University of Pittsburgh general subject pool participated in the experiment. For each session we recruited 18 subjects and ran with at most 16 from those attending the session, with an average of 14.6 subjects per session. Subjects earnings ranged from a minimum of \$5 to a maximum of \$77.75, where this includes a \$5 guaranteed show-up payment. Table 2.2 summarizes the experiments carried out and subject numbers per session.

Table 1.2: Experiment Summary

Treatment	**Sessions**	**Subjects**	**Cycles**	**Cycle Length**		**Activity**	**Active Choice Freq.**	
				Avg.	Max	$(t \geq 2)$	C	T |not C
S-75	3	14,16,13	21,23,24	4.3	21	0.936	0.580	0.018
S-85	1	16	12	7.8	17	0.940	0.494	0.014
S-95	1	16	24	4.6	17	0.867	0.739	0.066
S-105	1	16	23	4.4	9	0.699	0.583	0.115
S-115	1	16	18	2.4	6	0.778	0.537	0.098
S-125	3	12,16,14	18,20,17	5.1	20	0.624	0.607	0.143
S-135	1	16	21	5.0	21	0.572	0.536	0.129
No *T*	3	14,15,14	17,23,23	4.3	21	1.000	0.409	0.000
A-First	2	16,11	19,16	4.5	25	0.096	0.415	0.811
A-Last	2	14,15	24,20	6.0	13	0.775	0.519	0.072
A-Judge	2	11,16	13,20	5.7	25	0.790	0.834	0.142

Note: Number of cycles are given for each session, cycle lengths are in terms of payment rounds, observed rounds are nearest multiple of five above the final payment round.

1.3.1 Choices and Efficiency

We first examine aggregate choice behavior through some simple sample-averages, to illustrate broad patterns in the data. To start we look at the choice proportions within each treatment, aggregating over sessions, subjects, cycles and rounds. We then examine the within-session evolution of behavior, analyzing subject behavior as subjects gain experience with the environment. Next, we illustrate the patterns for within-cycle dynamics, comparing the cooperation and relationship activity in the first and fifth rounds of each cycle. Finally, we illustrate how these choices affect final outcomes, examining payoff efficiency across treatments, and comparing this to the hypotheses produced from weak dominance and individual rationality.

1.3.1.1 Aggregate Choices and Relationship Activity

The last three columns in Table 2.2 summarize sample averages for a nested series of binary outcomes. The *Activity* column summarizes the fraction of rounds $t \geq 2$ where the state is *Active*, where neither player has yet dissolved the partnership (all rounds are active by design in round 1). Where the state is *Inactive*, subjects had no choices to make, so the next two columns present choices conditional on the round being active. The penultimate column presents the cooperation rate in active partnerships. Finally, conditioning on both the round being active and that the subject chose not to cooperate, the last column illustrates the fraction of non-cooperative choices that terminate the relationship, with the residual being defections.

Aggregating all the symmetric treatments with the termination option available, subjects choose to cooperate 59 percent of the time, where the sample probability of cooperating varies mostly between 50 and 60 percent (the single *S-95* session is an outlier at 74 percent). Using session-level averages for *Active Cooperation*, we fail to reject equivalence using Mann-Whitney tests across the symmetric treatments.[23,24] Rather than raw cooperation rates, the main change across the symmetric treatments is over which non-cooperative action is selected. This is seen in the table through an increased termination rate as π heads past \$1.00 (and a matching reduction in activity). In treatments with

[23] All remaining tests in this section are one-sided Mann-Whitney tests using session averages against the small-sample U-distribution. We use this fairly conservative test to illustrate patterns, though for some comparisons the test is underpowered and cannot reject at the 10 percent level (any 3 session vs 2 session test), and so we will there examine tests against sessions from contiguous *S-X* treatments.

[24] For instance, we fail to reject a null of equivalence between *S-75* and *S-125* (three session observations for each), while there is no significant relationship between treatments with outside options lower than \$1.15 and treatments with values equal or greater than or equal to \$1.15 (six and five sessions, respectively).

$\pi_T > \$1.00$ subjects who have chosen not to cooperate end the relationship 10–15 percent of the time. In comparison, for dissolution payoffs between \$0.75 and \$0.95, the uncooperative subjects end the relationship between one and seven percent of the time. Examining session-level averages, all three *S-75* treatments have lower termination rates (and higher activity rates) than the three *S-125* treatments, so we can reject equivalence at the five percent level ($p = 0.050$).

However, a part of Hypothesis 1 (produced through risk-neutrality, weak dominance and individual rationality) proscribes termination as an observed action whenever $\pi_T < \$1.15$. Because this is a boundary prediction, the quantitatively small termination rates in *S-75* and *S-85* might be attributable to choice errors. However, given the size of the errors in *S-75–85*, the observed termination behavior in the *S-95* and *S-105* sessions are statistically larger ($p = 0.066$). With enough risk-aversion, one could rationalize observed termination in *S-105*, however, for *S-95* the termination payoff is lower than the minimal realization from choosing D, a payoff of \$1.00, so the Termination action is stochastically dominated by defecting forever.

In contrast to the *S-X* treatments, in *No T,* where termination is not an available action, the cooperation rate is significantly lower at 40 percent. We can reject equivalence in cooperation between *No T* and the symmetric-termination treatments *S-75–135* ($p = 0.011$).[25] Given the absence of a termination option in *No T,* all non-cooperative actions are necessarily defections, so there are no useful comparisons beyond cooperation.

The last three rows in Table 2.2 present the same data for the asymmetric treatments. In the *A-First* treatment the most obvious difference to the symmetric treatments is the very low activity rate (and correspondingly high use of termination). Indeed, the two

[25] Cooperation rates are also significantly lower in the *No-T* treatment compared to the symmetric treatment blocks where the weakly undominated IR hypotheses differ, *S-75–105* and *S-115–135* ($p = 0.048$ and $p = 0.018$, respectively). However, the test fails to reject when comparing the three sessions of *No-T* against the three sessions of *S-75* ($p = 0.200$), as the minimally cooperative session from *S-75* is smaller than the maximally cooperative session from *No-T*.

A-First sessions–where the partner who terminates gets a constant \$1.25 round payment to their partner's \$0.75—have the two lowest activity rates across the 20 experimental sessions. The observed activity rates drops below ten percent. Testing equivalence for termination/activity against any four other sessions (for instance, *S-125–135*), we reject in favor of greater termination and inactivity in *A-First* ($p = 0.066$). Though the overall cooperation rate in the *Active* state does not seem very low, at just over 40 percent, this figure oversamples pairs of cooperators who manage to get to an active round 2, where the vast majority enter *Inactivity* in round two onward. The raw termination rate in the very first round of each cycle is approximately 70 percent here. Given the round-one termination rate and independent matching, this explains the approximately 90 percent inactivity rate in rounds two and beyond, as $1 - (1 - 0.7)^2 \approx 0.9$.

In contrast, for *A-Last,* where those terminating receive the smaller payoff of \$1.25, compared to their partner's \$1.35, the observed activity rates are much higher. In fact both sessions have a higher activity rates than any session from *S-125–135*.[26] The small asymmetry in termination payoffs leads to relationships with longer durations. However, the institution does not have increased cooperation. In fact the average active cooperation rate is lower than both *S-125* and *S-135* (though not significantly so).

Finally, the last row in Table 2.2 provides data for the *A-Judge* treatment. Here we find the highest cooperation rates across all of our sessions. where 83 percent of the time the chosen action in an active partnerships is to cooperate (underpowered with $p = 0.101$ for tests against the *S-75*/*S-125*/*No-T* in isolation, but $p = 0.018$ against the nine session from the three treatments pooled together). Activity rates also indicate more-active partnerships ($p = 0.066$) than the symmetric treatments with high termination values *S-125–135*, and lower rates (again, *p=0.066*) than the low-termination-value treatments *S-75—95.*

Result Summary 1. *Results at the aggregate level indicate:*

[26] Termination rates are statistically lower (higher activity) when compared to the four pooled sessions of *S-125–135*.

- *As symmetric termination rates increase there is no significant effect on cooperation, but activity (termination) rates decrease (increase) with the outside option π_T.*
- *The presence of dissolution increases cooperation.*
- *Asymmetric treatments that reward players ending the relationship produce very high termination rates, while those that reward the player being jilted lead to low termination rates.*
- *The asymmetric division that favors the more cooperative player leads to very high cooperation rates and relatively low inactivity.*

1.3.1.2 Session Dynamics We here summarize the dynamics within sessions, providing more detail in Appendix B.0.1 for interested readers.

Result Summary 2. *Across sessions the data indicates:*

- *Reduced cooperation in* No T *relative to the symmetric termination treatments emerges quickly.*
- *Cooperation rates fall across all treatments' sessions except* A-Judge*, which exhibits increased cooperation.*
- *The direction of the trend in termination use depends on the outside option π_T. When above (below) the lower-bound realization for joint defection ($1), the termination rate increases (decreases) across the session.*
- *Subjects converge quickly within the session toward dissolution in round one of the* A-First *cycles.*

1.3.1.3 Cycle Dynamics Figure 1.10 illustrates the general patterns in response across cycles, comparing behavior in round one (the gray markers) and round five (white markers) within each cycle. The gray triangles indicate the average cooperation rate in round one (active in all cycles), while gray circles illustrate the termination rate in round one

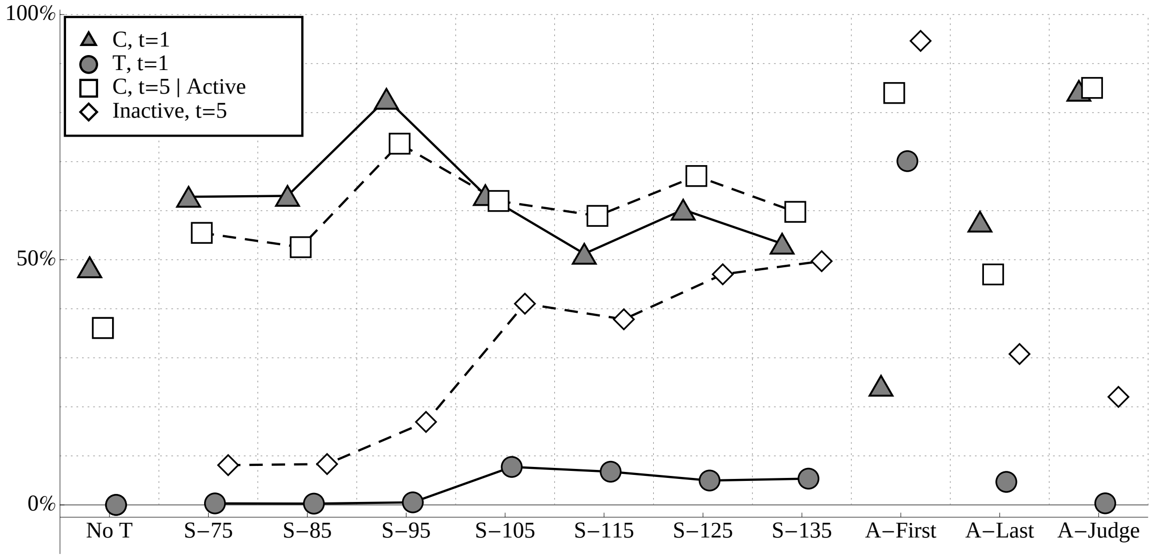


Figure 1.10: Actions and Activity across Cycles

(with the residual being defection choices). This behavior in round one of the cycle can be contrasted with the white shapes, indicating inactivity (white diamonds) and the active co-operation rate (white squares) by the cycle's fifth round. In addition, a table in Appendix B.0.2 provides greater detail on across-cycle response by providing the five-most-popular sequences of action-outcome pairs across cycles.

For *S-75–95* we observe reduced active cooperation across the cycle, but the vast majority of cycles are still *Active* in round five. In contrast, for *S-105–135* the pattern is reversed: the active cooperation rate increases across the cycle, but 40–50 percent of the cycles *Inactive* by this point.[27] In comparison to the symmetric termination treatments, the *No T* treatment starts out with lower cooperation, and the drop-off in cooperation

[27]The active cooperation rate within-cycle differences are significantly smaller ($p = 0.016$) when comparing *S-75*–95 to *S-105–135*, however, the relationship is not significant when comparing just *S-75* and *S-125* ($p = 0.200$).

across the cycle is comparable to that in *S-75–95*.

For our asymmetric treatments, the clearest effect is the very high termination rate in the first round of *A-First* cycles (70.1 percent), and low initial cooperation rate (24.3 percent). Matching this we find very high inactivity by the fifth round (94.6 percent), though for those partnerships which do survive to the fifth round, the cooperation rate is fairly substantial (84.0 percent cooperation for the 50 partnerships still active in round five). Second, within *A-Last*, despite observing similar initial cooperation rates to the comparable symmetric treatments (*S-125–135)*, we observe the opposite within-cycle response: many more cycles are still *Active* in round 5, and the active cooperation rate drops across the cycle. Finally, the *A-Judge* treatment has very high cooperation rates in active relationships, with little drop-off across the cycle. However, compared to the symmetric treatments where termination is frequently used (*S-105–135*), the *A-Judge* treatment has a substantially lower fraction of *Inactive* cycles by round five.

Result Summary 3. *Within cycle we find:*

- *Decreasing active cooperation rates for treatments where termination is not used (*S-75–95*,* No-T*).*
- *Where termination is used (*S-105–135*), active cooperation increase across the cycle, though this is potentially driven by selection, as uncooperatinve relationships become inactive.*
- *In asymmetric treatments,* A-Judge *sustains high cooperation across the cycle, while* A-Last *is the only treatment with substantial termination use where the active cooperation rate falls across the cycle.*

1.3.1.4 Payoff Efficiency We now examine payoff efficiency and the hypotheses from individual rationality and weak dominance detailed in section 1.2.4. Our efficiency measure will be the sample discounted-average payoff for cycles in the experimental treatment, relative to the discounted-average payoff from the in-relationship minmax ($1.15). This is then normalized by the difference in discounted-average payoffs between mutual cooperation and mutual defection ($1.47 − $1.15 = $0.32). The payoff efficiency for a particular subject-cycle is therefore

$$\Upsilon\left(\left\{a_1^t, a_2^t\right\}_{t=1}^{T}\right) = \frac{\hat{W}_i\left(\left\{a_1^t, a_2^t\right\}_{t=1}^{T}\right) - W_i\left(\{D, D\}_{t=1}^{\infty}\right)}{W_i\left(\{C, C\}_{t=1}^{\infty}\right) - W_i\left(\{D, D\}_{t=1}^{\infty}\right)},$$

where $\hat{W}_i\left(\left\{a_1^t, a_2^t\right\}_{t=1}^{T}\right) = {}^{(1-\delta)}/_{(1-\delta^T)} \sum_{t=1}^{T} \delta^{t-1} u_i(a_1^t, a_2^t)$ for cycles that end (exogenously) in round T.

Taking sample averages for Υ across all subject-cycles we illustrate the payoff efficiency by treatment in Figure 1.11. For each treatment, the figure indicates the discounted payoff efficiency across all cycles as the larger gray diamond, where the error bars indicate a 95-percent confidence region for this mean. In addition to the overall discounted average, we illustrate the average payoff efficiency for round one on its own, and rounds five onward as white circles and triangles. Payoff components from Hypotheses 1–5 (alongside an upper-bound from the best wSSPPE efficiency attainable, per section 1.2.4) are illustrated as the shaded region. Examining the lower and upper bounds on efficiency, all of our treatments except *S-135* fall within the identified region (for the discounted-average, as well as in rounds one and rounds four onward). With the exception of *S-135*, we therefore fail to reject any of the IR hypotheses.[28]

[28]Given $\delta = \frac{4}{5}$, the unique equilibrium payoff in *S-135* (all equilibria involve at least one player terminating in the first round) coincides with the IR payoff, producing a payoff efficiency of 62.5 percent. The sample-average efficiency in the *S-135* session is significantly lower at 56.5 percent, where a bootstrap indicates values greater than the IR prediction with probability 0.043. However, examining the discounted-average payoff from rounds five onward, the average efficiency in *S-135* is 60.8 percent, with a 0.362 probability the mean is larger that 62.5 percent. Rounds two–four drive the lower average, and the discounted-average efficiency across these three rounds is 44.8 percent, reflecting a high degree of (C, D) and (D, D) choices (expected efficiencies of 31.3 percent and 0 percent, respectively).

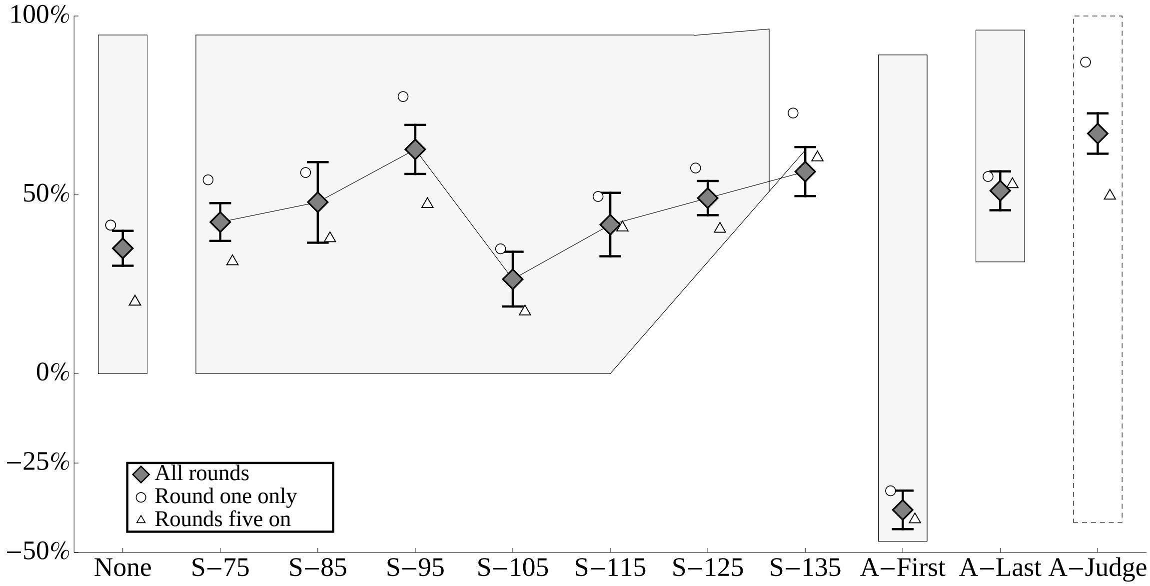


Figure 1.11: Payoff Efficiency by treatment

Note: Shaded areas represent lower-bound from individual rationality hypotheses (achievable with *Always Defect* or *Always Terminate* in majority of treatments), and upper bound from best symmetric pure-strategy memory 1 wSSPPE. For *A-Judge*, the indicated region represents a lower bound on the IR level at $\delta = \frac{4}{5}$, upper bound indicated at full efficiency. Error bars indicate 95 percent confidence region for mean payoff-efficiency calculated with a bootstrap of size 5,000.

For the symmetric termination treatments, five out of seven generate significantly higher efficiency than *No-T*: *S-75—95*, *S-125* and *S-135* when we examine averages over all subject-cycles.[29] The pattern in the figure indicates increasing efficiency as the outside option increases from 75 to 95, stemming from increased cooperation. However, a sharp drop in efficiency occurs at *S-105*, as subjects begin to use the (here highly inefficient) termination action more frequently. From here efficiency increases with the termination value, where this is driven by increasing payoffs from termination, rather than increased cooperation rates (seen by greater slope of the shaded region lower bound).

For the asymmetric treatments, the lower-bound efficiency in *A-First* is negative, as

[29]However, there is substantial variation at both the session and subject-levels.

symmetric *Always Defect* Pareto dominates symmetric *Always Terminate.* Moreover, as we have detailed, termination from the very first round is both an equilibrium outcome and the modal experimental response. As such this treatment generates the worst outcomes among all our treatments. The *A-Last* treatment performs similarly in efficiency terms to the comparable symmetric treatments (*S-125* and *S-135*). However, where most other treatments start out with higher efficiency and decrease consistently over the cycle, the *A-Last* treatment initially falls, then begins to increases again once the partnership is old enough. Finally, *A-Judge* generates the highest efficiency among all our treatments, however it also generates the highest drop-off in efficiency across the cycle.

Result Summary 4. *In terms of efficiency we find:*

- *Across all treatments except* S-135 *we fail to reject our weakly undominated, individually rational hypotheses.*
- *In total efficiency terms, the best treatment is* A-Judge *and the worst is* A-First*.*
- *Efficiency is not monotone in the outside option, where a middle region where termination is frequently used (but highly inefficient) reduces welfare.*
- *The majority of the symmetric treatments (*S-75–95 *and* S-125-135*) are more efficient than* No T*.*
- *Efficiency falls as the cycle progresses in almost all treatments.*

1.3.2 Strategy Estimation

This section investigates the strategies adopted by subjects within each treatment. We use the methods detailed in Dal Bó and Fréchette (2007), referred to as the *Strategy Frequency Estimation Method* (SFEM, also used in Fudenberg et al. (2010); Embrey et al. (2011)).[30] To use the SFEM method we specify a set of 38 strategies, motivated both

[30]In addition to this, Appendix B.0.2 provides a comparable reduced-form analysis of the data, looking at the one-period conditional response. This appendix effectively estimates an aggregate level memory-one public strategy (with probabilistic transitions).

by theory and the previous experimental literature. Given the strategy-set restriction, and an econometric error term (an independent probability of mistakes when implementing a strategy), we estimate the proportions of play for each strategy with a maximum likelihood approach, using data from the last six cycles in each experimental session. Appendix A outlines the method in more detail and reports full estimation results over the 38 strategies. We focus here on providing the broad families of strategy used, and the modal strategy selections.

Table 1.3 reports the proportion of estimated strategies that exhibit: i) initially cooperative behavior; ii) the possibility for ongoing cooperation; iii) lenience in response to failure; iv) forgiving punishment phases; and v) punishment phases with termination components.[31] In addition to these broad groupings, the table also indicates the three most-popular strategies in each treatment, and their estimated incidence.

[31] Possibility of cooperation is defined by there existing a future-round m and a sequence of public outcomes such that the strategy cooperates in all rounds $n > m$. A lenient strategy is any machine with a cooperative state that points to another cooperation state in both outcomes. A forgiving strategy is a machine with a defect-state which can transit back to a cooperation state. A terminating strategy is any machine which enters the *Inactive* state with positive probability.

Table 1.3: Strategy Classifications (Last six cycles)

	No T	***S-75***	***S-125***	***A-First***	***A-Last***	***A-Judge***
Initial *C*	52.5%	64.8%	59.8%	7.2%	54.8%	96.3%
Ongoing *C*	58.4%	64.9%	55.2%	0.0%	51.0%	88.9%
Lenient	10.4%	29.2%	40.9%	0.0%	24.0%	69.5%
Forgiving	27.0%	15.9%	33.7%	0.0%	10.5%	34.0%
(+ *All-C)*	(32.6%)	(33.7%)	(33.7%)	(0.0%)	(24.1%)	(61.9%)
Terminating	0.0%	7.2%	53.9%	92.8%	11.5%	28.6%
Popular	All-D (34%)	All-D (31%)	All-D (19%)	All-T (93%)	All-D (36%)	All-C (28%)
Strategies	Grim (26%)	Grim (24%)	Probation-21 (17%)	CDCD (7%)	Grim (25%)	Mono-31 (25%)
	Mono (7%)	All-C (18%)	2-Strike (12%)	–	All-C (14%)	2-Strike (13%)

In *No T*, where termination is not an option, the most-common strategies are always defect (*All-D*), the grim-trigger (*Grim*), and the monotone strategy (*Mono*). Just ten percent of the selected strategies exhibit lenience (*All-C* and the *Sum-2* strategy that cooperates if more successes have been observed than failures), while 27 percent are forgiving (primarily the *Sum-2, Mono* and *WSLS* strategies). However, just over half of the responses are initially cooperative, while 58.4 percent of the selected strategies are capable of sustaining cooperation.

In comparison to *No T,* the *S-75* treatment adds termination as an available action, but theory indicates its use is weakly dominated. Here the SFEM estimates indicate just 7 percent of selected strategies use termination, where this figure is much higher than the figures suggested by a reduced-form approach. The reason for the differing incidence is that the terminating strategies indicated by SFEM are the conditionally cooperative *2-*

and *3-Strike* responses. These lenient strategies require two or three failures to trigger dissolution, and otherwise cooperate. As such, termination is used less frequently along the path, despite a higher incidence of subjects selecting strategies with termination as a punishment.

The *S-75* treatment is more cooperative than *No T* (both initially, and in selecting strategies capable of ongoing cooperation) and has greater lenience, and so is more likely to remain at cooperation. The primary driver for this is a much larger proportion of subjects assessed as using the most cooperative strategy from the 38 specified, the *All-C* strategy. Always cooperate is selected at the expense of two forgiving strategies, the suspicious variants of *Mono* and *WSLS* (termed *S-Mono* and *S-WSLS* in Appendix A estimation tables) that start out in the defection state. Because *All-C* has no punishment phase it is not classified as a forgiving strategy. Potentially, what has been classified here as *All-C,* are in fact just very lenient, conditionally cooperative strategies. Because of the excessive lenience, the particular paths of play may not identify the punishment, and subsequently whether the punishment is forgiving or not. As such, below the *Forgiving* row we also provide the fraction of forgiving strategies including *All-C.*

Increasing the outside option from $0.75 to $1.25 has the effect of substantially increasing the selection of terminating strategies. Fifty-four percent of selected strategies use termination along the path, where the most-common termination strategy is a *Probation* variant. Labeled *Probation-21*, this initially cooperative strategy is lenient with an additional cooperation state after the first failure, and a round of defection after the second failure, but terminating on the third failure. Success in any of the probation parts of the punishment moves the state back into the first cooperation state. As such the strategy is both cooperative, lenient, forgiving and terminating. The estimates reflect an approximately 21 percent incidence of the *1-*, *2-* or *3-strike* strategies, where the most commonly chosen of the three is the (lenient) *2-strike* at 11.5 percent. Our estimates indicate that *S-125* is in fact more lenient *and* more forgiving (weakly if we include *All-C* as forgiving) than either *No T* or *S-75*, where forgiveness is driven by the 17 percent selection of *Probation*-21.

For the asymmetric treatments, the clearest selection is also obvious from the aggregate levels, the heavy use of *Always-T* in the *A-First* sessions, with 93 percent of subjects consistent with this strategy. In *A-Judge,* where active cooperation rates and efficiency are highest, the threat of arbitration induces high frequency of selection for *All-C* (27.9 percent) and a very lenient and forgiving monotone-strategy variant (*Mono-31*), with three cooperation states and a final defect state after four sequential failures. The vast majority of selected strategies are initially cooperative, and 89 percent are capable of sustaining cooperation given successes. Of the small minority that do play non-cooperative strategies, the most common are the false cooperator (*C-AllD*) which cooperates in round one, and then switches to always defect, the standard *All-D*, and the strategy which alternates between *C* and *D* regardless of the public outcome (*CDCD*), with each strategy selected at a three to four percent incidence. The majority of selected strategies are lenient and (if including *All-C*) forgiving.

