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ESSAYS IN EXPERIMENTAL ECONOMICS ON CONTRACT DESIGN

For the degree of Doctor of Philosophy

Is approved by the final examining committee:

Timothy N. Cason
Co-chair

Steven Y. Wu
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John M. Barron

Brian Roberson

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Approved by Major Professor(s): Timothy N. Cason, Steven Y. Wu

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ESSAYS IN EXPERIMENTAL ECONOMICS
ON CONTRACT DESIGN

A Dissertation

Submitted to the Faculty

of

Purdue University

by

Jacob A. Brindley

In Partial Fulfillment of the

Requirements for the Degree

of

Doctor of Philosophy

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ABSTRACT

Brindley, Jacob A. PhD, Purdue University, August 2016. Essays in Experimental Economics on Contract Design. Major Professors: Timothy N. Cason and Steven Y. Wu.

This dissertation contains three related essays which examine contracting environments with moral hazard. I use laboratory experiments to study how across treatment variations affect contractual outcomes including the types of contracts that principals design, the overall efficiency of the contractual relationship, and the surplus distribution between the principal and agent(s).

In the first chapter, which is joint work with Steve Wu, I investigate relational contracting within a bilateral relationship. Specifically, I explore how contracting is impacted by a reduction in the agent's market power as proxied by an exogenous decrease in the agent's expected outside option. Surprisingly, principals did not lower promised payoffs to agents. Instead, contracts are restructured to shift more strategic uncertainty onto agents. Thus, agents are no worse off under successful relational contracting but are significantly worse off when there is a breakdown in the relationship and/or performance outcomes are not favorable. Additionally, agents are more willing to engage in trade despite strategically riskier contracts thereby increasing efficiency via trading volume. An implication of these findings is that standard monopsony models may overestimate efficiency losses from a reduction in volume of trade if production occurs under a contract.

In the second chapter, I explore how the relational contract is impacted by the potential for the principal and agent to dispute over the agent's performance level. Both parties publicly observe the agent's performance value during each period in the baseline treatment. In the treatment of interest, the principal privately observes

the value and must send the agent unverifiable feedback concerning the value of the performance signal. Therefore, parties may have conflicting beliefs about the agent's performance in the latter treatment only. Despite the lack of contracts that facilitate relational contracting under the private signal, no efficiency losses, as measured by contract acceptance rates and agent effort provision, occur across treatments. Furthermore, principals honor their promised performance bonuses at similar rates. As in the first chapter, however, principals increase their discretion under the private performance signal by shifting more strategic uncertainty onto the agents, but they do not lower the agents' promised payoffs. Agents only experience significant payoff reductions across treatments when the principal under reports a good performance outcome and fails to pay the promised bonus. Principals, however, tend to provide accurate performance feedback so that the agent's overall welfare is unchanged across treatments. These results imply that relational contracting may be relatively no worse off in environments where the performance measure is subjective.

In the third chapter, I study a contracting problem with two agents whose production technologies are independent but verifiable. My treatment variation compares the types of team incentives that principals design across static and dynamic contracting relationships. Most principals implement cooperative compensation schemes, which compensate agents the most when both perform well. Furthermore, a larger proportion of observed contracts support cooperation in one-shot relationships. Assuming parties are self-interested, this finding contradicts the comparative static prediction where the proportion of contracts favoring cooperation should not decline when the relationship is repeated. The across treatment difference, however, is statistically insignificant although the types of incentives designed are significantly more variable in repeated relationships. Furthermore, the agent's expected compensation is similar across treatments. I also illustrate how agents who are inequity averse to the principal's earnings, but not to their peer's earnings, can explain why contracts favor cooperation and agents earn higher rents in one-shot interactions.

1. INTRODUCTION

Contract design, or the process in which parties construct agreements, has important implications for many economic relationships. In labor markets, employers strive to provide efficient incentives for their employees. Franchisers have a similar objective for their distributors. Regulators, furthermore, oversee public utilities and services that typically have access to valuable private information (Brousseau and Glachant (2002)). Finally, many agricultural commodities are produced under production or marketing contracts. In the United States, for example, contracts have accounted for an increasing value of total agricultural production, covering 39% of the value in 2008 compared to 11% in 1969 (MacDonald and Korb (2011)).

Although contractual relationships have many applications, field studies on contract design can be challenging in terms of identification. Besides issues of obtaining accurate field data, contracts are usually highly adapted to specific contexts. Because of this, empirical findings may critically depend on particular aspects of the contracting environment. Just and Wu (2009) note that testing theoretical predictions with field data is difficult due to aspects that are hard to measure yet influence the observed contract structure such as each party's beliefs, preferences, and private information. Because of these challenges, a controlled laboratory experiment has its advantages when attempting to analyze how variations in the contracting environment affect contractual outcomes.

In this dissertation, I conduct three laboratory experiments which expand upon the current empirical literature on contract design. More specifically, I study contracting environments with asymmetric information. With few exceptions, previous contracting experiments make all relevant information public to both the principal and the agent(s).[1] In contrast, at least one party has access to private information in all of my essays. Within each chapter I focus on a different moral hazard problem. That is, principals cannot perfectly observe each agent's action and, consequently, cannot directly contract on it. I therefore do not focus on adverse selection or signaling problems, which are also frequently studied in the contracting literature.

The basic structure of the experimental design remains common throughout the chapters and aligns closely with standard models on moral hazard. At the start each experimental period, a subject, acting as the principal, contracts with at least one other subject, acting as the agent, to carry out an action, which cannot be observed or verified by the principal. The agent, therefore, can shirk on the contracted action if he desires. Principals make take-it-or-leave-it offers to their exogenously matched agents, so there is no scope for agents to bargain over the contractual terms. Agents can however decide to either participate in the contract or reject the offer and receive their outside option. When participating, the agent makes a costly yet hidden effort choice that noisily maps into the value of the agent's performance measure. This realized value determines the amount of surplus to be divided between the principal and the agent(s). Furthermore, this value is retained by the principal as earnings although the principal may pay the agent a fixed payment and/or a performance-contingent bonus. To reiterate, the agent's effort choice helps determine the group's total surplus while the principal's payment decisions determine how the surplus is divided among the contracting parties. The stage game is repeated over many periods so that subjects can learn from their past decisions.

Within each chapter, I exogenously vary some aspect of the contractual environment across treatments to study how this variation affects contractual outcomes. The most important of these outcomes is the contract's structure, or the types of incentives that principals design in an attempt to motivate the agent to choose the contracted effort level. This main outcome is also related to other important results including

[1] For experiments where the agent's action remains hidden from the principal see Irlenbusch and Sliwka (2005) and Hoppe and Schmitz (2015).

the overall efficiency of the contractual relationship and the manner in which the surplus is distributed between the principal and the agent(s).

In the first two chapters, I focus on dynamic relationships between the principal and a single agent. Along with the agent's effort choice, the value of the agent's performance measure is unverifiable. Due to this assumption, the principal is not required to uphold any payments specified in the contract offer that are contingent upon the performance value. The agent's compensation consists of two components: 1) a fixed payment independent of the agent's performance implying that it must be honored and 2) a promised performance bonus that is discretionary. Principals, therefore, determine their amount of discretion through the incentives that they design in the contract. For instance, the principal may only promise to compensate the agent via the bonus leaving her with complete discretion. Subjects, however, are matched together in principal-agent pairs for an indefinite amount of periods, and the repeated relationship may create an incentive for the principal to honor the promised bonus.

Given this context, the first chapter, joint with Steve Wu, studies the effects of increasing the principal's market power across treatments. If the market is concentrated in favor of the principal, the agent faces fewer contracting alternatives. Because of this, we increase the principal's market power by reducing the value of the agent's expected outside option by one-half in the second treatment. With increased market power, contracts are significantly restructured to shift more strategic uncertainty onto the agents. Principals increase their discretion by lowering the guaranteed, upfront payment and increasing the discretionary performance bonus. Despite this restructuring, the agent's promised expected compensation, which is calculated under the assumption that all informal contractual agreements are honored, is similar across treatments. This implies that agent earnings do not decrease under successful relational contracting but are significantly reduced when the principal reneges on the promised bonus for good performance or the agent performs poorly and, therefore, receives a relatively small bonus payment. In terms of efficiency, the agent's effort provision and the total surplus per-period are similar across treatments. Agents, however, are significantly more likely to accept contract offers when the principal's market power increases despite contracts being strategically riskier. Efficiency gains therefore occur through an increased trade volume. These findings imply that traditional market power models may overstate efficiency losses if production takes place under a relational contract.

The second chapter adds additional asymmetric information into the contractual relationship. Instead of varying the agent's outside option across treatments, I compare environments where the agent's performance value is publicly observed by both parties to one where only the principal observes the value. The latter environment creates the potential for the parties to have conflicting beliefs over the agent's performance level, which can occur in relationships where performance evaluation is subjective. When deciding the actual performance bonus to pay to the agent, the principal must also send the agent unverifiable feedback about his performance level when the signal is private. This creates the potential for the principal to manipulate the performance feedback in her favor. Despite the lack of contracts that facilitate relational contracting under the private performance signal, across treatment changes in efficiency are minor. Contract acceptance rates and effort provision are not significantly different across treatments. Furthermore, contingent upon the observed performance value, principals honor their promised bonuses at similar rates. As in the first chapter, however, principals restructure contracts which increase their discretionary latitude under the private signal. The agent's promised expected payoff, however, remains unchanged. Agents only experience significant welfare reductions under the private performance signal when the principal understates good performance outcomes, which are associated with higher rates of shirking on the good performance bonus. In general, however, principals tend to provide accurate performance feedback, which cannot be explained by the predicted truth telling incentives outlined in the model. Therefore, these results imply that relational contracting may be just as successful in environments where the agent's performance is subjective or the principal has access to private information related to the performance measure.

The third chapter differs from the previous two in several respects. First, the contracting problem involves two agents who produce independent yet verifiable outputs. The principal now must honor the performance bonus specified in the contract offer. Second, the relationship need not be dynamic as the treatment variation compares static relationships, where subjects are randomly rematched into principal-agent-agent groups at the start of each period, to repeated relationships, where groups interact for an indefinite amount of periods. Few contracting experiments contain multiple agents, especially those with performance-contingent pay. Furthermore, since principals design the team's incentives, this experiment can test whether individuals prefer to implement certain types of group compensation schemes. Across treatments, a majority of principals implement cooperative evaluation schemes which reward the agents the most when both perform well. Furthermore, a larger proportion of observed contracts support cooperation in one-shot relationships, which contradicts the comparative static prediction. That is, assuming parties are self-interested, the proportion of contracts favoring cooperation should not decline when the relationship is repeated. When analyzing the data more closely, however, the types of team incentives designed are not significantly different across treatments. The agent's expected compensation is also similar across treatments although it is predicted to decrease when the relationship is repeated. I also illustrate how agents who are inequity averse to the principal's earnings, but not to their peer's earnings, can explain why the efficient contracts favor cooperation and why agents earn higher rents in one-shot interactions. Behavioral contracting models, therefore, can be useful in understanding how agents working in teams are compensated.

These chapters highlight important findings that can be further explored in future theoretical and empirical research on contract design. First, principals may be more likely to structure contracts that give them more discretion when they have an informational advantage over the agent or relatively better contracting alternatives. Although this discretion exposes the agent to more risk and uncertainty, it does not imply that the agent's welfare will decrease. If the parties can engage in successful relational contracting, the agent's welfare and the overall efficiency of the relationship may not decline. Second, non-standard preferences are sometimes relatively better at predicting the observed contract structure and surplus distribution. Revising the standard assumption of self-interest therefore may be a worthwhile exercise in models where the equilibria are fairly easy to derive with this revision. Finally, experimental work on contract design should study more complex contracting problems such as incorporating asymmetric information or multiple agents into the environment. Such results can speak to the robustness of current theories and provide avenues for future research.

2. RELATIONAL CONTRACTING AND MARKET POWER: EXPERIMENTAL EVIDENCE

This paper investigates the impact of market power on relational contracting outcomes, with a particular focus on efficiency, distribution and endogenous contractual form. While textbook microeconomic models predict welfare losses when firms have market power, it is not clear whether such models are reliable for predicting welfare losses characterized by both market concentration and contracting. Contracts are designed to mitigate agency problems whereby principals and agents have conflicting objectives. Intuitively, it seems plausible that when the principal has more power, the principal has more latitude to structure contracts that provide high-powered incentives to align objectives. Indeed, if the lack of competition facilitates the ability of principals to manage agency conflicts, then the efficiency losses due to decreased quantity under traditional market power models might be mitigated, making it more difficult to identify aggregate welfare losses in input markets characterized by both monopsony *and* contracting. While there is a long and vast literature on monopsony power, to the best of our knowledge, there is a paucity of research that explores the impact of buyer power on the agency/contracting dimension of the problem.

We also contribute to the experimental literature on relational contracts by showing that a reduction in the outside option of the agent does not necessarily lead to a drop in expected payoffs to the agent as theory or intuition might predict. Instead, our results show that principals restructure contracts in order to shift more strategic uncertainty onto agents so that in the event that relational contracts unravel, the agents are left "holding the bag." Thus, the distributional impacts largely occur following a breakdown of a relational contract rather than through an ex ante reduction in promised pay. Moreover, we find that overall efficiency *increases* because agents are more willing to accept even strategically riskier contracts thereby increasing the volume of trade. While prior experimental papers on relational contracts have documented the impact of competition on distribution, efficiency, and relationship length, our paper is the first to show the allocation of strategic uncertainty as a function of changes in outside options.

The importance of this research lies in the fact that contractually based trading can be controversial, particularly in industries where there is a perceived imbalance of market power. For example, in U.S. agriculture, the emergence of contract farming has raised concerns that increasing concentration and consolidation of agribusinesses limits the bargaining power of farmers when negotiating contracts (Domina and Taylor (2010)). As a consequence, some growers have complained that their incomes fall below expectations based on information provided by their counter parties (Schrader and Wilson (2001)). In many developing countries, there is an emerging trend of contract farming between smallholders and exporters. While many development strategists see contract farming as providing market access opportunities for smallholders, there is also some awareness that the potential imbalance of bargaining power can erode the long term viability of smallholders (Catelo and Costales (2008)). Another sector in which contracting and market power might be relevant is the U.S. retailing industry. There is concern that large retailers such as Walmart have exerted power over suppliers and squeezed their profit margins (Bloom and Perry (2001)).

In our analysis, we incorporate two stylized observations. First, transactions typically occur via incomplete contracts that do not cover every relevant contingency. Contracts typically combine legalistic components that are enforceable by a third-party, such as a court, along with implicit components that are not third-party enforceable. Second, many contracting relationships are not just one-shot transactions but take place repeatedly over time so that there is relational contracting. We believe these assumptions are fairly general and cover a wide array of contracting situations in practice.

When some important performance measures are not third-party enforceable, Bernheim and Whinston (1998) show that it is in the interest of the principal to design a contract that provides a high degree of discretionary latitude to the principal through, say, discretionary bonuses. The discretionary latitude benefits the principal by improving post-contractual incentives that induce the agent to exert effort. Intuitively, the principal can use the discretion to reward and punish unenforceable performance factors. We, however, highlight an important countervailing effect; namely, that highly discretionary contracts can weaken pre-contractual incentives for parties to contract in the first place. Intuitively, under a highly discretionary contract, the agent faces significant strategic uncertainty because the principal can use her discretionary flexibility for opportunistic purposes. Thus, agents face significant counter-party risk which may erode the agent's willingness to accept the contract.

Consequently, the principal faces a tradeoff between the provision of informal incentives and inducing agent participation. If the principal wants to maintain high-powered informal incentives, she must raise the level of pay, which provides the agent with a counter-party risk premium. Alternatively, she can keep the same level of pay, but then must reduce discretionary latitude, which weakens informal incentives. Thus, there is tension between distribution and efficiency. In this situation, market concentration in favor of the principal can actually improve efficiency by weakening this tension. That is, with market concentration, the agent is more willing to accept highly incomplete contracts as his outside option has eroded under concentration. Thus, market power potentially lowers the cost of incentive provision.

We test the theory using using experimental economics. Our experimental design is unique in that we allow for flexible contractual forms; i.e. our subjects who are placed in the role of principals can endogenously choose a contractual form that is consistent with how principals behave in standard relational contracting models. Most previous experimental studies on relational contracts impose specific contractual forms, such as efficiency wages, which would limit our ability to examine how contractual form responds to changes in market power. Most importantly, our design allows our subjects who are placed in the role of principals to endogenously leave themselves with greater or lesser amounts of discretionary flexibility which allows us to examine not only the response of expected payoffs to a change in market power, but also the way strategic uncertainty is allocated.

Our experimental design proxies a change in the principal's market power by adjusting the agent's expected payoff from his outside option. Compared to the control treatment, the agent's expected outside option in the market power treatment drops by 50%. One way to justify our reduction in the expected outside option as a proxy for market power is that the outside option is the agent's next best offer from another principal given that he stays in the industry. The expected payoff from the outside option is affected by whether the agent is on the long or short side of the market; i.e. if the agent is on the short-side, there will be excessive demand for his services so his expected outside option will be high. If he is on the long-side of the market, there is excess supply so his expected payoff from the outside option will be low and the principal has "market power."[1]

Several examples highlight situations in which a seller's outside option can be reduced relative to the buyer. Recent increases in concentration among processors within the American livestock industry have raised concern that processors will lower the compensation paid to suppliers who have difficulty selling their commodities to a competing firm (Sexton (2013); U.S. Government Accountability Office (2009)). Similarly, potential mergers in local health insurance and grocery retail markets have led to antitrust investigations due to concerns of increased buyer power over suppliers (Organisation for Economic Co-operation and Development (2008)). Finally, when

[1]An astute reader may wonder why we did not set up our experiment to explicitly allow for multiple principals and agents and vary the ratio of principals and agents. One problem with explicitly allowing for a general equilibrium type market experimental design is that there will be endogenous matching of principals and agents. It is now well known that endogenous matching can create significant biases when studying contractual form (Ackerberg and Botticini (2002)) . A second advantage of using a reduced form, exogenous matching approach as we have, is that the design is much simpler and statistical analysis is simpler and more transparent since we need not speculate about the size of outside options faced by each agent in a given trading period. For instance, we know that the main channel through which market power affects contracting outcomes is through the agent's outside option. With our setup, we explicitly control the size of the outside option. In a market based experiment, one has to infer the size of the outside option by accounting for all possible contracts that each subject received in each period and back out the size of the outside option. In short, our design is more parsimonious and much more consistent with standard partial equilibrium models of principal-agent relationships.

labor market conditions are unfavorable, such as a decrease in the demand for labor, workers searching for jobs face fewer alternative options (Acemoglu and Newman (2002)).

Overall, our experimental results are consistent with relational contract theory in that agents are more likely to accept contracts when the participation and the principal's self-enforcement (i.e. "promise keeping") constraints are satisfied and that agents' effort provision is higher when the incentive compatibility constraint is satisfied. Therefore, the theoretical framework performs fairly well, which lends confidence in the ability of the theory to make generalizable predictions about the impact of our main treatment effect which is how an exogenous decrease in the agent's expected payoff from his outside option affects contractual outcomes.

We find that an increase in the principal's market power causes a significant restructuring of relational contracts where principals reduce the size of the fixed, guaranteed payment and make a larger fraction of pay discretionary. Thus, agents are exposed to more strategic uncertainty since shirking on the discretionary bonuses by principals has a greater negative impact. Surprisingly, principals do not reduce agents' *expected promised* payoffs in response to a change in market power. The combination of same expected payoffs and riskier contracts implies that agents' *actual* payoffs are unaffected by market power when *all is well* in that the high output is realized and the principals pay the promised bonuses. However, if there is a deviation by the principal or output is low, then the agent suffers significant reductions in pay. In short, when agents lose market power, they are forced to bear significantly more counter-party risk even if contractual terms, as measured by expected pay, seem unaffected ex ante. We also find that total surplus per-trade is unaffected while principals' actual pay increases with market power. Finally, despite being offered riskier contracts, agents are more likely to accept contracts when the principals' market power increases and thus, overall efficiency increases since there is a greater volume of trade.

2.1 Related Literature

Our theoretical model is largely based on the moral hazard model of Levin (2003) though we simplified the model to facilitate experimental implementation. Like the theory, our experimental design allows for endogenous contract structure, which is important for examining how optimal contractual form responds to variations in market power.