Finally, examining *A-Last*, the small asymmetry in dissolution payoffs ($1.25 vs $1.35) leads to a much smaller incidence of terminating strategy selection. With the exception of *A-First*, the *A-Last* treatment has the lowest selection of cooperative strategies and the least forgiving responses. Always defect is selected 36.0 percent of the time, with the next most common strategy being the non-lenient, non-forgiving Grim. Comparing the selected strategies to *S-125*, we see a drop from just over half of strategies selected using termination to approximately one in ten. Of the terminating strategies that are selected, the most common is to terminate in the very first round (*All-T* at 5.8 percent) followed by the *D-2-strike* strategy at 3.4 percent (play *D* until two failures are observed, after which terminate).[32] Though the selected strategies are less likely to be terminating ones,

[32] A war-of-attrition style mixed-strategy wSSPPE exists for *A-Last* where each player defects with probability 0.758 and terminates with probability 0.242 with a discounted-average value of $1.262. Moreover, a *Grim*-like cooperative wSSPPE exists where this mixed-strategy is used as the punishment path following

the lack of lenience or forgiveness in the selections mean that dissolution is triggered more often. To illustrate this, the table indicates close to two and half times the incidence of terminating strategy selection for *A-Judge* over *A-Last.* But the inactivity rate for rounds two and beyond in *A-Last* exceeds that of *A-Judge.*

1.4 CONCLUSION

We experimentally investigate a series of prisoners' dilemma games with imperfect monitoring. Introducing a termination option into the PD stage game that can unilaterally end the relationship, our experiments manipulate the outside options available to the players.

Our first set of treatments examine the effect from varying the outside option symmetrically, where each partner receives the same payoff if the relationship dissolves. Here we contrast the observed outcomes to an imperfect-monitoring environment without a dissolution option. Our findings suggest that the use of termination as a punishment is directly related to the outside option's value: if remaining in an uncooperative relationship stochastically dominates walking away, subjects rarely end the relationship. However, the presence of a dissolution option does increase both cooperation, and the lenience of the strategies used. Without a termination option, the cooperative strategies selected are rarely lenient, with punishment phases triggered by a single failure. In contrast, the selection of lenience increases by a factor of three to four in our main symmetric treatments with a termination option.

Moreover, we do not observe drops in the selection of forgiving strategies. Where outside-options dominate the in-relationship punishment, we do see a majority of subjects using terminating strategies. Despite the use of the intrinsically unforgiving termination

a failure.

action, forgiveness does not decrease. The reason for this is a large proportion of subjects combining both in-relationship and dissolutions punishments. The selected probation strategies initially use in-relationship defections to punish, and in this phase are capable of returning to cooperation. The second-stage of the punishment uses termination only after continued bad outcomes. By using more sophisticated combinations of punishments, subjects retain the ability to forgive and therefore return to the cooperative path.

Our results do differ slightly from hypotheses generated from weak dominance and individual rationality. Ending a relationship is a weakly dominated action whenever the outside-option value is lower in expectation than the in-relationship minmax. However, we begin to observe termination use at significant frequencies below this level. Our results suggest a substitution toward terminating strategies whenever outside options are not stochastically dominated by staying within the relationship. Once the outside option exceeds the lowest individually-rational *realization* (as opposed to its expectation), we see higher rates of termination and relationship inactivity, increasing through sessions as subjects learn to use termination more often. Below this level, we see much-reduced termination use and inactivity, and this decreases over sessions. This leads to a non-monotonicity in observed efficiencies with respect to outside options. As outside options increase, their plausibility as punishments increases, which helps increase cooperation and therefore efficiency. However, because the termination rate increases so much at the minimal realization, we see large welfare losses: subjects are using a very costly form of punishment. Further increases to the outside option, decrease the costs of the termination punishment, and we again see increasing welfare. The results suggest an optimal friction for the dissolution process (incompleteness in contracts, costs in trials, etc.). These costs should be small enough to make the punishment just plausible enough to be a threat, but large enough to make the vast majority unwilling to use this option in all but the most extreme cases.

Our second set of treatments make payoffs on dissolution asymmetric, with one partner getting a higher payoff depending on the choices made while the partnership was active. Three simple institutions are analyzed: the party that terminates gets a higher payoff; the party being terminated gets a higher payoff; and, finally, the party that was more cooperative in the partnership gets the higher payoff. The effect from changing the asymmetric institutions are striking. Where first-movers have a payoff advantage, we see almost all subjects move towards termination in the very first round. Partnerships are very unlikely to get off the ground. The precise dissolution payoffs chosen actually result in outcomes Pareto dominated by the in-relationship minmax (itself dominated by mutual cooperation). In contrast, where the party being terminated receives a higher payoff, we observe a large drop in the fraction of subjects using termination, relative to comparable symmetric treatments. The quantitatively small asymmetry in dissolution payoffs leads to lower initial cooperation rates, and selected strategies selected that are less forgiving and lenient that those in the comparable symmetric treatments. Neither of these two asymmetric treatments indicate an efficiency gain from contracts specifying unequal divisions on dissolution.

However, our final asymmetric payoff treatment, *A-Judge*, is the most successful among all of our treatments. Selected strategies are mostly cooperative, forgiving and lenient, resulting in the high efficiency levels. This institution determines who receives the higher and lower dissolution payoffs by determining who cooperated the most within the relationship while it was active, and treatment mirrors an arbitrator or judge determining how to distribute payoffs (through some moral remit to assign it to the more-deserving party). In our experiments this judge player is automated and has access to a perfect forensic process. Future research might examine the extent to which the highly efficient outcomes we observe are retained when the arbitrator is a human subject without a set division rule and/or with imperfect information on the two player's actions. Such extensions will help gauge the robustness of this result, and may help guide remits for arbitration. Further to the discussion above, ex post inefficiencies can be useful to increase ex ante efficiency, and our *A-Judge* treatment has an expected dissolution payoff across the partners equal to the lowest *realization* for the in-relationship minmax. This low efficiency ex post allows for potentially very costly arbitration discovery hearings. Unlike the symmetric treatment with a comparable average dissolution payoff (*S-105*, representing comparable dissolution frictions), asymmetric division and assignment to the more-cooperative player combines a highly plausible threat, with far-greater punishment effect on deviators. The effect is large gains in cooperation rates, and reduced need to use the punishment. These gains are more than enough to offset the large costs when dissolution does occur.

2.0 COMMUNICATION IN INFINITELY REPEATED GAMES

2.1 INTRODUCTION

Communication is a prevalent form of interaction before a relationship convenes. In numerous scenarios of economic interest, agents communicate before they enter into a relationship: companies talk to workers about how they plan to behave before employment starts. Couples talk about life plans before marriage ensues. Partners discuss what each of them would do before they start a company. In some of these examples, communication is binding: companies sign contracts with workers on their promised behavior. Hence, deviation from promised behavior is costly. In contrast, in other examples, communication is non-binding: couples in general do not formulate contracts on their future behavior but rather rely on verbal commitments. Here deviation does not entail any cost. In these scenarios, the relationships are simple manifestations of the classical social dilemma, where individuals are faced with tradeoffs between individual benefits and group welfare. Communication, as a process of exchanging information and beliefs among individuals, might enhance efficiency in these social dilemmas.

The economic value of communication in dynamic, ongoing relationships depends on several factors. First of all, it depends on the binding power of the message. When the message is not binding, i.e. the talk is cheap, it depends on whether it is in the sender's best interest to signal and actually execute the efficiency-enhancing plan of actions. If it is, the message is credible to the receiver. When the message is binding, i.e. the cost of deviating is large enough, then it becomes a sequential decision problem where the sender directly selects and executes plans of actions in her best interest after which receivers best respond. In these two cases, the level of strategic uncertainty also varies. Cheap-talk partially reduces strategic uncertainty-it hinges on the likelihood that the message affects beliefs. On the other hand, fully-committed talk, i.e. a contract, removes all the strategic uncertainty. Besides this important determinant, several other factors also matter for the success of communication: attractiveness of the "secure" plan of action to the "risky" one, fixed matching protocol and information feedback,etc.. To understand how Cheap-talk and contracts create or destroy value, I conduct a laboratory study to examine selected outcomes and mechanics of the communication in a repeated partnership. A repeated partnership is abstracted with an infinitely repeated game with a fixed stage game-a prisoner's dilemma (PD) game with imperfect public monitoring. Moreover, I exogenously vary the existence of communication institution and the binding power of the message in presence of communication. When a communication institution exists, prior to each repeated partnership, it transits information unilaterally: a specific plan of action that the sender intends to carry out. A sender does not need to commit to the plan in a Cheap-talk but has to be fully committed to the plan in contracts.

In the static and finitely repeated PD game, mutual defection is the equilibrium and both players end up at this inefficient outcome. When repetitions are of indefinite horizon, more equilibria with efficient outcomes emerge. The classic folk theorem states that as long as agents are patient enough, all individually rational payoffs above mutual defection can be supported as equilibria (Fudenberg and Maskin (1986)). The theory does not provide further predictions as to the selection among these multiple equilibria, nor does it say how certain behavior is supported. Recent experimental evidence has explored which outcomes are selected, finding that agents fail to coordinate on the efficient outcome even under stringent conditions (equilibrium action, risk dominance) in repeated interactions but efficient equilibria can be supported when agents are patient enough or the payoff from cooperation is high enough. Moreover, in environments with imperfect monitoring, where actions in each stage game are unobservable and outcomes provide an imperfect signal, the previous literature (see Fudenberg et al. (2010)) finds that subjects tend to employ lenient and forgiving strategies. Subjects require multiple bad outcomes to enter a punishment phase, while punishment durations are short followed by a return to cooperation.

The above infinitely repeated games can be structured as symmetric coordination games with Pareto-ranked equilibria, a variant of the classic stag-hunt game. The concept of (pairwise) risk dominance by Harsanyi and Selten (1988) can be applied in the repeated game context to rank strategies (see Dal Bo and Frechette (2014)): Strategy A dominates Strategy B if A is a best response to the other play randomizing 50-50 between A and B. In a simple infinitely repeated PD game, if we focus on all two-state machines, suppose we have three equilibrium strategies: Always Defect (AD), Grim (G) and Tit-for-tat (TFT). Under some payoff structures, we can have TFT Pareto dominates G, which dominates AD. On the other hand, AD risk dominates G, which dominates TFT. Therefore the repeated game is reduced to an one-shot coordination game with Pareto-ranked equilibria. For this class of games, an extensive experimental literature has explored the role of communication in improving coordination (for reviews, see Camerer (2003) and Devetag and Ortmann (2007)). In particular, games with tradeoffs between Pareto efficiency and payoff security has been widely studied (Van Huyck et al. (1990); Cooper et al. (1990)). Among them, Cooper et al. (1992a) find that without communication the risk-dominant equilibrium tend to prevail. With Cheap-talk where players send a message about their desired actions, they tend to coordinate on the Pareto-efficient equilibrium.

In the environment of this study, however, assessing the coordination value of costless preplay communication raises a number of new questions. First of all, in the canonical stag-hunt game, the efficient equilibrium becomes the focal point with communication because it is a single action for the sender to choose and for the receiver to best respond with.

However in a repeated partnership, the efficient equilibrium comprises a strategy, i.e. a series of actions. Behaviorally, it makes the efficient equilibrium less salient: it comprises of actions as functions of a sequence of outcomes for senders to choose and for receivers to best respond with. In this situation, subjects might tend to choose less efficient but more salient strategies. Hence, whether preplay communication improves efficiency remains to be examined. Second, a message here is a plan of action for the entire forthcoming partnership. Therefore, as receivers go through the stage games, they learn the outcomes as noisy signals of senders' actual actions. Such informational feedback is partial and noisy, as compared to the full information feedback in coordination games explored in previous studies (e.g., Berninghaus and Ehrhart (2001); Brandts and Cooper (2006)), which was found to be efficiency-enhancing in repeated coordination games. Whether or not such noisy information feedback still improves efficiency remains an empirical question. Finally, messages of strategies create another dimension of behavior as to the role of communication in coordination: how the message of a plan of action affects the receiver's beliefs as it is verified repeatedly through the partnership. In summary, Cheap-talk does not change the set of equilibria but is likely to improve the coordination and efficiency. The value creation process, however, remains to be investigated. On the other hand, in the contract setting, the efficiency effect is straightforward: it hinges on whether the sender's best strategy is socially optimal or not.

In summary, the study addresses two main questions: i) Does preplay communication increases the cooperation and efficiency in a repeated partnership? ii) Which communication institution generates higher efficiency and how are more efficient outcomes reached? Moreover, I pay attention to the information transmission in each institution: the set of messages sent, senders' deviations from the messages (in Cheap-talk) and receiver's responses to the messages. The main result from the experiment confirms the conjectures: both communication institutions increase cooperation and efficiency relative to the baseline communication-free environment. Second, I find contracts generate higher in-relationship cooperation and efficiency than cheap-talk. Specifically, I find that actual cooperation of senders and receivers after both cheap-talk and commitment are higher than in the baseline. In cheap-talk, the actual cooperation from senders is lower than the implied cooperation (from the message) but the deviation rate is only about 10%. In response, receivers are less cooperative than senders by 10%. In contracts, senders' cooperation is higher than those in cheap-talk. Moreover, receivers are less cooperative than senders by only about 5%.

In terms of types of strategies selected, I separately analyze them in cheap talk and contracts, for senders and receivers. For both institutions, senders send four main strategies to receivers: AD (Always Defect), GRIM, T11 (One-period punishment) and AC (Always Cooperate). Among the strategies, AC is the most frequently sent message in both treatments: it was sent 47.5% and 65.7% of the time in Cheap-talk and Contracts respectively. Senders' deviations from these strategies in Cheap-talk are about 5%. Receiver's responses against AC are different as well. In Cheap-talk, the most common responses are AC (30.4%), AD (11.7%) and MONO21(13.9%) while those in contracts are AC (44.8%) and MONO22 (26.4%). For other strategies sent, except for AD, receivers' strategies are more cooperative and lenient in both institutions.

The results from the experiment suggest some departures from the theory. In Cheap-talk, receivers' behavior seems to be sensitive to the messages sent, and so sending messages conveying a dynamic response (one that will defect in some situations) can lead to the receivers focusing on the defection elements. On the other hand, sending the most cooperative message AC helps receivers focus on coordinating on efficient outcomes. In Contract, where receivers respond to a commitment, the AC contracts offered by senders can be thought of through the lens of the trust game. By offering to always cooperate, they are exhibiting a large degree of trust in the receiver in the hopes of getting the most efficient outcome. Though receivers have the option to extract the maximum possible payoff, for the most part they repay the trust shown in them through the offered AC contract by cooperating themselves.

Below I will first examine how my paper is connected to the current literature. Section 3 describes the game setup and explores some of the theoretical predictions. Section 4 presents the experimental design, followed by the report of results in Section 5. Section 6 concludes.

2.2 RELATED LITERATURE

2.2.1 Infinitely Repeated Games

The well-known folk theorem for repeated games (Fudenberg and Maskin (1986)) states that any feasible and individually rational payoff can be sustained in equilibrium when players are sufficiently patient. Repeated games with imperfect public monitoring are initially studied in dynamic Cournot competition settings, where firms see prices as imperfect signals of cartel members' joint quantity decisions, via (Porter (1983);Green and Porter (1984)). They find that when monitoring is imperfect, players have to enter a punishment phase of mutual defection to support any degree of cooperation, which is a stark difference from games of perfect monitoring.

The environment in this paper is closest to Radner et al. (1986), which studies the partnership game with two-sided imperfect monitoring and positive discount rates. The finding is that the supergame equilibria are bounded away from full efficiency uniformly in the discount rate. Fudenberg et al. (1994) subsequently extend the folk theorem to infinitely repeated games with imperfect public monitoring. They state the pairwise identification condition for the folk theorem, which requires that players' deviations can be statistically distinguished, so that punishment can be optimally used.

Early experiments on infinitely repeated games showed that cooperation is greater when it can be supported in equilibrium, but that subjects fail to make the most of the opportunity to cooperate (see Roth and Murnighan (1978);Murnighan and Roth (1983);Palfrey and Rosenthal (1994)). More recent experiments (Dal Bó and Fréchette (2011); Aoyagi and Fréchette (2009); Duffy and Ochs (2009)) have provided more positive results on subject's ability to support cooperation in infinitely repeated games. Dal Bó and Fréchette (2011) study the evolution of cooperation in infinitely repeated prisoner's dilemma games and find that when cooperation cannot be supported in equilibrium, the level of cooperative strategies decreases with experience. When cooperation can be supported on equilibrium, subjects fail to cooperate as much as they can, with Tit-for-Tat type strategies becoming more prevalent at higher discount rates and when the gains from cooperation are highest.

Experiments have also studied noise/imperfect public monitoring in repeated games. Fudenberg et al. (2010) study the repeated prisoner's dilemma with noise (implemented as a probability that a selection of cooperate is implemented by the computer as defect) and find that successful strategies were "lenient" in not retaliating after a single deviation, and that many used "forgiving" strategies in order to return to cooperation after a punishment phase. In a different setting, Aoyagi and Fréchette (2009) find that a rise in cooperation levels with increased quality of the public signal. Embrey et al. (2011) studies the role of renegotiation concerns in supporting cooperative equilibria. They find that a significant number of subjects reduce their level of cooperation in response to the addition of renegotiation concerns. It should be noted that they add stage-game communication into the experiment and find it to lead subjects to use more forgiving strategies to support cooperation, even in cases when such strategies are not incentive compatible. WILSON and WU (2014) investigates the effects from adding a termination action to a repeated partnership. They find that indicates selecting between in-relationship punishments or walking-away are dictated by individual rationality. Moreover, where dissolution is used to punish bad outcomes, subjects commonly use a compound punishment, with a forgiving probation phase before termination is used.

Dal Bó and Fréchette (2013) implements a strategy choice method prior to each repeated game to identify the set of strategies subjects employ. They ask subjects to design strategies that will play in their place and find that the strategy elicitation has negligible effects on behavior. They also find that the strategies elicited include some commonly mentioned strategies, such as tit-for-tat and grim trigger. In my experiment, I use a slightly more complex but general strategy elicitation method, which will be discussed in the experimental design section.

2.2.2 Preplay Communication

This paper is also related to the literature on preplay communication (i.e. Cheap-talk) [1]The theory of coordination via preplay communication starts from Farrell (1987, 1988) and Rabin (1991). Their analysis pose two conjectures: that preplay communication will yield and effective agreement to play an equilibrium in the underlying game; and that the agreed-upon equilibrium will be Pareto-efficient within that game's set of equilibria. Further they show that rationalizable preplay communication need not assure equilibrium; and that, although communication enhances coordination, even equilibrium with "abundant" communication does not assure that the outcome will be Pareto-efficient. Farrell and Rabin (1996)summarizes the theory on Cheap-talk and defines the types of messages existent in preplay communication: self-commiting and self-signaling messages. A message is *self-commiting* when it is optimal for the receiver to take the proposed action when the message is believed. A message is *self-signaling* when it is optimal for the sender to take the proposed action if the message is believed. They suggest that a self-commiting and self-signaling message is highly credible and therefore can enhance coordination, but even unlimited communication does not reliably lead to a Pareto-efficient outcome.

There is also a large experimental literature on preplay communication in coordination games. Two early studies suggest that coordination is a common phenomenon in the

[1]see Crawford (1998), Camerer (2003) and Devetag and Ortmann (2007) for related surveys

laboratory, namely, Cooper et al. (1990, 1992b). Cooper et al. (1989) consider two-player Battle of the Sexes with preplay cheap-talk. They find that coordination rates are much higher with one-way communication than with two-way communication and also that the sender in the one-way communication sends his favorite message, thereby inducing coordination on his favorite outcome, with a very large frequency. Cooper et al. (1990) finds that in games with conflict, one-way communication increases play of the Pareto-dominant equilibrium relative to the no communication baseline; two-way communication does not always decrease the frequency of coordination failures. In games with a less risky strategy, two-way communication always leads to the Pareto-dominant Nash equilibrium, while one-way communication does not. Some later studies [2] also find that costless preplay communication is efficiency-enhancing.

2.3 PREPLAY COMMUNICATION IN THE PARTNERSHIP GAME

The experimental game is a simplified example of the partnership game from Radner et al. (1986). Consider two individuals involved in a joint production task. In every period each partner has two actions they can take relating to their effort level: *High* or *Low.* In every period the partnership has two possible outcomes: *S(uccess)* or *F(ailure)*. Partners are imperfectly informed on the actions of each other with the public outcome signal stochastically related to the individual partner's choices. *High* effort is individually costly, but increases the likelihood of the *Success* outcome. In contrast, *Low* effort is less costly, but increases the likelihood of the *Failure* outcome. The parametrization implemented in the experiment, captures the classic tensions between individual and social welfare: the expectation of each action profile in our game produces a Prisoner's Dilemma game. I will

[2] e.g. Cooper et al. (1992b); Van Huyck et al. (1992); Blume and Ortmann (2007); Duffy and Feltovich (2002);Duffy and Feltovich (2006); Bangun et al. (2006))

therefore label High effort as C(ooperate) and Low effort as D(efect) to align to standard PD games.[3]

The stage game has each partner choosing an action $a_i \in \{C, D\}$, which yields the joint outcome/signal $y \in \{S, F\}$. In the experimental stage game the revenue from a success is \$6.00 while the revenue from failure is \$1.00 ($\pi(S) = 600$, $\pi(F) = 100$). Choosing high effort costs the agent \$0.95 for sure, while putting in low effort yields a certain gain of \$1.10 (so $c(C) = 95$ and $c(D) = -110$). The stage game payoff in cents are represented as in Table 2.1 and are given simply as $v_i(y, a_i) = \pi(y) - c(a_i)$.

Table 2.1: Stage-game payoffs

(a) Success outcome

1: \ 2:	C	D
C	(505, 505)	(505, 710)
D	(710, 505)	(710, 710)

(b) Failure outcome

1: \ 2:	C	D
C	(5, 5)	(5, 210)
D	(210, 5)	(210, 210)

(c) Expected outcome

1: \ 2:	C	D
C	(455, 455)	(280, 485)
D	(485, 280)	(335, 335)

The random success/failure variable $Y(a_1, a_2)$ depends on the actions of each of the two player. In particular, given both players cooperating there is a 90 percent chance the outcome is success. If both players defect the probability of success is just 25 percent. Finally, if one player cooperates and the other defects the probability of success is 55 percent ($\Pr\{\text{Success}\,|(C, C)\} = 0.90$,$\Pr\{\text{Success}\,|(C, D)\} = \Pr\{\text{Success}\,|(D, C)\} = 0.55$, and $\Pr\{\text{Success}\,|(D, D)\} = 0.25$). In the third panel I calculate the expected utility given the action profile $a = (a_1, a_2)$ as

$$u_i(a_1, a_2) = \Pr\{\text{Success}\,|(a_1, a_2)\} \cdot v_i(S, a_i) + \Pr\{\text{Failure}\,|(a_1, a_2)\} \cdot v_i(F, a_i).$$

[3]In the experiment the two actions are given the labels A and B.

Inspecting the third panel the game is clearly a prisoner's dilemma in expectation, however, the partners' choices induces a lottery rather than a fixed payoff. If both partners choose cooperate, they both receive the common lottery 90 percent on 505 and 10 percent on 5. If one partner defects and the other cooperates there is a 55 percent chance of the outcome 710 to the defector, 505 to the cooperator, and with 45 percent chance the outcome 210 to the defector 5 to the cooperator. Finally, if both defect they get a 25 percent chance of both getting 710 and a 75 percent chance of both getting 210. Finally, the discount rate in the partnership game is 80%.

Focusing on all up to two-state public strategies, the subgame perfect equilibria (SPE) in the partnership game are AD, GRIM, WSLS and T11, the explanations of which are as follows. The expected value of each strategy is in parenthesis.

- AD: Always Defect. (Value: \$3.35)
- GRIM: Cooperate until a Failure and Defect forever afterwards. (Value: \$4.21)
- WSLS (win-stay-lose-shift): Cooperate until a Failure and then switch to Defect; Defect until a Failure and then switch to Cooperate. (Value: \$4.44)
- T11(one-period punishment): Cooperate until a Failure and then switch to Defect; then switch back to Cooperate regardless of the outcome. (Value: \$4.46)

Denote the pairwise Pareto dominance relation as "$\succ^p$", where $X \succ^p Y$ means X Pareto dominates Y, i.e., X is Pareto more efficient than Y. In this game, $T11 \succ^p WSLS \succ^p GRIM \succ^p AD$. Similarly, denote the pairwise risk dominance relation as "$\succ^r$", where $X \succ^r Y$ means X risk dominates Y. Therefore, for this game, I have $AD \succ^p GRIM \succ^p WSLS \succ^p T11$.

2.3.1 Partnership Game with Preplay Communication: A Two-stage game

Here I consider two simple communication institutions, namely *Cheap-talk* and *Contract*, with one-way communcation from the sender to the receiver. The analysis above suggests that the supergame can be viewed as a coordination game, with risk dominance and Pareto dominance among the equilibria. Following Cooper et al. (1992b), the coordination game with one-way communication can be formalized as a two-stage game between two players. Here in the first stage the sender communicates to the receiver by sending a message comprising of a supergame strategy. In the second stage repeated game actions are chosen. A message here is a supergame strategy that specifies the action to take in every stage game by the sender. By restricting the attention on memory-1 machines, the set of potential messages available to the sender constitutes 32 strategies.

In Cheap-talk, payoffs are independent of messages, even though messages may influence actual play by affecting the beliefs of receivers about senders. In Contract, messages directly influence the payoffs: senders must commit to their messages and therefore their expected payoffs are determined by these messages, as if the sender formulates a unilateral "contract"; receivers freely choose actions in response to the contract specified by the sender. After the pre-supergame communication, players simultaneously choose actions in the second stage of the game.

I first analyze the set of equilibria in one-way cheap-talk. One equilibrium for the game is for the sender to randomly send messages and for these messages to be ignored. The equilibrium is called "babbling equilibrium" where messages are irrelevant and therefore communication does not change the set of equilibria of the repeated game. For messages to be relevant, it suffices to focus the subset of messages that transmit information of the four memory-1 equilibrium strategies. Farrell and Rabin (1996) assumes that for a message by the sender to be believed and honored, the message needs to be both self-committing and self-signaling: it would be optimal for the sender to commit to the message if the receiver believes the sender would commit to it. If announcing T11 leads to the play of T11, announcing and playing T11 is a dominant strategy for the sender. If this equilibrium occurs, sending T11 and both players playing T11 avoid coordination failures. Therefore, one equilibrium for the game with cheap-talk communication is the announcing T11, followed by both players playing T11. This equilibrium, in the same spirit as in stag-hunt type of games, demonstrates the value of preplay communication as a selection device towards more efficient outcomes. Such value can be seen as a way to reduce the riskiness of Pareto efficient equilibria, especially that of T11.

The plausibility of such efficient equilibrium as the experimental outcome is complicated by several behavioral factors in our environment. First of all, the key to successful communication in the stag-hunt game is the salience of the efficient equilibrium. Behaviorally, in our environment, multiplicity of dynamic equilibria and the noisy feedback channel make the efficient equilibrium less salient. Second, communicating a supergame strategy essentially specifies a function from outcomes to actions, which generates a series of actions through the repeated game. Here the credibility of these promised actions are dynamically related-the breakdown of credibility in previous actions might jeopardize that of later ones.