With regard to experimental studies, Brown et al. (2004) (BFF) focus on relational contracting based on the efficiency wage theory, complemented by social preferences. Subjects are allowed to trade with fixed-price contracts and form reputations within a marketplace where buyers and sellers can endogenously match with each other. Outcomes differ depending on whether the identities of the subjects are fixed, thereby allowing subjects to form reputations, or randomly generated each period. With fixed identities, the absence of third-party enforcement results in the formation of long-term pairings that are insulated from competitive pressures with performance disciplined by the threat of firing.

While BFF did not explicitly look at competition issues, an extension of BFF, Brown et al. (2012) (BFF12) includes a treatment in which there are more principals than agents in the market. This extension still finds that high performance can be sustained through relational contracts though agents are more willing to switch trading partners. Much of BFF and BFF12s' results are consistent with the efficiency wage theories of Klein and Leffler (1981) and Shapiro and Stiglitz (1984).[2] The main difference between our work and BFF/BFF12 is that we impose exogenous matching of principal-agent pairs, indefinite repetition, and allow for endogenous contractual form so that our results can be interpreted with respect to the principal-agent theory of Levin (2003) rather than these efficiency wage theories. Moreover, our work focuses more on contract design and the allocation of strategic uncertainty within the context

[2]For a recent survey of the experimental literature on efficiency wage contracts, see Casoria and Riedl (2013).

of a standard game-theoretic principal-agent model rather than the allocation of rents in an efficiency wage model of intrinsic motivations and fairness considerations.

Fehr et al. (2007) study the effectiveness of two different types of performance pay. They show that discretionary bonus contracts can increase efficiency relative to a contract where the agent is fined when caught shirking below the principal's preferred performance level. For this reason, a large majority of principals used discretionary bonus contracts. Although our study also analyzes a contracting environment with informal bonuses, we focus on repeat transactions rather than one-shot transactions. Thus, the primary focus of Fehr et al. (2007) is on how fairness considerations interact with contractual form to sustain productive one-shot trades, whereas we rely on relational contracts as the self-enforcing mechanism. Moreover, Fehr et al. (2007) does not study market power issues.

Furthermore, Erkal et al. (2015) (EWR) is similar to this paper in that it allows for endogenous contractual form. EWR find that experimental results are consistent with a number of canonical predictions from the relational contracts literature. In addition, EWR find that subjects play "semi-grim" style strategies in response to contractual deviations, which is consistent with the findings of Breitmoser (2015) in the context of prisoner's dilemma games. The main difference between our paper and EWR is that our study focuses on the impact of market power whereas EWR test a number of standard predictions from relational contract theory. Additionally, there are differences in the model and experimental design in that EWR base their work on a symmetric information model, whereas our model incorporates moral hazard where there is a noisy relationship between performance and effort.

Aside from experimental work, there are also some empirical papers on relational contracts using observational data. Antràs and Foley (2015) use transactions level data from an exporter of frozen food products to examine how payment terms are structured with importers. They find that strategic uncertainty and counter-party risk are significant drivers of contract structure. Like their study, we also find that contracts are structured to manage strategic uncertainty, although we mainly focus on the impact of market power while they focus on variations in contract enforcement. Moreover, our study is experimental whereas they rely on observational data. Nonetheless, both studies confirm each other in terms of identifying the importance of counter-party risk in relational contracting. Gil and Zanarone (2015) provide a recent survey of some additional empirical work on relational contracts.

2.2 Theoretical Framework

2.2.1 Model

A principal and agent are infinitely lived, risk neutral, and share a common discount factor δ. If the parties wish to trade in period t, they must enter into a contract. This contract summarizes the compensation promised to the agent, which depends on the agent's performance level in period t. Additionally, the contract may specify the likelihood of trading in future periods. Denote $q \in \{q_L, q_H\}$, where $0 < q_L < q_H$, as the realized value of the agent's performance measure in period t. The value of q accrues directly to the principal, is non-contractible, and is a stochastic function of the agent's effort choice $e \in \{e_L, e_H\}$, which the principal cannot observe. The principal can offer a take-it-or-leave-it contract to the agent, which consists of three components:

- *Payments*
  - Fixed Payment, $p \in \{\underline{p}, \ldots, \overline{p}\}$: If $p < 0$, p is transfered from the agent to the principal.[3] Likewise, if $p > 0$, p is transferred from the principal to the agent. Since p is fixed and does not depend on non-verifiable contingencies, it is third-party enforceable.
  - Performance-Contingent Bonuses, $b(q) \in \{0, \ldots, \overline{b}\}$: The principal specifies state-contingent payments: $b(q_L)$ and $b(q_H)$. Because $b(q)$ depends on

[3]This can be thought of as an upfront fee paid by the agent in order to engage in trade.

q which is non-verifiable, $b(q)$ is not third-party enforceable so that it is a discretionary bonus. As such, the principal can renege on $b(q)$.

- *Desired Effort Level*
  - The principal requests an effort level, either e_L or e_H, and uses incentive compatible payments to implement the requested effort.
- *Optional Termination Condition*
  - For completeness, we allow the principal the option of including a termination condition that depends on the agent's performance.[4] This condition specifies the likelihood of the relationship ending permanently after period t. If $q = q_L$ the relationship terminates with certainty after period t. In contrast, if $q = q_H$ the relationship continues with certainty.[5] This termination condition, however, does not have to be included in the offer. If the condition is not included in the offer, the relationship does not terminate in the case that $q = q_L$, which implies that the parties can trade in the next period.

If the principal makes an offer, the agent then decides to accept or reject it. If no contract is offered or it is rejected, the principal and agent receive their outside options for that period, which are $\bar{\pi}$ and $\bar{u}$ respectively.

If accepted, the agent then privately makes his effort choice. The high effort choice, e_H, costs the agent $c > 0$ while the low effort choice, e_L, is costless. The probability of realizing q_H given e_L is denoted as l. Similarly, h is the probability of realizing q_H given e_H with $0 < l < h < 1$. Therefore, when the agent exerts high effort it is more likely that the agent performs well. Both effort choices, however, can result in the realization of q_H or q_L, which makes the performance measure a

[4] Contracts in practice often specify termination conditions.

[5] In a more general contract, the principal could specify the values of the termination probabilities for both outcomes of q in the contract offer.

noisy signal of effort. The mapping between effort and performance is also common knowledge.

After q is observed by both parties, the principal must decide the actual bonus $B \in \{0, \ldots, \bar{b}\}$ to be paid to the agent since the promised bonuses are not enforceable. Payoffs are then realized for the current period. If a contract is formed, the principal and agent's payoffs, respectively, for period t are:

$$\pi = q - p - B$$
$$u = p + B - c(e)$$

As long as the relationship has not terminated in period t, the parties are free to enter into a new contract in period $t + 1$.

2.2.2 Optimal Contracts

This section characterizes the optimal contract structure which ensures that the contract if both self-enforcing and efficient. Self-enforcement implies that both parties have the incentive to uphold any informal agreements stated in the contract. Efficiency implies that the contract maximizes the expected joint earnings of the principal and agent.

Define the principal's expected payoff per-period for each effort choice as:

$$\pi_{e_L} \equiv E_q[q - p - b(q)|e = e_L] = lq_H + (1 - l)q_L - [p + lb(q_H) + (1 - l)b(q_L)]$$
$$\pi_{e_H} \equiv E_q[q - p - b(q)|e = e_H] = hq_H + (1 - h)q_L - [p + hb(q_H) + (1 - h)b(q_L)]$$

Similarly, the agent's expected payoffs per-period are:

$$u_{e_L} \equiv E_q[p + b(q) - c(e)|e = e_L] = p + lb(q_H) + (1 - l)b(q_L)$$
$$u_{e_H} \equiv E_q[p + b(q) - c(e)|e = e_H] = p + hb(q_H) + (1 - h)b(q_L) - c$$

Finally, the expected surplus per-period for each effort choice is:

$$s_{e_L} \equiv E_q[q - c(e)|e = e_L] = lq_H + (1-l)q_L$$
$$s_{e_H} \equiv E_q[q - c(e)|e = e_H] = hq_H + (1-h)q_L - c$$

To make the contract efficient when the agent exerts high effort, e_H, assume:

$$\bar{s} \equiv \bar{u} + \bar{\pi} < s_{e_H}$$
$$s_{e_L} < s_{e_H}$$

Therefore, e_H is the efficient effort level.

Finally, we focus on stationary contracts. Levin (2003) shows that when an optimal contract exists, then an equivalent stationary contract that is optimal also exists. Stationarity assumes that the principal offers the same payments p, $b(q_L)$, and $b(q_H)$ in all periods during which trade occurs. It also assumes that the agent chooses the same effort e in all periods as long as he has not observed the principal reneging on a bonus payment $b(q)$ during any previous period.[6]

If the principal fails to pay a promised bonus, Levin (2003) assumes that the agent responds by issuing the worst possible punishment.[7] In such a case, the agent breaks off trade and the principal receives her outside option, $\bar{\pi}$, in all subsequent periods.[8]

[6]Since contracts are assumed to be stationary, it is sufficient to focus on static incentive problems when solving for the optimal contract.

[7]The concept of the worst possible punishment follows the logic of Abreu (1988).

[8]Such an outcome, however, may not be renegotiation proof since the parties would be destroying surplus. Levin (2003), however, shows that "strongly optimal" contracts exist where the parties continue with an optimal relational contract even following a deviation but with terms adjusted to hold the breaching party at his reservation payoff. Since the parties are still on the Pareto frontier, there is no scope for renegotiation. Erkal et al. (2015) provide experimental evidence regarding how subjects actually respond to relational contract deviations. Their results provide no clear evidence of either termination or continuation with relational contracts; instead, subjects play a form of "semi-grim" strategies (see Breitmoser (2015)).

Conditions for Contract to be Optimal

In order for the contract to be self-enforcing and efficient, the following constraints must be satisfied:

1. The principal's participation constraint (PC_P) makes it worthwhile to enter the contract:

$$\pi_{e_H} \geq \bar{\pi} \tag{PC_P}$$

2. The agent's participation constraint (PC_A) makes it worthwhile to enter the contract:

$$u_{e_H} \geq \bar{u} \tag{PC_A}$$

3. The agent's incentive compatibility (IC) constraint prevents the agent from shirking:

$$u_{e_H} \geq u_{e_L} \tag{IC}$$

4. The principal's promise keeping (PK) constraint ensures that she will pay the discretionary bonus for all possible realizations of q:

$$\underbrace{\frac{\delta}{(1-\delta)}(\pi_{e_H} - \bar{\pi})}_{\substack{\text{expected, discounted net gain from} \\ \text{future interactions when paying bonus}}} \geq \underbrace{\max\{b(q_L),\, b(q_H)\}}_{\substack{\text{maximum gain from} \\ \text{withholding bonus}}} \tag{PK}$$

The last constraint emphasizes the relational aspect of the contract. Although bonuses are discretionary, the principal will not find it worthwhile to renege on any discretionary payment due to the net gain she expects to receive from future trades in which the agent acts efficiently. Intuitively, the promise keeping constraint emphasizes the upper bound on feasible bonuses. If a bonus is too large, the principal cannot credibly commit to paying it.

Furthermore, the optimal termination condition is *not needed* for the principal and agent to enter into an optimal contract. Including the condition would be inefficient as it can destroy future gains from trade by terminating the relationship in the case that $q = q_L$.[9]

2.2.3 Principal's Contract Design Problem

The next issue that needs to be addressed is the subset of contracts that the principal should choose to implement from the entire set of self-enforcing contracts. Assuming that both parties are self-interested, the principal seeks to maximize her expected earnings subject to the four conditions that ensure self-enforcement. Since e_H generates that largest expected surplus, the principal chooses p, $b(q_L)$, and $b(q_H)$ such that π_{e_H} is maximized conditional on satisfying (PC_P), (PC_A), (IC), and (PK). Since our theory is not new but is rather based on a simplified version of Levin (2003)'s moral hazard model, we will discuss optimal contracts within the context of our experimental parameterizations rather than through formal propositions. We subsequently provide some testable predictions under our experimental parameters.

Table 2.1 displays the parameters used in the experiment. In the baseline treatment, outside options are $\bar{u} = \bar{\pi} = 30$. In the second treatment, $\bar{u}$ equals either 0 or 30, both occurring with a 50% probability. Thus, the agent's expected outside option has fallen to 15. We refer to the latter treatment as the "market power" (MP) treatment since the agent may expect to receive less favorable terms from the principal since his outside option has fallen.

One way to justify our reduction in the expected outside option as a proxy for MP is that in typical bargaining problems, there is a disagreement point and an outside option point. The disagreement point is usually the payoff a party earns if he fails

[9] Theoretically, the termination condition has no impact on the set of optimal contracts. Furthermore, this condition does not fundamentally affect our qualitative results.

Table 2.1.: Parameter values used in experiment

Variable	**Value**	**Description**
δ	0.8	discount factor (probability of continuation)
$\bar{\pi}$	30	principal's outside option
$\bar{u}$	30	agent's outside option in baseline treatment
	0 or 30 each with a 50% chance	agent's outside option in market power treatment
q_L	30	low performance value
q_H	120	high performance value
l	0.25	probability of q_H given e_L
h	0.75	probability of q_H given e_H
c	10	cost of e_H
p	$\{-250, \cdots, 250\}$	bounds on fixed payment
$b(q_L)$, $b(q_H)$, B	$\{0, \cdots, 250\}$	bounds on promised and actual bonuses

to reach an agreement whereas the outside option is what a party can receive from accepting the next best offer. We normalize the agent's disagreement point to zero, which might represent his payoff from selling his assets and exiting the industry. The outside option, on the other hand, is his next best offer from another principal if he stays in the industry. The outside option is affected by whether the agent is on the long or short side of the market; i.e. if the agent is on the short-side, there will be excessive demand for his services so he is always guaranteed an outside option of 30. Hence, he has "market power." If he is on the long-side of the market, there is excess supply so he is not guaranteed to receive an offer from another principal since his market position has weakened. We model this by assuming that he will receive an outside option of 30 with only 50% probability and his *expected* outside option drops to 15.[10]

In the baseline treatment, $E[s|e = e_L] = 52.5 < \bar{\pi} + \bar{u} = 60 < E[s|e = e_H] = 87.5$. With MP, this changes to $\bar{\pi} + \bar{u} = 45 < E[s|e = e_L] = 52.5 < E[s|e = e_H] = 87.5$. Therefore, the optimal outcome occurs when the parties trade and the agent chooses e_H.

Under our experimental parameters, there is a large set of contracts that simultaneously satisfy all necessary constraints for optimality while implementing the efficient effort level, e_H. Therefore, rather than specifying an optimal contract, we will discuss comparative statics of how the *set* of optimal contracts change with MP.[11]

[10]We make two additional comments. First, rather than modeling this probabilistically, it is also possible to assume that excessive demand or supply will cause the outside option to adjust above or below 30 and the agent will always receive this new level with probability 1. We chose the probabilistic approach because frictions in practice often prevent employment or supply contracts from adjusting and instead, there are frequently layoffs. Regardless, both approaches lead to a change in *expected* outside options. Second, it should be noted that the outside option principle suggests that the outside option is only relevant if it exceeds the share of the surplus the agent can obtain through, say, Nash bargaining (see Muthoo (1999) for details). For example, if the agent had equal bargaining power, the agent would receive 1/2 the surplus so the outside option is only relevant if the next best offer is larger than what the agent gets from agreeing to a contract that offers him 1/2 the surplus. However, in our model, the principal is assumed to have all of the bargaining power and makes take-it-or-leave-it offers to the agent. Hence, the agent's outside option is relevant for all surplus levels above zero.

[11]We use numerical methods to generate comparative statics results.

2.2.4 Comparative Statics

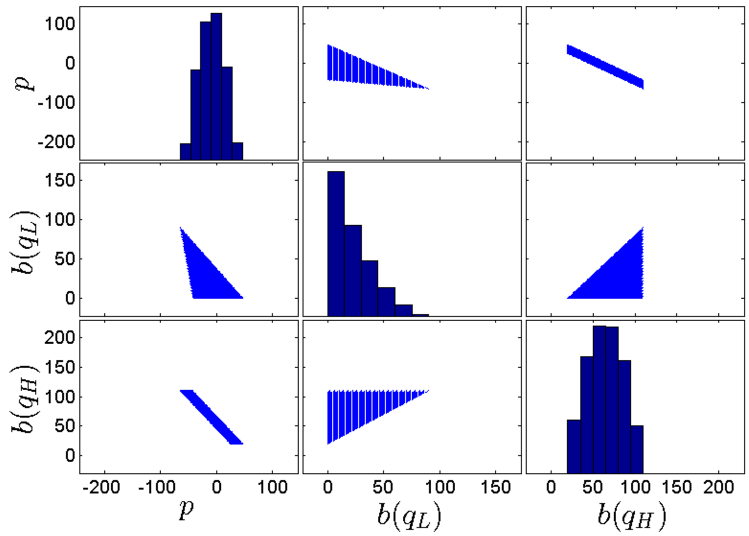
This section generates comparative statics predictions about the effects of MP on contracting outcomes.

Prediction 1: *The set of optimal contracts increases under MP, facilitating the use of relational contracts.*

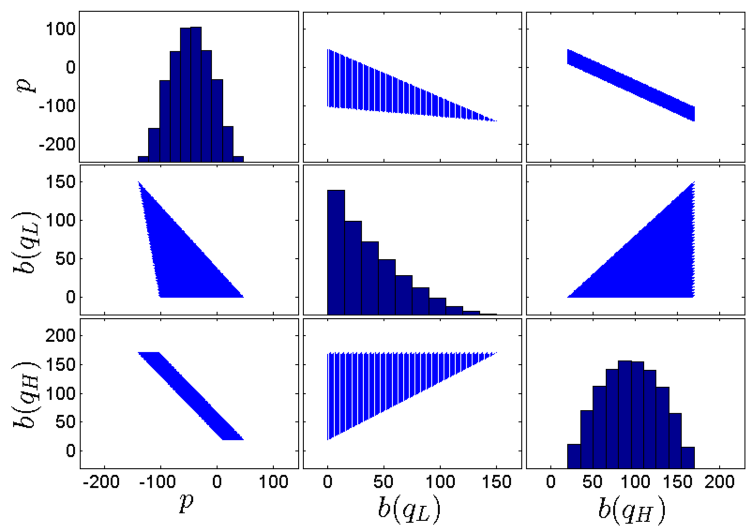
Intuitively, reducing the agent's outside option $\bar{u}$ creates slack in the agent's participation constraint (PC_A). Therefore, a larger set of contracts can now satisfy all four conditions ((PC_P), (PC_A), (IC), and (PK)) that guarantee self-enforcement. Figure 2.1 illustrates this fact by providing a graphical summary of the relationship between p, $b(q_L)$, and $b(q_H)$ for the offers that satisfy the four constraints in each treatment. Under our parameter values, 32,453 contract offers satisfy all of the constraints in the baseline treatment. In the MP treatment, 146,338 contracts satisfy all constraints, which is about 4.5 times larger. This can been seen in the figure as the bounds on feasible payments increases in the MP treatment. For instance, the minimum fixed payment, p, decreases from -65 in the baseline to -140 in the MP treatment, which creates a wider range of payments where the principal extracts more payment upfront from the agent, but credibly commits to paying the agent a large performance bonus. MP thus creates more latitude for the principal to structure contracts that shift more risk to agents.

Prediction 2: *Under MP, the principal restructures contracts to increase her own profit while reducing the agent's profit. A greater proportion of the agent's profit is also made discretionary, increasing the agent's exposure to strategic uncertainty.*

Within both treatments, a large subset of self-enforcing contracts hold the agent to her outside option $\bar{u}$. Therefore, the set of payments that maximize the principal's expected earnings make (PC_A) binding. As a result, the agent's expected profit, u_{e_H}, decreases from 30 points in the baseline treatment to 15 points in the MP treatment. Likewise, the principal's expected profit, π_{e_H}, increases by 15 points from 57.5 ($s_{e_H} - \bar{u}$) in the baseline to 72.5 in the MP treatment.



(a) Baseline treatment



(b) Market Power treatment

The top figure represents the baseline treatment while the bottom figure represents the market power treatment. The figures summarize the set of contracts for each treatment that satisfy the 4 necessary constraints: (PC_P), (PC_A), (IC), and (PK). Under the parameter values, 32,453 contract offers satisfy all constraints in the baseline treatment. In the MP treatment, 146,338 contracts satisfy all constraints. Elements on the main diagonal are histograms (with frequencies on the y-axis unreported) while off diagonal elements illustrate the relationship between p, $b(q_L)$, and $b(q_H)$.