In Contract, the game simply reduces to a sequential decision problem, where in the first stage the sender ex-ante chooses the strategy to implement and then the other in the second stage best responds. The asymmetry here is that the sender commits to a *ex-ante* optimal strategy while the receiver's behavior still needs to satisfy the one-deviation property to deliver a best response. For each of the 32 candidate memory-1 strategies for the sender, a unique best response by the receiver can be identified. Conditional on the best response from the receiver, the strategy that generates ex-ante highest value for the sender constitute the unique equilibrium. Under my experimental parameters, DMONO-AC[4] is the unique equilibrium. In the equilibrium, the sender sends a message of DMONO

[4]DMONO: MONO that starts with Defect. MONO is the imperfect monitoring version of TFT: cooperate until a failure and defect; defect until a success and cooperate. AC: Always Cooperate. This strategy pair works as follows.

and commits to it throughout a supergame and the receiver best responds by playing AC. This delivers an *ex-ante* value of $4.58 to the sender. Very close to the optimal DMONO contract in the sender's values, a sender's contract of either MONO or AC[5] has a value of $4.55. In terms of efficiency, the expected efficiency of MONO/AC contract is $8.86 and that of the DMONO contract is $9.25.

In summary, there are fundamental differences in the ways these two communication institutions operate to coordinate agents' behavior towards efficient outcomes. Cheap-talk improves efficiency by affecting the beliefs of the receivers about senders and reducing the riskiness of coordinating on efficient equilibria. It does not change the set of equilibria in the game, each of which is symmetric. Senders' contracts directly select the unique asymmetric equilibrium, which, given sender's individual rationality, induces higher efficiency for both parties than that by a cheap-talk message. The important task of the following laboratory experiment, is then to test the efficiency comparative statics and examine how the mechanics of these two institutions work empirically.

2.4 EXPERIMENTAL DESIGN

The experiment uses the partnership game as the baseline game throughout three treatments, Baseline (no communication institution), *Cheap-talk* and *Contract*. In each treatment, there are three parts: Phase 1, 2 and 3. I refer to a supergame as a *cycle*. In all treatments, Phase 1 is the play of the normal partnership game for three cycles. Phase 2 is 20-minute play of the partnership game with incentivized strategy elicitation prior to the start of each cycle. Phase 3 differs in three treatments but all lasts for 50 minutes. In the Baseline, Phase 3 is the play of the normal partnership game again, just as in Phase

[5]A contract of MONO is: the sender commits to MONO and the receiver best responds by AC. A contract of AC is: the sender commits to AC and the receiver best responds by playing the following strategy: start with Defect and stay with Defect only with a Failure last round.

1. Phase 3 of Cheap-talk and Contract implements preplay cheap-talk and contract respectively. Each subject receives experimental payment from one randomly drawn cycle among all cycles across three phases. In Phase 2, strategies are elicited for every cycle, using the language that is later for communication in Phase 3. The structure of each treatment intends to provide subjects with experience in the type of repeated interaction in this study (Phase 1 and 2), and more importantly in the use of message language (Phase 2), before subjects are engaged in the two communication institutions (Phase 3). One cycle, randomly selected from completed cycles in all phases, is paid to each subject.

2.4.1 A cycle

The infinitely repeated game in the experiment is played in blocks of 5 rounds. In this block design, whether or not the stage games in the block are counted towards payoffs are determined by the continuation probability. Subjects are informed at the end of each block whether or not the current supergame has ended. If the block has not ended, they play another block of five rounds.The advantage of the block design is that it guarantees a minimum number of rounds for observation without payoff discounting. The observation of longer sequences of play is essential to getting good estimates of the strategies used by each subject. This design is based on methodological research by Fréchette and Yuksel (2013), which compares lab mechanisms for infinitely repeated games and presents evidence in favor of this block design. The block design has additional tradeoffs in the experimental setup of this study. On one hand, to increase the statistical power of estimating strategies subjects use, the block design is beneficial in ensuring longer sequences of observations in expectation. On the other hand, to provide experience with the message language, it is advantageous to have more cycles, while the block design potentially decreases it.

2.4.2 Strategy Choice Method

To elicit strategies from subjects, the experiment implements a strategy choice method prior to the start of every cycle, by modifying the method used by Dal Bó and Fréchette (2013). They use a strategy choice method featuring questions that elicit two-state strategies and a menu of strategies including ready made popular strategies such as GRIM, MONO, WSLS (win-stay-lose-shift), etc. For the initial set of sessions, they elicit strategies prior to the start of a supergame by providing five questions. Subjects first answer "In round 1 select {1, 2} and answer four questions covering all permutations of "After round 1 if, I last selected [1, 2] and the other selected [1, 2], then select {1, 2}". Later, to examine whether these simple strategies are sufficient to explain subjects' behavior, they provide extra options with more complexed strategies: subjects can either "build" their simpler plans by answering these questions or directly choose more complex strategies from a menu. Moreover, for early sessions, they find that strategy elicitation has negligible effects on behavior.

I modified their strategy choice method and adapted it to the environment of this experiment in the following ways. First, since the public outcome is the only observable signal in the partnership game, I changed each of the four questions (four actions to take after round 1) to take the form of "After round 1 if, I last selected [A, B] and outcome in the previous round was [A, B], then select {A, B}". Second, after subjects answer the five questions, a diagram organized a two-state machine, i.e. memory-1 strategy, that uniquely corresponded to the strategy specified and was presented to the subjects. For Phase 2 through Phase 3, this diagram represents the elicited strategy from each subject and is also used as the language of communication in Phase 3. Finally, the elicitation is incentivized such that a subject receives 10 cents if the actual action she chooses corresponds to the action her strategy suggests.

2.4.3 Cheap-talk and Contract

The partnership game with communication is implemented in Phase 3 as a two-stage game and has the following features. First, communication takes place first, followed by the same partnership game in Phase 1 and 2. Second, communication is one-way. Third, messages in communication use as language the diagram of two-state machines, each uniquely representing a strategy. First of all, each of the two paired subjects specifies the intended strategy she plans to send to her partner, by answering the five questions as follows. After the strategies have been specified, one party is randomly selected as the sender and the other is the receiver. Then the sender's strategy is revealed to both parties in every round of the cycle, as well as the action suggested by that strategy. Receiver's strategy is not revealed. In Cheap-talk, both parties can still freely choose actions in every round of the cycle. In Contract, the sender's strategy will play the partnership game for the sender while the receiver can still freely choose actions.

2.5 RESULTS

Six sessions of the experiment were conducted, two for each treatment, at PEEL (Pittsburgh Experimental Economics Lab), recruiting University of Pittsburgh undergraduates as participants. A trio of three sessions, each for a distinct treatment, shares a common sequence of random numbers that determine the length of each cycle. Such implementation renders the comparison of cooperation and efficiency across treatments free of any effects coming from different lengths of cycles. On the other hand, this also guarantees that subjects' experience in the repeated game environment is comparable across all treatments. In this section, the analysis of the main results is conducted in three parts: Summary Statistics, Cooperation and Efficiency and Strategies. As a first step, I report a brief summary of the experiment in Table 2.2.

Table 2.2: Experiment Summary

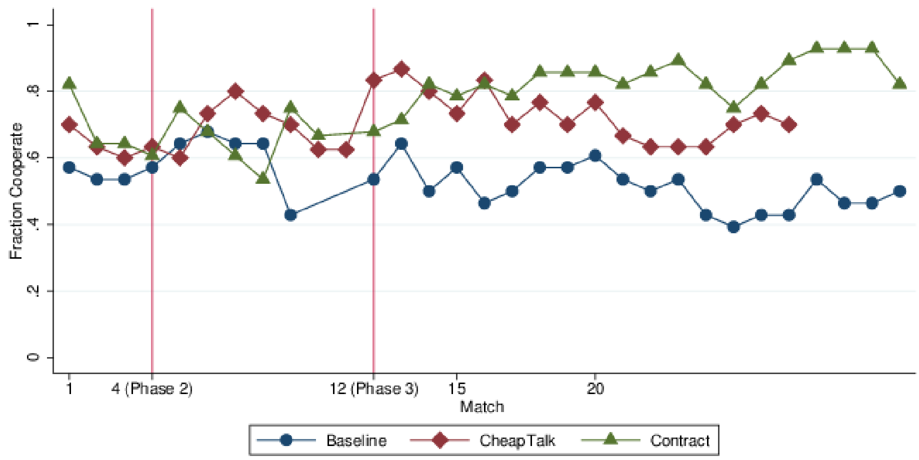
Treatments	Sessions	Subjects	cycles				Avg. cycle Length	Disc. Avg Payoff
			Phase 1	Phase 2	Phase 3	Total		
Baseline	2	14,14	3	6, 5	18, 20	27, 28	4.9	$30.2
Cheap-talk	2	14,16	3	6, 5	16, 18	25, 27	4.3	$31.2
Contract	2	12,16	3	8, 7	16, 20	27, 30	4.5	$32.9

Note: Number of cycles are given for each session, the cycle lengths are in terms of payment rounds. Disc. Avg Payoff is the discounted average cycle payoff.

2.5.1 Evolution of Cooperation

Next, I examine the evolution of cooperation for all treatments. For all treatments, subjects are treated exactly the same in Phase 1 and 2. Therefore, the evolution of cooperation in these three parts are expected to be statistically similar with sufficient sessions of data. Based on the similar cooperation profile through the end of Phase 2, I then only need to compare differences in cooperation rates in Phase 3 across all treatments. However, in practice, because I have limited sessions of data, possible differences of the evolution of cooperation can arise across treatments. In such case, I employ a Difference-in-Difference method to identify the effect of the treatment, i.e. communication institutions on the cooperation rates.

Figure 2.1 and 2.2 illustrate the evolution of cooperation in all three treatments. For both first and all round cooperation, I observe that cooperation rates start at similar levels in at the beginning of Phase 1 for all treatments. At the end of Phase 2, cooperation rates are similar for Cheap-talk and Contract but are lower in Baseline. In Phase 3, cooperation rates in Contract are higher than those in Cheap-talk and both are higher than those in the Baseline across all cycles in that Phase. Because I only have two sessions of data (about 30 observations per cycle per treatment), there can be session-level variances so that the cooperation rates through the end of Phase 2 differ significantly across all treatments. Hence, next I investigate the differences of cooperation rates across all treatments. Table 2.3 summarizes the results.



Note: Because some session has more cycles than another in one Phase, I label the first cycle in Phase 2 and 3 by the largest cycle number across all sessions, using red lines. Therefore cycle numbers are adjusted for sessions where the first cycle in one phase is smaller than that number.

Figure 2.1: Evolution of Cooperation: All Treatments First Round

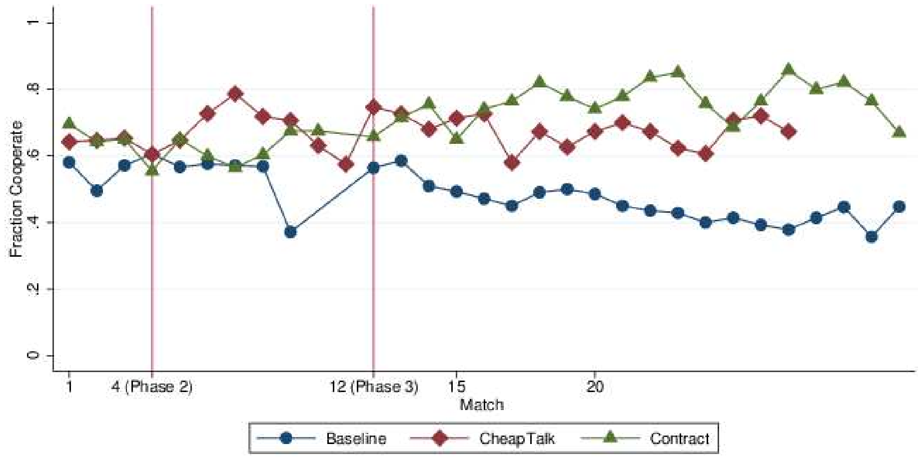


Figure 2.2: Evolution of Cooperation: All Treatments All Rounds

Table 2.3: Cooperation Rates by Treatment (%)

Treatments		Baseline		Cheap-talk		Contract	
Cycle / Round / Phase		Phase 2	Phase 3	Phase 2	Phase 3	Phase 2	Phase 3
Last Cycle	First Round	60.7	50.0	76.7	70.0	53.6	82.1
	Fifth Round	46.4	35.7	60.0	63.3	67.9	67.9
	All Rounds	57.6	43.6	68.0	67.3	60.4	66.9
All Cycles	First Round	61.7	50.9	68.9	73.1	64.6	83.2
	Fifth Round	53.2	43.8	64.2	60.0	57.9	73.0
	All Rounds	56.9	45.9	67.8	67.5	60.1	75.4

From Table 2.3, I find that cooperation rates in both the last cycle and all cycles through Phase 2 significantly differ across all three treatments. In Baseline, cooperation rates do not change much across all cycles in Phase 2. For the last cycle in Phase 2, both first round and all rounds cooperation are highest in Cheap-talk and lower in other two treatments. Therefore, I use the Difference-in-Difference method to estimate the true effect of communication institutions on cooperation rates. To put the experiment environment in the DID settings, consider period 0, the control period as Phase 1 through 2 and period 1, the treatment period as Phase 3. I are interested in whether communication institution has an effect on the cooperation, which can be estimated as the difference in changes in the mean cooperation in the treatment period, relative to the control period. Following this idea, I estimate the effects of communication institutions on cooperation by running the DID estimation for three pairs: Baseline v.s. Cheap-talk, Baseline v.s. Contract and Cheap-talk v.s. Contract. In the first two paired comparisons, Baseline is the control and in the third one Cheap-talk is the control group. The DID method estimates the "normal" difference in the mean cooperation between the two treatments: the difference that would still exist if neither treatment experienced the treatment, represented by the dotted line. The treatment effect is then the difference between the observed mean cooperation in Phase 3 and the hypothetical mean cooperation in Phase 3 if the treated group doesn't receive the treatment. Table 2.4 reports the estimation results for first round cooperation.

From Table 2.4, the positive effect of each of the two communication institutions on cooperation is significant: the positive changes in cooperation in Cheap-talk or Contract relative to the control period are significantly greater than those in Baseline. The changes in mean cooperation in Baseline are negative while both those in Cheap-talk and Contract are positive. Remarkably, the "difference-in-difference" of Baseline and Contract is 26%, indicating a substantially greater increase in cooperation rates in Contract. Comparing the two communication institutions, more boost in cooperation is induced in Contract: the "difference-in-difference" is 14.5%. Hence, I conclude that both communication institutions significantly improve cooperation and Contract generates highest cooperation rates.

Table 2.4: Difference-in-Difference Estimation: First Round Cooperation

Compared Groups	Phase 1 & 2			Phase 3			
	Control	Treated	Diff.	Control	Treated	Diff.	Diff-in-Diff
Baseline v.s.	0.597	0.694	0.097***	0.507	0.720	0.212***	0.115***
Cheap-talk	0.004	0.003	0.004	0.003	0.004	0.006	0.003
Baseline v.s.	0.597	0.662	0.066***	0.507	0.833	0.326***	0.260***
Contract	(0.003)	(0.005)	(0.007)	(0.003)	(0.003)	(0.004)	(0.007)
Cheap-talk v.s.	0.694	0.662	-0.032***	0.720	0.833	0.114***	0.145***
Contract	0.003	0.005	0.005	0.003	0.002	0.004	0.006

Note: Coefficients and standard errors are bootstrapped. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$

2.5.2 Payoff Efficiency

Next I examine the efficiency of relationships in different treatments. First I quantify payoff efficiency and then estimate the effect of communication institutions on efficiency. To define payoff efficiency of a round, I use the relative difference between the expected joint payoffs implied by the choices of a pair and those from the one-shot Nash choices. A cycle's efficiency is therefore defined as: (Expected Joint Payoff of a cycle - One-shot Nash Equilibrium)/(Expected Joint Payoff of Full Cooperation-One-shot Nash Equilibrium). Next I first examine the evolution of payoff efficiency of a cycle, then estimate the effect of communication institutions on the efficiency of a cycle using the DID method. Figure 2.3 illustrates the evolution of payoff efficiency for all treatments.

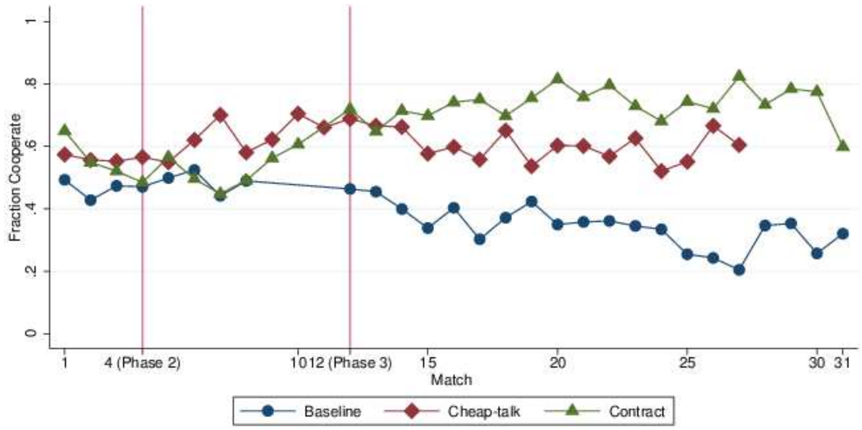


Figure 2.3: Evolution of Payoff Efficiency for All Treatments

Payoff efficiency has clear separation between three treatments in Phase 3: efficiency in Contract is higher than Cheap-talk and that in both is higher than Baseline. Similarly, I observe substantial differences in the efficiency when Phase 3 starts across three treatments, especially between either one of the communication treatment and Baseline. There I use DID method to estimate the effect of communication institution on efficiency. Results are reported in Table 2.5.

Table 2.5 shows qualitatively, the conclusion on the comparison of cooperation rates for all treatments applied to payoff efficiency. Remarkably, the DID effect of Contract relative to Baseline on the payoff efficiency is about 30% while that of Contract relative to Cheap-talk is about 14%. This further suggests that both communication institutions not only improves cooperation but efficiency, among which Contract generates higher efficiency.

Table 2.5: Difference-in-Difference Estimation: Payoff Efficiency

Compared Groups	Phase 1 & 2			Phase 3			
	Control	Treated	Diff.	Control	Treated	Diff.	Diff-in-Diff
Baseline v.s.	46.6	58.0	11.4***	35.1	61.4	26.4***	**15.0*** **
Cheap-talk	(0.007)	(0.007)	(0.010)	(0.004)	(0.007)	(0.008)	(0.012)
Baseline v.s.	46.6	54.5	7.9***	35.1	71.8	36.7***	**28.8*** **
Contract	(0.007)	(0.008)	(0.011)	(0.005)	(0.006)	(0.008)	(0.012)
Cheap-talk v.s.	58.0	54.5	3.5***	61.4	71.8	10.3***	**13.8*** **
Contract	(0.008)	(0.008)	(0.010)	(0.007)	(0.006)	(0.010)	(0.014)

Note: Coefficients and standard errors are bootstrapped.***p<0.01, **p<0.05,*p<0.1

2.5.3 Strategy Prevalence

The "reduced-form" finding about cooperation rates and efficiency seems to be consistent with initial conjectures. However, cooperation rates and payoff efficiency still do not inform us on the channels communication institutions generate high efficiency. It is not clear still how the mechanics of each communication institution work to achieve higher efficiency. Moreover, we still need to understand why Contract performs better than Cheap-talk: how the one-sided binding power results in higher cooperation rates and efficiency. This section answers these questions by closely examining the 1) the strategies specified by each subject and the associated deviations in Phase 2; 2) the strategies specified or sent in Phase 3 and their associated receiver responses.

2.5.3.1 Phase 2 Strategies Table C1 reports the strategies specified at the start of every cycle in Phase 2. The overall frequency indicates that the most prevalent specified strategies are AC and AD. Other subgame perfect strategies are also specified with frequencies close to 5%: WSLS (5.5), GRIM (4.9) and T11 (5.5). MONO, a strategy similar to WSLS is specified with 5.5%. This result somewhat suggests that subjects are able to specify a strategy that is or close to the set of subgame perfect strategies. Obviously, I are more interested in their deviations from specified strategies. The final column reveals that for the above six main strategies, the deviation rates are not higher than 5%: deviations associated with AD and AC are almost 0. This implies that the subgame strategies specified are adhered in general by subjects. Beyond reporting the deviation rates, which do not inform us on the consequences of deviation, I further report the difference between actual choices and predicted choices by subjects' specified strategies. The latter are generated using the specified strategies, the first round choices of subjects and realized outcomes in a cycle. I look for the consequences of deviation: whether subjects behave more or less cooperative than what their strategies imply. Figure 2.4 reveals that the cooperation rates change very little as a result of deviation. Therefore I conclude that subjects are able to specify and follow subgame perfect strategies. However, they favor AC most.

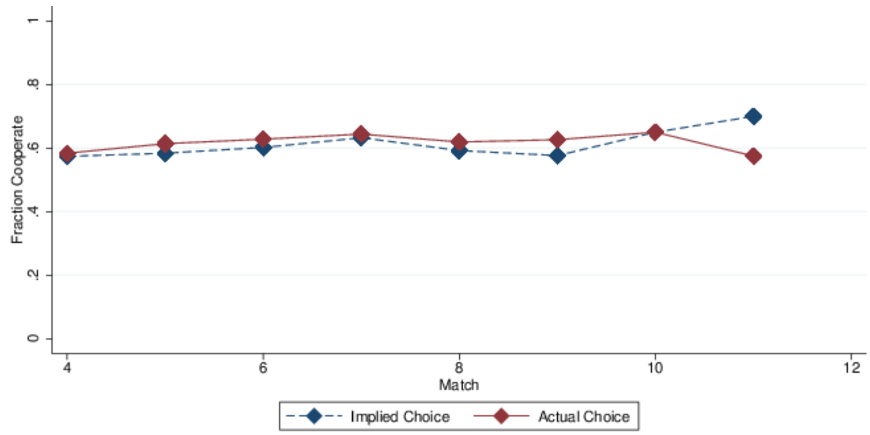


Figure 2.4: Actual and Implied All Rounds Cooperation Rates in Phase 2: All Sessions

2.5.3.2 Phase 3 Strategies In Phase 3, senders send strategies they would like to play without commitment at the beginning of each cycle in Cheap-talk and with full commitment in Contract. I examine these strategies as follows. First, I calculate the frequencies of strategies specified by all subjects and those by senders. Second, I analyze the interactions between senders and receivers. For each of the two communication treatments, I examine the evolution of cooperation of senders and receivers separately. Further, I restrict our attention to the four main strategies sent in the two treatments and examine the evolution of cooperation.

Table 2.6: Frequencies of Elicited and Sent Strategies (All cycles in Phase 3)

Strategy	Cheap-talk				Contract		
AKA	Overall	Sent	Rec. Coop	Dev.	Overall	Sent	Rec.Coop
AD	14.0	**12.5**	37.7	8.6	7.3	**5.7**	15.0
GRIM	10.4	**9.2**	60.9	0.1	10.9	**11.1**	67.6
T11	13.8	**10.8**	64.6	19.3	5.5	**7.9**	75.2
AC	39.0	**47.5**	69.4	8.5	61.1	**65.7**	84.9

2.5.3.3 Overall and Sending Frequencies of Strategies Table 2.6 reports the frequencies of strategies specified by each subject and those actually sent in Phase 3 for both communication treatments. Remarkably, the four most frequently sent strategies coincide in the two treatments: AD, GRIM, T11 and AC. Among them, AC is the most frequently sent message/contract: the frequency is 47.5% in Cheap-talk and 84.9% in Contract.

2.5.3.4 Cooperation Dynamics by Sender and Receiver Next I examine the interactions between senders and receivers by analyzing the evolution of sender and receiver's cooperation. Figure 2.5 and 2.6 report the dynamics for first round cooperation. For Cheap-talk, in addition, I look at the evolution of cooperation of implied senders' choices from senders' strategies. The sequence of implied choices for a cycle is derived as a function of the sequence of realized outcomes in the cycle and the strategy sent by the sender. In Figure D1, I report the evolutions of all round cooperation.

For first round cooperation, I observe that in Cheap-talk, senders' actual choices are less cooperative than the implied choices, but the difference is small: on average about 5%. However, receivers' choices are much less cooperative than senders' actual choices: on average receiver cooperation rates are about 10% less than senders' actual cooperation rates. On the other hand in Contract, receivers' cooperation rates are also in general less than senders' but close to it: on average the difference is 5%. Further, actual cooperation in Cheap-talk is less than that in Contract.

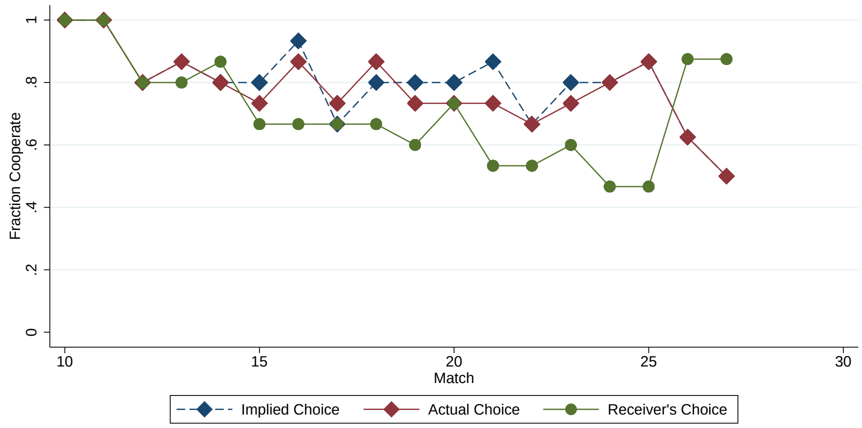


Figure 2.5: Aggregate Evolution of Sender and Receiver's Cooperation in Cheap-talk: First Round

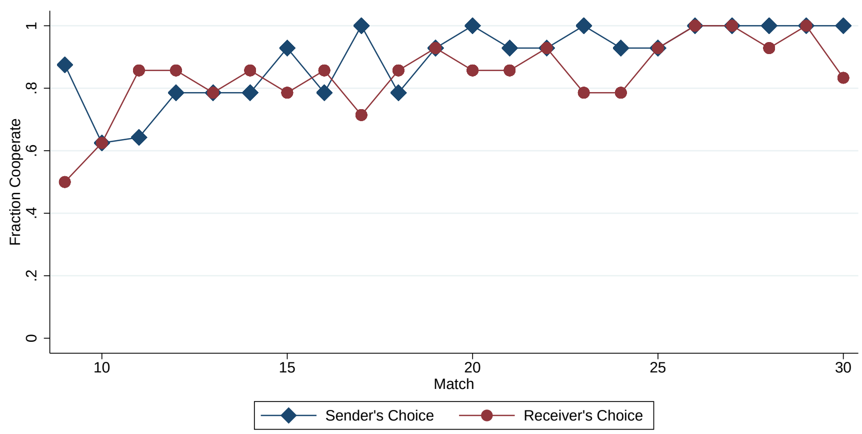


Figure 2.6: Aggregate Evolution of Sender and Receiver's Cooperation in Contract: First Round

Because four main strategies account for about 90% in total among all the strategies sent in both treatments, I now restrict our attention to the evolution of cooperation for these four strategies in both treatments. Figure 2.7 and 2.8 illustrates the results for first round cooperation.

AD (Sending Frequency=12.5% in Cheap-talk; =5.7% in Contract). In Cheap-talk, senders' deviations from the message are small: sender's actual cooperation rate is 8.6% while the implied metric is 0%. Receivers behave more cooperative than senders in Cheap-talk (36.6%) and Contract (13.3%). Receivers are more cooperative in Cheap-talk than their counterparts in Contract, partly because senders' actual cooperation rate is higher.

GRIM (Sending Frequency=9.2% in Cheap-talk; =11.1% in Contract). Senders' deviations from this strategy are little in Cheap-talk: the actual cooperation rate is 68.8% while the implied metric is 68%. Receivers are in general as cooperative as senders in both Cheap-talk (64%) and Contract (63.2%).