Fig. 2.1.: Set of optimal contracts for baseline and market power treatments

As for strategic uncertainty, table 2.2 summarizes the subset of self-enforcing offers that maximize the principal's expected profit for each treatment. This applies to 1,081 combinations of p, $b(q_L)$, and $b(q_H)$ in the baseline treatment and to 2,926 combinations in the MP treatment. The average fixed payment p decreases from -27.25 points in the baseline to -77.25 points under MP. Furthermore, the performance bonus for low and high outcomes respectively rises by 20 and 40 points on average. Therefore, the principal is more likely to increase discretionary pay to agents. Figure 2.2 reinforces this fact by presenting histograms for each individual payment specified in the contract offer. Fixed payments tend to be much lower under MP. For instance, 78% of the feasible offers have $p < -50$ in the MP treatment compared to just 11% in the baseline. Demanding large upfront payments also requires the principal to pay relatively larger performance bonuses. For high performance, the set of offers with $b(q_H) > 100$ increases by 51% in the MP treatment. Therefore, the agent faces more strategic uncertainty when his outside option decreases.

Prediction 3*: Acceptance rates and effort provision should not vary across treatments.*

Given that there is a large set of feasible contracts within both treatments that satisfy all constraints, there should be no expectation that effort or acceptance rates should vary across treatments. Nonetheless, the set of optimal contracts in the MP treatment is substantially larger, making it easier for subjects to design optimal contracts in the MP treatment. Thus, although this prediction is our null hypothesis, the impact of market power on effort and acceptance rates is ultimately an empirical question.

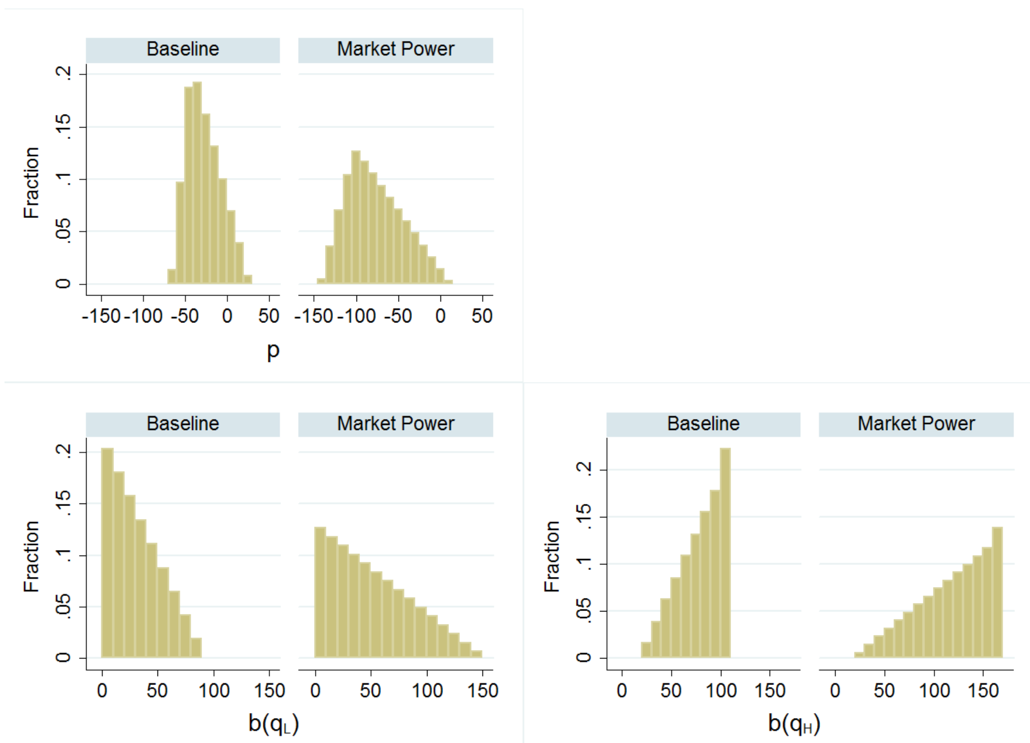
2.3 Experimental Design

Subjects participated in experimental sessions which consisted of multiple rounds of trading between subjects who were assigned to be buyers (principals) or sellers (agents). At the start of a session, subjects were randomly assigned ID numbers

Table 2.2.: Summary statistics for contracts that maximize principal's profit

		Average	Median	Min	Max
fixed payment, p	Baseline	-27.25	-30	-65	25
	Market Power	-77.25	-82	-140	10
low performance bonus, $b(q_L)$	Baseline	30.49	27	0	90
	Market Power	50.49	44	0	150
high performance bonus, $b(q_H)$	Baseline	79.51	83	20	110
	Market Power	119.51	126	20	170

For the baseline, 1,081 feasible contract offers maximize the principal's profit. For the market power treatment, 2,926 contract offer maximize the principal's profit.



For the baseline, 1,081 feasible contract offers maximize the principal's profit. For the market power treatment, 2,926 contract offer maximize the principal's profit. The y-axis reports the fraction of offers that fall into the given range (e.g. 0-10 points).

Fig. 2.2.: Self-enforcing contracts that maximize the principal's expected profit by treatment

and a role (buyer or seller), which remained fixed throughout the session. Subjects interacted via computers and remained anonymous. Each subject participated in only one session. The treatment (MP or Baseline) for a session was randomly assigned and subjects did not know which treatment they would participate in when they signed up for the experiment.

The stage-game interaction in each round mirrors the theoretical stage-game described earlier. At the beginning of each round, the buyer could make an offer, if she desired, to her randomly matched seller. Only the ID of one's trading partner was revealed to each subject. The terms of the offer were endogenously specified (in integers) by the principal within the provided bounds listed in the parameter table. An offer consisted of a four components: p, $b(q)$, a requested effort level, and the optional termination condition. To specify whether or not to include the optional termination condition, buyers simply had to click a "yes" or "no" button on the offer screen. If the seller received an offer, he then decided to accept or reject it. If accepted, the seller privately made his effort choice, framed as numbers with $e_L = 0$ and $e_H = 1$. After this, the value of q was observed by both parties. If no contract was entered, both parties received their outside options for that round.

At the end of each round, the screen displayed the subject's earnings (in points) for that round along with any choices that the subject made or observed. Points were converted into cash at the end of the experiment.

Each unique principal-agent pairing lasted for an indefinite number of rounds with δ serving as the probability of the pair being rematched at the start of the next period. This method is common for implementing laboratory infinitely repeated-game experiments (Roth and Murnighan (1978); Dal Bó (2005)).[12] Therefore, once a round was completed, subjects faced an 80% ($\delta = 0.8$) probability of being rematched with the same partner in the next round. This was conveyed to the subjects with a randomly drawn integer between 1 and 10 displayed on their screens. If a 9 or 10 was

[12]Note that this concept is *not* related to the endogenous termination condition outlined earlier. Here, δ describes the likelihood of paired subjects being exogenously rematched together in the next period.

drawn, subjects were exogenously matched with a different trading partner. Subjects could not, however, encounter an old trading partner in the rematching process. That is, a perfect strangers matching protocol for supergames was used. Note that if a pair had the endogenous termination clause in the contract in the previous period and the clause was triggered, then the pair cannot engage in trade in future periods when rematched together.

Since trading was indefinitely repeated, subjects were informed that the session would end according to one of the following rules: 1) If subjects encountered all unique trading partners, or 2) If the subjects had played at least 18 periods across all supergames, then the session would end once the current supergame randomly terminated. To keep the number of rounds and the length of each interaction consistent across sessions, the random numbers were predetermined. Subjects were made aware of this decision before the experiment started and could verify that the random draws were consistent with the numbers displayed on their screen by opening a sealed envelope at the end of the experiment. Random rematchings occurred three times after rounds 3, 11, and 16, resulting in four supergames with unique trading partners.

Sessions were conducted during Spring 2014 at Purdue University's Vernon Smith Experimental Economics Laboratory (VSEEL), which is a dedicated experimental economics laboratory with an explicit no deception policy. Subjects were recruited from the VSEEL ORSEE database, which consisted of mostly undergraduate students that had participated in other experiments but not the specific treatments used in this study. Each session lasted roughly 2 hours consisting of instruction, 2 practice periods, and 19 paid periods of trading. zTree was used to conduct the experiment (Fischbacher (2007)). Seven sessions of the baseline treatment were conducted with a total of 64 subjects. Eight sessions of the MP treatment were conducted with a total of 70 subjects. Average earnings were equal to \$25.76 and \$25.41 for the baseline and MP treatments respectively.

2.4 Results: A First Look at the Data

Prior to presenting our main results, which relate to the impact of market power, we first examine whether within treatment contracting outcomes are consistent with the theory. If so, this would lend confidence that the experimental design properly reflects the theoretical model, that the theory robustly organizes patterns of behavior, and that our subjects understood the instructions and the nature of the games that they played.

Specifically, the theory predicts the following patterns of behavior:

1. Satisfying the agent's participation constraint (PC_A) significantly increases the agent's willingness to accept a contract offer.

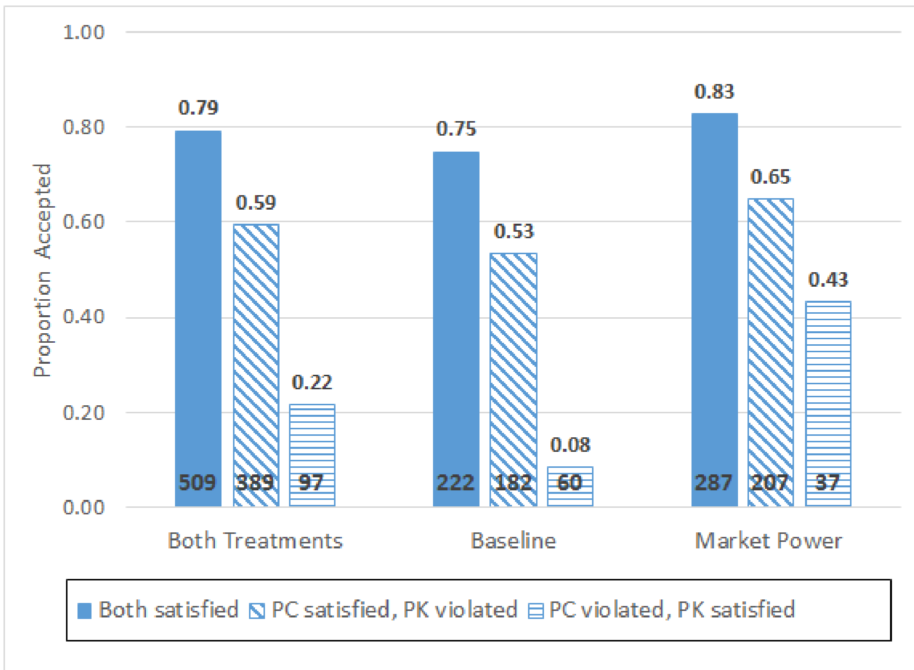
2. The incentive compatibility (IC) constraint significantly increases high effort provision.

We will examine each of these patterns in the following subsections.

2.4.1 Participation Constraint and Acceptance Rates

Result 1*: Satisfying the agent's participation constraint (PC_A) significantly increases the agent's willingness to accept a contract offer. Satisfying the promise keeping constraint (PK) significantly increases contract acceptance.*

Figure 2.3 reports the fraction of accepted contract offers depending on whether or not some combination of the (PC_A) and (PK) constraints is satisfied. When aggregating the data across treatments, satisfying both constraints has the largest acceptance rate at 79%. Offers that satisfy (PC_A) but violate (PK) have an acceptance rate of 59%. Because no observations violate both constraints simultaneously, the lowest fraction of accepted offers, 22%, occurs when (PC_A) is violated but (PK) is not. The evidence suggests that the agent is relatively more concerned about the size of the promised rent rather than the credibility of the bonus payments when deciding to accept an offer.



The numbers located in the base of each bar chart reflect the total number of contract offers observed for that category. Note that no offers violate both the (PC) and (PK) constraints simultaneously.

Fig. 2.3.: Contract acceptance rates: agent's participation (PC_A) and promise keeping (PK) constraints

Table 2.3 reports the marginal effects from linear probability models (LPM) where the dependent variable takes a value of "1" if the agent accepted the contract offer.[13] The main interest concerns the effects of the (PC_A) and (PK) constraints on contract acceptance, though additional covariates were added as controls. One control that requires explanation is what we define as cooperation in the last period. First, cooperation entails that the two subjects engaged in trade previous period. Second, conditional on the agent's performance level, the principal paid a bonus greater than or equal to the promised bonus stated in the contract offer.[14]

Note that regression (1) does not take advantage of the panel structure of the data whereas regression (2) includes agent fixed effects, since unobserved agent heterogeneity might induce self-selection into certain types of contracts so that the error term could be correlated with the (PC_A) and (PK) dummies. Moreover, the cooperated last period dummy in regression (2) was included to control for the possibility that an agent might update beliefs about the principal's reliability based on previous interactions.

The coefficients for the (PC_A) dummy are positive and significant across both specifications, which is consistent with theory. While the coefficient in regression (2) is smaller (0.380 versus 0.578), the qualitative conclusions are robust and suggests that the participation constraint is an important determinant of agent contract acceptance. The (PK) dummies also have the expected signs and are statistically significant, though the magnitude of the coefficients are lower than the (PC_A) coefficients. Overall, it appears that agents are more willing to accept contracts that satisfy the participation and promise keeping constraints. These results are largely consistent with theory.

[13]We also estimated marginal effects from a Probit model. While some of the numeric results changed, the qualitative conclusions did not change.

[14]Cooperation did not require that the agent picked the principal's requested effort level because the principal could not observe effort. Therefore, the principal could not verify that the agent was violating or honoring their agreement.

Table 2.3.: Linear probability estimates for contract acceptance

	(1)	(2)
(PC_A) satisfied dummy	0.578***	0.380***
	(0.067)	(0.086)
(PK) satisfied dummy	0.198***	0.103**
	(0.036)	(0.047)
Market power dummy	0.116**	0.054**
	(0.054)	(0.019)
Cooperated last period dummy		0.204***
		(0.049)
Length of relationship		-0.007
		(0.011)
$\frac{1}{\text{Period}}$	0.212***	0.304**
	(0.058)	(0.12)
Constant	-0.088	0.219
	(0.092)	(0.111)
Agent fixed effects	No	Yes
Observations	995	745
R^2	0.16	0.38

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimates are linear probability models. The dependent variable takes a value of 1 if the agent accepted the contract offer and a value of 0 otherwise. Robust standard errors clustered at the session level in parentheses. (PC_A) *satisfied dummy* takes a value of 1 if the (PC_A) constraint is satisfied. *Market power dummy* takes a value of 1 the observation belongs to the market power treatment. *Cooperated last period dummy* takes a value of 1 if the pair traded and the principal honored her bonus payment for the realized quality level in the previous period. *Length of relationship* indicates the number of periods in which the pair of subjects have interacted with each other.

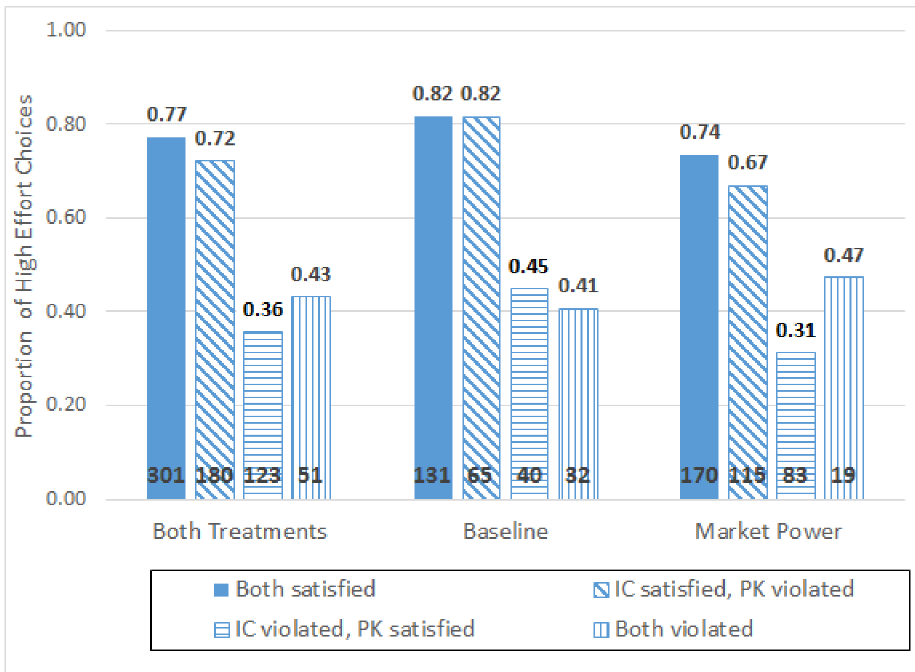
2.4.2 Incentive Compatibility and Effort Provision

Result 2: *Satisfying the incentive compatibility* (IC) *constraint significantly increases high effort provision.*

Figure 2.4 reports the fraction of high effort choices depending on whether or not some combination of the (IC) and (PK) constraints is satisfied. While in theory, the (PK) constraint should not have a direct effect on effort choice, it is possible that it could have an indirect effect in that a forward looking agent who does not believe that the promised discretionary bonus is credible may refuse to exert high effort. When aggregating data across treatments, satisfying both constraints produces the largest percentage of high effort choices at 77%. This percentage falls by 5% for offers that violate (PK) but satisfy (IC) with bonuses that are not credible. When the (IC) constraint is violated, however, the percentage of high effort choices declines to around 40% irrespective of whether or not (PK) is satisfied. Therefore, incentive compatibility is more associated with high effort provision relative to promise keeping.

Table 2.4 reports LPM marginal effects, with a dependent variable that takes a value of "1" if the agent chose high effort. The difference between the two regressions is that (1) ignores the panel structure of the data while (2) includes agent fixed effects to account for potential selection biases. Regression (2) also includes the cooperated last period dummy and length of relationship variable to control for the possibility that agents may update their beliefs about the principal's actions given prior behavior.

Both regressions suggest that satisfying (IC) has a positive and significant effect (0.363 with $p < 0.01$ in (1) and 0.336 with $p < 0.01$ in (2)). The estimates for (PK), however, are not significant, which indicates that promise keeping does not significantly influence effort provision. Overall, the results are consistent with theoretical predictions in that discretionary bonuses that satisfy incentive compatibility increase the probability of high effort.



The numbers located in the base of each bar chart reflect the total number of observations for that category.

Fig. 2.4.: High effort (e_H) provision: incentive compatibility (IC) and promise keeping (PK) constraints

Table 2.4.: Linear probability estimates for high effort

	(1)	(2)
(IC) satisfied dummy	0.363***	0.336***
	(0.061)	(0.074)
(PK) satisfied dummy	0.014	-0.0006
	(0.036)	(0.058)
Market power dummy	-0.103	-0.019
	(0.081)	(0.088)
Cooperated last period dummy		0.149**
		(0.056)
Length of relationship		-0.003
		(0.010)
$\frac{1}{\text{Period}}$	-0.121	-0.199
	(0.083)	(0.188)
Constant	0.464***	0.04
	(0.079)	(0.065)
Agent fixed effects	No	Yes
Observations	655	483
R^2	0.14	0.44

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions are linear probability models. The dependent variable takes a value of 1 if the agent chose e_H and 0 if they chose e_L. Robust standard errors clustered at the session level in parentheses. *(IC) and (PK) satisfied dummy* take a value of 1 if the incentive compatibility and the promise keeping constraints are respectively satisfied. *Market power dummy* takes a value of 1 the observation belongs to the market power treatment. *Cooperated last period dummy* takes a value of 1 if the pair traded and the principal honored her bonus payment for the realized quality level in the previous period. *Length of relationship* indicates the number of periods in which the pair of subjects have interacted with each other.

2.5 Major Results: Impact of Exogenous Reduction in Agent's Outside Option

The previous sections showed that our data patterns are consistent with some of the major predictions of the theoretical model, which inspires some confidence in the theory and experimental implementation. We now explore our main question, which is how relational contracting outcomes are affected by a reduction in agents' market power as proxied by an exogenous decrease in the agent's expected outside option. Recall that under the baseline treatment (B), an agent can assure himself a payoff of 30 points if he rejects the contract. Under the market power treatment (MP), there is a 50% chance that the agent would receive 30 and a 50% chance of receiving nothing, so that the expected payoff from taking the outside option drops to 15.

2.5.1 Contract Structure

Table 2.5 highlights the differences in contract structure across treatments. The p-value for a two-sample Mann-Whitney test, using individual session averages as the unit of observation, is also reported. The fraction of accepted offers is significantly higher by 15% in the MP treatment. In contrast, the fraction of high effort choices, bonus payments honored, and use of the optional termination condition is not significantly different across treatments.

Furthermore, the structure of the agent's compensation has some noticeable differences across treatments, particularly when focusing on the fixed payment, p. For all offers, the average p significantly increases from 6.2 points in the MP treatment to 21.6 points in the B treatment. Although these payments did not completely cover the agent's outside option, the average values for the accepted offers are close to $\bar{u}$ in each treatment—14.9 and 31.7 points respectively—and the difference is once again significant.

Additionally, the principal offers the agent more discretionary pay under MP. For instance, the average high performance bonus offered to the agent rises by 24.8 points

Table 2.5.: Across treatment comparison of contract terms

All Offers	Baseline	Market Power	Mann-Whitney p-value
Fraction offers accepted	.58	.73	.09
Fraction offers with termination condition	.26	.22	.35
Fraction of accepted offers with termination condition	.24	.19	.56
Average price (p) offered	21.6	6.2	.03
Average low bonus ($b(q_L)$) offered	17.6	31.9	.73
Average high bonus ($b(q_H)$) offered	48.4	73.2	.13
Completed Trades	Baseline	Market Power	Mann-Whitney p-value
Fraction high effort (e_H) choices	.71	.61	1
Fraction of promised bonuses honored	.53	.50	.77
Fraction honored when $q = q_L$	.48	.40	.24
Fraction honored when $q = q_H$	.57	.58	.82
Fraction honored if termination condition included	.35	.43	.77
Average price (p)	31.7	14.9	.04
Average low bonus ($b(q_L)$) offered	14.7	19.6	.39
Average bonus paid when $q = q_L$	2.2	1.0	.06
Average high bonus ($b(q_H)$) offered	43.3	58.6	.15
Average bonus paid when $q = q_H$	25.3	31.1	.20

Two sided Mann-Whitney p-values reported. Session averages used (7 sessions for baseline treatment, 8 sessions for market power treatment).

in the MP treatment. This gap decreases slightly for accepted offers with a difference of 15.3 points on average.

To study this issue further, we define the variable

$$DPR = \begin{cases} \frac{max\{b(q_L),b(q_H)\}}{max\{b(q_L),b(q_H)\}+p}, & if\ p > 0 \\ 1 & if\ p \leq 0 \end{cases}$$

which falls in the interval $[0, 1]$. The discretionary payment ratio (DPR) denotes the fraction of total pay promised to the agent that is discretionary rather than guaranteed upfront. Note that upfront payment is guaranteed by the size of p but when p is zero or negative, then nothing is guaranteed to the agent upfront so $DPR = 1$.

Table 2.6 reports regressions for completed trades of DPR on the market power dummy variable which takes a value of "1" if the observation belongs to the MP treatment. To determine if the results are robust, we also include several other variables as controls and run both random-effects regressions at the session-principal-agent levels as well as Tobit regressions to account for the fact that DPR is censored.
Main Result 1*: The ratio of discretionary to total payment (DPR) increases with the principal's market power. This suggests that agents are offered fewer payment guarantees and thus face more strategic uncertainty.*

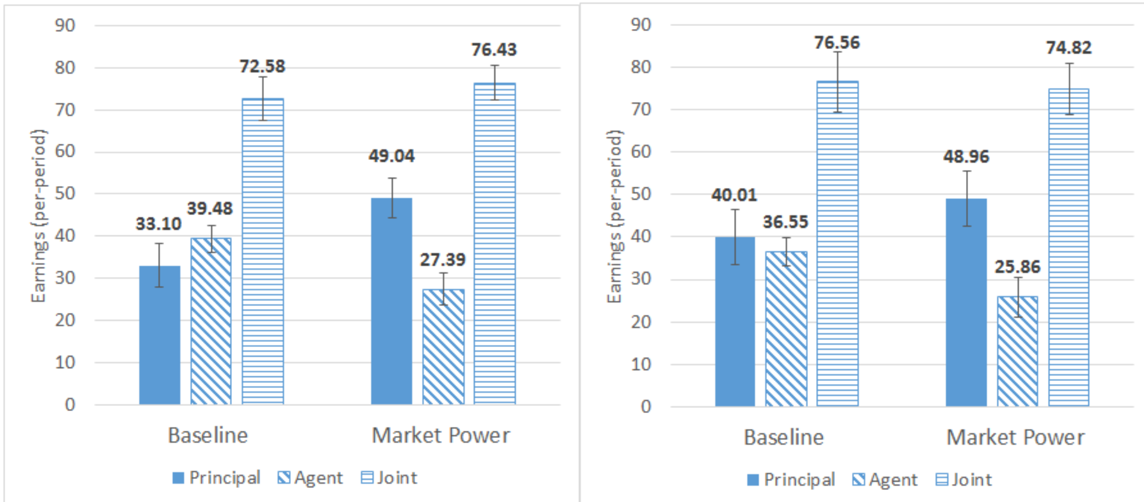
Main Result 1 is confirmed by the fact that the coefficient for the market power dummy variable is positive and significant across all four specifications. The coefficient ranges from a low of 0.145 in regression (3) to a high of 0.154 in regression (2), which is a fairly tight range across very different specifications. Thus, Main Result 1 also appears to be robust.

The takeaway is that MP increases the DPR so that agents are exposed to more strategic uncertainty since they receive fewer payment guarantees. Thus, if the principal shirks on the discretionary bonus and/or the poor outcome q_L is realized, the agent is in a more vulnerable position relative to the baseline.

Table 2.6.: Discretionary payment ratio (DPR) regressions

			Tobit Estimates	
	(1)	(2)	(3)	(4)
Market power dummy	0.150***	0.154***	0.145**	0.148*
	(0.053)	(0.053)	(0.0707)	(0.0753)
Cooperated last period dummy		-0.023		-0.0531
		(0.018)		(0.0465)
Length of relationship		-0.0007		0.00452
		(0.004)		(0.00639)
$\frac{1}{\text{Period}}$	-0.152***	-0.312***	-0.132***	-0.272***
	(0.041)	(0.089)	(0.0435)	(0.0993)
Constant	0.60***	0.629***	0.591***	0.611***
	(0.037)	(0.04)	(0.0533)	(0.0598)
Observations	655	483	655	483

* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. Regressions (1) and (2) estimated using multi-level random effects (at the session-principal-agent levels) with robust standard errors clustered at the session level. Regressions (3) and (4) are Tobit regressions with robust standard errors clustered at the session level. *Market power dummy* takes a value of 1 if the observation belongs to the market power treatment. *Cooperated last period dummy* takes a value of 1 if the pair traded and the principal honored her bonus payment for the realized quality level in the previous period. *Length of relationship* indicates the number of periods the pair of subjects have interacted with each other.



(a) All rounds (b) Last 10 rounds only

The earnings are expressed in experimental points. For all rounds, the number of observations are 268 and 387 for the baseline and market power treatments respectively. 95% confidence intervals for the mean are also depicted.

Fig. 2.5.: Average per-period earnings: completed trades only

2.5.2 Payoffs and Surplus

We now look at how efficiency and distribution are impacted by the MP treatment. In practice, one of the concerns about market power held by principals, such as large firms or employers, is that it negatively impacts suppliers/workers.

Figure 2.5 reports the average per-period earnings for principals and agents if the parties engaged in trade. The sum of these averages is also reported for efficiency comparisons. Overall, principals' average earnings are roughly 16 points higher in the MP treatment. When looking at the last 10 rounds only, this difference falls to 9 points. In comparison, agents' average earnings are roughly 12 and 10 points *lower* in the MP treatment for all rounds and the last 10 rounds respectively. Furthermore, the joint earnings (surplus) differ by only 2 to 4 points on average across treatments.

Thus, the manner in which the earnings are divided changes across treatments. For the last 10 rounds, principals capture 52% of the joint earnings in the baseline treatment leading to a fairly equitable split. In the MP treatment, however, principals

Table 2.7.: Regression estimates of the impact of market power on per-period payoffs and surplus

	Dependent Variables		
	(1)	(2)	(3)
	Principal Payoffs	Agent Payoffs	Surplus
Market power dummy	21.10***	-15.76**	3.985
	(8.000)	(7.020)	(3.887)
$\frac{1}{\text{Period}}$	-17.17*	9.025	-10.23***
	(9.752)	(8.684)	(3.723)
Constant	34.96***	35.99***	74.44***
	(3.905)	(2.124)	(3.136)
Observations	655	655	655

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions estimated using multi-level random effects (at the session, principal, and agent levels) with robust standard errors clustered at the session level. *Market power dummy* takes a value of 1 if the observation belongs to the market power treatment.

capture 65% of the joint earnings and, as a result, have an average payoff roughly twice as large as agents do. Therefore, principals extract more surplus from agents in the MP treatment, but efficiency remains similar across treatments for completed trades.

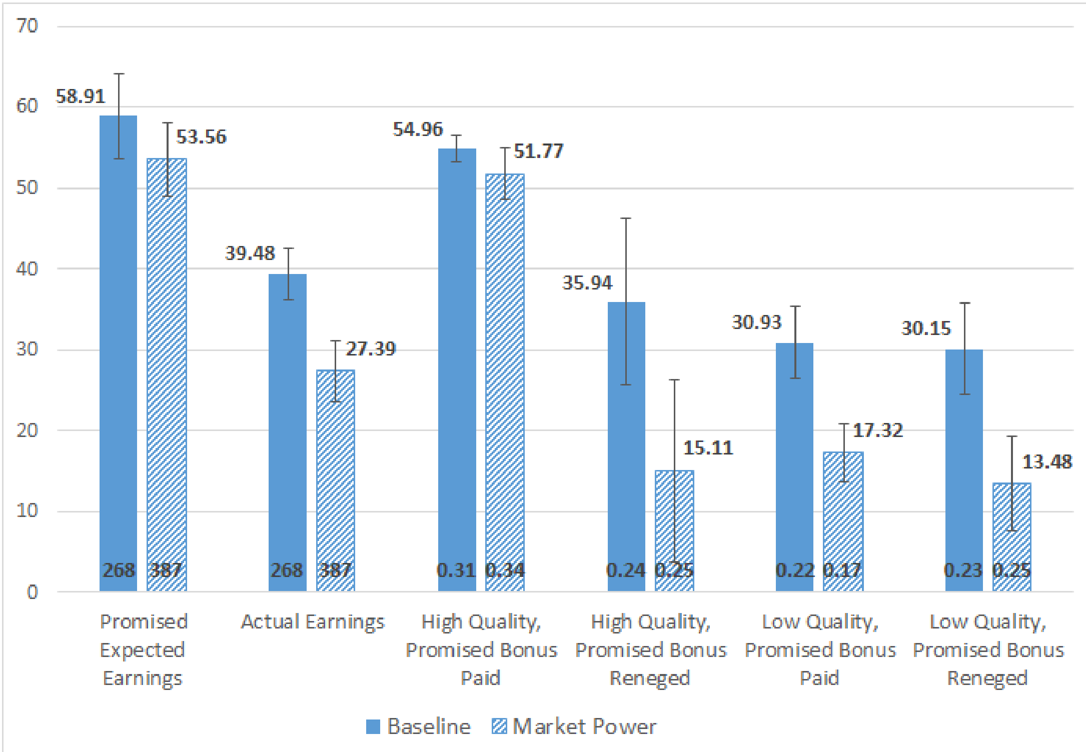
Table 2.7 presents estimates from three multi-level random-effects regressions (session-principal-agent levels). The sign and significance of the MP dummy coefficient estimates largely verify what we saw in figure 2.5. The MP treatment induces a 21.1 increase in principals' per-period payoff (p<0.01), a reduction in agents' payoff (-15.76, p<0.05), and no statistically significant change in surplus. Hence, an increase in the principal's market power appears to have no impact on efficiency but does cause distributional shifts.

Main Result 2*: An increase in the principal's market power does not impact efficiency but does have distributional effects by increasing the principal's payoff while lowering the agent's payoff.*

We now examine agents' earnings in more detail to try to understand the mechanisms through which their earnings decreased. Figure 2.6 reports average earnings for agents across treatments for different subsets of the data. The first set of bar charts presents the agents' *expected promised payoff*, which is what agents expect to earn under their contracts assuming the contracting parties honor the terms of the contracts. The expected promised payoffs are quite similar across treatments, 58.91 points in the baseline versus 53.56 under MP.

Despite the fact that principals do not lower promised payoffs significantly in MP relative to B, we see from regression (2) in Table 2.7 that agents' *actual* payoffs drop significantly by about -15.76 points. We now explore the channels through which this drop in payoffs occurs. We know from the results in Table 2.6 that principals decrease payment guarantees in MP by lowering the fixed payment while increasing the size of discretionary payments. This offers some clues as where to look when attempting to identify the channels through which agents suffer payoff losses under MP. An increased discretionary payment ratio implies that an agent will face more risk, both in terms of strategic uncertainty if the principal shirks on the discretionary bonus, and in terms of exogenous risk since high effort does not map deterministically into q_H. Figure 2.6 confirms these findings as average payoffs are only similar across treatments when agents perform well and principals honor bonuses. If, however, principals fail to pay bonuses under high quality or agents perform poorly, there is a noticeable decline in the average payoff in the MP treatment.

Table 2.8 presents four regressions of agents' payoff estimates that confirm these findings. In regression (1), which focuses on agents' expected promised payoff, the MP coefficient is -5.009 but not statistically different from zero. Hence, it appears that principals do not actually lower payoffs promised to agents when there is market power. Regression (2) was estimated on the subset of data for which principals paid discretionary bonuses so that strategic risk was not realized. We can see here that there is no statistically significant difference in per-period agent payoffs. Thus, market power does not cause agents' payoffs to drop when everything is going well. However, agents are not so fortunate when things go awry. Regression (3), which is estimated on the subset of data where principals shirks on promised bonuses, shows that agents



95% confidence intervals for the mean reported. There were 268 completed trades in the baseline treatment and 387 in the market power treatment. The proportions reported at the base of the bar charts on the right side state the fraction of observations for each treatment that fall into the listed category. For example, 31% of the completed trades in the baseline treatment had high quality where the principal honored the bonus payment compared to 34% of the completed trades in the market power treatment.

Fig. 2.6.: Average agent earnings comparisons for completed trades

Table 2.8.: Regression estimates of the impact of market power on agent payoffs across scenarios

	(1) Expected Promised Payoff	(2) Actual Payoff Bonus Paid	(3) Actual Payoff Bonus not Paid	(4) Actual Payoff q_L realized
Market Power dummy	-5.009	-3.011	-21.08**	-16.85***
	(8.735)	(3.257)	(8.674)	(5.399)
$\frac{1}{\text{Period}}$	4.758	6.188	12.31	7.517*
	(9.093)	(4.439)	(14.64)	(4.109)
Constant	60.95***	43.14***	29.91***	28.03***
	(7.640)	(2.144)	(3.382)	(3.715)
Observations	655	337	318	281

* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. Regressions estimated using multi-level random effects (at the session, principal, and agent levels) with robust standard errors clustered at the session level. *Market power dummy* takes a value of 1 if the observation belongs to the market power treatment.

are exposed to significant strategic uncertainty under MP as their profit drops by an average of -21.08. Similarly, regression (4) shows that agents are also exposed to considerable exogenous risk as agents' payoffs are an average of -16.85 lower when q_L is realized under MP.[15]

Main Result 3: *An increase in the principal's market power does not lower expected profit promised to agents. Moreover, agents' actual profits are not lower in the MP treatment when circumstances are favorable to agents (i.e. bonuses paid and q_H realized). However, agent payoffs are significantly lower when either the principal shirks on the bonus and/or q_L is realized.*

Finally, one might conjecture that these across treatment results are driven by an increased *rate* of shirking on bonuses by principals under MP rather than riskier contracts. The rate of shirking, however, is almost identical across treatments as seen in table 2.5. Principals shirked on bonuses 50% of the time under MP compared to 47% in B. Indeed, when we ran a random-effects LPM with bonus paid (equals "1") as the dependent variable against the same independent variables as those in table 2.8, we found that the MP dummy coefficient is 0.020 with a robust standard error clustered on sessions of 0.13 ($p-value$=0.88).[16] The fact that the rate of shirking on bonuses is no higher in the MP treatment despite strategically riskier contracts is not surprising. Theoretically, an exogenous decrease in outside options should strengthen self-enforcement so it appears that subjects assigned to be principals took advantage of the more relaxed self-enforcement constraints by increasing the size of discretionary payments, though not to the point of causing more systematic breaches of the self-enforcement constraints.

In summary, it appears that the primary impact of market power on agents is that principals restructure contracts that increase agents' exposure to both strategic and exogenous risk.

[15]The qualitative results also remain true when separating the data by *both* quality level and whether or not the principal honored the promised bonus payment.

[16]We did not create a table for this regression in order to conserve space.

2.5.3 Acceptance Rates by Agents

The previous subsection examined efficiency and distribution in terms of trades that occurred but a second major determinant of gains from trade is whether people are willing to engage in more trade. So far, we have seen that agents are offered more risky contracts in the MP treatment so an obvious question arises: do agents reject these riskier contracts at a higher rate?

If so, then one can argue that market power reduces efficiency since we have already seen that surplus per-trade does not change. However, if agents are more willing to accept contracts, then market power actually enhances efficiency by facilitating a higher volume of trade.

Referring back to figure 2.3, one can see that acceptance rates are higher in the MP treatment relative to the B treatment across all configurations of the (PC) and (PK) constraints. We also report regression results in Table 2.9. Regressions (1) and (2) are random-effects LPMs where the dependent variable is whether an agent accepted (= “1”) a contract offer. Regressions (3) and (4) are Probit specifications. We ran various specifications to check for robustness. One can see indeed that the estimated marginal effects are positive (in the 0.13-0.151 range) across all four specifications and statistically significant at the 10% or 5% levels.

Main Result 4: *An increase in the principal's market power increases the probability that agents will accept the contract. This combined with the finding that there is no difference in surplus per-trade implies that an increase in market power in favor of the principal increases efficiency.*

In terms of absolute numbers, out of 535 total contracting opportunities (i.e. situations in which the principal could make an offer to the agent) across all sessions of the B treatment, approximately 50% resulted in trades. Out of 576 opportunities in the MP treatment, approximately 67% resulted in trades. Thus, significantly more surplus was created in the MP treatments.

Table 2.9.: Marginal effects of the impact of market power on seller acceptance

	(1) LPM	(2) LPM	(3) Probit	(4) Probit
Market power dummy	0.136*	0.130*	0.151**	0.144**
	(0.0768)	(0.0704)	(0.079)	(0.073)
Cooperated last period dummy		0.255***		0.392***
		(0.0447)		(0.044)
Length of relationship		-0.00542		-0.016
		(0.00898)		(0.011)
$\frac{1}{\text{Period}}$	0.116*	0.200*	0.123	0.204
	(0.0602)	(0.114)	(0.069)	(0.133)
Constant	0.573***	0.497***		
	(0.0697)	(0.0892)		
Observations	995	745	995	745

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Linear probability models estimated using multi-level random effects (at the session, principal, and agent levels) with robust standard errors clustered at the session level. Probits were estimated with robust standard errors clustered at the session level. *Market power dummy* takes a value of 1 if the observation belongs to the market power treatment. *Cooperated last period dummy* takes a value of 1 if the pair traded and the principal honored her bonus payment for the realized quality level in the previous period. *Length of relationship* indicates the number of periods the pair of subjects have interacted with each other.

2.6 Conclusion

The purpose of this paper is to determine how an exogenous increase in the principal's market power affects contracting outcomes. With regard to efficiency, our key findings are that market power enhances the ability of principals to resolve agency conflicts since it increases their leverage to maintain high-powered informal incentives while increasing the agent's willingness to accept contracts. Since high effort is maintained, surplus per-trade is unaltered and all efficiency effects operate through an increase in the volume of trade. Consequently, market power relaxes the tension between incentive compatibility and contract participation which enhances the ability of principals to solve moral hazard problems. The key implication of our findings is that traditional market power models that predict welfare losses due to a reduction in the volume of trade may overstate the restraint of trade caused by market power. Many modern input market and supply chains are characterized by contracting relationships so the ability of market power to relax contractual constraints can actually increase the volume of trade and work in the opposite direction of traditional market power effects.