T11 (Sending Frequency=10.8% in Cheap-talk;=7.9% in Contract). Senders are less cooperative than what their messages implied in Cheap-talk: the actual cooperation rate is 88.3% while the implied metric is 92.4%. Receivers are less cooperative than senders in both treatments and receivers are less cooperative in Cheap-talk (64.1%) than their counterparts in Contract (72%).

AC (Sending Frequency=47.5% in Cheap-talk;=65.7% in Contract). Senders are less cooperative than what their messages implied in Cheap-talk: the actual cooperation rate is 91.5% while the implied metric is 100%. Meanwhile, receivers are less cooperative than senders in both treatments. However in Contract (69.1%), receivers are less cooperative than their counterparts in Cheap-talk (84.1%).

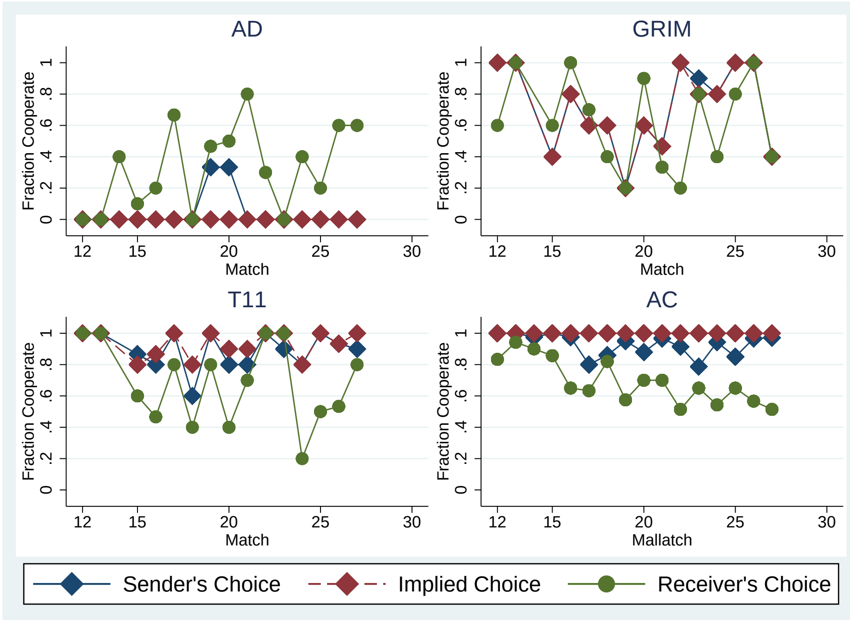


Figure 2.7: Evolution of Sender and Receiver's Cooperation in Cheap-talk by Four Main Strategies

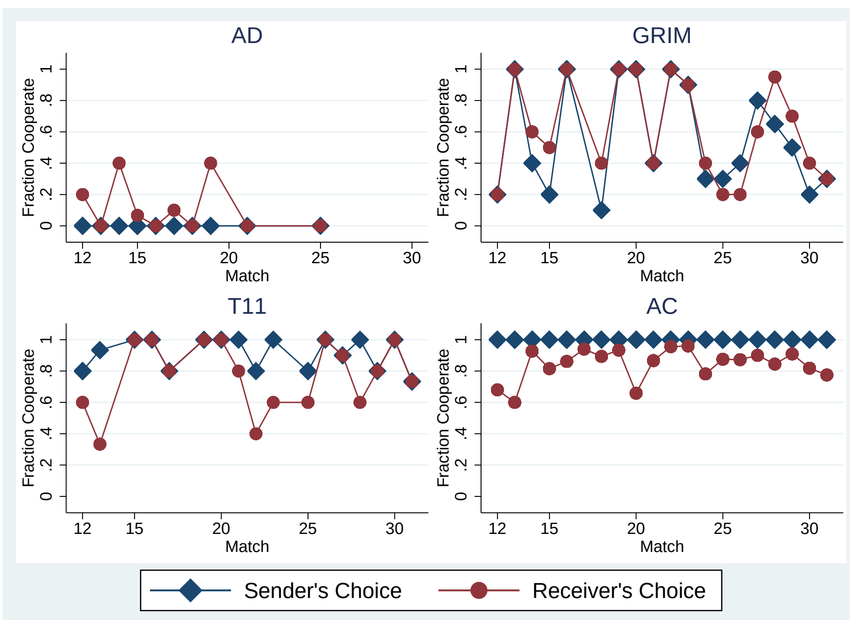


Figure 2.8: Evolution of Sender and Receiver's Cooperation in Contract by Four Main Strategies

2.5.3.5 Receiver Strategies

Next I explore receivers' behavior against the four sender strategies in Cheap-talk and Contract. For each sender strategy, we estimate the strategy distribution from receivers' actual choices and outcomes. We employ the strategy frequency estimation methods outlined in Dal Bó and Fréchette (2011). The details of the methodology is in Appendix E. After specifying a set of 18 strategies motivated by theory and previous experimental literature[6], I estimate the proportions of each strategy, using data from all receiver responses. Table 2.7 reports the results.

In Table 2.7, I first focus on the most popular sender strategy in both Cheap-talk and Contract: AC. About 30% of the receivers play AC in Cheap-talk and about 45% of the receivers do so in Contract. In Cheap talk, on average about 12% of the receivers play AD, T2 and MONO21 and about 6% of the receivers play FC, GRIM and WSLS. In contrast, receivers' responses are more cooperative and lenient in Contract: about 26% of the receivers play MONO22; about 6% of the receivers play AD and T2. In summary, receivers are more lenient and cooperative when AC is the contract than when AC is a non-binding message.

Now I focus our attention on other sender strategies: AD, GRIM and T11. When AD is sent in Cheap-talk, only 27% of the receivers' responses are AD and the rest are more cooperative responses: WSLS (12.3%), DGRIM2 (13.6%), MONO21 (11.3%), MONO12(22.9%) and MONO22 (11.0%). In contrast, 66% of the responses are AD and about 26% of the responses are GRIM in Contract. However, note that the sending frequency of AD is half in Contract of that in Cheap-talk. When GRIM is sent, most of the responses are cooperative and lenient strategies in both treatments: MONO21 and MONO12 both account for about 40% each in Cheap-talk; AC accounts for about 40% in Contract. When T11 is sent, in Cheap talk, most of the subjects respond by playing T11. This shows that even the salience of the efficient equilibrium is weakened in the partnership game, it is selected by a substantial proportion of subjects (10%).

[6] I use a similar set of strategies mainly from Fudenberg et al. (2010); Embrey et al. (2011).

Table 2.7: Strategy Estimation of Receiver Responses Based on Main Sender Strategies from Cheap-talk and Contract

Receiver /	Cheap-talk				Contract				Baseline
Sender Strategy	AD	GRIM	T11	AC	AD	GRIM	T11	AC	
AC	0.0	0.0	18.1	**30.4**	0.0	42.7	31.3	**44.8**	21.5
AD	27.4	0.0	7.0	**11.7**	65.7	12.1	6.2	**7.8**	50.8
FC	0.0	0.0	0.0	**5.2**	0.0	0.0	0.2	4.2	2.8
GRIM	0.0	0.0	0.0	**5.9**	25.5	4.6	2.4	0.0	
WSLS	12.3	12.1	0.0	**7.5**	0.0	1.9	0.0	4.1	
MONOD	0.0	5.4	0.0	2.6	0.0	2.2	0.0	0.0	6.9
DWSLS	0.0	0.0	26.7	0.0	0.0	3.3	1.9	3.8	9.4
T2	0.0	0.0	0.0	**11.2**	0.0	4.4	2.6	**6.3**	
T11	0.0	0.0	48.0	0.0	0.6	4.6	50.0	0.9	
DGRIM2	13.6	0.0	0.3	0.0	0.5	5.9	0.2	0.0	
MONO21	11.3	40.7	0.0	**13.9**	0.0	1.5	0.0	0.0	3.0
MONO12	22.9	41.8	0.0	0.1	0.5	4.6	0.0	0.0	
MONO22	11.0	0.0	0.0	0.0	3.2	0.0	0.2	**26.4**	5.8
Gamma	0.47	0.69	0.73	0.57	0.38	0.74	0.33	0.44	0.52
Beta	0.89	0.81	0.80	0.85	0.93	0.79	0.95	0.91	0.87

The above analysis of senders and receivers' strategies illustrate some departures from our conjectures. First, most senders did not send any of the equilibrium strategies. In Cheap-talk, only about 10% senders sent and the most efficient symmetric equilibrium strategy T11. Instead, they send the most cooperative but non-dynamic strategy AC and deviate little from it. In Contract, senders did not send the sequential equilibrium strategy

DMONO but instead again send AC. Second, receivers did not best respond to senders' messages. In Cheap-talk, though receivers discount the message and are much less cooperative than senders, they still employ cooperative and lenient strategies such as AC and MONO21. In Contract, the best response given the AC contract is AD. However, most of the responses are AC. Finally, if I compare the distributions of senders' and receivers' strategies to the distribution in Baseline, the proportion of cooperative and lenient strategies is higher in both Cheap-talk and Contract. This shows that both senders and receivers are more cooperative when two communication institutions are in place.

The message and response patterns in communication institutions indicate that both senders and receivers are employing very cooperative yet non-dynamic strategies. For senders in Cheap-talk, sending AC signals highest level of trust: any dynamic message that involves the Defect state shows lower trust. For the receiver, if they were to respond by playing AD, then the initial trust signaled by the sender would collapse and both players might be stuck in mutual-defection, which gives each lowest payoffs. This applies to subsequent responses of senders as well, which could explain the low deviation rate (8.5%) associated with AC. Therefore in this dynamic environment, trust is maintained: senders and receivers coordinate on AC.

In Contract, even when senders cannot change their actions, both senders and receivers coordinate more on AC. To interpret the result, one can think of the repeated partnership with preplay contract as a variant of the trust game. Here the sender's endowment is the expected payoff of offering an AD contract-the joint defection payoff. Similar to findings from trust game experiments [7], the sender "sends" part of her endowment to the receiver: such offer can be thought of as the difference between the sucker's payoff and the joint defection payoff. By offering such a trusting contract, the sender expects some reciprocity from the receiver. And the receiver's behavior is consistent with the findings from trust game experiments as well: the receiver is trustworthy by coordinating with the sender on

[7] see Camerer (2003) for a review of the literature

AC, i.e. returning a favor rather than exploiting the trust. Of course, such trustworthiness could stem from costs of non-reciprocity: triggering the distrust among players and therefore getting a less trusting contract later, etc.

2.6 CONCLUSIONS

I experimentally investigate two forms of communication in partnership games: Cheap-talk and contract. Such communication is unilateral and introduced prior to the start of each repeated game. By varying whether the message is binding or not, the experiment tests the effect of communication institutions on efficiency.

I find that both communication institutions (Cheap-talk and Contract) increases cooperation rates and efficiency relative to communication-free Baseline. Moreover, Contract generates higher cooperation rates and efficiency than Cheap-talk and Baseline, after controlling for session-level variances in experience level using difference-in-difference methods. I find that in Cheap-talk, senders' actual cooperation rates are lower than implied cooperation of their messages and receivers' cooperation rates are much lower than senders' actual cooperation rates. The same result applied to Contract, but the magnitude of difference between receivers and senders' cooperation rates is smaller. Further after examining the strategies sent and employed by senders and receivers, I find that AC, the most efficient strategy is most frequently sent by senders in both communication institutions. Senders' deviations from AC are small in Cheap-talk. In both communication institutions, senders also send cooperative and lenient strategies.

Preliminary findings highlight the importance of trust in communication to improve coordination and efficiency in repeated partnerships. In Cheap-talk, the sender sends AC mainly to signal one's intention to cooperate rather than intended strategy; receivers prove their trustworthiness by coordinating on AC. Trust is maintained by both parties to avoid mutual defection. In Contract, behavior is similar to that in trust game experiments: senders offer AC as a trusting contract and receivers are trustworthy in coordinating on AC.

3.0 DETERMINANTS AND SHAREHOLDER WEALTH EFFECTS OF THE SALES METHOD IN M&A

3.1 INTRODUCTION

A vast literature investigates the wealth effects accruing to acquirer and target shareholders mergers around the post-public announcement period of the acquisition process (see e.g., Andrade et al. (2001)). Less is known about the sales method choice which takes place during the private takeover process before the public announcement of the deal[1]. The theoretical literature has two competing schools of thought regarding the sales method and its effect on takeover premiums. Bulow and Klemperer (2009) predict that auctions increase competition and, compared to negotiations, should result in higher takeover premiums. In contrast, French and McCormick (1984) and Hansen (2001) hypothesize that when information costs associated with auctions are considered takeover premiums in auctions should be no different than those offered in negotiated deals.

Recent empirical studies, most notably Boone and Mulherin (2007, 2008) (hereafter BM), analyze the private takeover process in detail and investigate whether takeover premiums differ between the two sales methods. They find that competition during the pre-announcement takeover process is much higher than what had been previously documented with auctions and negotiated deals each accounting for roughly half of their sample. They

[1] For an illustration of timeline of a typical M&A process, see Boone and Mulherin (2009).

find no difference in takeover premiums between these two sales methods. This result is consistent with their competition tradeoff hypothesis, which predicts that while auctions increase target premiums by introducing greater competition, greater information costs inherent in the more intensive due diligence process offset the increase.

This paper revisits the question of whether there is an association between the sales method and shareholder wealth effects in takeovers. We contribute to the literature based on four innovations. First, we argue that the tradeoff between information costs and competition benefits is likely affected by the relative size of the target in the acquisition. Specifically, we hypothesize that size has a greater impact on information costs than it has on competition premiums. For example, relatively larger targets may have more proprietary information than smaller ones, but might have a similar competition premium. This suggests that for relatively small targets that use auctions, the positive effect of competition on takeover premiums may outweigh the negative effect information costs have on takeover premiums. In contrast, for large targets the competition premium decreases due to a smaller set of potential bidders, but larger information costs could offset this. In short, we predict that relative size will affect the targets' tradeoff between competition premiums and information costs and label this the extended competition tradeoff hypothesis. We note that BM's sample covers the 1990s and includes relatively large targets. The mean (median) target size is \$2.69 (\$0.69) billion and the mean (median) relative size is 56% (27%)[2]. To put this in perspective, \$2.69 billion represents roughly the 93rd percentile in terms of size of all NYSE, NASDAQ, and Amex firms during that period and approximately the 75th size percentile for takeover targets.

Second, to the best of our knowledge, we are the first paper to investigate the relation between the sales method and wealth effects extending beyond the target premium, such as bidder, dollar-denominated, and synergy returns, and the distribution of gains between

[2] BM acknowledge that target sizes in their sample are larger than usual: "... a relatively large value which reflects the fact that the sample comes from firms listed on the Value Line Investment Survey", (BM, 2007b, p. 13).

the bidder and the target. We propose that bidders, as targets do, face a tradeoff because the information gain from the due diligence process to each bidder should be unaffected by increased competition. However, while increased competition is a cost to the bidder if it increases the premium it must offer to win, bidders may benefit from competition by acquiring more information about the target's valuation, specifically about other bidders' private signals of the target's valuation. This information is sought to understand the common-value component of that valuation[3]. The value discovery process improves by aggregating bidders' private information and leads to more precise valuation of the target, and ultimately more efficient acquisition. We expect that the bidder's tradeoff between the information aggregation benefit and the competition cost will also be affected by the relative size of the target. For relatively small targets, the potential benefit of information aggregation for bidders is greater because the set of private signals from other bidders is larger. We label this the bidder competition tradeoff hypothesis.

Third, we investigate the determinants of the sales method. The question of what determines the sales method remains largely unanswered in the literature as it has focused on estimating the relationship between sales method and target firm uncertainty[4]. Little is known about how characteristics, such as financial constraints and industry liquidity, affect the sales method. Therefore, in addition to previously documented determinants of the sales method choice we consider a number of previously ignored determinants such as the target's operating performance, financial constraints, and industry characteristics[5]. Understanding the determinants of the sales method is necessary for designing our empir-

[3] Gorbenko and Malenko (2014)models financial bidders' M&A auction as a common-value auction and strategic bidders' as private-value auctions. However, we find in Section 3 that auctions are associated with problem-fixing types of synergy which requires a less unique set of bidders. In this sense, strategic bidders' auctions are private-value auctions with common-value components

[4] An exception, for example, is Anilowski et al. (n.d.) who study how managers' incentives to manage earnings relate to the sales methods. Section 2 will provide a more detailed review of the relevant literature on the determinants of the sales method.

[5] For example, we focus on more direct measures of both the level of operating performance and also its dynamics. Previous literature has looked mostly at indirect measures. Marquardt and Zur (2014) finds that financial accounting quality, measured by accruals quality, is related to sales method. Anilowski Cain et al. (2009) finds that auctioned targets have greater future cash flow uncertainty.

ical tests exploring the wealth effects associated with the sales method. Furthermore, we examine how deal initiation is related to the sales method and hypothesize that the sales method serves as a device to maximize the probability of finding a bidder that matches the financial needs of the target.

Fourth, we offer a number of methodological improvements. For example, we redefine the sales method to better capture both the actual competition that results from the sales method and the potential competition implied by the method. Potential competition has been previously ignored, but is important for target shareholders' wealth, as shown in Aktas et al. (2010). Also, for our premium analysis we pay special attention to proper identification of our empirical model and conduct a series of two-stage IV regression analyses of target returns and sales method that control for endogeneity (between the two). We validate our models using out-of-sample testing.

Based on a sample of 575 M&A deals announced between 1998 and 2012, we find the following results. Target firms are more likely to use auctions when they are more leveraged, financially more constrained, have smaller information costs and higher debt burdens, higher operating expenses, and lower industry shocks. They also experience stronger sales growth during the year prior to the deal announcement and are associated with lower market share growth. We interpret these results to suggest that targets that use auctions are able to induce competition from bidders because they experience attractive economic performance, but given their financial constraints, may be searching for a bidder who is able to finance their internal growth opportunities (see e.g., Masulis and Simsir (2013); Fidrmuc and Xia, 2014). In this case, an auction may be preferred because the set of potential bidders is less limited by concerns of finding a specific buyer that satisfies the target's specific operational needs. Similarly, targets are more likely to initiate a deal when they are more levered, financially constrained, and operate in industries with fewer industry shocks. This suggests alignment of incentives between deal initiation and the sales method.

Our two-stage regression analyses show that auctions are associated with significantly higher target returns. The impact of the sales method is economically relevant as a ten percent increase in the predicted probability of doing an auction leads to an increase in target returns of approximately 3.5 percentage points, which represents more than 13% of the average target return in the sample. The positive relation between auctions and target returns continues to hold when we control for fixed effects, add different industry characteristics, and estimate richer two-stage model specifications. When we split the sample on the median (terciles) of relative size we find that the positive relation between auctions and target returns continues to hold only for below median (lowest tercile) of targets in the sample. This is consistent with the extended competition tradeoff hypothesis and suggests that competition tradeoffs differ between small and large targets.

In terms of bidder wealth effects, we find weak evidence consistent with the bidder competition tradeoff hypothesis. For example, auctions increase bidder returns for bidders associated with relatively small targets but there is no overall effect. According to the bidder competition hypothesis, bidders are faced with tradeoffs between the information aggregation benefit and the competition cost. These tradeoffs are also affected by the relative size. Given that it is the smaller targets for which we find the positive association between auctions and percentage returns, not surprisingly, we find that target dollar denominated wealth gains are lower in auctions. Similarly, we find that auctions are associated with higher bidder dollar returns, which is driven by relatively larger bidders among small bidders in auctions. This suggests that bidders with lower competition cost are more likely to enter into the auctions.

The rest of the paper is organized as follows. We discuss the literature and our contribution in Section 2. Section 3 introduces the sample. Section 4 reports the results from the logistic analysis of the sales method. Section 5 reports 2SLS regression analyses of wealth effects and the sales method. Section 6 reports a variety of robustness checks and Section 7 concludes.

3.2 PREVIOUS LITERATURE

Our paper complements and contributes to a large literature analyzing the wealth effects associated with mergers and acquisitions. Specifically, we aim to contribute to the literature that investigates the determinants and wealth effects of the sales method. The theoretical literature has two competing points of view with respect to the wealth effects, especially insofar as target premiums are concerned, of the sales method choice.

Bulow and Klemperer (2009)show that auctions are preferred over negotiations and the value of negotiating skills is small relative to additional competition. They also suggest it might be wealth enhancing to the target shareholders to restrict the number of bidders if an auction would diminish value by revealing private information. In general, they hypothesize that auctions have positive shareholder wealth effects. However, it is unclear from their model how information costs, as the tradeoff for competition, match the benefits of competition.

In contrast, French and McCormick (1984) argue that the seller bears the cost of information gathering in the sales process. One such cost from selling the firm is the revelation of proprietary information to rivals (Hansen (2001)). Taking into account these information costs, target returns could be the same for negotiations and auctions.

Incorporating information costs therefore sets up the competition tradeoff hypothesis proposed in BM (2007a, 2007b). They examine a sample of 400 M&A deals between 1989 and 1999 and conclude that auctions and negotiations generate similar target wealth effects from both multivariate and two-stage regression models. An important characteristic of BM's sample is that it contains, on average, relatively large targets. Our paper proposes the extended competition tradeoff hypothesis, which predicts that size has a greater impact on information costs than it has on competition premiums: For relatively small targets that use auctions, the positive effect of competition on takeover premiums may outweigh the negative effect information costs have on takeover premiums. For large targets, in contrast, the competition premium decreases because of the smaller set of potential bidders and these two effects may cancel each other out.

Our paper relates to the literature that studies the wealth effects and the determinants of the sales method. Anilowski Cain et al. (2009) studies the impact of incentives of target managers to manage earnings on the method of sale. When target management engages in "window-dressing" their financial statements, selling through an auction, instead of through a one-one-one negotiation, can limit the level of timing of the information that bidders can scrutinize. This reduces the risk of earnings management detection. In addition, controlling for potential earnings management, both abnormal returns and premiums increase for targets when they sell through an auction. Their findings suggest that both the market and the bidder expect lower value acquisitions when the uncertainty about reported earnings is higher.

Anilowski Cain et al. (2009) control for self-selection bias stemming from management's private information inherent in the sales mechanism and finds that auctions are associated with higher target cumulative abnormal returns and offer premiums. With a sample of target initiated deals, they focus on the impact of various proxies of adverse selection risk and the uncertainty of future cash flows on the sale and find that targets with more aggressive (or opaque) financial reporting are more likely to auction the firm and this strategy proves beneficial only when the uncertainty of future cash flows is high. When financial reporting is more transparent or the uncertainty of future cash flows is low, targets benefit more from single bidder negotiations. While this paper regards sales methods and target returns as a problem of self-selection, we follow BM and take the view that it is a problem of endogeneity.

Marquardt and Zur (2014) examines the role of target firms' accounting quality (AQ) on the course of the acquisition process. They find that target AQ is positively associated with the likelihood that the deal will be initiated as a negotiation rather than as an auction, the speed in which the deal is completed, and the likelihood of deal completion. Moreover, they find that AQ has a more pronounced effect on transactions when the target firm is sold via auction versus negotiation. Again, the results are consistent with the idea that corporations with high AQ are easy to value given their low uncertainty about future cash flows, while corporations with poor AQ are harder to value because of the increased uncertainty.

Rogo (n.d.) find that the costs of disclosing proprietary information in auctions are offset against the benefits of competition among bidders. Targets with valuable proprietary information choose negotiations to mitigate valuation uncertainty at lower costs and maximize expected revenues. Specifically, the level of valuation uncertainty is positively associated with the likelihood of negotiations. We incorporate the idea of information cost in our estimation of the likelihood of conducting an auction.

Shleifer and Vishny (2003)point to industry factors that might affect the sales method. They find that government regulation can constrain some potential bidders in buying a firm. Hence, selling firms in regulated industries may be less likely to conduct auctions. Based on these findings, we incorporate the role of industry effects into our analysis.

Our paper also contributes to the literature on deal initiation. Masulis and Simsir (2013) show that target deal initiation is associated with lower announcement abnormal returns and link this finding to information asymmetries concerning the quality of target firms. Xie (2010) argues that deal initiation reveals both selling firm bargaining power, but also bidder valuations and thus buyer initiated deals result in higher premiums. Xie (2010) also shows that target initiated deals are more often organized as auctions whereas bidder initiated deals are most likely privately negotiated. Fidrmuc et al. (2012) finds that target initiation and high profitability are important determinants of whether firms are sold in auctions or private negotiations. De Bodt et al. (2014) find that a higher willingness to sell through target initiation is associated with lower premiums, but also increases deal success probability. Finally, Fidrmuc and Xia (2014) find that targets that initiate deals are financially constrained.

While these papers address similar issues relating to the sales method, they do not take the relative size of the deal explicitly into account when analyzing the tradeoffs between competition and information costs relevant to the target shareholders. Specifically, we consider the bidder's tradeoff between competition costs and benefits from information aggregation and analyze the relation between the sales method and, various shareholders wealth measures.

3.3 SAMPLE CONSTRUCTION AND SUMMARY STATISTICS

Our main sample comprises 575 takeovers of U.S. listed firms announced between 1998 and 2012. We construct the sample as follows. First, we extract all M&A deals during 1998-2012 from SDC that satisfy the following criteria:

1. Both targets and acquirers are public U.S. companies;
2. Deal value is be greater than $10 million;
3. Deals are completed[6] ;
4. Acquisitions are of majority interest: the percent of shares owned after the acquisition is greater than or equal to 100%;
5. Percent of shares held by acquirer 6 months prior to announcement is less than 50%

These selection criteria result in 3,050 transactions from SDC. We merge this sample with COMPUSTAT and EVENTUS to obtain target and acquirer financial characteristics. There 1,367 deals remaining that satisfy our data availability requirements[7]. Because

[6] We focus on completed deals because the sales method information is available in SEC filings. In most withdrawn deals, both parties do not need to submit SEC filings.

[7] As we require a rich set of variables to be available for the target and bidder of each deal, there are three variables that cause big drops in the sample size: acquirer size and two financial constraint indices

information regarding each deal's initiation and sales method is not reliably available in electronic format, we must rely on careful hand-collection. We randomly choose 575 deals to collect information regarding deal initiation and sales method from the "Background of the Merger" section of the merger document for each deal filed with the U.S. Securities and Exchange Commission (SEC)[8]. The 575 deals form our main sample in the subsequent analyses. As a control group, we also have the sample of the remaining 792 deals without any hand-collected information regarding the private takeover process.

For each takeover in our main sample, we review the filings from the EDGAR system of the SEC. We collect details of the sales process for each takeover from the background section of 14A and S-4 filings (mergers) and 14D filings (tender offers).

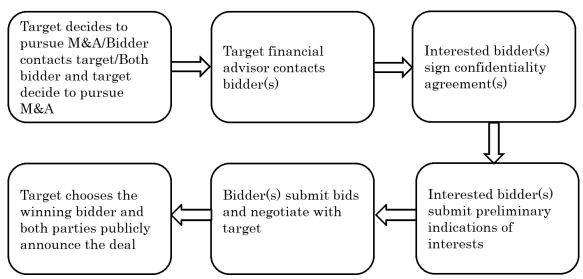


Figure 3.1: Timeline of an M&A Process

Figure 3.1 illustrates the timeline of a typical private takeover process as described in the SEC filings. Deals can start in many different ways: a target can put itself for sale; a bidder can proactively approach the target about the possibility of a business combination; a target can be approached by a third-party and then later contact the (final) bidder to gauge the bidders' interest, or be contacted by the (final) bidder. Both the target and the bidder engage financial advisors after a deal is initiated. The target usually asks the

(WW and KZ Index).