With regard to distribution, we find that an increase in the principal's market power does not cause principals to directly reduce expected payoffs promised in contracts to agents. Rather, contracts are restructured to shift significantly more risk—both strategic risk as well as exogenous risk—onto agents. Thus, when relationships are stable and performance outcomes are favorable, agents do not suffer profit losses. However, agents are exposed to significantly more downside payoff losses when either the principal defaults on discretionary bonuses or performance outcomes are not favorable.

Our findings suggest that smallholders or suppliers contracting with large firms or exporters may fair well in environments characterized by stable relationships with low exogenous risk. However, in other situations characterized by significant production or price risk, or in environments that are exposed to significant political or technological change where discount factors are small or time inconsistencies are significant, market power may have significant negative consequences for agents. One promising avenue of future research is to conduct relational contract experiments with market power that also involve variations in discount factors or exogenous shocks. This might provide additional insights into how market power affects efficiency and distribution.

Appendix: Instructions for the Market Power Treatment

You can earn money during this experiment, with the exact amount depending on the decisions you make during the experiment. Your experimental income is calculated in points, which will be converted into cash at the rate of: $1 = 30 points. We will start you off with a balance of 150 points ($5).

All written information you received from us is for your private use only. You are not allowed to pass over any information to other participants in the experiment. Talking during the experiment is not permitted. Violations of these rules may force us to stop the experiment.

General Information

This experiment is about how people buy and sell goods for which quality matters. Participants are divided into two groups: half will be buyers and the other half sellers. Then a trading period will start in which a buyer and seller will trade one unit of a good that can vary in quality. The price agreed upon between the buyer and seller and the quality of the good traded will determine how much money each party makes in that period. There will many trading periods throughout the course of this experiment.

Who will you trade with? The computer will randomly match each participant in the room with another participant to form a buyer-seller pairing. You will be informed whether you are the buyer or seller in your pairing. You will trade with your pair-member. You will not be informed of the actual identity of the other person (and s/he will not be informed of your identity). All sellers and buyers are assigned a numeric ID which is not associated with their real identity. You will also retain your ID and role (e.g. buyer or seller) through the entire experiment.

For how many periods will you trade with the same person? All participants will remain matched with their pair-member for a random number of periods. How is this determined? At the end of each period, the computer will determine randomly whether the same pairings will continue for the next period or whether new pairings will be formed. In any given period, there is an 80% chance that the same pairings will continue for the next period. In other words, there is an 80% chance that you will continue to trade with the same person in the next period. To help you understand this, imagine the computer has been programmed to spin a roulette wheel. If it lands on 1,2,3,4, 5, 6, 7, or 8 then you will continue to trade with the same person the next period. But if it lands on 9 or 10 the current pairings are immediately terminated. And then for the next period, the computer will randomly match you with a different person in the room to form a new pairing. This process will repeat for every new pairing. At the beginning of each period, you will be notified on-screen whether the random matching process has kept you with the same person or matched you with a new person.

When does the entire experiment end? If one of two conditions holds: (1) The experiment will end if all participants have already been matched with all possible trading partners. This is because no participant will be matched with the same person more than once during this experiment. For example, if there are 4 buyers and 4 sellers, then no buyer or seller will have more than 4 unique pairings. After 4 unique pairings, the experiment ends. (2) Even if all unique pairings have not been exhausted, the last pairing will occur once the experiment has lasted at least 18 periods. In other words, if you have traded at least 18 periods for the experiment, then your current pairing is your last one. This does not mean the experiment stops at 18 rounds exactly; it only means that when your last pairing randomly ends, you will not be paired with a new partner.

To summarize, if you have had less than 4 different trading partners during the experiment, and the experiment has not lasted at least 18 total periods, then when your current match is randomly terminated, the computer will match you with a new person and the experiment would continue. However, if the experiment has lasted at least 18 total periods, then the experiment will end once your current pairing is randomly terminated.

Conducting Trades

Each trade occurs within a trading period. Each trading period is then divided into a proposal phase followed by an effort determination phase and then followed by a quality realization & payment determination phase.

1. During the proposal phase, the buyer can make a proposal on the terms of trade to the seller. The seller can either accept or reject the proposal.

2. If the seller accepts the proposal, then during the effort determination phase, the seller privately chooses the effort level to supply.

3. After the seller makes an effort choice comes the quality realization & payment determination phase. During this phase, both the buyer and the seller observe the realized quality. The buyer can then make final adjustments in payment depending on the initial terms of the proposal

Specific details of each phase are given below:

1. The Proposal Phase

Each period starts with a proposal phase. A proposal allows the parties to agree to the terms of trade by including a list of promises and obligations of both parties (see below for details). The buyer can submit a single proposal during the proposal phase. Once a proposal is submitted, the seller will decide to accept or reject the proposal.

How does a buyer make a proposal? A proposal screen will appear that will require the buyer to enter values for the following terms: price, two quality-contingent bonuses, and a requested effort level. The buyer must also specify whether to include an optional ending condition for the trading relationship. These four components are described below:

- **Price** This allows the buyer to state the price s/he will pay for the good traded. The buyer enters a price in the “Price” field. The price ranges from -250 to 250 (whole numbers).

Important: The price the buyer specifies will be *binding*. This is similar to an upfront payment or a legally binding obligation once the proposal is agreed upon, the computer will ensure that the price is paid to the seller.

b) **Quality-contingent bonus** There are two potential quality levels for the good traded: 30 or 120. The quality level realized depends partially on a decision made by the seller in the effort determination phase (see below for explanation). In the proposal, the buyer states that s/he will pay a bonus linked to each possible quality level. That is, a bonus level must be specified for two cases: when the realized quality is 30 and when realized quality is 120. To enter a bonus for the case when quality is 30, enter a number into the "Bonus amount for quality 30" field to specify the size of the bonus (whole numbers from 0 to 250).
Similarly, for the case when quality is 120, enter a number into the "Bonus amount for quality 120" field to specify the size of the bonus (whole numbers from 0 to 250).
Important: The stated bonuses are *not binding*. During the payment determination phase, to come later, the buyer can choose any bonus level s/he wishes. Thus, the stated bonuses, one for each possible quality level, are discretionary. Only the price is ensured to be paid to the seller.
The total offered payment to the seller is: price + quality-contingent bonus.

- **Requested effort level** In the next phase, sellers who accept an offer must choose an effort level of either 0 or 1 to supply. The seller's effort choice helps determine whether the quality level will be 30 or 120 for that period. Buyers must request an effort level that they want their seller to choose. To indicate the requested effort level, click the corresponding button next to the question, "What effort level do you request the seller to provide?"
  Note: If the offer is accepted, the seller is *not* obligated to choose the effort level requested by the buyer.

- **Optional ending condition** The buyer may also decide to include an ending condition for the trading relationship in the proposal. To include the condition, the buyer clicks the "Yes" button next to the question, "Would you like to include the condition that the relationship will end if quality 30 is realized?" If the buyer does not wish to include the ending condition, he/she clicks the "No" button.

  Note: If included and the offer is accepted by the seller, then the buyer and seller will *not* trade in future rounds if the realized quality is 30 in the *current* trading period (see phase #3 below). This does not mean that the experiment will end if the condition is satisfied. It does, however, mean that the two parties will not be able to make or accept offers in their pairing until randomly rematched with a new trading partner.

After the buyer has specified these terms, s/he needs to click the "Commit Decision" button to submit it. Then comes the effort determination phase.

2. Effort Determination Phase

Following the proposal phase, sellers can accept or decline the buyer's proposal. Sellers who accept the agreement will choose between the two possible effort levels: 0 or 1. The seller clicks the button (0 or 1) corresponding to his or her effort choice. **Important:** Only the seller observes his/her effort choice. The buyer does not observe effort but can make inferences about the chosen effort based on the quality realized in that period.

3. Quality Realization and Payment Determination Phase

During this phase, after both the buyer and seller observe the realized quality, the buyer will decide the actual bonus to be paid to the seller. The buyer will enter his/her bonus choice in the bonus payment field. *Nothing restricts the buyer from choosing a bonus level that is different from the bonus that was specified in the proposal.* The actual bonus can range from 0 to 250 (whole numbers) at the buyer's discretion.

Buyers will also be asked to make a guess about the seller's effort choice in that period. Click the button corresponding to your guess (0 or 1).

Note: If the proposal included the optional ending condition and realized quality was 30 in this round, then the buyer and seller will not trade after the current period. That is, if the ending condition is satisfied, offers cannot be made or accepted in the pairing until the buyer and seller are randomly rematched with a new trading partner.

At the end of each period, the buyer and seller will be shown a summary screen. The following information is displayed on this screen:

- the points that you individually earned (or lost) in this period.

Note: **Your trading partner's points will not be displayed on your screen. Similarly, your points will not be displayed on their screen.** Furthermore, note that buyers and sellers can incur losses in each period. These losses are subtracted from your points balance.

If an agreement was formed, the following information will also be displayed:

- the Price the buyer offered.
- the Proposed Bonuses (for the two possible quality levels)
- the Actual bonus granted (in bold)

Please enter all the information on the screen in the documentation sheet supplied to you. This will help you keep track of your performance across periods so that you can learn from your past results.

At the beginning of the next period, the computer will inform you if you have been randomly matched with the same trading partner or with a different partner.

How do Buyers Make Money?

- If the **buyer does not make an offer or the seller rejects the offer**, the buyer will receive 30 points for that period. If the trading relationship has ended as a result of the ending condition, the buyer will receive 30 points in each subsequent period **until randomly rematched with a new seller**.

- If the buyer's offer is accepted, the buyer's points for the period depend on the realized quality, the price, and the actual bonus paid. That is,

Buyer Points = Realized Quality - Price - Actual Bonus

- As you can see, the higher the realized quality, the more points the buyer earns. At the same time, the lower total payments (price plus actual bonus), the more points the buyer earns.
- In summary, higher quality and lower payments means more points for the buyer.

How is realized quality determined? The realized quality depends, in part, on the seller's effort choice. The table below summarizes the likelihood of observing quality levels 30 or 120 given the seller's effort choice:

	Quality = 30	Quality = 120
Effort = 0	0.75	0.25
Effort = 1	0.25	0.75

After the seller makes his/her effort choice, the actual quality is randomly determined by the computer based on the above probabilities. That is, for either effort choice, there is a chance observing a quality level of 30 or a quality level of 120. An effort choice of 1, however, increases the chance of observing a quality level of 120. To help you understand this, imagine we randomly pick one ball from a bingo cage containing 4 balls numbered 1 through 4. If the seller's effort choice was "0" then drawing the ball labeled 1 would result in the quality level of 120 while balls labeled 2, 3, or 4 result in a level of 30. If the seller's effort choice was "1" instead, then drawing balls labeled 1, 2, or 3 would result in a quality level of 120 while the ball labeled 4 would result in a quality level of 30.

How do Sellers Make Money?

- If the **seller rejects the proposal or the buyer does not make an offer, the seller will receive 30 points with a 50% probability and 0 points with a 50% probability** for that period. This is like flipping a coin where if it comes up heads, the seller gets 30 points but if it is tails the seller gets 0 points.

  If the trading relationship has ended as a result of the ending condition, the seller will receive 30 points in each subsequent period **until randomly rematched with a new buyer**.

- If the seller has accepted an offer, then the seller's points depends on the price, actual bonus, and effort costs s/he incurs. The points of a seller are determined as follows:

Seller Points = Price + Actual Bonus - Effort Costs

How are effort costs calculated? Effort costs depend on the seller's private effort choice of 0 or 1. The table below summarizes the seller's costs:

Effort:	0	1
Cost:	**0**	**10**

An effort choice of "1" costs the seller 10 points while a choice of "0" costs the seller no points.

- As you can see, the higher the actual payments, the more points a seller earns. At the same time, the higher effort cost reduces points.

3. A RELATIONAL CONTRACTING EXPERIMENT ON SUBJECTIVE PERFORMANCE

This paper uses a laboratory experiment to study how potential disputes over the agent's performance level affect relational contracting outcomes in a bilateral principal-agent relationship. More specifically, I focus on changes in contractual form, efficiency, and surplus distribution when moving from an environment where the two parties cannot disagree over the agent's performance measure to one where the principal and agent may have conflicting beliefs concerning the agent's performance level. In both environments, the performance measure is a stochastic function of the agent's effort choice, which the principal cannot observe. The performance signal, however, is only publicly observed in the baseline treatment. In the second treatment, the principal privately observes the performance signal. A potential source of disagreement exists in the latter treatment because both parties have important asymmetric information related to the performance signal. The principal is unsure about the level of effort provided while the agent is unsure about how well he actually performed. Therefore, since effort maps imperfectly into performance, the agent can realize a poor performance signal after working hard due to "bad luck" but may believe that he performed well while the principal may attribute low performance to the agent shirking. Such conflict may hinder the success of the dynamic contracting relationship.

Furthermore, the principal has additional strategic considerations when she privately observes the performance signal. In the model, which is adapted from Levin (2003), the principal can offer the agent two types of monetary incentives. The first is a verifiable fixed payment, independent of the agent's performance level, which is paid upfront. The second is a promised performance bonus, which the principal need not pay because performance is assumed to be unverifiable by a neutral party and, therefore, performance is non-contractible. Along with deciding the actual performance bonus to pay, the principal must also send unverifiable performance feedback to the agent when she privately observes the signal. Therefore, the principal may manipulate performance feedback to her advantage such as under reporting the performance value after receiving the good signal in order to avoid paying a high performance bonus. In such a case, the agent cannot verify that the principal is breeching the contractual agreement unless she fails to pay the promised bonus for low performance. Because of this, the principal may restructure contracts advantageously across treatments by increasing her amount of discretion as payment deviations become harder to detect.

The importance of this research stems from the fact that performance evaluation is inherently subjective in most tasks or occupations. Because of this, subjective measures, such as discretionary bonuses, frequently provide incentives within many contractual relationships. A 2014 survey of 1,452 American firms, for instance, reports that 58.5% of employers used bonuses to compensate middle managers. For those in non-management positions, bonuses were the second most popular form of performance pay (Bruce (2014)). Although commonly used, Prendergast (1999) notes the lack of empirical studies evaluating the effectiveness of subjective performance systems. Existing work, instead, focuses on data where output is easily measured and the worker's subsequent performance is fairly easy to quantify. An important aspect not explored in these studies is that workers and firms typically disagree, or at least have somewhat conflicting perceptions, on the worker's performance, making the potential for conflict an important aspect of the bilateral relationship. In terms of laboratory experiments on contract design, very few studies incorporate asymmetric information into the contracting relationship. This is the first relational contracting experiment, to my knowledge, to make the agent's performance measure private, therefore leaving the agent uncertain as to how well he actually performed.

Furthermore, although subjective measures are more practical in terms of how compensation is tied to performance, they can potentially create problems that can be easily observed in the lab. First, when disputes over performance pay arise, the relationship between the worker and firm may be permanently strained. For example, in the early 1990s many managers left First Boston and Goldman Sachs after promised bonuses were much lower than expected (Stewart (1993)). Another problem stems from the evaluation process. When conducting performance reviews, supervisors may be concerned about factors beyond the accuracy of their appraisals. Longenecker et al. (1987) surveyed 60 executives and found that many manipulated performance feedback in their favor. Some inflated performance to encourage the worker to perform well in the future while others deflated performance for the same reason. Therefore, in addition to the difficulty of agreeing on adequate compensation, evaluations may not even reflect the firm's true perception of the employee's performance if other motivational factors are present. The results of this experiment can easily measure how the potential for conflict alters the efficiency of the relationship, via contract acceptance rates and effort provision, and whether principals tend to manipulate performance feedback for their personal gain.

Turning to the main findings, across treatment changes in efficiency as a result of the subjects' actions are minor. Contract acceptance rates are 58% and 57% in the public and private performance signal treatments respectively. For completed trades, the percentage of efficient effort choices is also not significantly different across treatments at 71% and 64%. Finally, contingent on the true value of the performance measure, principals honor their promised performance bonuses at remarkably similar rates, 53% and 52% of the time. Across treatment comparisons, therefore, indicate no efficiency losses despite the theoretical lack of contracts that facilitate successful relational contracting under the private performance signal. In terms of earnings for completed trades, the principal's per-period earnings significantly increase under the private performance signal while the agent's earnings are not significantly different. Furthermore, pairwise earnings are significantly higher when the performance signal is private, but this result is not driven by significant changes in the agent's effort provision. Agents in the treatment of interest instead experience a relatively higher amount of "good luck" with the noisy mapping between effort and performance resulting in a larger frequency of good performance signals. Only principals benefit from this good luck, however, implying that principals capture a larger fraction of the joint surplus when the signal is private.

Principals in the private performance signal treatment also significantly restructure contracts by lowering the upfront, guaranteed payment to the agent while increasing the promised discretionary performance bonus for high performance outcomes, thereby exposing the agents to more strategic uncertainty. The agent's promised, expected earnings, however, do not significantly vary across treatments. This implies that although principals have increased discretion under the private signal, the agent's overall welfare will not decrease as long as principals honor their promised bonus payments at similar rates, which indeed occurs. The agent's earnings, however, are significantly reduced for high performance realizations across treatments when the principal falsely reports that his performance was poor. These cases are associated with much larger relative rates of shirking on the promised bonus. On occasion, therefore, principals manipulate performance feedback to their advantage by understating the agent's performance.

In terms of overall performance feedback, however, principals in the private performance signal treatment tend to report performance honestly. Principals evaluate performance honestly for 73% of the good performance realizations. Similarly, for poor performance outcomes principals are honest 83% of the time. Furthermore, this tendency to report truthfully cannot be explained by the theoretical framework as satisfying the model's incentives for truth telling result in minor increases in accurate feedback. The general preference for truth telling, therefore, must be driven by some aspect outside the scope of the theoretical framework such as lying aversion or beliefs that frequent misreporting will strain the contracting relationship.

3.1 Related Literature

The experiment is adapted from the theoretical framework of MacLeod and Malcomson (1989) and Levin (2003). MacLeod and Malcomson (1989) characterize the conditions which ensure that the relational contract is self-enforcing in an environment where the principal and agent have symmetric information related to the performance measure. Levin (2003) extends their model among several dimensions. First, he solves for the optimal contract structure under moral hazard. In another extension, the principal also privately observes the agent's performance measure and sends unverifiable performance feedback to the agent. To analyze these environments in the laboratory, I study a simplified version of Levin (2003)'s model.

Related experimental work on contract design begins with Brown et al. (2004) who study how contracting partnerships emerge under gift exchange contracts within a market where principals and agents endogenously match with one another. They find that when identities of the subjects are fixed, as opposed to being randomly generated each period, subjects engage in repeated contracting with the same partner. Agents also choose higher efforts and receive higher earnings on average when identities are fixed despite the market containing an excess supply of agents. In this study, I also analyze dynamic relationships but impose an exogenous matching process of principal-agent pairs as this aligns more closely with Levin (2003)'s model. Furthermore, principals can implement a wide variety of contractual forms, not just gift exchange contracts, which allows me to study how potential performance disputes alter the types of contracts that principals design. Fehr et al. (2007) compare the effectiveness of a contract with a discretionary bonus to an incentive contract where the agent must pay a fine to the principal if caught shirking below the principal's requested effort level. They find that most principals prefer to implement contracts with a discretionary bonus. Bonuses also increase both the agent's effort choice and the principal's earnings on average. The authors, however, focus on one-shot interactions whereas this experiment looks at a dynamic principal-agent relationship. Both of these studies also do not make the agent's effort choice hidden from the principal. Irlenbusch and Sliwka (2005) study moral hazard in a gift exchange contract. They find that average effort choices are not lower although they are more variable compared to environment where effort is perfectly observed. Once again, however, I focus on contracts with informal bonuses.

Furthermore, these experiments do not create the potential for the principal and agent to disagree over performance since they jointly observe the agent's performance value. Ederer and Fehr (2016) allow the principal to privately observe information about the performance difference between two agents engaged in a two-stage tournament. I, however, do not focus on a one-shot environment with multiple agents. Finally, Sebald and Walzl (2014) study a real-effort task in which the principal observes the agent participating in the task. The agent's payoff may depend on how well the principal rates his performance. After observing the performance evaluation, the agent could punish the principal at a cost. They find that agents tend to punish principals when their self-evaluation is higher than the principal's reported evaluation. Although they create the potential for the two parties to dispute over the agent's performance level they analyze one-shot instead of dynamic relationships. Furthermore, I do not focus on a real-effort task as this does not align with the assumptions of Levin (2003)'s model. In my environment, principals also endogenously specify the performance incentives instead of just evaluating the agent's performance. Therefore, additional strategic considerations are present in this experiment as the principal has to decide the actual bonus to pay along with evaluating the agent's performance. To my knowledge, this is the first experiment that studies the implications of allowing the principal to privately observe the agent's performance signal within the relational contracting framework.

3.2 Theoretical Framework

3.2.1 Model

The framework of the model is closely related to the one found in the first chapter. For conciseness, I provide a summary of the main aspects of the model and refer readers to the previous chapter for a more detailed description. A risk-neutral principal and agent are infinitely lived and share discount factor δ. In each period, the principal can offer the agent a take-it-or-leave it contract offer. In the offer the principal specifies two types of payments: 1) a fixed, enforceable payment $p \in \{\underline{p}, \ldots, \overline{p}\}$ which may be negative and 2) a nonnegative discretionary bonus, $b(q) \in \{0, \ldots, \overline{b}\}$. The latter depends upon the value of the agent's performance level, $q \in \{q_L, q_H\}$, in the current period where $0 < q_L < q_H$. Furthermore, q is a stochastic function of the agent's hidden effort choice, $e \in \{e_L, e_H\}$, where e_H costs the agent c while e_L is costless. Denote l and h as the probability of realizing q_H given an effort choice of e_L and e_H respectively. Although e_H is more costly to the agent, $0 < l < h < 1$, which implies that high effort, e_H, increases the likelihood of the agent realizing the high performance level, q_H.

Along with specifying payments in the offer, the principal also states the effort level, either e_L or e_H, that she wants the agent to choose. Since the principal cannot observe e, however, the agent does not have to honor this request. Finally, the principal can include an optimal termination condition, which contingent upon certain outcomes or actions, specifies the likelihood of the two parties engaging in future trade. This condition will be explained in more detail as a I discuss the two variations of the contractual environment.

If the agent rejects the offer or the principal does not make an offer in a given period, the principal and agent receive an outside option of $\bar{\pi}$ and $\bar{u}$ respectively. If the agent accepts the offer, he privately makes his effort choice e. Depending on the environment, the appropriate party/ies observe the value of q. Finally, the principal decides the actual bonus, $B \in \{0, \ldots, \overline{b}\}$, to pay to the agent. The principal's payoff when contracting equals $\pi = q - p - B$ while the agent's payoff equals $u = p + B - c(e)$.

3.2.2 Efficient and Stationary Contracts

To characterize the set of optimal contracts, Levin (2003) focuses on contracts that satisfy several properties. First, contracts are efficient in that they maximize the expected joint earnings of the principal and the agent. Here, efficiency implies that the agent picks e_H.

To illustrate this, I first define the relevant expected payoffs per-period contingent on the agent's effort choice, e, and the principal honoring the promised performance bonus, $b(q)$. For each effort choice, the principal's expected per-period payoff is:

$$\begin{aligned}\pi_{e_L} \equiv& E_q[q - p - b(q)|e = e_L] = lq_H + (1-l)q_L - [p + lb(q_H) + (1-l)b(q_L)] \\ \pi_{e_H} \equiv& E_q[q - p - b(q)|e = e_H] = hq_H + (1-h)q_L - [p + hb(q_H) + (1-h)b(q_L)]\end{aligned}$$

Similarly, the agent's expected per-period payoff for each effort choice is:

$$\begin{aligned}u_{e_L} \equiv& E_q[p + b(q) - c(e)|e = e_L] = p + lb(q_H) + (1-l)b(q_L) \\ u_{e_H} \equiv& E_q[p + b(q) - c(e)|e = e_H] = p + hb(q_H) + (1-h)b(q_L) - c\end{aligned}$$

Finally, the expected surplus per-period for each effort choice is:

$$\begin{aligned}s_{e_L} \equiv& E_q[q - c(e)|e = e_L] = lq_H + (1-l)q_L \\ s_{e_H} \equiv& E_q[q - c(e)|e = e_H] = hq_H + (1-h)q_L - c\end{aligned}$$

To make the contract efficient when the agent exerts high effort, e_H, assume:

$$\begin{aligned}\bar{s} \equiv \bar{u} + \bar{\pi} <& s_{e_H} \\ s_{e_L} <& s_{e_H}\end{aligned}$$

Therefore, trading under e_H maximizes the expected joint earnings of the principal and agent. Second, Levin (2003) assumes that contracts are stationary, which implies that the principal offers the same payments p, $b(q_L)$, and $b(q_H)$ in all periods during which trade occurs. Levin (2003) also assumes that the agent chooses the same effort e in all periods as long as he has not observed the principal reneging on $b(q)$ during any previous period. If a discretionary payment is not honored, the agent issues the worst punishment to the principal by rejecting all future offers.[1] These assumptions make it possible to focus on the static, rather than the dynamic, incentive problem.

3.2.3 Optimal Contracts when Both Parties Observe the Performance Signal

In the first environment, both parties publicly observe the value of q when they engage in contracting. Therefore, there is no scope for the principal and agent to disagree over the agent's performance measure.

Along with specifying payments in the offer, the principal must also decide whether or not to include an optional termination condition. This condition specifies the likelihood of the relationship ending permanently after the current period, therefore ending the contractual relationship. If $q = q_L$ the relationship terminates with certainty after the current period. That is, in all future periods the parties receive their outside options. In contrast, if $q = q_H$ the relationship continues with certainty.[2] If the condition is not included in the offer, however, the relationship does not terminate in the case that $q = q_L$, which implies that the parties can trade in the next period.

To ensure that the contract is efficient and all informal agreements are honored, the following four constraints must be satisfied:

1. The principal's participation constraint (PC_P) makes it worthwhile to enter the contract:

$$\pi_{e_H} \geq \bar{\pi} \tag{PC_P}$$

[1]Levin (2003) also illustrates that an equivalent contract exists where the parties can continue trading under a relational contract following a deviation. The terms of the contract, however, are renegotiated to hold the deviating party to his or her outside option. Since the parties remain on the Pareto frontier, this outcome is renegotiation proof.

[2]In a more general contract, the principal could specify termination probabilities for each possible value of q. I fix these probabilities in order to make the contract design problem less demanding for subjects and to make the experiment easier to understand.

2. The agent's participation constraint (PC_A) makes it worthwhile to enter the contract:

$$u_{e_H} \geq \bar{u} \tag{PC_A}$$

3. The agent's incentive compatibility constraint (IC) prevents the agent from shirking:

$$u_{e_H} \geq u_{e_L} \tag{IC}$$

4. The principal's promise keeping constraint (PK) ensures that she will pay the discretionary bonus, $b(q)$, for all possible realizations of q:

$$\underbrace{\frac{\delta}{(1-\delta)}(\pi_{e_H} - \bar{\pi})}_{\substack{\text{expected, discounted net gain from} \\ \text{future interactions when paying bonus}}} \quad \geq \quad \underbrace{\max\{b(q_L),\, b(q_H)\}}_{\substack{\text{maximum gain from} \\ \text{withholding bonus}}} \tag{PK}$$

Furthermore, in this environment the optimal termination condition is *not needed* for the principal and agent to engage in successful relational contracting. Including the condition would be inefficient as it destroys future gains from trade when $q = q_L$.

3.2.4 Optimal Contracts when the Principal Privately Observes the Performance Signal

In the second environment, the principal privately observes q. Along with deciding the actual bonus to pay to the agent, she must also send unverifiable performance feedback to him during each period.[3] The principal's message, m, can either report that performance was high, $m =$ "$q = q_H$" or that performance was low, $m =$ "$q =$

[3]Note that the assumption that feedback is unverifiable is critical. Otherwise, contingent the performance feedback sent to the agent, the principal would not be able to renege on the corresponding promised performance bonus.

q_L". Therefore, the principal is restricted from sending an uninformative performance report but it does not have to be truthful.

Furthermore, the optional termination condition now depends on the message sent and *not* the value of q that the principal observes. If the principal includes the termination condition and reports that performance was low (m = "$q = q_L$") the relationship terminates with certainty after the current period. If she reports high performance (m = "$q = q_H$"), however, the relationship continues with certainty.[4] Once again, the principal is not required to include this condition implying that parties can continue to trade in future periods when the condition is not included and the principal reports low performance.

In order for the contract's informal agreements to be self-enforcing, the contract must have additional structure when the principal privately observes the performance signal. The issue stems from guaranteeing that the principal will uphold her promised performance bonus $b(q)$ for all realizations of q. In addition to the promise keeping constraint, which theoretically ensures that it is worthwhile for the principal to uphold the bonus due to the expected gain from future interactions, the principal now needs the incentive to report q truthfully. Otherwise, the principal can renege on $b(q)$ by strategically manipulating the performance report to her advantage. More specifically, the principal may understate the agent's performance after privately observing a good performance outcome but pay the agent $b(q_L)$. When this occurs, the principal reneges on $b(q_H)$ but the agent cannot detect that the principal is violating the agreement. To guarantee that all promised bonuses are honored, therefore, the principal needs to have the incentive to report q truthfully.

Levin (2003) illustrates that to ensure truthful reporting for both values of q, the principal must remain indifferent between reporting m = "$q = q_L$" or m = "$q = q_H$". When indifferent, the principal has no incentive to overstate or understate the agent's performance. Without additional structure on the contract, this requires that $b(q_L) = b(q_H)$ as this equates the principal's expected payoff across q_L and q_H realizations.

[4] Levin (2003) assumes that the principal specifies a value for the termination probability for each possible message that she can send to the agent. Here, I fix these probabilities to be 1 or 0.

The problem, however, is that this contract violates the agent's incentive compatibility (IC) constraint. In order to provide dual incentives for the agent to choose e_H and for the principal to report q honestly, the principal's expected payoff cannot vary with q while the agent's expected payoff must increase when performing well. Levin (2003)'s solution to this problem is to introduce the possibility of the relationship terminating with the likelihood of termination varying with the report, m, that the principal sends to the agent. Since $b(q_H)$ must be greater than $b(q_L)$ to incentivize the agent, the principal can remain indifferent between the two performance outcomes if the likelihood of termination increases when the principal reports that performance was low. Although the principal pays the agent a larger bonus for good performance outcomes, her future expected payoff is equal across the two reports since her expected gain from future interactions is smaller when reporting that performance was poor due to the increased likelihood of termination. Therefore, equating the principal's payoff across the two outcomes requires that the termination condition is included in the offer. As noted below, however, guaranteeing equality also depends upon the specific value of the payments p, $b(q_L)$, and $b(q_H)$ specified in the contract.

To characterize the set of optimal contracts under the private performance signal, the expected payoffs must account for the probability of the relationship terminating since the condition must be included in the offer to satisfy truth telling. These payoffs, described in terms of their average present-discounted values, for the principal and agent respectively are defined below:[5]

$$\tilde{\pi}_{e_L} = \bar{\pi} + \frac{(1-\delta)(\pi_{e_L} - \bar{\pi})}{1 - l\delta} \qquad \tilde{\pi}_{e_H} = \bar{\pi} + \frac{(1-\delta)(\pi_{e_H} - \bar{\pi})}{1 - h\delta}$$

$$\tilde{u}_{e_L} = \bar{u} + \frac{(1-\delta)(u_{e_L} - \bar{u})}{1 - l\delta} \qquad \tilde{u}_{e_H} = \bar{u} + \frac{(1-\delta)(u_{e_H} - \bar{u})}{1 - h\delta}$$

To ensure that the contract is efficient, all informal agreements are honored, and the principal reports q truthfully, the following six constraints must be satisfied:

[5] See the appendix for more details.

1. The principal's participation constraint ($\tilde{PC_P}$) makes it worthwhile to enter the contract:

$$\tilde{\pi}_{e_H} \geq \bar{\pi} \tag{$\tilde{PC_P}$}$$

2. The agent's participation constraint ($\tilde{PC_A}$) makes it worthwhile to enter the contract:

$$\tilde{u}_{e_H} \geq \bar{u} \tag{$\tilde{PC_A}$}$$

3. The agent's incentive compatibility constraint ($\tilde{IC}$) prevents the agent from shirking:

$$\underbrace{hb(q_H) + (1-h)b(q_L)}_{\substack{\text{expected discretionary payment} \\ \text{in current period with } e_H}} - c + \underbrace{\frac{\delta}{1-\delta} h(\tilde{u}_{e_H} - \bar{u})}_{\substack{\text{expected, discounted net gain from future} \\ \text{interactions with } e_H \text{ given truthful feedback}}} \geq$$

$$\underbrace{lb(q_H) + (1-l)b(q_L)}_{\substack{\text{expected discretionary payment} \\ \text{in current period with } e_L}} + \underbrace{\frac{\delta}{1-\delta} l(\tilde{u}_{e_L} - \bar{u})}_{\substack{\text{expected, discounted net gain from future} \\ \text{interactions with } e_L \text{ given truthful feedback}}} \tag{$\tilde{IC}$}$$

4. The principal's truth telling constraint ($\tilde{TT}$) provides her with the incentive to report feedback honestly:

$$\underbrace{\frac{\delta}{(1-\delta)}(\tilde{\pi}_{e_H} - \bar{\pi})}_{\substack{\text{expected, discounted net gain from} \\ \text{future interactions when reporting } q_H}} - b(q_H) = \underbrace{0}_{\substack{\text{expected, discounted net gain from} \\ \text{future interactions when reporting } q_L}} - b(q_L) \tag{$\tilde{TT}$}$$

5. Conditional on observing the high performance value, q_H, the principal's promise keeping constraint ($\tilde{PK_H}$) ensures that she will honor the discretionary bonus, $b(q_H)$:

Table 3.1.: Parameter values used in the experiment

Variable	Value	Description
δ	0.8	discount factor (probability of continuation)
$\bar{\pi}$	30	principal's outside option
$\bar{u}$	30	agent's outside option
q_L	30	low performance value
q_H	120	high performance value
l	0.25	probability of q_H given e_L
h	0.75	probability of q_H given e_H
c	10	cost of e_H
p	$\{-250, \cdots, 250\}$	bounds on fixed payment
$b(q_L)$, $b(q_H)$, B	$\{0, \cdots, 250\}$	bounds on promised and actual bonuses

$$\frac{\delta}{(1-\delta)}(\tilde{\pi}_{e_H} - \bar{\pi}) \geq b(q_H) \qquad (P\tilde{K}_H)$$

6. Conditional on observing the low performance value, q_L, the principal's promise keeping constraint $(P\tilde{K}_L)$ ensures that she will honor the discretionary bonus, $b(q_L)$:

$$0 \geq b(q_L) \qquad (P\tilde{K}_L)$$

To reiterate, $(\tilde{TT})$ makes the principal indifferent between reporting $m =$ "$q = q_L$" and $m =$ "$q = q_H$" since her expected payoff is equal across the two outcomes. Furthermore, note that $(P\tilde{K}_L)$ implies that $b(q_L) = 0$. If the principal knows the relationship will terminate after the current period, she has no incentive to pay the agent a performance bonus.

3.3 Experimental Design

Table 3.1 displays the parameters used in the experiment. Subjects participated in one of two treatments with the agent's performance value being either publicly observed by both parties or privately observed by the principal. Experimental sessions consisted of multiple rounds of trading between subjects who were assigned to be buyers (principals) or sellers (agents). At the start of a session, subjects were randomly assigned ID numbers and a role (buyer or seller), which remained fixed throughout the session. Subjects interacted via computers and remained anonymous. Each subject participated in only one session.

The stage-game interaction for each treatment follows the theoretical stage-game described earlier. At the beginning of each round, the buyer could make an offer, if she desired, to her randomly matched seller. Only the ID of one's trading partner was revealed to each subject. The terms of the offer were endogenously specified (in integers) by the principal within the provided bounds listed in the parameter table. An offer consisted of a four components: p, $b(q)$, a requested effort level, and the optional termination condition. To specify whether or not to include the optional termination condition, buyers simply had to click a "yes" or "no" button on the offer screen. If the seller received an offer, he then decided to accept or reject it. If accepted, the seller privately made his effort choice, framed as numbers with $e_L = 0$ and $e_H = 1$. After this, the value of q was observed by the party/parties that could observe it. In both treatments, the principal then decided the actual bonus B to pay to the agent. Additionally, if the signal was private, the principal decided the performance feedback ($m =$ "$q = q_L$" or $m =$ "$q = q_H$") to send to the agent. If no contract was entered, both parties received their outside options for that round.

At the end of each round, the screen displayed the subject's earnings (in points) for that round along with any choices that the subject made or observed. Points were converted into cash at the end of the experiment.

Each unique principal-agent pairing lasted for an indefinite number of rounds with δ serving as the probability of the pair being rematched at the start of the next period. This method is common for implementing laboratory infinitely repeated-game experiments ([Roth and Murnighan (1978), Dal Bó (2005)]).[6] Therefore, once a

[6]Note that this concept is *not* related to the endogenous termination condition outlined earlier. Here, δ describes the likelihood of paired subjects being exogenously rematched together in the next period.

round was completed, subjects faced an 80% ($\delta = 0.8$) probability of being rematched with the same partner in the next round. This was conveyed to the subjects with a randomly drawn integer between 1 and 10 displayed on their screens. If a 9 or 10 was drawn, subjects were exogenously matched with a different trading partner. Subjects could not, however, encounter an old trading partner in the rematching process. That is, a perfect strangers matching protocol for supergames was used. Note that if a pair had the endogenous termination clause in the contract in the previous period and the clause was triggered, then the pair could not engage in trade in future periods when rematched together.

With the exception of a pilot study for the private signal treatment, subjects were informed that the session would end according to one of the following rules: 1) if subjects encountered all unique trading partners, or 2) if the subjects had played at least 18 periods across all supergames, then the session would end once the current supergame randomly ended. To keep the number of rounds and the length of each interaction consistent across sessions, the random numbers were predetermined. Subjects were made aware of this decision before the experiment started and could verify that the random draws were consistent with the numbers displayed on their screen by opening a sealed envelope at the end of the experiment. Excluding the pilot, random rematchings occurred three times after rounds 3, 11, and 16, resulting in four supergames with unique trading partners.[7]

Sessions were conducted during Spring 2014 at Purdue University's Vernon Smith Experimental Economics Laboratory (VSEEL). Subjects were recruited from the VSEEL ORSEE database, which consisted of mostly undergraduate students that had participated in other experiments but not the specific treatments used in this study. Each session lasted roughly 2 hours consisting of instruction, a quiz, 2 practice periods, and 19 paid periods of trading. zTree was used to conduct the experiment

[7]The only revision to the pilot study was to extend the number of experimental rounds. Therefore, subjects in the pilot study were informed that the experiment would end if 1) subjects had encountered all unique trading partners (as before) or 2) the subjects had played at least 15 periods across all supergames. The pilot therefore ended after round 16 with three supergames completed. No other revisions were made to the experimental design, instructions, or zTree program.

(Fischbacher (2007)). Seven sessions of the public performance signal treatment were conducted with a total of 64 subjects. Nine sessions of the private performance signal treatment were conducted with a total of 80 subjects. Average earnings were equal to \$25.76 and \$27.25 for the public and private signal treatments respectively.

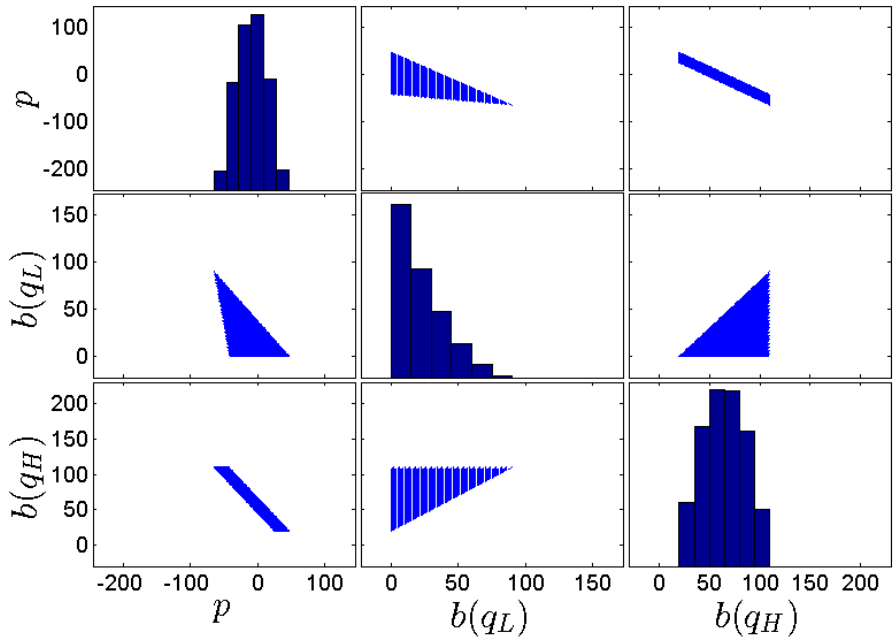
3.4 Comparative Statics

This section summarizes the comparative static predictions about the effects of making the performance signal private within the contractual relationship. I use numerical methods with the parameter values found in table 3.1 to derive the comparative statics.