[8]For each year, we randomly choose a fixed proportion of deals from the sample of 1,367 deals.

financial advisor to contact other bidders to join in the bidding or just proceed with the bidder for one-to-one negotiation. After bidder(s) are contacted, each interested bidder is requested to sign a confidentiality agreement with the target. When negotiations continue, remaining bidders are asked by the target to submit a preliminary indication of interest as an initial offer and then to revise its bid until the target accepts the bid or the bidder quits from the negotiation. To characterize this process, we collect the following variables of the private takeover process: deal initiation party (bidder/target/both), deal initiation date, date target and bidder CEO first meet; number of bidders contacted, number of bidders that signed the confidentiality agreement, and number of bidders that submitted preliminary indications of interest.

We then classify the deal initiation party and the sales method with the variables collected for each deal. Targets and bidders might terminate their merger discussions at some point for various reasons and later reinitiate them. We focus on the takeover process that ultimately leads to the deal announcement without interruptions, which is the most recent takeover process to the deal announcement date [9]. There are several important differences in our classifications relative to those made in previous studies. First, we classify the sales method of a deal as an auction when the number of bidders contacted is greater than one and negotiation when there is only one bidder contacted. This is different from BM's definition based on the number of bidders that signed the confidentiality agreement to delineate between auctions and negotiations. Our approach is motivated by the idea that the decision of the sales method by the target takes place before bidders begin to respond. Also, our classification reflects the potential competition introduced by the target's sales method to the bidder(s), as noted in Aktas et al. (2010). We note that the number of bidders contacted includes those contacted prior to and during the formal bidding process. For example, if there is only one bidder contacted before the formal bidding process and

[9] The existing literature does not mention whether the number of bidders contacted or signed the confidentiality agreement is calculated including all takeover processes (including potential termination of discussions and reinitiation of discussions) in the background of SEC filings.

after the bidder submits the initial bid other bidders choose to come in and make a play for the firm, we classify the sales method as auction. We classify a deal as "target-initiated" when the target contacts the bidder and as "bidder-initiated" when the bidder approaches the target. If there is no clear indication about such move by the target or bidder in the merger document, we classify the deal as a "both initiated" deal. For a deal where a third party approaches the target first, we classify it as target-initiated (bidder-initiated) when the target (bidder) later contacts the bidder to gauge their interest [10].

Table 3.1: Summary of the Sales Process

Sample	Observations	Initiation			Contact	Confidential	Interest	Length
		Target	Bidder	Both				
Panel A. The Full Sample								
Full Sample	575	211	302	62	6.13	2.96	1.72	197
Panel B. The Sample Categorized by Sales Method								
Auction	313	157	135	21	10.43	4.6	2.32	235
Negotiation	262	54	167	41	1	1	1	152

Table 3.1 summarizes the sales process in our sample. Auctions account for 54.4%, which is similar to the 51% in BM. In target-initiated deals, auctions account for 74.4% and in bidder-initiated deals, negotiations account for 55.2%. The proportions are in line with those reported in the literature, e.g., BM (2008) and Aktas et al. (2010). The intensity of potential competition in our sample is 11 bidders on average. The intensity of competition in the final bidding process is 3 bidders on average. The length of an auction deal is shorter than a negotiation deal, consistent with the results in Fidrmuc and Xia (2014). This may reflect the incentive of targets to mitigate the cost of information disclosure in auctions

[10] In BM (2008), unsolicited deals are initiated by third-party and bidder. We categorize third-party initiated deals into target or bidder initiated ones.

when confidential information is revealed to multiple bidders (e.g., Anilowski et al. (n.d.)).

Table 3.2: Sample By Year

Year	Full Sample	Auction	Negotiation	Initiation		
				Target	Bidder	Both
1998	75	27	48	20	51	4
1999	63	26	37	22	31	10
2000	55	22	33	16	31	8
2001	49	27	22	29	15	5
2002	31	20	11	12	17	2
2003	30	21	9	12	13	5
2004	37	20	17	12	23	2
2005	34	19	15	11	19	4
2006	38	27	11	15	18	5
2007	37	22	15	10	22	5
2008	28	21	7	15	10	3
2009	23	17	6	6	11	6
2010	31	17	14	11	19	1
2011	19	12	7	9	9	1
2012	25	15	10	11	13	1

Table 3.2 reports the number of deals and the proportion of auctions/negotiations over our sample period. In years with relatively high M&A activity (1998, 1999 and 2000), there are substantially more bidder-initiated than target-initiated deals. In 1998, the number of bidder-initiated deals is more than double the number of target-initiated deals. In these merger wave years, there are also substantially more negotiations than auctions. In 1998, the number of negotiations is almost twice that of auctions. This

appears counter-intuitive to the idea posed by the theory of economic shocks on merger activities. Results from the literature (e.g.Gort (1969); Ravenscraft, 1987; Mitchell and Mulherin (1996); Harford (2005)) suggest that merger waves are induced by unexpected "economic shocks" within industries, such as rapid changes in technology and demand, movements in capital markets, and changes in entry barrier within industries. Also, during these periods uncertainty in firm value rises and merger activity increases. Given these explanations, we should expect to witness a dominant use of auctions in these wave periods. During economic shocks, the internal growth opportunities deplete because of the industry uncertainty and we would expect more firms seeking business combinations, particularly through acquisitions. Therefore, an auction would be welfare-increasing to the target because there are potentially more bidders. We further explore the time-series pattern in Section 5 where we present the analysis of the sales method.

Table F1 reports summary statistics for firm, deal, industry characteristics and wealth measures in our sample. All variable definitions, calculations, and sources are listed in Appendix G of the paper. We report p-values to indicate whether the difference of these variables between auctions and negotiations is statistically significant. There are no significant differences in target, bidder, and relative size between auctions and negotiations. Compared to the sample in BM (not reported), we note there is a substantial difference in relative size. In BM, target and bidder size are defined as the value of equity and relative size as the log of the target equity divided by bidder equity. We use the book value of assets as a measure of firm size and to capture the magnitude of firm's proprietary information[11]. Relative size is defined as the log of target book value of assets divided by the bidder's. Our sample includes more relatively small targets compared to the BM sample, which translates into a mean (median) of relative size is 33% (16%) compared to 57% (27%) in BM.

[11] Wangerin (2012)discusses the type of information exchanged between targets and acquirers in the due diligence process. He points out that most of the attention in the due diligence process is directed towards accounting identification and valuation of assets and liabilities of targets, and to obtain the fair value of targets. We believe book value of assets is able to capture the majority of the type of information revealed in the due diligence process and proxies for the costs of revealing information.

Deal characteristics differ significantly between the auction and negotiation samples. Specifically, the auction sample contains significantly more cash deals, fewer tender offers, and fewer bidder initiated deals compared to the negotiation sample. Second, for firm characteristics, we find that in our sample, both targets and bidders in auctions are on average smaller than those in negotiations, at 10% significance level. We also include three indices for financial constraints: revised KZ index (Baker et al. (2003)), WW index (Whited and Wu (2006)) and SA index (Hadlock and Pierce (2010)). Lamont et al. (2001) creates the KZ index based on the regression estimates by Kaplan and Zingales (2000). The KZ index is a linear combination of Q, leverage, operating cash flow, cash, and dividends.Baker et al. (2003) points out a disadvantage of the index in including Q that might deliver opposite predictions for investment and this use a revised index that omits Q. These indices measure financial constraints, where a higher value indicates a more financially constrained firm.

For target industry and year characteristics, we include a liquidity index, an economic shock index, the Herfindahl index, and deal volume. The liquidity index is defined as the total value of corporate control transactions divided by the total book value of assets of firms in the target's industry in each year (Schlingemann et al. (2002)). This index measures the intensity of corporate asset transactions within an industry and proxies for that industry's asset market liquidity. The economic shock index is calculated as in Harford (2005), as the first principal component of the seven economic shock variables: profitability (net income/sales), ROA, employee growth, R&D intensity, capital expenditure and asset turnover. Each shock variable is measured as the industry absolute median change of the corresponding economic variable. Hence we also include the seven shock variables independently. Harford (2005) uses the economic shock index to capture an industry's operating environment and finds that the beginning of merger waves is significantly and positively related to the index. Deal volume is the total number of deals in each year in our main sample and describes the aggregate M&A activity annually. All these industry indices are calculated based on Fama-French 48 industrial classifications. In our sample, target (bidder) sizes do not differ significantly between auctions and negotiations, as is the opposite in BM. For target characteristics, we find that auctioned targets are associated with higher debt ratio, lower ROE, lower Tobin's Q, lower sales growth, lower industry liquidity index, lower economic shock index and lower M&A activity.

Our wealth measures are target returns (TCAR), bidder returns (BCAR), target wealth gains (T$CAR), bidder wealth gains (B$CAR), synergy wealth gains (S$CAR), synergy returns (SCAR) and target relative gains (DELTACAR). Specific definitions of these measures are discussed in Section 5.3. In our sample, bidder returns are on average negative; there are no differences in TCAR (BCAR) between auctions and negotiations, but T$CAR (B$CAR, S$CAR) are significantly lower (higher) in auctions. We do not find a significant difference in SCAR and DELTACAR between auctions and negotiations.

3.4 DETERMINANTS OF SALES METHOD

Before revisiting the wealth effects of the sales method, we consider the determinants of the sales method, where we focus, respectively, on auctions versus negotiations, the number of bidders contacted, and deal initiation.

3.4.1 Auctions versus Negotiations

While we are not the first paper to analyze the determinants of the sales method, we propose, however, a richer set of explanatory variables. The specifications include uncertainty, target financial characteristics and operating performance, but also cover target financial constraints and target industry characteristics.

Table F2 (in Appendix F) reports the results for seven logistic model estimations of the sales method. The dependent variable is the binary auction dummy. We organize the explanatory variables into five categories: (i) target financial characteristics, (ii) target uncertainty, (iii) target information cost, (iv) target operating performance, (v) target financial constraints, and (vi) target industry characteristics.

Target financial characteristics include Target Size, Leverage, ROA, ROE and Tobin's Q. Target uncertainty is proxied by Return Standard Deviation (Return Std.). Target information cost is measured by R&D intensity: the higher R&D intensity is, the greater the magnitude of the private information is. Target operating performance includes Beta, Liquidity, Market Share Growth, SG&A (selling, general and administrative expenses), Change in ROA and Change in Sales Growth. We also define two dummies: Change in ROA/Sales Growth Negative/Positive dummy that equals 1 when change in ROA/sales growth rate is negative/positive and include two interaction variables of Change in ROA/Sales Growth and their associated dummies. Target financial constraints include two dummies: High SA/WW Index that equals 1 when target's SA/WW Index is higher than the industry median. Finally, target industry characteristics include the Economics Shock Index and Herfindahl index.

Model (1) in Table F2 focuses on target financial characteristics, uncertainty and information cost. Model (2) adds target operating performance. In both models, auction is significantly associated with higher leverage, lower ROE and information cost (R&D intensity), and lower market share growth, decreases in ROA, and with positive changes in sales growth. The relation between auctions and these variables is consistent across all of the models presented in Table F2. Since EBIT is the numerator of ROA, our results suggest that auctioned targets are usually not big players in the market and are more likely to have higher operating expenses that decrease their returns on equity. Such operating expenses also cause their ROA to dramatically drop over the year before the M&A deal. In contrast, sales growths increases over the year before the M&A deal. Models (3) – (5) add financial constraint indices as explanatory variables. Both the SA and WW index are functions of target size. For example, the two dummy variables that indicate High SA and high WW index have correlations of approximately 60% with target size. To avoid multicollinearity, we omit target size in these models. Model (3) adds the High SA Index dummy and Model (4) adds the High WW Index dummy. The SA index dummy is not significant, but the WW index dummy is significant. This provides some evidence that more financially constrained targets are more likely to use an auction in a corporate takeover. In Model (5) we add the Economics Shock Index and the Herfindahl index. The Economics Shock Index is significantly and negatively related to the likelihood of an auction, which is in line with the earlier result from the summary statistics showing a clear time series pattern. We see more bidder-initiated deals and negotiations and fewer auctions during active M&A years. We revisit this time-series relationship later in this section. For model (5), we report both the logit coefficients and marginal effects of the explanatory variables on the likelihood of conducting an auction for easier interpretation.

3.4.2 Bidder competition

We next analyze the determinants of the number of bidders contacted in the sales process. We use a Tobit model where the dependent variable is the number of bidders contacted, right censored at the maximum number of bidders in the sample: 113. Table F3 reports the results. We start with the same explanatory variables as in Model (2) from Table F3. For brevity, we do not report specifications separately with the WW and SA Index dummies), as their coefficients are insignificant and they do not affect the other coefficient estimates in any of the specifications. Instead, we use the KZ Index dummy to measure financial constraints. Model (1) shows that a negative change in ROA and positive change in Sales Growth and the KZ Index dummy are significant determinants of bidder competition. Model (2) we drop leverage from Model (1) and ROA becomes significantly and negatively related to bidder competition. Model (3) and (4) adds the Economic Shock

Index, Herfindahl Index and Potential Competition. In general, bidder competition is positively associated with targets with lower ROA, lower information cost, lower market share growth, negative change in ROA, positive change in Sales Growth, tighter financial constraints, and lower industry economic shocks. Overall, the determinants of bidder competition are similar to those in the sales method estimations[12].

3.4.3 Deal initiation

Next, we examine the determinants of deal initiation and compare these to the determinants of the sales method. The timing of the sales method differs between target-initiated and bidder-initiated deals. For example, for target-initiated deals, the sales method occurs simultaneously with the target's decision to sell the firm. For bidder-initiated deals, however, the choice of method occurs after the bidder expresses its interest in merging with the target. The simultaneity between the deal initiation and sales method in target-initiated deals suggests that the incentives behind these two might be similar.

We report the results of this analysis in Table F4. In each specification, the dependent variable equals 1 when the target initiates the deal and 0 otherwise. We start in Model (1) with a similar set of explanatory variables that we used in the sales method estimation of Model (5) in Table F2, but, without affecting our results, exclude the Herfindahl Index. We find that target-initiated deals, like auctions, are associated with higher leverage, positive changes in sales growth, and lower industry economic shocks. Model (2) drops Change in ROA, Change in ROA negative and Change in ROA $\times$ Change Negative. In addition, we find that target-initiated deals have lower Tobin's Q. Model (3) adds sales growth into Model (2). While the results from Model (2) hold, we also find that target-initiated deals have higher sales growth rates, lower market share growth and tighter financial constraints.

[12] We also estimate the last model in the auction subsample and find that only Change in ROA $\times$ Change Negative, Change in Sales Growth $\times$ Change Positive and KZ Index Dummy are positively related to the number of bidders contacted.

In Models (4) - (6) we drop Economics Shock Index, ROA, ROE and R&D intensity sequentially. Our results continue to hold, in addition we find Uniqueness, a measure of administrative and advertising expenses, increases with the likelihood of a target initiated deal. Relative to Model (6), Model (7) adds the industry median Tobin's Q. We find that the coefficients on High WW index and (firm-specific) Tobin's Q are not significant, while higher industry Tobin's Q is significantly related to bidder-initiated deals[13].

These results suggest that when targets initiate a deal, they tend to operate in more competitive industries. Also, these industries have fewer growth opportunities. In addition, they are more leveraged, but experience positive sales growth. They also bear the burden of higher administrative and advertising expenses. The commonality in deal initiation and sales method determinants suggests that they are motivated by the similar incentives. Tobin's Q provides an interesting exception as it is an important determinant for deal initiation, but is unrelated to the sales method.

3.5 M&A WEALTH EFFECTS AND THE SALES METHOD

We proceed with our analysis of the valuation consequences of the sales method, taking into account the determinants for the sales method and deal initiation from the previous section. To test the extended competition tradeoff hypothesis we analyze the target wealth effects of auctions and negotiations and compare our findings with the results reported in BM. We conduct an event study analysis of target returns followed by cross-sectional multiple regression analyses. We then proceed to a two-stage simultaneous regression framework to control for endogeneity between target returns and sales method. In Section 5.2 we re-estimate the two-stage simultaneous equation system where the number of bidders

[13]Tobin's Q is often used as a proxy for operating performance. For example, Gompers et al. (2003) conclude that firms with more shareholder rights are better governed since they have a higher Tobin's Q. Yermack (1996) also proxies board performance by Tobin's Q.

contacted proxies for bidder competition and investigate its relation with target returns. In Section 5.3 we test the bidder competition tradeoff hypothesis and analyze bidder and combined (synergy) wealth effects of the sales method.

3.5.1 The extended competition tradeoff hypothesis and target returns

Table 3.3 reports the average cumulative market-model adjusted abnormal returns for the target, TCAR, based on a (–1, +1) window where day 0 is the announcement date and the market index is the CRSP value-weighted index. The average of TCAR for both the auction and negotiation subsamples are significantly positive and in line with earlier studies. There is no significant difference in the average TCAR between auctions and negotiations.

Table 3.3: Event Study Analysis: [-1, +1] Window

Sample	Target Return (N=575)
Panel A. Full Sample: Mean Return (p-value)	
Full Sample	26.0%
	(0.000)
Panel B. Analysis by Sales Procedure: Mean Return (p-value)	
Negotiation	26.2%
	(0.000)
Auction	25.8%
	(0.000)
Panel C. Paired t-tests: t-statistic (p-value)	
Negotiation	0.4%
v.s. Auction	(0.856)

We next conduct a multivariate analysis of TCAR based on cross-sectional OLS regressions. To conserve space we do not tabulate these results. Our main variable of interest is the dummy variable for the sales method (Auction) and we include a set of control variables that have been shown in the literature to affect target returns. All variables are defined in Appendix G. Specifically, we include various combinations of relative size, a cash dummy to indicate a pure cash deal; a tender offer dummy, a bidder-initiated deal dummy, and target return standard deviation. The coefficient on auction is never significantly different from zero in any of our specifications, suggesting there is no difference in target premiums between negotiations and auctions. In contrast, relative size, cash dummy, tender dummy, and return standard deviation each have coefficients significantly different from zero.

A concern with the OLS framework is that the coefficient on auction will likely be biased because of endogeneity. For example, Hansen (2001), Smith (1987) and BM point out the potential endogeneity bias arising from the method of sales and target returns. We confirm the presence of an endogeneity bias between the two variables in our sample using the Durbin-Wu-Hausman test and therefore follow the previous literature and conduct a series of two-stage simultaneous regression analyses[14].

In our two-stage regression models, we follow our multiple regression analysis and Boone and Mulherin (2007) in specifying our target returns equation by including a similar set of explanatory variables. For our first stage equation, we choose two instruments for Auction, which results in an over-identified system where the additional instruments can be used to increase the precision of the estimates as they provide more exogenous variation in predicting the endogenous variable[15]. With a single instrument there would be less exogenous variation in the endogenous variable and the estimators less efficient.

Following Roberts and Whited (2012), we rely on economic arguments to justify our

[14]This is based on a two-stage regression, in which the first-stage has an auction dummy as the dependent variable and the other variables from the target returns equation as explanatory variables.

[15]For example, Angrist and Krueger (1991) show that using 180 instruments in the schooling application study gives tighter correct confidence intervals than using 3 instruments.

instruments, but acknowledge that any instrument is subject to criticism in empirical corporate finance research. We use target sales growth and deal volume, measured as the total number of deals in each year in our main sample as instruments for Auction[16]. For target sales growth, Comment and Schwert (1995) and Schwert (2000) document that target sales growth is not significantly related to target returns. We are also unable to find evidence of a significant relation between aggregate M&A activity and target returns, despite a large literature investigating the determinants of target returns. Moreover, Bradley et al. (1988) finds that institutional and regulatory change, as partially described by deal volume, does not have a significant impact on target returns (instrument exclusion)[17].

We report the results of our two-stage regression models in Table 3.4[18]. Model (1) is the baseline model. In the first-stage, Auction is regressed on the set of exogenous variables, including the two instruments. The p-value for the joint F-test is less than 0.001 and indicates that our instruments are significantly related to Auction.Hall et al. (1996) use Monte Carlo simulation to show that simply having an F-statistic that is significant at the typical 5% or 10% level is not sufficient. Stock et al. (2002) suggest that the F-statistic should exceed 10 for inference based on the 2SLS estimator to be reliable when there is one endogenous explanatory variable. Our F-statistic is 11.515, therefore the instrument relevance criteria appear to be satisfied. In the second stage, TCAR is regressed on the fitted value of Auction (Auction*) from the first-stage and on the same set of exogenous variables, excluding the two instruments. To test instrument exclusion, we conduct Sargan's test of over-identification restrictions and report the results in the table. The null hypothesis is that instruments are valid and a significant test statistic could therefore represent either invalid instruments or an incorrectly specified model. Our p-value for Sargan's test is 0.913, suggesting our instruments are valid. For each of the

[16] We think it is sensible to use deal volume in our restricted sample, since our target returns estimation is focused on this sample.

[17] Harford (2005)shows that deregulatory events are associated with the beginning of merger waves.

[18] BM use target size as an instrument, but we find that this does not satisfy the relevance condition.

subsequent two-stage regression models, we report a joint first-stage F-test statistic and p-value, and Sargan's test statistic and p-value.

Table 3.4: Two-Stage Regression Analysis: (-1, +1) Window

	(1)		(2)		(3)		(4)	
Variables	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	(a)	(b)	(a)	(b).
Intercept	1.470***	-0.083	1.766	-0.348	-0.570*	0.042	-0.299**	0.072
	(0.324)	(0.076)	(1.388)	(0.271)	(0.331)	(0.104)	(0.149)	(0.083)
Auction*		0.358***		0.325***	0.641*	0.138	0.450***	0.119
		(0.111)		(0.107)	(0.330)	(0.155)	(0.156)	(0.137)
Relative Size	-0.032	-0.010	0.001	-0.010	-0.044**	-0.055*	-0.028**	-0.041**
	(0.053)	(0.006)	(0.057)	(0.006)	(0.018)	(0.031)	(0.011)	(0.021)
Cash	0.485**	-0.002	0.619***	-0.001	0.049	0.057	0.005	0.053
	(0.219)	(0.030)	(0.238)	(0.031)	(0.040)	(0.059)	(0.035)	(0.049)
Tender	-0.982***	0.174***	-0.984***	0.153***	0.291***	-0.006	0.248***	0.041
	(0.249)	(0.039)	(0.274)	(0.039)	(0.087)	(0.056)	(0.055)	(0.054)
Bidder Initiated	-0.761***	0.084***	-0.717***	0.068**	0.204*	0.047	0.140***	0.038
	(0.182)	(0.030)	(0.197)	(0.029)	(0.105)	(0.041)	(0.054)	(0.033)
Return Std.	3.918	1.290**	6.381	1.520**	2.194**	1.564*	1.960**	1.091
	(4.641)	(0.531)	(5.571)	(0.613)	(1.097)	(0.924)	(0.839)	(0.701)
Sales Growth	-0.336*		-0.412**					
	(0.179)		(0.196)					
Deal Volume	-0.023***		-0.026***					
	(0.006)		(0.006)					
N	575		575		192	191	288	287
Industry FE	No	No	Yes	Yes	No	No	No	No
Pseudo R2/Adj. R2	0.092	0.068	0.134	0.088	0.090	0.02	0.09	0.029
F-statistics	11.516		11.635					
p-value	0.000		0.000					
Sargan's Chi-sq		0.012		0.101	0.656	0.000	1.679	0.01
p-value		0.913		0.751	0.418	0.990	0.195	0.918

The coefficient estimate in the second stage on Auction* is 35.8% and significant at the one-percent level, which suggests that auctions significantly increase target returns. We will further discuss this result and its implications later in the section.

In Model (2) we add industry fixed effects in both of our two stage models. The coefficient on Auction* is 32.5% and remains significant at the one-percent level[19]. Model (3) and (4) investigates how relative size affects our result from Model (2). In particular, Models (3a) and (3b), respectively, split the sample into deals where the relative size of the target is equal or below the median and above the median. Similarly, Models (4a) and (4b) split the sample into deals where the relative size of the target is at the bottom and upper tercile. For both Models (3a) and (4a), where we analyze relatively smaller targets we find a positive and significant coefficient on Auction*. The coefficients for these two specifications are and 45.0% and 64.1% respectively, while the coefficients for Models (3b) and (4b) are 13.8% and 11.9% and both statistically insignificantly different from zero percent. Together, our results are consistent with the predictions of the extended competition tradeoff hypothesis and show that for relatively smaller targets we find the strongest relation between the sales method and target returns.

3.5.2 Bidder competition and target returns

The two-stage simultaneous equation system allows us to estimate the relationship between bidder competition proxied by the number of bidders contacted and target returns. We use the specifications from Table 3.4 in Section 5.1. In the first-stage, we use a Tobit model where the dependent variable is the number of bidders contacted. The Tobit model is right-censored at 113, which is the maximum number of bidders contacted in the sample.

[19]The results are robust to adding more controls variables to the specification of Model (2)., such as indicator variables for pure stock deals, related deals, and Deal Value for deal characteristics and ROA, Tobin's Q and Leverage for target characteristics. We also use target returns measured in [–3, +3] window around the deal announcement date for Model (2) and the result still holds.

Table F5 reports the result of the Tobit models. In Model (1) and (2) we see that our result goes beyond the binary method of sales: greater bidder competition also significantly increases target returns. This result is related to Aktas et al. (2010), who focus on the role of ex-ante competition as a threat for the bidder if the first-stage one-to-one negotiation fails, in the number of bids. They find a positive relation between ex-ante competition and bid premiums. They use industry deal frequency to proxy for ex-ante competition. Note that we classify a deal as an auction when the number of bidders contacted is greater than one and competition is measured prior to the process of actual bids being submitted. Therefore, the results from the Tobit estimations in Table F5 suggest that when ex-ante competition is higher, target returns are higher. Because we measure the ex-ante competition specific to the target and the deal, our results are more informative and contribute to our understanding of how true competition for the target contributes to target shareholders' wealth.

In summary, we find a positive and significant association between auctions and target returns. However, this does not imply that firms should always use auctions. We show that when information costs are relatively small, as is the case with relatively small targets, auctions are, on average, preferred. However, as information costs increase auctions no longer are associated with higher target returns.

3.5.3 The bidder competition tradeoff hypothesis: Alternative wealth measures

The primary focus of the literature on wealth effects associated with the sales methods is on target premiums. An important, yet largely unanswered question is whether and how the sales method affects bidder returns, synergy returns, and the distribution of gains between the bidder and the target. According to the bidder competition tradeoff hypothesis, bidders, as a result of competition, face a tradeoff between the cost of offering a higher premium in order to win the bid, but also benefit from acquiring more information about the target's valuation, for example, about other bidders' private signals of the target's valuation. Note that the information gain in the due diligence process to each bidder is not affected by the increased competition. Especially for relatively small targets where the set of private signals from other bidders is larger, the potential benefit from more competition through information aggregation may outweigh the cost of offering a higher premium.

Next, we include, in addition to the variables from our baseline model in Table 3.4, target (bidder) size, bidder return std., and the number of target (bidder) SIC codes, relative market share (target/bidder), hostile, target (bidder) operating cash flow and relative Tobin's Q (target/bidder). To avoid multicollinearity we only include target size in models explaining target wealth effects and bidder size in all other models. As before, we recognize the importance of proper identification and conduct a Durbin-Wu-Hausman endogeneity test to determine whether there is potential endogeneity between each wealth measure and auction choice for each model. We conclude that an endogeneity bias is again likely between TCAR and the sales method, but not for our alternative wealth measures. Therefore, we maintain the two-stage regression framework for TCAR, but use OLS for the other wealth measures. We continue to use Target Sales Growth and Deal Volume as instruments for Auction as they continue to satisfy the exclusion and relevance conditions.