Prediction 1*: The set of optimal contracts is large under the public performance signal while the set is empty under the private performance signal. Therefore, the model predicts that relational contracting should be much more successful when both parties observe the performance signal.*

Figure 3.1 provides a graphical summary of the relationship between p, $b(q_L)$, and $b(q_H)$ for offers that satisfy the four constraints—(PC_P), (PC_A), (IC) and (PK)—in the public signal treatment. 32,453 contract offers satisfy all of the constraints in this treatment. Furthermore, the principal can design a wide range of incentives that maximize her expected per-period payoff. Table 3.2 presents summary statistics for the 1,081 feasible contract offers that equate the agent's expected per-period payoff to his outside option, $\bar{u} = 30$. Therefore, the principal expects to earn $s_{e_H} - \bar{u} = 57.5$ when maximizing her per-period earnings.[8] In contrast, no contract offer satisfies all six constraints when the principal privately observes the performance signal. The truth telling constraint $(\tilde{TT})$ must be satisfied with equality making it difficult to ensure truthful feedback for both values of q as p, $b(q_L)$, and $b(q_H)$ are restricted to

[8]This result assumes that parties are self-interested. Previous contracting experiments, however, illustrate that more equitable distributions of the joint surplus are fairly common. The 32,453 feasible contracts contain many different distributions of the joint surplus including outcomes where the principal is held to her outside option $\bar{\pi}$. Relational contracting, therefore, can be supported under many different surplus distributions.



The figures summarize the set of contracts for the public performance signal treatment that satisfy the 4 necessary constraints: (PC_P), (PC_A), (IC), and (PK). Under the parameter values, 32,453 contract offers satisfy all of these constraints. Elements on the main diagonal are histograms (with frequencies on the y-axis unreported) while off diagonal elements illustrate the relationship between p, $b(q_L)$, and $b(q_H)$.

Fig. 3.1.: Set of optimal contracts for the public signal treatment

Table 3.2.: Summary statistics for contracts that maximize the principal's expected payoff under the public signal

	Average	Median	Min	Max
fixed payment, p	-27.25	-30	-65	25
low performance bonus, $b(q_L)$	30.49	27	0	90
high performance bonus, $b(q_H)$	79.51	83	20	110

Under the public signal, 1,081 feasible contract offers maximize the principal's payoff by holding the agent's expected payoff to the value of his outside option $\bar{u} = 30$.

be integers. As a result, the model predicts that all of the self-enforcing agreements cannot be simultaneously satisfied under the private signal, thereby hindering the success of the relational contract.

***Prediction 2**: Due to the lack of optimal contracts, the model predicts that the private performance signal should result in efficiency losses.*

Efficiency losses could occur through reduced trading volume such as the agent being less willing to accept contract offers or the contractual relationship breaking down over time due to violations of the informal agreements. Losses could also result from reduced effort provision.

3.5 Across Treatment Results

Table 3.3 provides a general overview of across treatment comparisons. The p-value for a two sided Mann-Whitney test, using individual session averages as the unit of observation, is also reported.

In terms of efficiency, contract acceptance rates are very close across treatments at 58% and 57%. Furthermore, for completed trades the proportion of high effort choices is not significantly different across treatments. When the performance signal is public, agents chose e_H 71% of the time, which is slightly larger than the 64% observed when the performance signal is private. Due to the lack of across treatment differences, changes in efficiency via trading volume or effort provision are minor.[9]

In relation to the theoretical framework, the theory predicts that the termination condition is only needed when performance is privately observed in order to induce the principal to provide truthful feedback. The data, however, illustrates that the termination condition is only used in roughly one-third of all contract offers when the signal is private.[10] Furthermore, the percentage of contracts using the condition does

[9]These results also hold true for linear probability models with multi-level random effects (at the session-principal-agent levels). To conserve space, I did not report these results. For contract acceptance, the dependent variable equals "1" if the agent accepted the offer and for effort provision, the dependent variable equals "1" if the agent chose e_H for the completed trade. With controls, the estimate on the private signal treatment dummy was -0.012 with a robust standard error clustered at the session level of 0.059 (p=0.832) for contract acceptance. With controls, the estimate on the private signal treatment dummy was -0.031 with a robust standard error clustered at the session level of 0.095 (p=0.749) for effort provision.

[10]One caveat, however, is that principals may not be including the condition because truth telling can not be guaranteed simultaneously for both q_L and q_H realizations. Another possible explanation for the infrequent use of the termination condition is that principals may be aggregating performance

not significantly differ across treatments with 26% of offers containing the condition under the public signal.

Finally, one might conjecture that principals may be more likely to renege on promised bonus payments when the agent cannot observe the performance value, especially due to the lack of theoretical incentives to report q truthfully. Contingent on the observed value of q, however, the proportion of trades where the actual bonus paid is at least as large as the bonus initially promised in the contract offer does not significantly vary across treatments. Principals honor their promised bonus, $b(q)$, 53% of the time with a public signal and 52% of the time with a private signal. Therefore, principals in general uphold their promises at remarkably similar rates.

3.5.1 Contract Structure

Although the previously discussed treatment differences are insignificant, the environments differ in terms of the types of compensation that the principal offers to pay to the agent. More specifically, when the performance signal becomes private, principals tend to lower the guaranteed, upfront payment p but increase the promised, discretionary bonus for high performance $b(q_H)$. For completed trades, the average value of p under a private signal is 18.1 experimental points which is significantly lower ($p = 0.01$) than the average value of 31.7 points under the public signal. In contrast, the average value of $b(q_H)$ for completed trades increases by 11 points when moving from the public to the private signal and this difference is marginally significant ($p = 0.10$). These findings suggest that principals become more reluctant to offer agents payment guarantees under the private signal, thereby increasing their discretion.

evaluations across periods and, therefore, are unwilling to invoke the termination condition until the agent has performed poorly after several rounds of interaction (see Levin (2003) and Fuchs (2007)). The data, however, suggests that this is not occurring as there is no clear relationship between the agent's past performance and the principal's use of the termination condition. For instance, the correlation between the average value of q in the previous periods and the use of the termination condition in the current period is -0.08 for the offers made in the private signal treatment.

Table 3.3.: Across treatment comparison of contract properties

	Public Signal	Private Signal	Mann-Whitney p-value
All Offers			
Fraction offers accepted	.58	.57	.71
Fraction offers with termination condition	.26	.34	.31
Fraction of accepted offers with termination condition	.24	.31	.53
Average price (p) offered	21.6	8.9	.03
Average low bonus ($b(q_L)$) offered	17.6	26.0	.96
Average high bonus ($b(q_H)$) offered	48.4	65.3	.12
Completed Trades			
	Public Signal	Private Signal	Mann-Whitney p-value
Fraction high effort (e_H) choices	.71	.64	.83
Fraction of promised bonuses honored	.53	.52	.92
Fraction honored when $q = q_L$	.48	.54	.96
Fraction honored when $q = q_H$	.57	.51	.96
Fraction honored if termination condition included	.35	.38	.75
Average price (p)	31.7	18.1	.01
Average low bonus ($b(q_L)$) offered	14.7	16.9	.64
Average bonus paid when $q = q_L$	2.2	3.3	.15
Average high bonus ($b(q_H)$) offered	43.3	54.3	.10
Average bonus paid when $q = q_H$	25.3	29.0	.37

Two sided Mann-Whitney p-values reported. Session averages used (7 sessions for public signal treatment, 9 sessions for private signal treatment).

To study how the principal's degree of discretion changes across the two environments, I follow a similar approach used in the first chapter by defining the discretionary payment ratio (DPR) which falls in the interval $[0, 1]$:

$$DPR = \begin{cases} \frac{max\{b(q_L),b(q_H)\}}{max\{b(q_L),b(q_H)\}+p}, & if\, p > 0 \\ 1 & if\, p \leq 0 \end{cases}$$

Contingent on the largest promised bonus stated in the contract offer, the DPR denotes the fraction of total pay promised to the agent that is discretionary rather than guaranteed upfront. When DPR equals 1, this implies that $p \leq 0$ so that all of the agent's pay is discretionary. Likewise, when DPR equals 0, the agent receives all of his compensation upfront through the fixed payment (i.e. a gift exchange contract). Therefore, larger values of DPR indicate that the principal has more discretion.

Figure 3.2 presents the distribution of the values of DPR for completed trades within each treatment. The average value when the signal is public is 0.54, which is lower than the value of .68 when the signal is private. Furthermore, the distribution is clearly skewed left under the private signal, indicating that the treatment favors higher values of DPR, while the distribution is relatively more symmetrically distributed under the public signal. To test for significant differences across treatments, table 3.4 presents the results of both a multi-level random effects regression (at the session-principal-agent levels) and a Tobit regression where the dependent variable equals the value of the DPR for the completed trade. Furthermore, robust standard errors are clustered at the session level. The coefficient on the private signal treatment dummy, which takes a value of "1" if the principal privately observes the value of q, is positive and significant for both specifications with estimates of 0.11 and 0.12.[11] As a result, the agent is exposed to more strategic uncertainty when he cannot observe his

[11] If rounds 1-3 are excluded from the analysis, the treatment estimates become marginally insignificant with p=0.12 although the values of the estimates are similar. Furthermore, when analyzing all contract offers, instead of completed trades only, the results are similar.

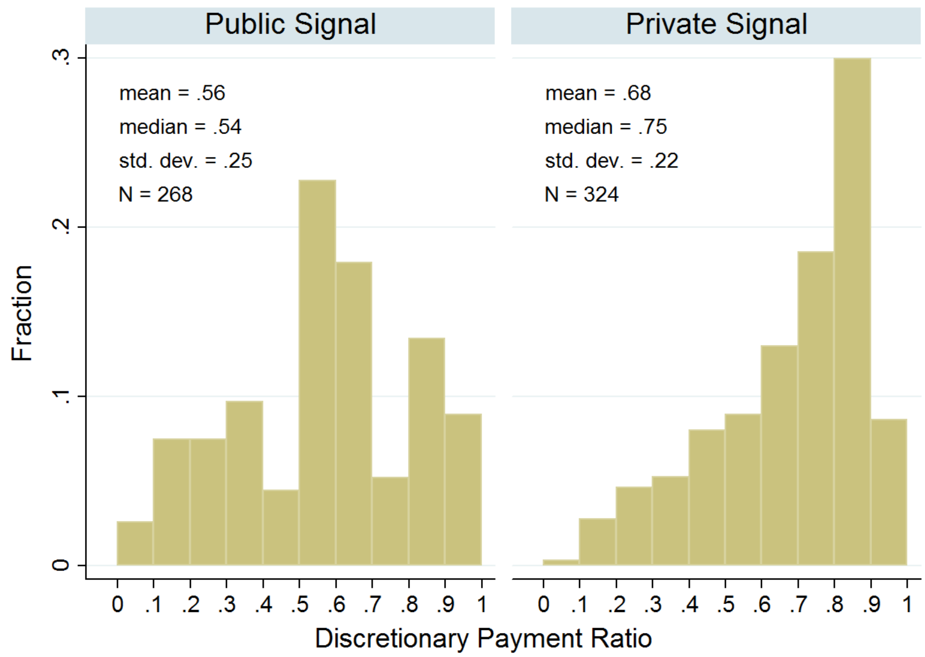


Fig. 3.2.: Discretionary payment ratio (DPR) histograms for completed trades by treatment

performance signal since a relatively larger of proportion of his promised compensation comes from the discretionary bonus.

***Main Result 1**: The discretionary payment ratio (DPR) significantly increases when the principal privately observes the performance signal. Principals therefore restructure contracts which increase their discretion, thereby exposing agents to more strategic uncertainty.*

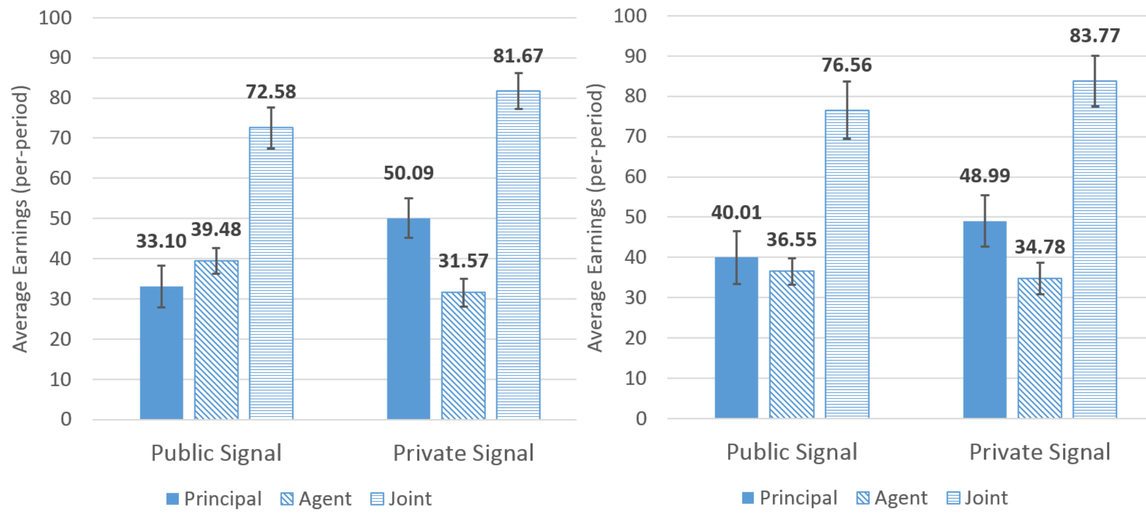
3.5.2 Payoffs and Surplus for Completed Trades

Knowing that principals increase their discretion in the private signal treatment, I now explore how efficiency and the earnings distribution change across treatments for completed trades. Figure 3.3 displays the average per-period earnings for principals and agents. Their average joint earnings, which is simply the sum of the two parties' payoffs, are also displayed. For all rounds, the principal's average payoff is roughly

Table 3.4.: Discretionary payment ratio (DPR) regressions for completed trades

		Tobit Estimates
	(1)	(2)
Private signal dummy	0.110**	0.122*
	(0.0514)	(0.0650)
$\frac{1}{\text{Period}}$	-0.0559	-0.0452
	(0.0397)	(0.0513)
Length of relationship	-0.00105	0.00343
	(0.00446)	(0.00667)
Constant	0.579***	0.563***
	(0.0314)	(0.0473)
Observations	592	592

* $p < 0.10$, ** $p < 0.05$, $^{***}p < 0.01$. Regression (1) estimated using multi-level random effects (at the session-principal-agent levels) with robust standard errors clustered at the session level. Regression (2) is a Tobit regression with robust standard errors clustered at the session level. *Private signal dummy* takes a value of 1 if the observation belongs to the treatment where the principal privately observed q. *Length of relationship* indicates the number of periods in which the pair of subjects have interacted with each other.



(a) All rounds

(b) Last 10 rounds only

The earnings are expressed in experimental points. 95% confidence intervals for the mean are also depicted. For all rounds, the number of observations are 268 and 324 for the public and private signal treatments respectively. For the last 10 rounds, the number of observations are 137 and 159 for the public and private signal treatments respectively.

Fig. 3.3.: Average per-period earnings for completed trades

17 points higher when the signal is private. In contrast, the agent's average payoff is roughly 8 points lower under the private signal. These two findings suggest two things when the signal becomes private: 1) principals capture a larger share of the joint earnings and 2) joint earnings increase by about 9 points on average per-period. When excluding the analysis to the last 10 rounds of play, the results are somewhat similar although the surplus distribution tends to be less extreme. Now, under the private signal the principal's average earnings increase by about 9 points while the agent's average earnings decrease by only 1.75 points.

As established earlier, the frequency in which agents choose e_H is not significantly different across treatments. In fact, the percentage of high effort choices is 7% *higher* under the public signal. This implies that the increase in average joint earnings for completed trades under the private signal must be driven by the noisy mapping between the agent's effort choice e and his performance realization q. Indeed, when the signal is public, e_L results in q_H 16% of the time while e_H results in q_H 71% of the time. For the private signal, q_H is the result 32% and 83% of the time for e_L and e_H respectively. Therefore, the agents experience a disproportional amount of "good luck" in the private signal treatment, thereby increasing average joint earnings.[12] Efficiency gains are therefore driven by exogenous factors to the contracting relationship.

Table 3.5 presents the results of multi-level random effects regressions (at the session and principal levels) in order to test for significant differences in per-period earnings across treatments.[13] In order to control for extreme observations that occurred in early rounds of the experiment, rounds 1 through 3 are excluded from the analysis. The dependent variables analyzed are the earnings of the principal, the agent, and their joint earnings. The estimates on the private signal treatment dummy are positive and significant for both principal payoffs and joint payoffs with estimates of 12.17 and 8.58 respectively. In contrast, the estimate for agent payoffs is slightly negative and insignificant. Therefore, the "good luck" driving the increase in efficiency across treatments benefits the principal but has no significant effect on the agent's payoff.

Main Result 2: *For completed trades, the principal's earnings significantly increase when the performance signal is private. The agent's earnings, however, are not significantly different across treatments. Therefore, principals act more advantageously by capturing a larger share of the joint earnings under the private signal.*

Although the agent's payoff, in the aggregate, does not significantly differ across treatments, are there certain situations that lead to significant differences in the agent's welfare? To explore this issue further, figure 3.4 displays the agent's average earnings across treatments for relevant subsets of the data. Only completed trades after round 3 are considered in the calculations. Interestingly, the agent's

[12]When aggregating the data across treatments, the mapping between e and q is much more in line with the expected values. For e_L, q_H results 25.3% of the time and for e_H, q_H results 77.4% of the time.

[13]I encountered computation issues when attempting to include both principal and agent random effects. The results, however, are robust when including agent random effects instead of principal random effects.

Table 3.5.: Regression estimates of the impact of the private signal on per-period payoffs and surplus for completed trades (rounds 4-19)

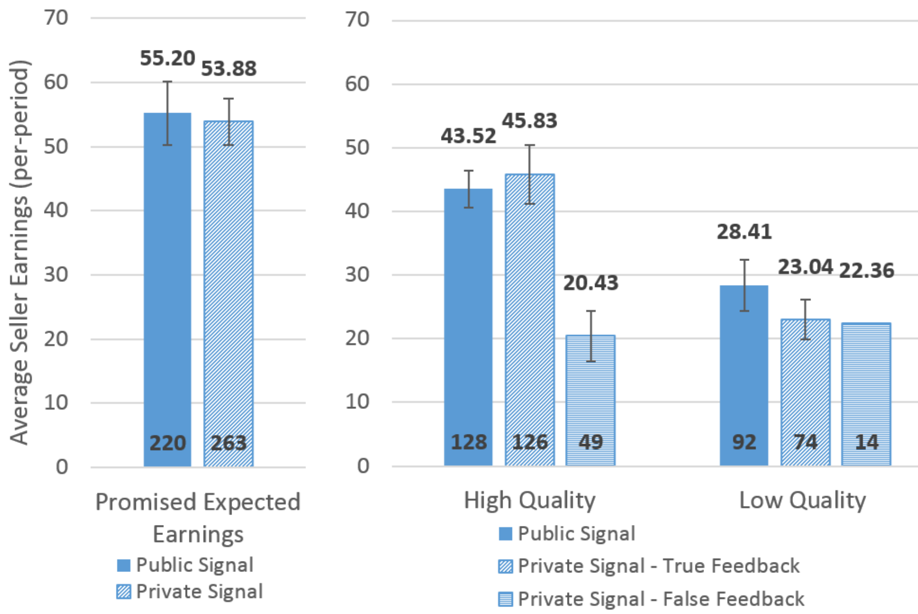
	(1) Principal Payoffs	(2) Agent Payoffs	(3) Joint
Private signal dummy	12.17***	-3.247	8.581**
	(3.938)	(2.669)	(3.885)
$\frac{1}{\text{Period}}$	-17.95	-6.933	-30.26
	(25.92)	(20.36)	(25.94)
Constant	39.25***	36.58***	78.12***
	(4.485)	(2.414)	(4.489)
Observations	483	483	483

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions estimated using multi-level random effects (at the session and principal levels) with robust standard errors clustered at the session level. Observations from periods 1-3 excluded as they alter the significance of the private signal dummy on the agent's payoff. *Private signal dummy* takes a value of 1 if the observation belongs to the treatment where the principal privately observed q.

promised expected earnings, which are calculated under the assumption that all informal agreements specified in the contract are upheld, are similar across treatments. That is, assuming that 1) the agent picks the principal's requested effort level and 2) the principal honors the corresponding promised bonus for the observed value of q, agents expect to earn similar amounts across treatments. Principals, however, have increased discretion in the private signal treatment due to lower upfront payments. They can also send false performance feedback to the agents. This suggests that agents may be worse off when the principal under reports performance when $q = q_H$ and reneges on $b(q_H)$.