Table F7 the results for the full sample. Model (1) confirms that for TCAR the coefficient on Auction* remains positive and significant using the expanded specification. In model (2) we investigate the relation between the sales method and bidder returns (BCAR). BCAR is measured in the same way as TCAR and represents the bidder's cumulative abnormal return in a (-1, +1) window around the deal announcement. The coefficient on Auction is insignificant, suggesting that the sales method does not affect bidder returns. We next consider dollar denominated returns ($CAR), which proxies for the net present value of the transaction (Malatesta, 1985). For the target (bidder), the T$CAR (B$CAR) is defined as the product of the TCAR (BCAR) and the market value of the target (bidder) measured one year prior to the deal announcement. In model (3) we find that the coefficient on Auction is negative and significant if we focus on dollar dominated target wealth gains (T$CAR). As F1 shows, targets in auctions are substantially smaller than those in negotiations. The opposite signs of the coefficient on Auction in models (1) and (3) suggest that the positive relation between target percentage returns and auctions is driven by smaller targets. These auctioned targets have greater returns but are smaller, resulting in smaller dollar gains. In summary, shareholders of smaller targets benefit from auctions in terms of greater percentage returns, which represent smaller dollar gains. Vice versa, shareholders of larger targets experience smaller returns measured in percentage terms that translate into a greater dollar gains. This is consistent with our extended target competition tradeoff hypothesis: smaller targets are more likely to enter into auctions because they are able to attract a larger set of potential bidders and hence induce greater competition premium; meanwhile their information costs in the auctions are much smaller[20]. The results based on the dollar denominated returns, compared to those obtained with traditional announcement period returns (TCAR), also illustrate that targets would not be uniformly better off choosing an auction over a negotiation.

A similar observation can be made with respect to bidder percentage returns versus dollar denominated returns, as shown in models (2) and (4). In model (2), the coefficient on Auction is insignificant; in model (4), the coefficient on Auction is positive and significant at the five percent level. From the summary statistics in F1, we know that bidder returns are negative on average and that bidders are smaller in auctions than in negotiations. This suggests that while there is no difference in bidder returns between auctions and negotiations, smaller bidders in auctions are associated with smaller dollar denominated

[20]In unreported tables, we calculate the distribution of relative size, target and bidder market size. The intersection between the terciles representing the smallest (largest) targets and the largest (smallest) relative size in the sample contains only 18% (19%) of the sample observations. Similarly, the intersection between the terciles representing the largest (smallest) bidders and the largest (smallest) relative size in the sample contains only 7% (4%) of the sample observations. The correlation between relative size and, respectively, target and bidder size is 0.222 and -0.587, both significant at the one-percent level.

wealth losses. This is consistent with our bidder competition tradeoff hypothesis: smaller bidders are more likely to enter into auctions because they rely more than the information aggregation benefits of auctions to obtain more precise valuations of targets.

In unreported analysis, we re-estimate model (3) for, respectively, the bottom and top terciles, and below and above median sub-samples of relative size. We confirm that BCAR exhibits a positive, but marginally significant (p-value=0.101) relation with Auction for the bottom tercile of relative size (i.e., on average smaller targets), but not for the other terciles. This finding is consistent with the idea that relative size affects the bidder competition tradeoffs: for bidders associated with relatively small targets, the benefits of information aggregation are greater because the set of private signals from other bidders is larger, which leads to a more precise valuation of the target, in the spirit of the Law of Larger Numbers.

Next, we consider the synergy returns based on the percentage and dollar denominated return measures, SCAR and S$CAR. Synergy returns (SCAR) are defined as the market value-weighted average of TCAR and BCAR, following Bradley, et al., (1988). The sum of T$CAR and B$CAR, S$CAR, represents to the total dollar gain associated with the transaction. In model (5) we find no association between the sales method and SCAR, which we expect based SCAR representing a value-weighted average where the size effects likely cancel each other out. In model (6) we consider the total dollar gain (S$CAR), which is the sum of T$CAR and B$CAR. Since S$CAR is not value-weighted and because bidders are generally larger than targets, it is no surprise to see that the coefficient on Auction is similar to the one reported in model (4) for B$CAR.

Finally, we investigate whether the sales method is associated with the distribution of the total gains, S$CAR, between the bidder and the target. We follow Ahern (2012) and define relative gains accruing to the target shareholders (DELTACAR) as the difference between target and bidder dollar gains, divided by the total market value. This measure represents the relative gain of the target gains versus the acquirer for each dollar of total market value and avoids the concern that total merger gains may be negative. Model (7) shows no relation between the sales method and DELTACAR.

Taken together, the results in Table F7 suggest the sales method is associated with wealth effects beyond those of target returns and that the results are consistent with the predictions of our hypotheses. First, though on average bidder competition tradeoffs cancel out and result in no wealth effect from the auction, the value of the discovery process for the bidder associated with a relatively small target in an auction outweighs the competition cost, because of the larger set of private signals from other bidders. Second, the results on wealth gains illustrate the size effect of sales method. Targets have greater return in auctions but they are smaller, hence they have lower wealth gains. Bidder returns on average are not affected by the sales method but bidders are smaller in auctions, hence they have lower wealth losses. This size effect is consistent with our hypotheses. Smaller targets enter into auctions because the set of potential bidders is larger and the information cost is smaller. Smaller bidders enter into auctions because they rely more than the information aggregation function of auctions to obtain more precise valuation of the targets.

3.6 ROBUSTNESS CHECKS AND MODEL VALIDITY

In this section, we evaluate the robustness of our results and also validate our empirical model using an out-of-sample test.

3.6.1 Robustness

We first address the concern that announcement period returns may be biased because they may not properly reflect differences in perceived likelihoods of deal completion based on the sales method. If auctions are considered to be more likely to complete than negotiated deals, our results may reflect the difference in this likelihood. To mitigate this concern we estimate buy-and-hold abnormal returns starting one day before the public announcement of the deal until one day after the effective date of the deal where there is no more uncertainty regarding deal completion. We find that our results continue to hold when we replace TCAR with its long-term counterpart. For brevity we do not report the results in separate tables.

Industry variation could play a role in the tradeoffs between information costs and the competition premium related to the sales method, which, in turn, affects returns. We control for industry fixed effects in our basic specifications in F6. However, to better understand what industry characteristics these industry dummies precisely capture and these affect our results, we replace the industry dummies with four industry indices: the economic shock index, the in-wave dummy, the Herfindahl index, and the liquidity index. The in-wave dummy equals 1 when there is an industry merger wave in the target industry during the deal announcement year. We estimate industry merger waves from our pre-merging original SDC sample following the method in Harford (2005). We first divide the sample into two subsamples: 1998-2005 and 2006-2012. Taking the total number of bids over the entire subsample period for a given industry, we simulate 1,000 distributions of that number of occurrences of industry member involvement in a bid over a 120-month period by randomly assigning each occurrence to a month where the probability of assignment is 1/96 for each month in the years 1998-2005 and 1/84 for the years 2006-2012. Since waves are defined to be in 24 months, we compute the highest 24-month concentration of activity from each of the 1,000 draws[21]. Finally, we compare the actual concentration of activity from the potential wave to the empirical distribution of 1,000 peak 24-month concentrations. If the actual peak concentration exceeds the 95th percentile from that empirical distribution, that period is coded as a wave. The estimation results in 44 waves in 19 industries. Table F6 shows that the coefficients on Auction* continue to be positive and significant at a 5% level when we replace the industry dummies with the four industry

[21] We follow Harford (2005) and Mitchell and Mulherin (1996) in defining the length of merger waves

indices and.

Finally, Bao and Edmans (2011) and Golubov et al. (2012) find that financial advisors may possess a heterogeneous set of skills in advising M&A deals. In particular, for target financial advisors, they may influence the choice of the sales method. In unreported results, we control for financial advisor fixed effects in our baseline two-stage regression model. The number of observations drops to 531 and the coefficient on Auction* is still statistically significant. This implies that our results are robust to controlling for unobserved heterogeneity in financial advisors across deals as well. This further suggests that the different set of skills of financial advisors[22] alone cannot explain the auction's wealth effects.

3.6.2 Out of sample evaluation

To validate our empirical models about target returns, we re-evaluate such results and our estimations of the sales method using an out of sample methodology. Out-of-sample validation addresses the questions of how models predict data, which is a different question from how models describe the data.

We proceed as follows. Our estimation sample includes the 575 deals for which we have information on deal initiation and sales method. Our validation sample includes the 792 deals without this information. We use the logistic models based on the estimation sample to generate predicted probabilities of these two variables: deal initiation and sales method and derive the optimal cutoff probabilities for the predicted probabilities. In our validation sample, we use our estimated models to generate predicted probabilities and classify sales method and deal initiation using cutoffs and evaluate our two-stage model of target returns of sales method in the validation sample. The out-of-sample methodology is described in Appendix H.2.

[22] Bao and Edmans (2011)) shows that certain banks have ability in identifying acquisitions or negotiating terms, or trustworthiness in turning down bad deals. Golubov et al. (2012) shows that certain banks might be better at one sales method (auction or negotiation) in conducting M&A deals.

Table F8 reports the results of re-estimating our two-stage regression model for the validation sample with the predicted auction and bidder-initiation dummies. The coefficient on Auction* remains positive and significant at the five percent level, which implies that the positive target wealth effect of auctions holds in the validation sample. The coefficient is 0.291, which is similar to the effect found in the estimation sample. This suggests that the results on target returns from our two-stage regression models can be generalized to a larger and independent data set and do not come from potential small sample bias.

3.7 CONCLUSIONS

We investigate the determinants and the shareholder wealth effects of the sales method in a sample of 575 M&A transactions during 1998-2012. Firms sold through auctions have higher leverage, idiosyncratic risk, operating and interest expenses, lower market share growth, smaller industry economic shocks; greater positive change in sales growth, and negative changes in ROA. We estimate the determinants of deal initiation and conclude that targets choose auctions to optimize their search for bidders that can relieve their financial constraints and pay a higher premium.

We extend the hypothesis that the choice between auctions and negotiations is determined by a tradeoff between the competition premium and information costs and argue that relative size plays an important role in this tradeoff. We find that for relatively small targets the competition premium outweighs information cost, whereas for relatively large targets, these two effects may cancel out. We find that auctions are associated with higher target announcement returns, particularly for smaller targets. This result is robust to controlling for industry characteristics and a large set of deal characteristics.

Bidders are expected to face tradeoffs between competition cost and information aggregation benefit in auctions where information aggregation benefits outweigh competition costs for bidders associated with relatively small targets, but these effects might cancel out for other bidders. Our empirical analysis provides weak support for the bidder competition hypothesis. We show that targets have greater percentage returns in auctions, but are smaller in size. Bidder percentage returns are unaffected by the sales method, but bidders are smaller in auctions. As a result, we find that auctions are associated with lower target dollar gains and higher bidder dollar gains, which lends further support to the target and bidder competition tradeoff hypotheses.

In terms of social efficiency and wealth redistribution, we find that synergy percentage returns are positively related to auctions. This suggests that the sales method is not a zero-sum game where target shareholder gains are just wealth transfers. Consistent with this, we find no association between the sales method and a measure of the relative gain captured by the target shareholders.

APPENDIX A

STRATEGY FREQUENCY ESTIMATION-CHAPTER 1

We will briefly describe the econometric model adopted in SFEM, discuss about the set of strategies we include in the estimation and then reports the estimated frequency of strategies for each of the treatments. To use SFEM, we assume that each subject chooses a fixed strategy for the last six cycles (the supergames). These chosen strategies are implemented with the possibility of independent mistakes, where another choice than the intended action is made. In this section, we report the results from our SFEM estimations. We first briefly describe the econometric model adopted and then report the estimated strategy weights for each of the main treatments.

Denote the choice made by subject i in round t of cycle m by c_{imt}. We specify a priori a set of K possible public perfect strategies $\Phi = \{\phi_1, \ldots, \phi_K\}$, where the choice of the strategy ϕ_k prescribes the choice $s^k_{imt} = \phi\left(y_{jm1}, ..., y_{jm(t-1)}\right)$ following the public history $\left(y_{jm1,}, \ldots, y_{jm(t-1)}\right)$. Each subject is assumed to follow a particular strategy ϕ_k, but they make independent mistakes each round with a probability $(1 - \beta)$, and chose the prescribed action with probability β. Given three actions (C, D and T) a random uniform choice is represented by $\beta = 1/3$, while a perfect match with a strategy by $\beta = 1$. The econometric model assumes a mixture model across the available strategies, so that strategy ϕ_k is selected with probability q_k.

Define the indicator, $I^k_{imt} = 1\{c_{imt} = s^k_{imt}\}$, which assigns a value of one if the observed subject choice matches the strategy choice σ_k match. The likelihood that the observed choices for subject i were generated by the strategy ϕ_k are given by

$$\Pr_i(\phi_k; \beta) := \prod_{m \in M_i} \prod_{t=1 T_{im}} \beta^{I^k_{imt}} (1-\beta)^{1-I^k_{imt}},$$

where M_i is the set of cycles, and T_{im} the set of active rounds. Combining across all subjects in a treatment we obtain the following likelihood function:

$$\sum_{i \in \mathcal{I}} \ln \left(\sum_{\phi_k \in \Phi} q_k \Pr_i(\phi_k; \beta) \right)$$

for the specified set of strategies Φ and summing the log-likelihoods across all subjects $\mathcal{I}$ in the treatment. The parameters to be estimated by maximum likelihood are the vector of probabilities $\mathbf{q} = (q_1, \ldots q_K)$ and the strategy-match probability β, under constraints that $\beta \in [1/3, 1]$, and that the vector of probabilities $\mathbf{q}$ lies in the probability K-simplex. The numerical maximization, and bootstrapping of the results, was completed in *Mathematica* using a differential-evolution constrained-optimization algorithm, using starting points for the estimation obtained following the same techniques followed in Dal Bó and Fréchette (2011).[1]

In total we allow for 38 different strategies, motivated by theory and the previous experimental literature. Many of the important strategies are defined in the main text, but a full list and definition of each strategy are available from the authors by request.

[1] We also conducted the same exercise in *Matlab* using modified code provided by Guillaume Fréchette, obtained qualitatively similar results. However, the *Mathematica* numerical routines seemed to be slightly better at attaining a global solution.

Table A1: Strategy Estimates: Main Treatments (last six cycles)

	None	Sym-75	Sym-125	Asym-First	Asym-Last	Asym-Moral
AC	0.056	0.178**			0.136*	0.279*
	(0.041)	(0.090)			(0.075)	(0.148)
AD	0.335***	0.307***	0.189***		0.360***	0.037
	(0.086)	(0.097)	(0.066)		(0.121)	(0.037)
AT			0.052	0.928***	0.058	
			(0.053)	(0.070)	(0.043)	
CDCD			0.024	0.072		0.034
			(0.032)	(0.056)		(0.031)
DCDC	0.025	0.022	0.037			
	(0.032)	(0.028)	(0.028)			
C-AllD	0.057	0.022	0.070		0.038	0.040
	(0.042)	(0.021)	(0.058)		(0.049)	(0.051)
C-T						
D-T						
Grim	0.255***	0.240*			0.247***	
	(0.094)	(0.124)			(0.092)	
Mono	0.070	0.023	0.028			
	(0.058)	(0.069)	(0.022)			
WSLS	0.036	0.070				0.040
	(0.031)	(0.051)				(0.050)
T11 CD	0.005		0.019			
	(0.019)		(0.037)			

S-Mono	0.070		0.022		
	(0.044)		(0.026)		
S-WSLS	0.046	0.023			
	(0.033)	(0.022)			
Grim-2					
Grim-3					
Mono21			0.073	0.082	
			(0.060)	(0.070)	
Mono31				0.003	0.246*
				(0.025)	(0.147)
Mono12					
Mono22					
Sum2	0.045	0.042		0.020	
	(0.044)	(0.033)		(0.040)	
Sum3					
Sum4					
S-Sum2					0.037
					(0.050)
C-1-Strike			0.049	0.022	0.099
			(0.070)	(0.032)	(0.088)
C-2-Strike		0.027	0.115*		0.133
		(0.041)	(0.065)		(0.131)

C-3-Strike		0.045	0.050			
		(0.058)	(0.051)			
D-1-Strike			0.022			
			(0.024)			
D-2-Strike			0.054		0.034	
			(0.048)		(0.041)	
D-3-Strike						
CD-11-Strike						
CD-12-Strike						
CD-21-Strike						0.054
						(0.103)
CD-22-Strike						
Probation						
S-Probation			0.026			
			(0.033)			
Probation21			0.171*			
			(0.094)			
Probation12						
β	1.000	1.000	1.000	1.000	1.000	1.000
	(0.033)	(0.028)	(0.025)	(0.039)	(0.027)	(0.020)

Note: Bootstrapped standard errors (across sessions, subjects and cycles) in parentheses. Significance indicated by: ***–1 percent level; **–5 percent level; *–10 percent level.

APPENDIX B

ADDITIONAL ANALYSES-CHAPTER 1

B.0.1 Within-Session Dynamics

Examining subjects' initial response in the experiment—the first round of the very first cycle, before they have interacted with other subjects—we do not see a stark cooperation differences across treatments. Across all sessions the median initial cooperation rates is 70 percent, with an interquartile range of 64–73 percent.[1] Expanding beyond this very first choice in the experiment, and analyzing the first six cycles in the sessions, the cooperation rate in active rounds in symmetric treatments is generally within the 60–70 percent range. For the other treatments, outliers in initial cooperation are the *No T* treatment, with an already much-lower active cooperation rate of 50 percent, and the *A-Judge* treatment with 76.5 percent cooperation.[2]

[1] Extreme outliers for initial cooperation in cycle-one-round-one are one session of *No T* and one for *S-75* on the low side (36 and 46 percent, respectively), and one session of *S-75* and one for *S-125* on the high (89 percent and 92 percent).

[2] *No T* is significantly lower ($p = 0.030$) and *A-Judge* is significantly higher ($p = 0.051$) than the symmetric treatments *S-75–135*. *A-First* also has significantly lower initial cooperation than the symmetric treatments ($p = 0.051$), however the difference is quantitatively smaller, at 57 percent active cooperation.

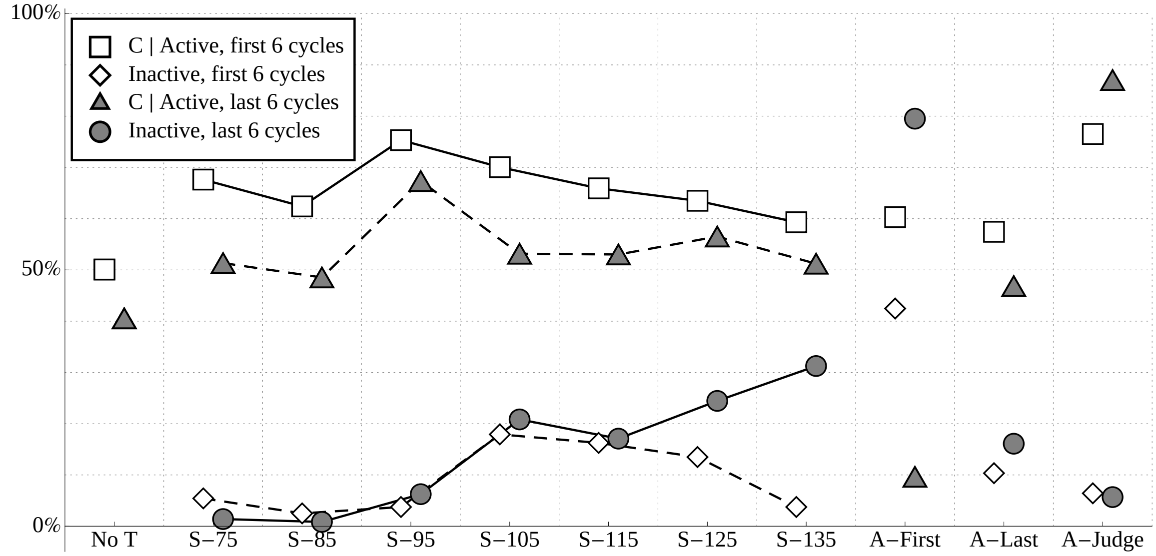


Figure B1: Cooperation and Inactivity across Sessions

As the session progresses, the active cooperation rate decreases by approximately 10 percent in each treatment, with the exception of *A-Judge* and *A-First*. Looking at the final six cycles in each session, the symmetric treatments have active cooperation rates of 50–55 percent (with a single outlier in the S-95 session). *No T* decreases to 40.5 percent active cooperation, while *A-Last* has a similar ten percent decrease to 46.8 percent. For *A-First*, the cooperation rate slide is much larger, decreasing to just 9.6 percent active cooperation in the final six cycles, with a matched increase in inactivity across the session. Finally, *A-Judge* is the only treatment which shows an *increase* in cooperation across session. The final six-cycles in *A-Judge* have active cooperation rates of 87.0 percent, while activity rates remains fairly constant across the sessions at approximately 94 percent.[3]

For the symmetric treatments with termination, the activity-rate trend across sessions is more varied. All sessions for *S-75–95* show increased relationship activity (and reduced

[3]Figure B1 in Appendix B.0.1 illustrates the activity and cooperation across sessions by treatment.

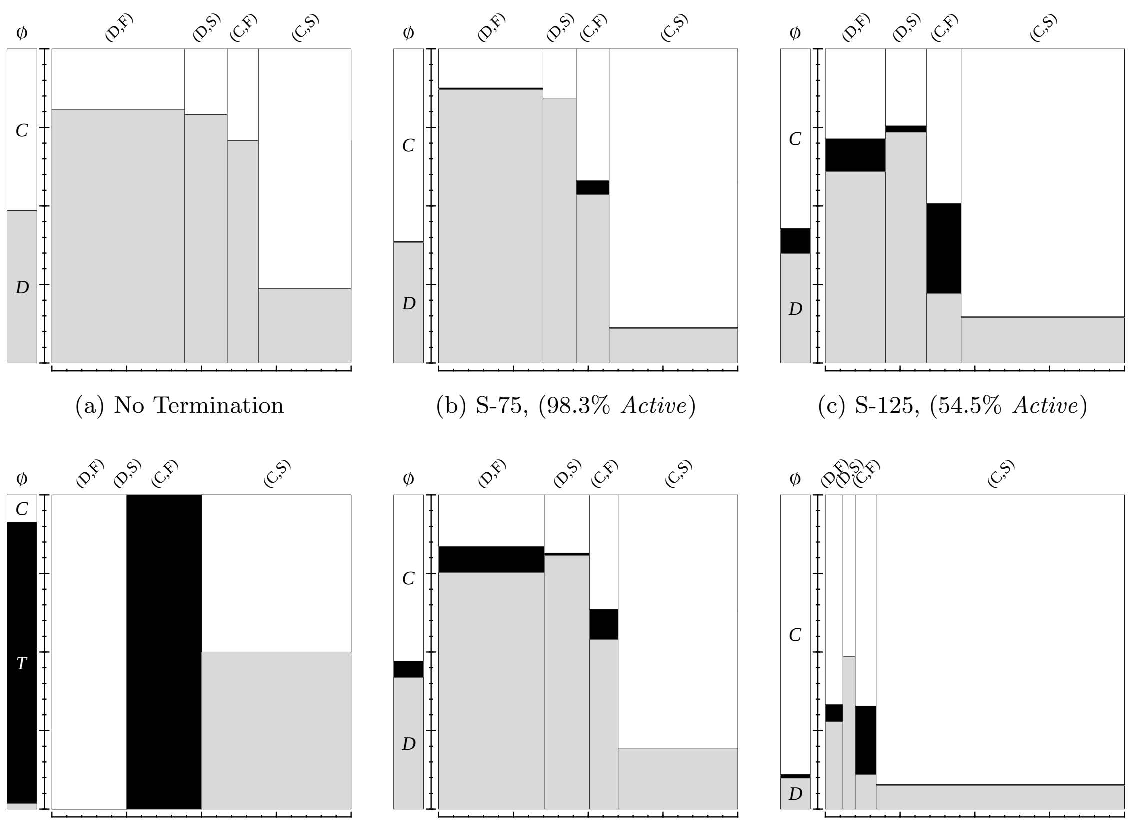
termination rates) in the last six cycles when compared to the first six, as subjects move away from using the termination action as the session progresses. In contrast, every session from *S-105–135* shows decreased relationship activity (increased termination) in the final six cycles (though the activity drop in the single *S-115* session is quantitatively small).

B.0.2 Conditional Response

To examine aggregate strategic responses, Figure B2 illustrates the proportion of action choices (either C, D or *Terminate*) conditioned on the previous round's history in our multi-session treatments (similar graphs for the single-session symmetric treatments are provided in Appendix B.1). Given the imperfect monitoring environment, subject i observes the sub-history $\left(a_i^{t-1}, y^{t-1}\right)$ for all rounds $t \geq 2$, her own previous action choice and the public outcome, and the empty history $\emptyset$ in the very first round. Each figure indicates the proportion of choices in *Active* rounds over the last six cycles in the session.[4] The main square in each figure represents all active rounds in rounds two and onward, while the thinner bar to the left indicates the actions chosen in the first round. The set of all active rounds is then sub-divided horizontally to indicate the previous round's history. The square is therefore divided into at most four regions, as all active rounds have a sub-history in $\{C, D\} \times \{S, F\}$. The proportion of active rounds with the relevant sub-history is given by the area covered by each of these four bands, here proportional to their width. For instance, in the *No-Termination* treatment the modal action-outcome pair in the previous round was *(D,F)*, while the least common was (C, F). In contrast, for *S-125*, the most common action-outcome the previous round was *(C,S)*, though it should be noted that only 54.5 percent of the relevant rounds in this treatment are active.[5] Each history-specific band (including the

[4]Given M total cycles in a session, all analyses referring to the last six cycles use session cycles $M-3$ to $M-1$, dropping the last cycle in the session to remove an end-session effect stemming from some subjects realizing the session was about to end where the hour had elapsed. For each cycle, each subject has two distinct partnerships, so we have a total of six distinct cycles.

[5]For all treatments except *No Termination*, we provide the activity rate for all rounds other that the first one in the treatment label.



(a) No Termination

(b) S-75, (98.3% *Active*)

(c) S-125, (54.5% *Active*)

(d) A-First, (0.6% *Active*)

(e) A-Last, (70.1% *Active*)

(f) A-Judge, (88.7% *Active*)

Figure B2: Conditional response in last five cycles, $a_i^t \,\big|\, \big(a_i^{t-1}, y^{t-1}\big)$

Note: For each treatment the main square indicate all active rounds for $t \geq 2$ in the last six cycles. This is then divided horizontally into the proportion of active rounds with the relevant (a_i^{t-1}, y^{t-1}) history. The proportion of action choices following each history are represented by dividing the band into three regions: D in gray, at the bottom, C in white from the top, and the *Terminate* action in black. The bar to the left in each plot indicates the proportion of actions chosen in the very first round.

first round empty-history band) is itself divided up vertically into three regions, indicating the proportion of actions chosen, given the last period's sub-history. The bottom gray region reflects the proportion of defection choices, the top white region reflects cooperation choices, while the black middle region indicates termination choices. Examining the *No-Termination* treatment, the figure indicates that the initial action choice in the first round is to play $a_i^1 = C$ in 52 percent of subject-cycles, while D is played 48 percent of the time, with no termination decisions (by design).

After the first round, conditional on cooperating and getting a successful outcome last round,the (C, S) sub-history, subjects continue to cooperate 75 percent of the time in *No T*, while for every other sub-history defection is the more common response (with defection rates between 75–80 percent). In fact, the modal response to successful cooperation last round is to cooperate again across all of our treatments, and at higher rates than *No T*.[6] The highest cooperation rate following the (C, S) sub-history is for *A-Judge,* where subjects continue to cooperate 93 percent of the time, though this is closely followed by *S-75* at 91 percent. The large efficiency differences between *A-Judge* and *S-75*—67.1 percent vs. 42.3 percent payoff efficiency overall, and 69.6 percent vs 32.7 percent in the last six cycles—are mostly attributable to greater first-round cooperation in *A-Judge.*

The figures partially illustrates leniency and forgiveness through the cooperation rates after histories other than (C, S). In terms of symmetric memory-one strategies, cooperating after the history (C, F) indicates lenience (a willingness to delay entering a punishment phase) while cooperation following (D, F) and (D, S) indicates forgiveness (attempts to enter a new cooperative phase from punishment). Across treatments, the most lenient and forgiving treatment using this rubric is *A-Judge*. In comparison to *No T*, *S-75* and *S-125*—where defection is the modal response to every sub-history bar (C, S)—the *A-Judge* treatment has cooperation as the modal response regardless of previous round's history.

[6]The *No T* cooperation rate following (C, S) is matched by *A-First*, though we will mostly ignore this treatment for much of the discussion as there are only four *Active*-state observations in rounds two and onward in the last six cycles, due to very high termination rates in round one.

The treatment with the least lenient behavior is *No T*, with 29 percent cooperative decisions following (C, F) the previous round. In contrast, cooperation rates following (C, F) in symmetric treatments with termination are 42 percent in *S-75* and 49 percent in S-125. The presence of a dissolution option therefore increases the selection of lenient responses.[7]

In contrast to lenience, the presence of an unused, low-payoff termination option has a negative effect on forgiveness. Pooling the decisions where (D, F) and (D, S) occurred the previous round, the cooperation rate is 20 percent in *No T* compared to just 13 percent in *S-75*. Where termination is present and utilized, the raw cooperation rate after a defection the last round increases in *S-125* to 27 percent, so forgiveness seems to increase. In this situation however, the reduced-form figure is harder to parse, as selection effects through partnership dissolution are more pronounced. Because many more cycles are dissolved in the punishment phase, those that are not are over-sampled. We address this in the paper body through the SFEM analysis.

Examining the black shaded regions in each figure, the termination action is primarily used either: i) as a decision to opt out in the very first round; ii) as a punishment following failed cooperation, (C, F); or iii) to exit an inefficient relationship following failed-defection, (D, F). In addition to *A-First*—where 90 percent of first-round actions are termination and subsequently 99 percent of partnerships in the last six cycles end in round one—round-one dissolution is most common in the *S-105–135* treatments (with 7–8 percent round-one termination) and *A-Last* (7.5 percent). Outside these treatments, round-one termination is rare (less than 0.5 percent across *S-75–95* and 1.2 percent in *A-Judge*).

Conditional on being in an active relationship past round one, the highest dissolution rates follow failed cooperation, and can thus be interpreted as unforgiving punishments. The incidence of termination following (C, F) varies from low rates in *S-75* and *S-85* (4.5 percent and 1.6 percent, respectively), to middling rates in *A-Last* (9.5 percent), to the

[7] The other symmetric termination treatments mostly have cooperation rates following (C, F) of 43–53 percent, with the one outlier being *S-135* with 33 percent cooperation.

more substantial in *S-95–135* and *A-Judge* (mostly in the 20–25 percent range, with two-outliers *S-115* at 14.6 percent and *S-135* at 45.8 percent). Termination is rarely used following successes last round, and termination rates are generally lower than 0.5 percent after either (C, S) or (D, S).[8] Finally, termination rates are non-negligible following a failed defection the previous round. However, in all treatments but *A-Last* the termination rate following (D, F) is a fraction of that following (C, F), and matches the idea that the participant shares more of the blame for the observed failure, given their defection.

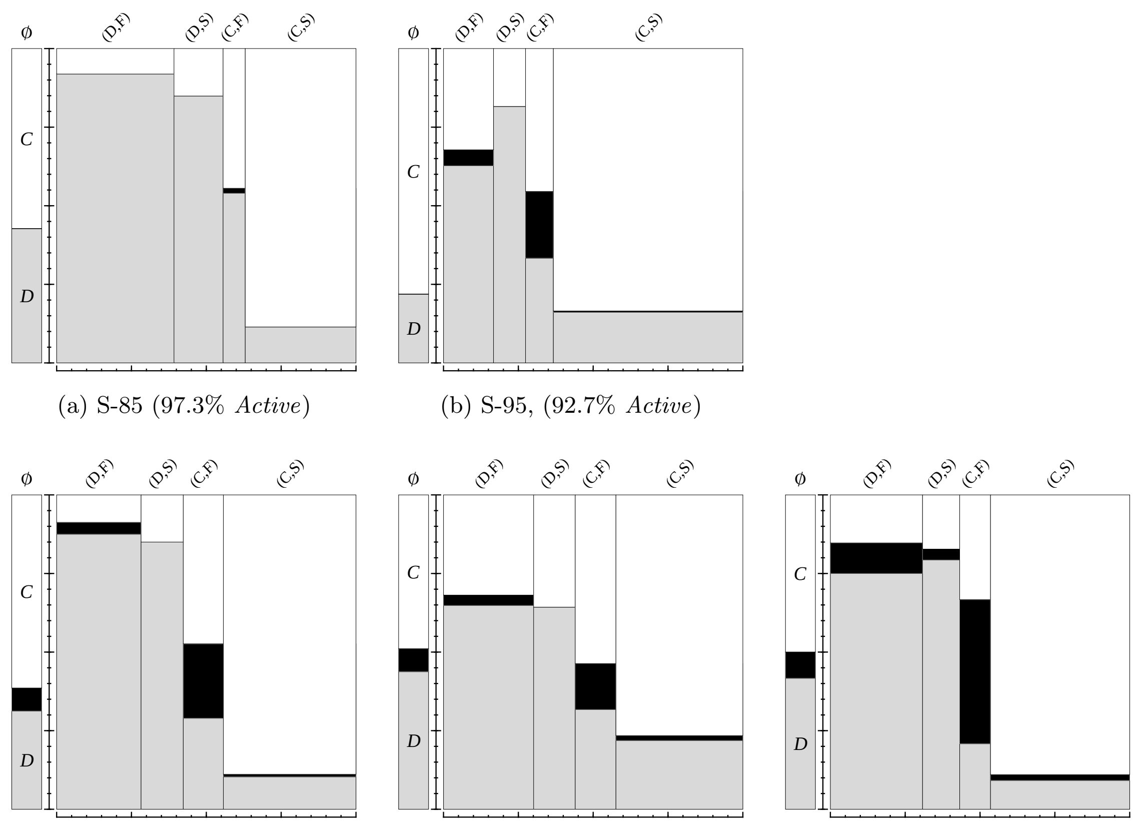
[8] The only notable exceptions are termination rates of 2.0 and 3.5 percent following the (D, S) history in *S-125* and *S-135*, respectively.

B.1 SUPPLEMENTAL FIGURES AND INSTRUCTIONS (FOR ONLINE PUBLICATION)

Table B1: Most Common Sequences

	No-T	***S-75***	***S-125***	***A-First***	***A-Last***	***A-Judge***
1	$\overline{CCCCC}$	$\overline{CCCCC}$	$\overline{CCCCC}$	$\hat{T}\hat{T}\hat{T}\hat{T}\hat{T}$	$\overline{CCCCC}$	$\overline{CCCCC}$
	(0.092)	(0.267)	(0.185)	(0.701)	(0.130)	(0.390)
2	$\underline{DDDDD}$	$\underline{DDDDD}$	$\hat{T}\hat{T}\hat{T}\hat{T}\hat{T}$	$\hat{C}\hat{T}\hat{T}\hat{T}\hat{T}$	$\hat{T}\hat{T}\hat{T}\hat{T}\hat{T}$	$\overline{CCCC}\underline{C}$
	(0.082)	(0.052)	(0.050)	(0.143)	(0.047)	(0.026)
3	$\underline{CDDDD}$	$\overline{D}\underline{DDDD}$	$\hat{C}\hat{T}\hat{T}\hat{T}\hat{T}$	$\overline{CCCCC}$	$\underline{DDDDD}$	$\overline{CCC}\underline{C}\overline{C}$
	(0.035)	(0.022)	(0.028)	(0.035)	(0.029)	(0.022)
4	$\overline{D}\underline{DDDD}$	$\overline{CCCC}\underline{C}$	$\underline{C}\hat{T}\hat{T}\hat{T}\hat{T}$	$\hat{D}\hat{T}\hat{T}\hat{T}\hat{T}$	$\hat{D}\hat{T}\hat{T}\hat{T}\hat{T}$	$\overline{CC}\underline{C}\overline{CC}$
	(0.032)	(0.022)	(0.026)	(0.025)	(0.028)	(0.022)
5	$\underline{DD}\overline{D}\underline{DD}$	$\underline{DDD}\overline{D}\underline{D}$	$\underline{D}\hat{D}\hat{T}\hat{T}\hat{T}$	$\overline{C}\hat{T}\hat{T}\hat{T}\hat{T}$	$\overline{CCCC}\underline{C}$	$\overline{C}\underline{C}\hat{T}\hat{T}\hat{T}$
	(0.019)	(0.016)	(0.026)	(0.006)	(0.018)	(0.017)
N	468	739	488	847	328	441
	(0.259)	(0.379)	(0.315)	(0.910)	(0.252)	(0.477)

Note: Five most frequent sequences of the form $\{(a_i^1, y^1), \ldots, (a_i^5, y^5)\}$, where each cell indicates the action sequence $a^1a^2a^3a^4a^5$, where $\overline{a}^t$ represents y^t =*Success*, $\underline{a}^t$ represents y^t =*Failure*, and $\tilde{a}^t$ indicates the relationship state became/was *Inactive* in period t. The number N_5 indicates the total number of subject-cycles for the five most popular sequences, while figures in parentheses indicate the fraction of the total subject-cycles represented by the sequence/top-five sequences.



(a) S-85 (97.3% *Active*)

(b) S-95, (92.7% *Active*)

(c) S-105, (64.9% *Active*)

(d) S-115, (77.6% *Active*)

(e) S-135, (41.3% *Active*)

Figure B3: Conditional response in last five cycles, $a_i^t \,\middle|\, \left(a_i^{t-1}, y^{t-1}\right)$

Note: For each treatment the horizontal axis indicate the proportion of active rounds $t \geq 2$ in the last five cycles given the relevant (a_i^{t-1}, y^{t-1}) history. The vertical axis indicates the proportion of each of the three possible actions: D in gray, C in white, and the *Terminate* action in black. The bar on the left of each plot provides the proportion of actions chosen at the empty history (round 1)

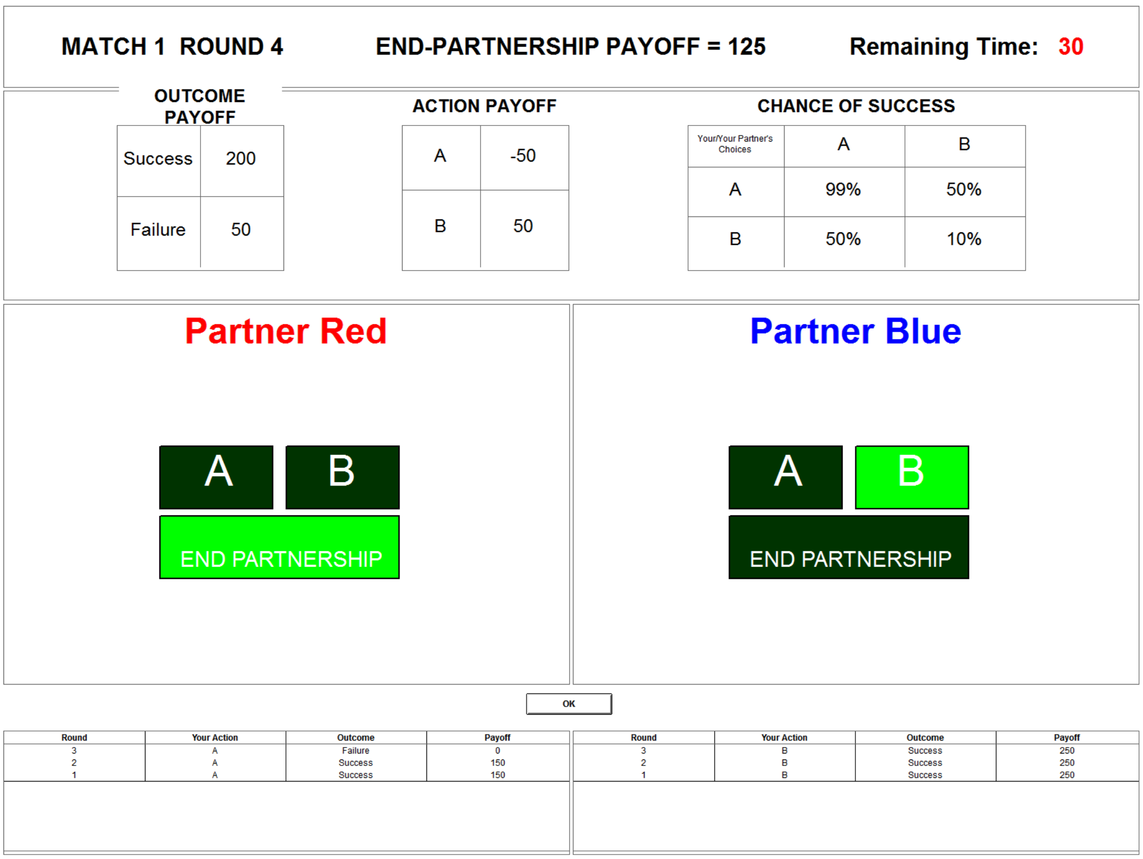


Figure B4: Screenshot of Experimental Interface

APPENDIX C

ADDITIONAL TABLES-CHAPTER 2

Table C1: Distribution of Elicited Strategies (All cycles in Phase 2, %)

No.	1^{st}	A (S, C)	A (S, D)	A (F,C)	A (F, D)	AKA	Freq.	Dev.
1	D	D	D	D	D	**AD**	**16.2**	–
2	D	D	D	D	C		1.9	10.0
3	D	D	D	C	D		0.8	25.0
4	D	D	D	C	C		2.6	7.1
5	D	D	C	D	D		1.1	66.7
6	D	D	C	D	C		1.7	55.6
7	D	D	C	C	D		2.8	33.3
8	D	D	C	C	C		1.1	16.7
9	D	C	D	D	D		0.6	66.7
10	D	C	D	D	C	DWSLS	3.0	25.0
11	D	C	D	C	D		1.1	50.0
12	D	C	D	C	C		1.9	40.0
13	D	C	C	D	D		1.1	50.0
14	D	C	C	D	C		0.8	50.0
15	D	C	C	C	D		0.8	50.0
16	D	C	C	C	C		0.9	0.0
17	C	D	D	D	D		1.5	50.0
18	C	D	D	D	C		1.1	16.7
19	C	D	D	C	D		0.9	40.0
20	C	D	D	C	C		1.7	33.3
21	C	D	C	D	D		0.4	50.0
22	C	D	C	D	C		0.9	–
23	C	D	C	C	D		3.0	–

24	C	D	C	C	C		0.9	20.0
25	C	C	D	D	D	**GRIM**	**4.9**	**7.7**
26	C	C	D	D	C	**WSLS**	**5.5**	3.4
27	C	C	D	C	D		2.3	–
28	C	C	D	C	C		3.4	5.6
29	C	C	C	D	D	MONO	**5.3**	–
30	C	C	C	D	C	**T11**	**5.5**	–
31	C	C	C	C	D		1.9	–
32	C	C	C	C	C	**AC**	**22.3**	0.8

Note: AC' (AD') denotes that a strategy will behave as AC (AD) in every history it will reach if choices are perfectly implemented. Subgame perfect strategies are denoted in bold. The letters in the strategy names denote the recommended action after each possible contingency: initial round (Firsta); SC (a_SC); SD (a_SD); FC (a_FC); FD (a_FD), where the first letter designates the outcome last round. Only strategies with positive frequencies are included.

APPENDIX D

ADDITIONAL FIGURES-CHAPTER 2

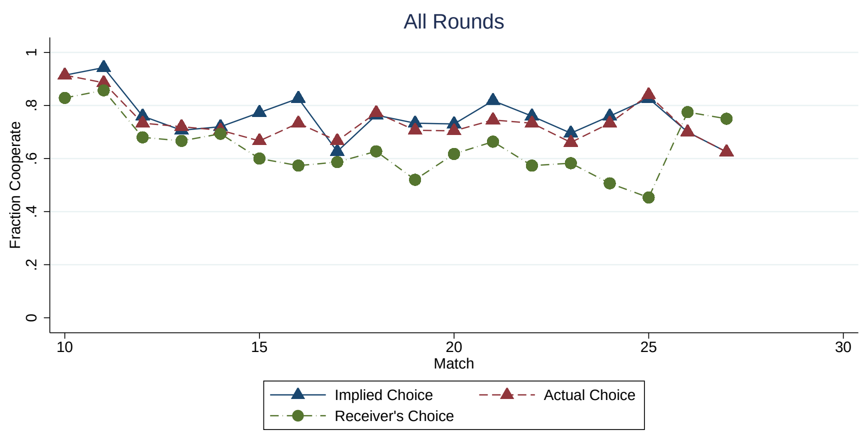


Figure D1: Aggregate Evolution of Sender and Receiver's Cooperation in Cheap-talk: All Round

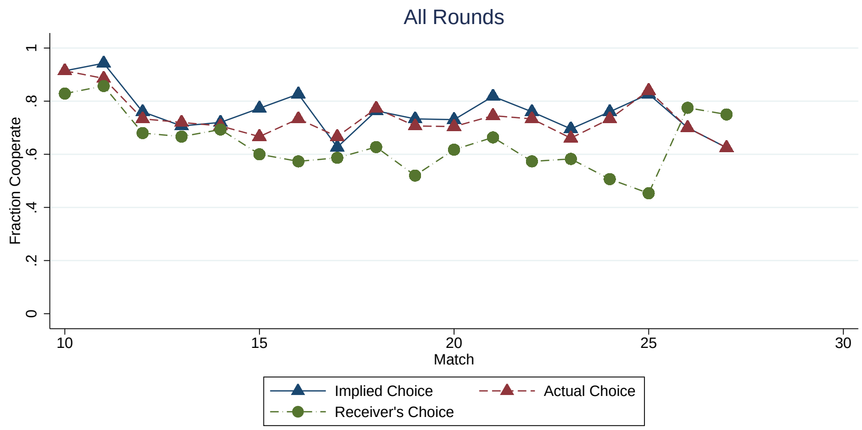


Figure D2: Aggregate Evolution of Sender and Receiver's Cooperation in Contract: All Round

APPENDIX E

STRATEGY FREQUENCY ESTIMATION-CHAPTER 2

Here we briefly describe the econometric model adopted in SFEM, discuss about the set of strategies we include in the estimation and then reports the estimated frequency of strategies for each of the treatments. To use SFEM, we assume that each subject chooses a fixed strategy for the last six cycles (the supergames). These chosen strategies are implemented with the possibility of independent mistakes, where another choice than the intended action is made. In this section, we report the results from our SFEM estimations. We first briefly describe the econometric model adopted and then report the estimated strategy weights for each of the main treatments.

Denote the choice made by subject i in round t of cycle m by c_{imt}. We specify a priori a set of K possible public perfect strategies $\Phi = \{\phi_1, \ldots, \phi_K\}$, where the choice of the strategy ϕ_k prescribes the choice $s^k_{imt} = \phi\left(y_{jm1}, ..., y_{jm(t-1)}\right)$ following the public history $\left(y_{jm1,}, \ldots, y_{jm(t-1)}\right)$. Each subject is assumed to follow a particular strategy ϕ_k, but they make independent mistakes each round with a probability $(1-\beta)$, and chose the prescribed action with probability β. Given three actions (C, D and T) a random uniform choice is represented by $\beta = 1/3$, while a perfect cycle with a strategy by $\beta = 1$. The econometric model assumes a mixture model across the available strategies, so that strategy ϕ_k is selected with probability q_k.

Define the indicator, $I_{imt}^k = 1\{c_{imt} = s_{imt}^k\}$, which assigns a value of one if the observed subject choice cycles the strategy choice σ_k cycle. The likelihood that the observed choices for subject i were generated by the strategy ϕ_k are given by

$$\Pr_i(\phi_k; \beta) := \prod_{m \in M_i} \prod_{t=1 T_{im}} \beta^{I_{imt}^k} (1-\beta)^{1-I_{imt}^k},$$

where M_i is the set of cycles, and T_{im} the set of active rounds. Combining across all subjects in a treatment we obtain the following likelihood function:

$$\sum_{i \in \mathcal{I}} \ln \left(\sum_{\phi_k \in \Phi} q_k \Pr_i (\phi_k; \beta) \right)$$

for the specified set of strategies Φ and summing the log-likelihoods across all subjects $\mathcal{I}$ in the treatment.

In total we allow for 13 different receiver strategies, motivated by theory and the previous experimental literature.

APPENDIX F

ADDITIONAL TABLES-CHAPTER 3

Table F1: Sample Characteristics

Variable	Full Sample (N=575)		Auction (N=313)		Negotiation (N=262)		p-value
Panel A. Firm Size	Mean	Median	Mean	Median	Mean	Median	
Target ($ bil)	2.96	0.21	3.69	0.23	2.09	0.21	0.576
Bidder ($ bil)	21.82	2.46	21.12	2.56	22.67	2.37	0.818
Relative Size	33%	16%	31%	13%	36%	20%	0.818
Panel B. Deal Characteristics							
Cash	36%		40%		32%		0.040
Tender	20%		13%		27%		0.000
Bidder	53%		43%		64%		0.000
Panel C. Firm Characteristics							
Target M.V. ($ bil)	1.35		1.11		1.63		0.258
Bidder M.V. ($bil)	22.04		18.48		26.27		0.084
Return Std.	0.04		0.04		0.04		0.731
R&D Intensity	0.15		0.13		0.16		0.411
Intangible Assets	0.11		0.11		0.12		0.964
Debt Ratio	0.50		0.52		0.48		0.043
ROE	0.05		0.01		0.10		0.038
Liquidity	-0.20		-0.21		-0.20		0.415
Book Leverage	0.49		0.51		0.46		0.022
Market-to-Book	2.82		2.74		2.91		0.536
Capital Expenditure	0.05		0.05		0.05		0.308
Uniqueness	0.39		0.41		0.36		0.113
Operating Cash Flow	0.03		0.03		0.04		0.215
Tobin's Q	2.01		1.88		2.15		0.088
Sales Growth	0.22		0.17		0.27		0.020

ROA	0.01	0.00	0.02	0.130
Beta	0.87	0.87	0.87	0.954
KZ Index	0.13	0.20	0.04	0.490
WW Index	-0.25	-0.25	-0.25	0.417
SA Index	-3.09	-3.10	-3.08	0.746
Panel D: Industry and Year Characteristics				
Liquidity Shock Index	0.06	0.05	0.06	0.039
Economics Shock Index	0.35	0.30	0.41	0.000
Econ Shock 1:Profitability	0.10	0.10	0.10	0.925
Econ Shock 2: ROA	0.05	0.05	0.06	0.042
Econ Shock 3: Employee Growth	0.10	0.10	0.10	0.906
Econ Shock 4: Sales Growth	0.35	0.30	0.41	0.000
Econ Shock 5: R&D	0.01	0.01	0.01	0.241
Econ Shock 6: Capital Expenditure	0.01	0.01	0.02	0.000
Econ Shock 7: Asset Turnover	0.11	0.10	0.12	0.005
Herfindahl index	0.02	0.02	0.02	0.752
Deal Volume	44.31	40.83	48.47	0.000
Panel E: Wealth Measures				
TCAR	0.262	0.259	0.265	0.808
BCAR	-0.020	-0.019	-0.021	0.729
T$CAR	105.01	154.76	259.77	0.013
B$CAR	-206.32	-74.79	-362.05	0.007
S$CAR	-20.47	62.52	-118.74	0.073
SCAR	0.018	0.018	0.018	0.966
DELTACAR	0.044	0.043	0.045	0.713

Table F2: Logistic Model Estimations of Sales Method Choice

Variables	(1)	(2)	(3)	(4)	(5)	
Intercept	0.435	0.306	-0.118	-0.143	0.064	
	(0.308)	(0.356)	(0.247)	(0.247)	(0.257)	
Target Size	-0.048	-0.063				
	(0.039)	(0.047)				
Leverage	0.458**	0.532**	0.410*	0.412*	0.341	0.135
	(0.213)	(0.242)	(0.220)	(0.220)	(0.222)	(0.088)
ROA	-0.429	-0.437	-0.442	-0.365	-0.373	-0.148
	(0.481)	(0.604)	(0.604)	(0.607)	(0.619)	(0.245)
ROE	-0.267*	-0.304**	-0.301**	-0.303**	-0.279*	-0.111*
	(0.142)	(0.151)	(0.151)	(0.151)	(0.153)	(0.061)
Tobin's Q	-0.021	-0.034	-0.026	-0.027	-0.025	-0.010
	(0.031)	(0.038)	(0.038)	(0.038)	(0.038)	(0.015)
Return Std.	-4.705	-4.813	-3.259	-3.855	-3.073	-1.218
	(3.499)	(3.866)	(3.468)	(3.509)	(3.545)	(1.405)
R&D Intensity	-0.273*	-0.426**	-0.399**	-0.389**	-0.383**	-0.152**
	(0.161)	(0.172)	(0.172)	(0.173)	(0.173)	(0.069)
Beta		0.158	0.126	0.141	0.104	0.041
		(0.114)	(0.105)	(0.106)	(0.107)	(0.043)
Liquidity		0.014	-0.133	-0.077	-0.207	-0.082
		(0.377)	(0.366)	(0.366)	(0.372)	(0.147)
Market Share Growth		-0.497***	-0.503***	-0.467***	-0.491***	-0.195***
		(0.139)	(0.138)	(0.140)	(0.141)	(0.056)
Uniqueness		0.248	0.239	0.243	0.220	0.087
		(0.180)	(0.179)	(0.180)	(0.179)	(0.071)

Change in ROA		1.278*	1.274*	1.313*	1.301*	0.516*
		(0.720)	(0.719)	(0.715)	(0.720)	(0.285)
Change in Sales Growth		-0.050	-0.036	-0.034	-0.048	-0.019
		(0.104)	(0.103)	(0.103)	(0.103)	(0.041)
Change in ROA Negative		0.235*	0.230*	0.237*	0.222*	0.088*
		(0.134)	(0.134)	(0.134)	(0.135)	(0.054)
Change in Sales Growth		-0.105	-0.104	-0.113	-0.121	-0.048
Positive		(0.124)	(0.124)	(0.125)	(0.125)	(0.050)
Change in ROA ×		0.817	0.781	0.728	0.696	0.276
Change Negative		(1.014)	(1.007)	(1.008)	(1.012)	(0.401)
Change in Sales Growth		0.411**	0.387*	0.388*	0.459**	0.182*
× Change Positive		(0.209)	(0.208)	(0.209)	(0.211)	(0.084)
High SA Index			0.158			
			(0.126)			
High WW Index				0.220*	0.202*	0.081*
				(0.127)	(0.128)	(0.050)
Economics Shock Index					-0.503***	-0.199***
					(0.155)	(0.062)
Herfindahl Index					-0.427	-0.169
					(2.200)	(0.872)
N	575	575	575	575	575	
Pseudo R^2	0.054	0.054	0.055	0.055	0.069	
Model p-value	0.000	0.000	0.000	0.000	0.000	

Table F3: Tobit Estimation of Bidder Competition

Variables	(1)	(2)	(3)	(4)
Intercept	3.848	4.004	8.682	9.676
	(5.988)	(5.977)	(6.078)	(6.422)
Financial Characteristics				
Target Size	-1.133	-0.840	-1.127	-1.132
	(0.817)	(0.732)	(0.733)	(0.734)
Leverage	3.392			
	(4.153)			
ROA	-14.004	-16.984*	-16.081*	-16.824*
	(10.336)	(9.685)	(9.729)	(9.862)
ROE	-2.046	-1.811	-1.267	-1.369
	(2.426)	(2.417)	(2.412)	(2.425)
Sales Growth	-3.718	-3.349	-0.834	-1.746
	(5.749)	(5.723)	(5.785)	(6.104)
Uncertainty				
Return Std.	-55.804	-50.874	-45.251	-44.301
	(65.230)	(64.862)	(64.465)	(64.554)
R&D Intensity	-5.558*	-5.746*	-5.496*	-5.556*
	(2.981)	(2.973)	(2.950)	(2.955)
Operating Performance				
Beta	0.170	-0.269	-0.576	-0.647
	(1.868)	(1.787)	(1.780)	(1.788)
Liquidity	1.564	0.219	-1.407	-1.466
	(6.273)	(6.050)	(6.063)	(6.068)

Market Share Growth	-6.818	-7.099	-9.217*	-9.017*
	(4.821)	(4.813)	(4.869)	(4.895)
Uniqueness	5.019*	4.581	4.499	4.447
	(2.981)	(2.930)	(2.907)	(2.910)
Change in ROA	8.390	8.870	8.173	8.863
	(12.001)	(11.973)	(11.919)	(12.021)
Change in	-2.336	-2.145	-2.472	-2.452
Sales Growth	(1.703)	(1.685)	(1.671)	(1.675)
Change in	1.742	1.766	1.328	1.457
ROA Negative	(2.279)	(2.277)	(2.268)	(2.286)
Change in	-1.301	-1.359	-1.562	-1.808
Sales Growth Positive	(2.119)	(2.116)	(2.105)	(2.167)
Change in ROA	36.660**	38.297**	36.653**	36.696**
×Change Negative	(16.865)	(16.728)	(16.591)	(16.591)
Change in Sales Growth	10.835***	10.507***	11.424***	11.679***
× Change Positive	(3.757)	(3.723)	(3.730)	(3.775)
Financial Constraints				
High KZ Index	3.376*	3.650*	3.421*	3.501*
	(1.936)	(1.905)	(1.894)	(1.903)
Industry Characteristics				
Economics Shock Index			-9.781***	-9.639***
			(2.829)	(2.846)
Herfindahl Index			0.667	-0.184
			(36.719)	(36.806)
Potential Competition				-2.714
				(5.639)
sigma	19.656***	19.644***	19.473***	19.484***
	(0.830)	(0.829)	(0.820)	(0.821)
N	575	575	575	575
Pseudo R2	0.013	0.013	0.017	0.017
Model p-value	0.000	0.000	0.000	0.000

Table F4: Logistic Estimations of Deal Initiation

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-1.109***	-1.159***	-1.182***	-1.377***	-1.270***		-0.637
	(0.428)	(0.396)	(0.397)	(0.386)	(0.341)		(0.439)
Financial Characteristics							
Leverage	1.108***	1.197***	1.139***	1.232***	1.180***	1.221***	0.975***
	(0.368)	(0.360)	(0.362)	(0.359)	(0.338)	(0.335)	(0.349)
ROA	-0.189	0.478	0.346	0.390			
	(1.018)	(0.876)	(0.878)	(0.879)			
ROE	0.047	0.061	0.090	0.056			
	(0.230)	(0.228)	(0.229)	(0.228)			
Tobin's Q	-0.110	-0.111*	-0.119*	-0.120*	-0.110*	-0.105*	-0.049
	(0.068)	(0.065)	(0.066)	(0.066)	(0.063)	(0.060)	(0.064)
Uncertainty							
Return Std.	7.549	7.570	9.321	8.192	7.081	6.867	8.271
	(5.769)	(5.748)	(5.823)	(5.783)	(5.490)	(5.418)	(5.468)
R&D Intensity	0.229	0.287	0.291	0.287	0.202		
	(0.288)	(0.283)	(0.281)	(0.281)	(0.241)		
Operating Performance							
Beta	-0.148	-0.129	-0.151	-0.113	-0.120		
	(0.175)	(0.173)	(0.174)	(0.173)	(0.172)		
Liquidity	0.673	0.755	0.675	0.800	0.793	0.809	0.531
	(0.600)	(0.595)	(0.597)	(0.591)	(0.587)	(0.585)	(0.597)
Market Share	-0.379	-0.315	-1.189**	-1.033**	-1.031**	-1.043**	-1.074**
Growth	(0.234)	(0.221)	(0.502)	(0.489)	(0.488)	(0.487)	(0.492)

Uniqueness	0.378	0.439	0.436	0.450	0.371	0.438*	0.589**
	(0.292)	(0.279)	(0.280)	(0.279)	(0.247)	(0.233)	(0.245)
Sales Growth			1.155**	0.974*	0.984*	0.949*	0.980*
			(0.574)	(0.560)	(0.558)	(0.556)	(0.559)
Change in ROA	0.574						
	(1.224)						
Change in Sales	-0.371**	-0.361**	-0.322*	-0.308*	-0.302*	-0.324*	-0.370**
Growth	(0.179)	(0.176)	(0.176)	(0.175)	(0.174)	(0.173)	(0.179)
Change in ROA	0.219						
Negative	(0.226)						
Change in Sales	-0.311	-0.297	-0.309	-0.305	-0.293	-0.284	-0.260
Growth Positive	(0.213)	(0.209)	(0.210)	(0.209)	(0.208)	(0.207)	(0.208)
Change in ROA *	1.558						
Change Negative	(1.721)						
Change in Sales Growth	1.169***	1.146***	0.966***	0.922**	0.883**	0.941***	0.968***
× Change Positive	(0.358)	(0.357)	(0.366)	(0.365)	(0.358)	(0.355)	(0.359)
Financial Constraints							
High WW Index	0.336	0.347	0.351*	0.364*	0.349*	0.387*	0.312
	(0.212)	(0.211)	(0.212)	(0.211)	(0.210)	(0.199)	(0.202)
Industry Characteristics							
Economics Shock Index	-0.470*	-0.477*	-0.560**				
	(0.278)	(0.276)	(0.279)				
Industry Tobin's Q							-0.561**
							(0.236)
N	575	575	575	575	575	575	575
Pseudo R2	0.067	0.063	0.068	0.063	0.062	0.059	0.059
Model p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Table F5: Tobit Two-stage Analysis of Target Returns

	(1)		(2)	
Variables	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage
Intercept	12.154***	-0.101	9.430	-0.371
	(3.134)	(0.079)	(19.053)	(0.270)
Competition*		0.448***		0.436***
		(0.133)		(0.130)
Relative Size	0.147	-0.014**	0.392	-0.013**
	(0.512)	(0.006)	(0.525)	(0.006)
Cash	4.324**	-0.002	4.443**	0.003
	(2.082)	(0.029)	(2.130)	(0.030)
Tender	-7.554***	0.159***	-8.566***	0.154***
	(2.536)	(0.036)	(2.648)	(0.038)
Bidder Initiated	-11.569***	0.126***	-11.494***	0.118***
	(1.799)	(0.039)	(1.838)	(0.038)
Return Std.	78.505*	0.955*	93.939*	1.208*
	(45.030)	(0.532)	(50.745)	(0.617)
Sales Growth	-3.078*		-3.045	
	(1.857)		(1.905)	
Deal Volume	-0.230***		-0.250***	
Sigma	18.812***		18.151***	
	(0.786)		(0.758)	
N	575		575	
Industry FE	No	No	Yes	Yes
Pseudo R2/Adj. R2	0.028	0.070	0.041	0.093
F-statistic	5.526		10.173	
p-value	0.011		0.016	
Sargan's Chi-sq		0.058		0.015
p-value		0.811		0.904

Table F6: Robustness Checks: Two-Stage Regression Analysis with Industry Controls

	(1)		(2)		(3)		(4)	
Variable	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage	1^{st} Stage	2^{nd} Stage
Intercept	1.505***	-0.103	1.534***	-0.023	1.493***	-0.087	1.447***	-0.093
	(0.338)	(0.096)	(0.331)	(0.077)	(0.329)	(0.077)	(0.325)	(0.080)
Auction*		0.386***		0.277**		0.357***		0.373***
		(0.134)		(0.110)		(0.111)		(0.117)
Relative Size	-0.032	-0.010	-0.029	-0.011*	-0.031	-0.010*	-0.030	-0.010
	(0.053)	(0.006)	(0.053)	(0.006)	(0.053)	(0.006)	(0.053)	(0.006)
Cash	0.485**	-0.005	0.482**	0.007	0.489**	-0.003	0.494**	-0.004
	(0.219)	(0.031)	(0.220)	(0.029)	(0.220)	(0.030)	(0.220)	(0.030)
Tender	-0.982***	0.180***	-0.964***	0.153***	-0.978***	0.173***	-0.981***	0.176***
	(0.249)	(0.042)	(0.249)	(0.039)	(0.249)	(0.039)	(0.250)	(0.040)
Bidder-Initiated	-0.766***	0.088***	-0.753***	0.068**	-0.761***	0.084***	-0.764***	0.087***
	(0.182)	(0.032)	(0.182)	(0.030)	(0.182)	(0.030)	(0.182)	(0.030)
Return Std.	4.034	1.279**	3.790	1.384***	3.892	1.292**	6.007	1.053*
	(4.653)	(0.533)	(4.647)	(0.534)	(4.639)	(0.532)	(4.928)	(0.567)
Sales Growth	-0.336*		-0.340*		-0.339*		-0.314*	
	(0.179)		(0.179)		(0.180)		(0.179)	
Deal Volume	-0.025***		-0.025***		-0.023***		-0.022***	
	(0.008)		(0.006)		(0.006)		(0.006)	
Economics Shock	0.126	0.012						
Index	(0.348)	(0.036)						
In-Wave Dummy			0.238	-0.050*				
			(0.234)	(0.025)				

Herfindahl index					-1.283	0.333		
					(3.510)	(0.417)		
Liquidity Index							-1.631	0.174
							(1.234)	(0.154)
N	575		575		575		575	
Industry FE	No	No	No	No	No	No	No	No
Pseudo R2/Adj R2	0.092	0.067	0.093	0.07	0.092	0.067	0.094	0.066
F-statistic	10.856		11.608		11.531		10.4	
p-value	0.000		0.000		0.000		0.000	
Sargan's Chi-sq		0.000		0.138		0.007		0.019
p-value		0.999		0.810		0.933		0.890

Table F7: Analysis of Other Wealth Measures

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-0.064	0.039	-1227.817***	1457.545***	540.107	0.073***	0.013
	(0.137)	(0.025)	(139.270)	(412.198)	(399.737)	(0.023)	(0.024)
Auction		-0.000	-76.443**	261.152**	187.242*	0.003	0.003
		(0.007)	(36.888)	(110.843)	(107.492)	(0.006)	(0.006)
Auction*	0.278**						
	(0.122)						
Target Size	-0.005		173.660***				0.004*
	(0.009)		(14.235)				(0.002)
Bidder Size		-0.006**		-211.853***	-85.472**	-0.007***	
		(0.002)		(41.508)	(40.253)	(0.002)	
Relative Size	-0.009	-0.007**	-0.330	-131.178***	-15.895	0.006**	0.011***
	(0.009)	(0.003)	(13.741)	(46.305)	(44.905)	(0.003)	(0.002)
Cash	-0.013	0.026***	79.712*	-7.865	53.243	0.018**	-0.020***
	(0.032)	(0.008)	(42.314)	(127.430)	(123.577)	(0.007)	(0.007)
Tender	0.144***	0.009	-27.398	101.692	89.858	0.009	-0.006
	(0.041)	(0.009)	(47.469)	(142.651)	(138.339)	(0.008)	(0.008)
Bidder Initiated	0.073**	0.002	31.199	79.988	125.526	0.005	0.005
	(0.031)	(0.007)	(36.264)	(109.078)	(105.781)	(0.006)	(0.006)
Target Return Std.	1.819**	0.144	3292.788**	843.803	2072.171	0.077	-0.194
	(0.868)	(0.245)	(1372.180)	(4108.893)	(3984.681)	(0.233)	(0.232)
Bidder Return Std.	-0.234	-1.344***	2530.464	-12582.179**	-9866.994*	-0.926***	0.918***
	(1.420)	(0.357)	(1994.197)	(5987.943)	(5806.927)	(0.340)	(0.337)
No. of SIC	0.047**	0.003	71.702**	-45.164	-7.443	0.005	0.000
Target	(0.020)	(0.006)	(31.061)	(93.512)	(90.685)	(0.005)	(0.005)

No. of SIC	0.011	-0.003	96.028***	-1.238	96.523	0.004	0.006
Bidder	(0.020)	(0.006)	(31.763)	(95.607)	(92.717)	(0.005)	(0.005)
Relative Market Share	0.006	0.002	-18.343	28.575	16.535	0.009***	0.004
	(0.011)	(0.003)	(18.207)	(54.705)	(53.051)	(0.003)	(0.003)
Hostile	-0.044	0.007	26.214	202.620	342.705	0.020	0.008
	(0.128)	(0.037)	(205.740)	(617.758)	(599.083)	(0.035)	(0.035)
Target Operating	-0.073	0.032	-142.514	417.942	350.199	0.038	-0.033
Cash Flow	(0.086)	(0.025)	(138.205)	(415.213)	(402.661)	(0.024)	(0.023)
Bidder Operating	0.309**	-0.014	581.317***	-1739.453***	-1182.642**	0.016	0.040
Cash Flow	(0.144)	(0.034)	(192.492)	(578.492)	(561.004)	(0.033)	(0.033)
Relative Tobin's Q	-0.027**	-0.007**	17.168	-23.255	-0.840	0.006*	0.010***
	(0.011)	(0.003)	(18.152)	(54.529)	(52.880)	(0.003)	(0.003)
N	547	546	547	546	546	546	546
adj. R-sq	0.08	0.11	0.33	0.06	0.00	0.10	0.20

Table F8: Two-Stage Regression Analysis in Validation Sample

Variables	1^{st} Stage	2^{nd} Stage
Intercept	-0.070	0.099***
	(0.298)	(0.031)
Auction*		0.291**
		(0.119)
Relative Size	-0.015	-0.017***
	(0.049)	(0.005)
Cash	-0.115	0.070***
	(0.206)	(0.020)
Tender	-0.442*	0.092***
	(0.247)	(0.026)
Bidder Initiated	-0.712**	0.039
	(0.335)	(0.031)
Return Std.	2.194***	-0.043
	(0.270)	(0.066)
Sales Growth	-0.234*	
	(0.134)	
Deal Volume	-0.027***	
	(0.006)	
N	792	
Industry FE	No	No
Pseudo R2/Adj. R2	0.034	0.012
F-statistic	11.23	
p-value	0.002	
Sargan's Chi-sq		0.217
p-value		0.641

APPENDIX G

VARIABLE DEFINITIONS

Table G1: Variable Definitions

Variable	Definition	Source	Calculation
Sales Process			
Auction	The number of bidders contacted >1	SEC	
Negotiation	The number of bidders contacted =1	SEC	
Target	Target CEO approaches Bidder CEO first	SEC	
Bidder	Bidder CEO approaches Target CEO first	SEC	
Both	No clear indication who approaches first	SEC	
Contact	The number of bidders contacted by the target financial advisor	SEC	
Confidential	The number of bidders that signed the confidentiality agreement	SEC	
Interest	The number of bidders that submitted preliminary indications of interest	SEC	
Length	Days between the deal initiation and date of deal announcement	SEC	
Deal Characteristics			
Cash	dummy=1 if it is a pure cash deal	SEC	
Tender	dummy=1 if it is a tender offer deal	SEC	
Stock	dummy=1 if it is a pure stock deal	SEC	
Related	dummy=1 if the two-digit SIC codes of the target and bidder are the same	SEC	
Bidder-initiated	dummy=1 if it is a bidder-initiated deal	SEC	

Target Characteristics			
Return Std.	the standard deviation of target stock returns in the period -317 to -64 days prior to the deal announcement	Eventus	
R&D Intensity	R&D expenses divided by sales	COMPUSTAT	xrd/sale
ROE	ebit divided by common equity	COMPUSTAT	ebit/at
Liquidity	total net liquid assets divided by book value of assets	COMPUSTAT	(ch+msa-lct)/at
PE Ratio	common equity divided by net income	COMPUSTAT	csho*prcc_f/ni
Leverage	book value of debt divided by the book value of assets		
Market-to-Book	market value of equity divided by book value of equity	COMPUSTAT	csho*prcc_f/ceq
Capital Expenditure	capital expenditure divided by book value of assets	COMPUSTAT	capx/at
SG & A	selling and general administrative expenses divided by sales	COMPUSTAT	xsga/sale
Tobin's Q	market value of assets divided by the book value of assets.	COMPUSTAT	(lt+pstkl-txditc+csho*prcc_f)/at *
Sales Growth	percent growth of total sales over the past year	COMPUSTAT	[salet-salet-1]/salet-1
ROA 1	ebit divided by book value of total assets.	COMPUSTAT	ebit/at
Beta	computed using target returns and market model and daily returns between(-316, -64) prior to the deal announcement	Eventus	

Market Share Growth	the percent growth in the market share of the target firm. For a given year, the market share of a company is the ratio of its annual salesto the total sales of the firms in its industry. Industries are defined using FF48	COMPUSTAT
Change in ROA/Sales Growth	change in ROA/Sales Growth over the past year prior to the deal announcement	
Change in ROA/Sales Growth Negative/Positive	dummy=1 if Change in ROA/Sales Growth is negative/positive	
Change in ROA /Sales Growth× Change Negative/Positive	interactions between Change in ROA /Sales Growth and their associated dummies	
KZ Index	KZ-index=-1.001909× (Cash flow) +3.139193× (Leverage)-39.36780× (Dividend) -1.314759× (Cash holdings).Calculations follow Baker et.al. (2002). Note that flow and leverage variablesare calculated differently from that of the WW index.	COMPUSTAT
WW Index	WW-index=-0.091*(Cash flow) -0.062× (Dividend payer dummy) + 0.021× (Leverage) -0.044×log (Book value of Assets)+0.102×(Industry sales growth)-0.035×(Firm sales growth). Calculations follow Whited and Wu (2006)	COMPUSTAT

SA Index	SA-index=-0.737× (Size) +0.043× (Size^2)-0.04× (Age), where size is the natural log of the inflation adjusted (to 2004 USD)book value of assets and age is the number of years the firm has been on Compustat with a non-missing stock price. We winsorize size at 4.5 billion USD and age at 37. Calculations follow Hadlock and Pierce (2010).	COMPUSTAT
High SA Index	dummy=1 if SA Index is higher than the industry median	
High WW Index	dummy=1 if WW Index is higher than the industry median	
High KZ Index	dummy=1 if KZ Index is higher than the industry median	
Industry and Year Characteristics		
In-wave Dummy	dummy=1 when the year of deal announcement and the target industry (defined by FF48) is during the identified 24-month industry merger waves	COMPUSTAT/SDC
Liquidity Shock Index	the number of M&A transactions in the target's industry (defined by FF48 industrial classifications) divided bythe total book value of assets over the year prior to the transaction. Calculations follow Schlingemann et.al. (2002)	COMPUSTAT/SDC

Economics Shock Index	following Harford (2005), we estimate, for each industry (defined by FF48),the median absolute change in the Profitability, Asset Turnover, R&D, Capital Expenditures, Employee Growth, ROA and Sales Growth figures. The economic shock index is the first principal component of these seven variables. The calculation of the variables is explained below.	COMPUSTAT	
Econ Shock 1:Profitability	the median absolute change in net income./sales for target industry	COMPUSTAT	ni/sale
Econ Shock 2: ROA	the median absolute change in ROA for target industry	COMPUSTAT	ebit/at
Econ Shock 3: Employee Growth	the median absolute change in employee growth for target industry	COMPUSTAT	[empt-empt-1]/empt-1
Econ Shock 4: Sales Growth	the median absolute change in sales growth for target industry	COMPUSTAT	[salet-salet-1]/salet-1
Econ Shock 5: R&D	the median absolute change in R&D for target industry	COMPUSTAT	
Econ Shock 6: Capital Expenditure	the median absolute change in capital expenditure for target industry	COMPUSTAT	capx/at
Econ Shock 7: Asset Turnover	the median absolute change in sale/at for target industry	COMPUSTAT	sale/at
Herfindahl index	the sum of squares of market share of each firm in the target industry	COMPUSTAT	
Deal Volume	the number of deals per year	SDC	

APPENDIX H

METHODOLOGICAL ISSUES

H.1 TWO-STAGE REGRESSION ANALYSES

To motivate our two-stage model, consider the abstraction of the ordinary least squares (OLS) regression model[1]:

$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + ... + \beta_K x_K + \mu,$

where $E(\mu) = 0$, $Cov(xj, \mu) = 0$,$j = 1, 2, ..., K - 1$, and y is the target announcement return, CAR, x_1 to x_{K-1} are the exogenous control variables, x_K is the auction indicator variable (Auction) and μ is the residual. Auction is the endogenous explanatory variable that is correlated with μ. To conduct the two-stage least square analysis, we need instrument(s) that satisfy the relevance and exclusion conditions. Following Roberts and Whited (2012), with one instrument, Z_1, we need:

1a) Instrument relevance in the reduced form equation: $x_K = \delta_0 + \delta_1 x_1 + \delta_2 x_2 + ... + \delta_{K-1} x_{K-1} + \theta_1 Z_1 + r_K, \theta_1 \neq 0$.

2a) Instrument exclusion: $Cov(Z_1, \mu) = 0$

[1] Here we provide a summary of econometrics of instrumental variables from Wooldridge (2010) and Roberts and Whited (2012).

Note that the instrument relevance condition means that Z_1 is partially correlated with μ once the other exogenous variables have been netted out. We should also point out that while instrument relevance is testable with a simple t-test of θ_1 in the reduced form equation, instrument exclusion is not testable because it involves the unobservable μ. However, having more than one instrument enables us to test whether the additional instruments are valid in the sense that they are uncorrelated with μ.

Suppose we have two instruments Z_1 and Z_2 for x_K. We can now restate the above two conditions:

1b) Instrument relevance: in the reduced form equation: $x_K = \delta_0 + \delta_1 x_1 + \delta_2 x_2 + ... + \delta_{K-1} x_{K-1} + \theta_1 Z_1 + \theta_2 Z_2 + r_K, \theta_1 \neq 0.$, $\theta_1 \neq 0 \& \theta_2 \neq 0$.

2b) Instrument exclusion: $Cov(Z_1, \mu) = 0 \& Cov(Z_2, \mu) = 0$.

In this case instrument relevance can be tested directly using a joint F-test on θ_1 and θ_2. Instrument exclusion can be tested using Sargan's test of the over-identifying restrictions[2].

H.2 OUT-OF-SAMPLE ESTIMATION

The purpose of the out-of-sample estimation is to assess how the results of a statistical analysis will generalize to an independent data set. Out-of-sample validation addresses the questions of how models predict data, which is a different question from how models describe the data. It starts with splitting the sample into an estimation sample, where models are constructed to estimate the outcomes and a (usually smaller) validation sample, where estimated models are used to predict the outcome. A comparison between actual outcomes and predicted outcomes yields the forecasting power of estimated models.

[2]We recognize the shortcomings of using a test for over-identification as explained in Roberts and Whited (2012). For example, the test implicitly assumes that at least one of the instruments is valid, but does not specify which one and there may be a lack of power resulting from model misspecification.

The inputs to the two-stage regression models are the bidder-initiated and auction dummy variables. Therefore, we start by identifying variables that are similar between the estimation and validation samples[3]. While we do not tabulate this, we find that deal characteristics do not differ between two samples, but some target characteristics, such as Leverage, Liquidity, Change in ROA, Economics Shock Index do. However, because some variables, such as Leverage are important variables for our estimation of the sales method, we relax our requirement that explanatory variables should not significantly differ between two samples[4]. Specifically, while we allow variables to differ significantly, we restrict the effect of the difference on the sales method/deal initiation likelihoods to be smaller than 1%[5]. Such effect is calculated as the marginal effect of the variable in the logit model of sales method/deal initiation times the difference of mean of that variable between two samples. In summary, we look for a model that explains sales method/deal initiation consistently with models in Section 5 and minimizes the bias between the two samples.

We select ROE, R&D Intensity, Market Share Growth, SG&A, Beta, High WW Index and KZ index based on significant differences in their means. Another consideration is that variables need to deliver similar qualitative predictions to those in Section 5. Therefore, we also include Leverage, Tobin's Q, Change in Sales Growth, its associated dummy and the interaction variable between the two. While the medians are significantly different between the two samples for these variables, the effects of the differences of these variables on the sales method/deal initiation likelihood are below 1%. After estimating sales method and

[3]We note that the validity of our approach depends on the assumption that the relations described by the models on deal initiation and sales method in the estimation sample are the same in our validation sample. A potential bias would also occur if there are significant differences in the explanatory variables used to samples to estimate and predict.

[4]We believe these differences do not question the randomness of our sample, but is caused by our small estimation sample (n=575). In our subsequent analysis we show that these differences do not affect our main result on the auction's wealth effects

[5]To measure the effects of difference on the likelihoods, we first estimate the marginal effect of a variable in the logistic models. The effect of the difference is then calculated as the difference of the variable between the two samples times the marginal effect of that variable. Even with our variable selection, any remaining differences can bias the quantitative predictions, such as derived predicted probabilities in the validation sample. The larger the estimation sample, the smaller this bias will be.

deal initiation party, we generate predicted probabilities associated with sales method and deal initiation and determine the optimal cutoffs. The optimal cutoff allocation rule uses standard statistical measures of the performance of a binary classification test: sensitivity and specificity. Sensitivity is the true positive rate, defined as the number of actual auctions (bidder-initiated deals) correctly classified as negotiations (bidder-initiated deals) / the number of predicted auctions (bidder-initiated deals). Specificity is the true negative rate, defined as the number of actual negotiations (non-bidder-initiated deals) correctly classified as negotiations (non-bidder-initiated deals) / the number of predicted negotiations (non-bidder-initiated deals). Given every possible cutoff between 0 and 1, we calculate sensitivity and specificity and plot them on Figure 2 and 3. The optimal cutoff is where the absolute difference between sensitivity and specificity, i.e., the error, is minimized. The optimal cutoff for auctions and bidder initiation are 0.55 and 0.53, respectively, indicated by the vertical line in Figures 2 and 3.

Using the optimal cutoffs, we classify auctions and bidder-initiated deals in our validation sample. Finally, we predict 30.3% auction and 12.7% bidder-initiated deals. The proportion of auctions is closer to that in the estimation sample but that of bidder-initiated deals appears to be low[6]. We then estimate our base two-stage model in the validation sample using the classified sales method and deal initiation party.

[6]Such a bias comes from the intrinsic small sample bias of the estimation sample, rather than from the methodology

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