Figure 3.4 also displays the agent's actual payoff, on average, across high and low realizations of q. For the private signal treatment, the data is separated based on whether the principal provided truthful or false feedback about the realized value of q. Independent of the feedback provided for q_L realizations, average payoffs are fairly close to one another across treatments. The q_H realizations, however, depict a different story. In the private signal treatment, agents on average earn 2.3 points more when the principal provides truthful feedback. In contrast, if the principal reports that $q = q_L$ the agent's average payoff drops by roughly 23 points. This finding suggests that the agent's welfare is reduced whenever the principal under reports the good performance signal. Table 3.6 confirms these findings using multi-level random effects regressions (at the session and principal levels) where the dependent variable equals the agent's payoff for the different scenarios described above. All estimates on the private signal dummy are insignificant except for the case where q_H realizations in the public signal treatment are compared to the subset of q_H realizations in the private signal treatment where the principal falsely reported that $q = q_L$. The estimate predicts a significant decrease in the agent's payoff by roughly 16 points.

This significant payoff reduction for the agent implies that principals more frequently shirk on the promised bonus for high performance, $b(q_H)$, when they under report the performance signal. An easy way to analyze the extent to which principals shirk on $b(q_H)$ is to look at the fraction of the promised bonus paid to the agent, $\frac{B}{b(q_H)}$,



95% confidence intervals for the mean reported. The numbers at the base each bar chart reflect the number of observations for that category. Promised expected earnings calculated under the assumption that the agent picks the principal's requested effort level and that the agent pays the promised bonus for the realized value of q.

Fig. 3.4.: Average agent earnings comparisons for completed trades (rounds 4-19)

Table 3.6.: Regression estimates of the impact of the private signal on agent payoffs across scenarios (rounds 4-19)

	(1)	(2)	(3)	(4)	(5)
	Expected Promised Payoff	Actual Payoff			
		q_H realized		q_L realized	
Private Signal:		true report	q_L reported	true report	q_H reported
Private signal dummy	-4.008	4.529	-15.98***	-6.684	-4.909
	(8.974)	(5.392)	(5.562)	(4.569)	(12.80)
$\frac{1}{\text{Period}}$	28.03	15.11	33.45**	-5.963	-51.60
	(26.37)	(14.40)	(16.03)	(20.90)	(61.86)
Constant	57.43***	37.72***	35.08***	29.99***	35.34***
	(8.496)	(3.640)	(3.414)	(4.133)	(8.057)
Observations	483	254	177	166	106

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions estimated using multi-level random effects (at the session and principal levels) with robust standard errors clustered at the session level. *Private signal dummy* takes a value of 1 if the observation belongs to the treatment where the principal privately observed q.

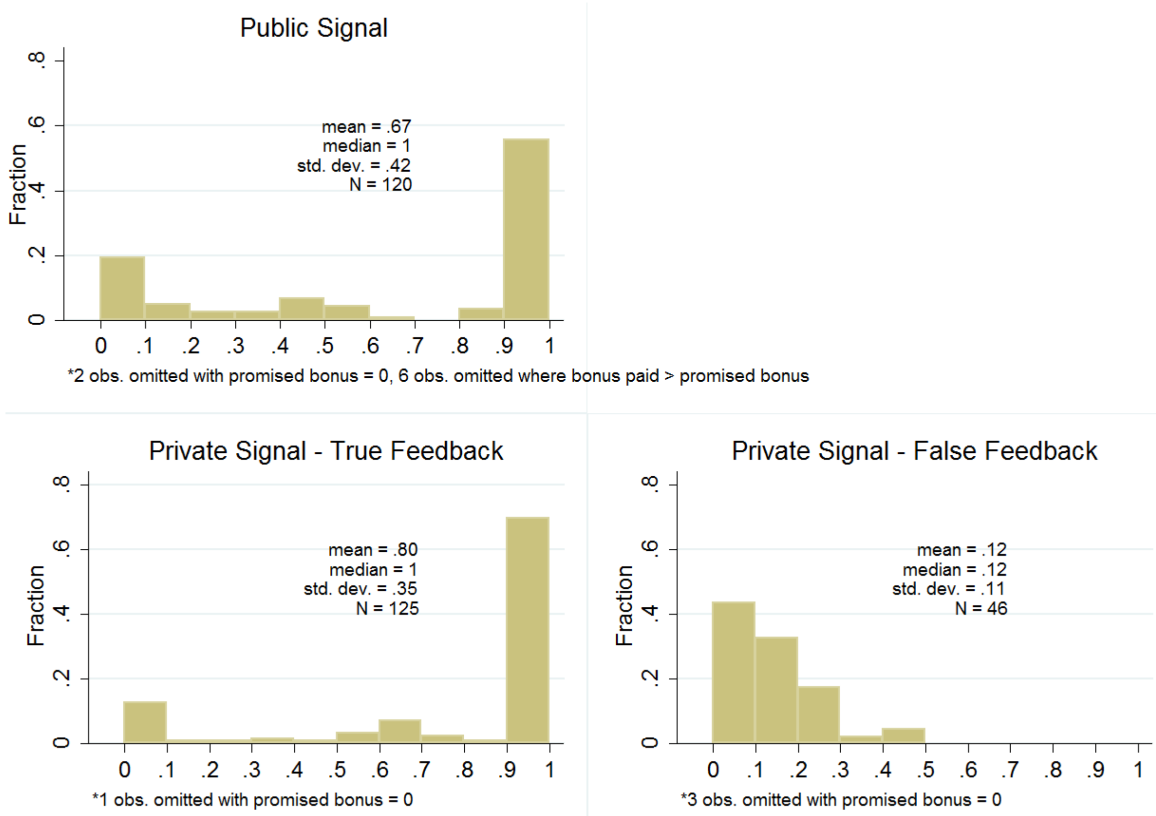


Fig. 3.5.: Fraction of promised bonus paid ($\frac{B}{b(q_H)}$) across treatments for q_H realizations (rounds 4-19)

where B represents the actual bonus paid. Figure 3.5 graphs distributions of $\frac{B}{b(q_H)}$ for q_H realizations where $b(q_H) > 0$ so that the principal has an option to shirk on her promise. Under the public signal or the private signal with truthful feedback, principals tend to honor their promises. The median fraction for both cases is 1, indicating that principals honor $b(q_H)$ a majority of the time. In contrast, principals shirk on all promises when they under report the private signal and the median observation pays 12% of $b(q_H)$.[14]

Main Result 3: *For completed trades, the agent's earnings are significantly reduced under the private signal in situations where the agent performs well (i.e. q_H realized)*

[14]Given these findings, the regression results concerning the across treatment impact on the agent's welfare in table 3.6 are similar when dividing the data based on whether or not the principal honored the promised bonus instead of dividing it based on the principal's feedback.

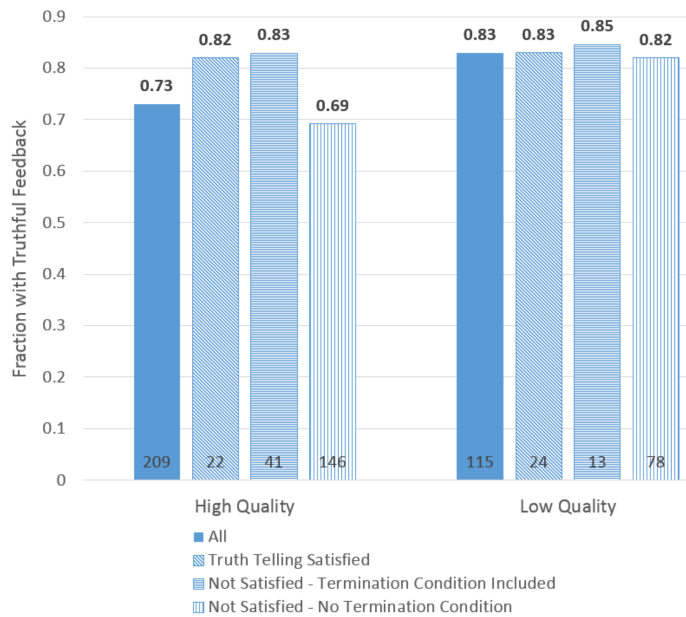
but the principal falsely reports that performance was poor. In such cases, principals shirk on their promised bonus for good performance.

3.6 Feedback in the Private Signal Treatment

Given these across treatment results, I now explore one potential explanation for why contracting remains just as successful in the private signal treatment despite the lack of contracts that theoretically support relational contracting. Within the treatment of interest, principals tend to provide honest performance feedback, which implies that the truth telling constraint ($\tilde{TT}$) may not be necessary to facilitate contractual relationships. Figure 3.6 presents the fraction of truthful performance reports separately for q_H and q_L realizations. Overall, principals report q truthfully 76.5% of the time. This percentage is slightly higher for q_L realizations at 83% compared to 73% of q_H realizations.

Furthermore, contingent upon the observed value of q, the truth telling incentives found in the model fail to elicit sizable increases in truthful performance feedback. To see this, ($\tilde{TT}$) can be relaxed so that the principal has the incentive to either report that performance was high or that performance was low. Regardless of the true value of the performance signal, the model predicts that the principal will report $m =$ "$q = q_H$" if $\frac{\delta}{(1-\delta)}(\tilde{\pi}_{e_H} - \bar{\pi}) - b(q_H) > -b(q_L)$ and the termination condition is included in the contract. Similarly, when the inequality is reversed the model predicts that the principal will report $m =$ "$q = q_L$".

As illustrated in figure 3.6, however, subjects tend to report q truthfully even when the relaxed ($\tilde{TT}$) is violated. When truth telling is predicted for high performance outcomes, the percentage of truthful reports is 82%. If the termination condition is included in the offer but the relaxed truth telling constraint predicts that q_L will be reported, however, the percentage of q_H reports is similar at 83%. Finally, when the termination condition is not included in the offer, the fraction of truthful reports falls to 69%. Therefore, satisfying the termination condition alone results in a small



The numbers at the base of each bar chart reflect the number of observations for that category. Truth telling is satisfied if: 1) the optional termination condition is included in the contract and 2) contingent upon the true value of q, the relaxed truth telling constraint is satisfied. For q_H realizations the relaxed constraint is $\frac{\delta}{(1-\delta)}(\tilde{\pi}_{e_H} - \bar{\pi}) - b(q_H) \geq -b(q_L)$. For q_L realizations the relaxed constraint is $\frac{\delta}{(1-\delta)}(\tilde{\pi}_{e_H} - \bar{\pi}) - b(q_H) \leq -b(q_L)$.

Fig. 3.6.: Proportion of truthful performance reports by performance level

increase in truthful feedback for high performance realizations independent of whether or not the relaxed $(\tilde{TT})$ constraint is satisfied. For low performance outcomes, there is a negligible difference in the fraction of truthful reports across all subsets of the data. Principals tend to report q_L truthfully regardless of whether the termination condition is included in the offer and/or the relaxed $(\tilde{TT})$ is satisfied. Therefore, the general preference for truth-telling does not seem to be driven by the theoretical framework.

Now I look the reporting behavior of individual principals to see if the data supports any general trends in truth telling. I exclude six principals who completed less than five trades from the analysis leaving 34 principals. In terms of the performance feedback given the principals can be classified into 5 groups, which are summarized in table 3.7. 18% of subjects always report q truthfully. The largest group of subjects, at 38%, always report q_L truthfully but sometimes under report q_H. In contrast,

Table 3.7.: Description and distribution of performance feedback classifications

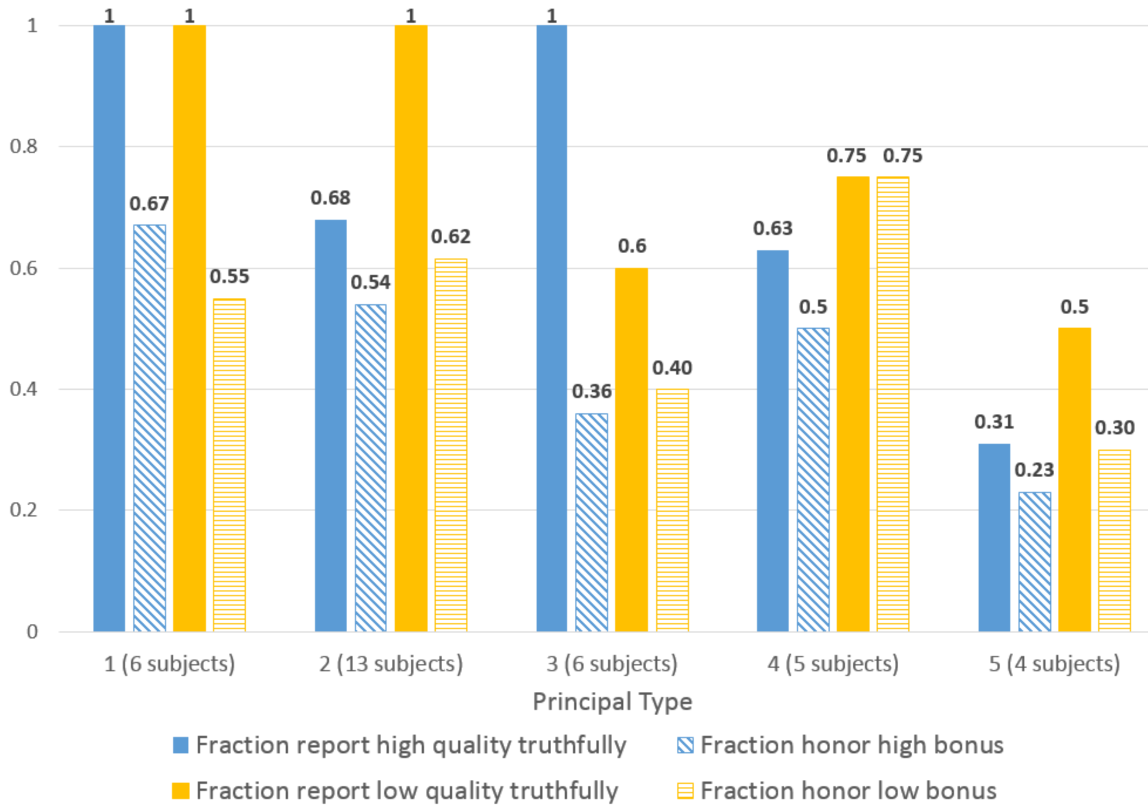
Description	**# subjects**	**%**
(1) always report q truthfully	6	18%
(2) always report q_L truthfully but sometimes under report q_H	13	38%
(3) always report q_H truthfully but sometimes over report q_L	6	18%
(4) misreport both q_L and q_H but mostly truthful	5	15%
(5) usually misreport q	4	11%

18% of subjects always report q_H truthfully, but sometimes overstate q_L realizations. Finally, there is a subset of participants that misreport both q_L and q_H realizations.

Given these classifications, do these groups of principals renege on promised bonuses at different rates, thereby alluding to certain types of strategic behavior? Figure 3.7 summarizes this information for each principal type described in table 3.7. For q_H realizations 4 out of the 5 types report high performance truthfully more than 60% of the time. Furthermore, figure 3.5 illustrates that when principals under report performance, they shirk on $b(q_H)$ at much higher frequencies. Since principals tend to report q_H honestly, however, no noticeable pattern in terms of the principal's propensity to honor $b(q_H)$ appears across subject types. Similarly, most principal types tend to report q_L truthfully and there are no noticeable trends between the rate at which types report q_L honestly and the rate at which they honor $b(q_L)$.

Furthermore, previous studies have found that subjects can be averse to lying, which may explain why the truth telling incentives of the model are unnecessary for performance feedback to be accurate.[15] For instance, Gneezy (2005) compares preferences over two types of monetary allocations between two players. Only player one, however, observed the true value of the allocations before one was chosen. The first allocation (i.e. generous) gave player two a relatively larger payoff while the

[15]Besides Gneezy (2005), see Hurkens and Kartik (2009), Sánchez-Pagés and Vorsatz (2009), and Fischbacher and Föllmi-Heusi (2013).



Principal types correspond to the descriptions found in table 3.7.

Fig. 3.7.: Frequency of truthful reporting and honoring bonuses by the principal's performance feedback classification

other allocation (i.e. selfish) gave player one a relatively larger payoff. In the control treatment, player one simply picked between the two allocations. In the second, player one had to send a message to player two who then chose the allocation. Enticing player two to pick the selfish allocation, however, required the sender to lie and falsely claim that it would increase player two's payoff. Gneezy (2005) finds that a significantly higher proportion of subjects choose the selfish option in the control treatment compared to sending the deceptive message, thereby providing evidence of lying aversion. Furthermore, Hurkens and Kartik (2009) analyze the data in more detail and find that it supports an outcome where 50% of subjects never lie while the other half lie whenever they prefer the outcome from lying over the one from telling the truth.

In comparison to Hurkens and Kartik (2009)'s finding, this data supports a less obvious preference for truth telling or lying. Only one-fifth of the subjects always report q honestly and these subjects do not uniformly honor the promised bonus. Furthermore, subjects do not necessarily lie when doing so provides them an opportunity to increase their immediate payoff. For instance, subjects become less transparent when reneging on $b(q_H)$ by under reporting q_H but paying $b(q_L)$. The data, however, suggests that principals in general tend to provide honest performance feedback, which may be explained by lying aversion. This, however, is not the only potential explanation. Subjects may not misreport q in every period if they believe that doing so will hinder successful contracting in future periods. Therefore, other motivations such as reputational effects may also support truth telling.

3.7 Conclusion

This paper has studied the implications of making the agent's performance measure disputable within a relational contracting environment. In terms of efficiency, across treatment changes in contract acceptance and the agent's effort provision are minor. Therefore, pairwise efficiency is unaffected as a result of the subjects' actions.

Pairwise earnings for completed trades, however, are significantly higher when the principal privately observes the performance signal due to the noisy mapping between the agent's effort choice and his performance value. Agents in that treatment realize a relatively higher amount of good performance signals, resulting in a larger surplus to be split between the principal and the agent. The larger surplus, however, benefits the principal only as the agent's earnings for completed trades are not significantly different across treatments while the principal's earnings significantly increase under the private signal.

Furthermore, principals restructure contractual payments across treatments by increasing their discretionary latitude when the performance signal is private, thereby shifting more strategic uncertainty onto the agents. The agent's welfare, however is only significantly reduced across treatments when he performs well and the principal understates the performance value. Sending an inaccurate performance report, however, only happens in roughly one-quarter of the trades. In general, principals tend to provide honest performance feedback to the agents, which cannot be explained by the predicted truth telling incentives outlined in the theoretical framework.

The results of this experiment suggest that subjective relationships are no worse off under relational contracting as relationships where the agent's performance is objective or easy to measure. Furthermore, the agent's welfare may not be significantly reduced in environments where the principal has access to important private information related to the agent's performance level. The experimental design, however, was not able to identify the specific reasons as to why principals generally report performance feedback honestly. Future work must investigate the underlying factors that promote truth telling within the relational contracting framework.

Appendix: Deriving Payoffs under the Private Performance Signal

To characterize the set of optimal contracts, the expected payoffs, which depend on e, the payments p and $b(q)$, and the principal's message m, must be derived under the private performance signal. In a more general contract the principal specifies a probability that the relationship terminates, $1 - \alpha\{m(q)\}$, contingent upon the message she sends to the agent. Denote α_L as the probability of the relationship *continuing* when the principal sends the message $m =$ "$q = q_L$" while α_H denotes the probability of the relationship continuing with the message $m =$ "$q = q_H$".

Using this notation define the present-discounted payoffs, expressed as a per-period average, for the principal as:

$$\tilde{\pi} = (1-\delta)E_q[q - b(q) - c(e)|e] + \delta\bar{\pi} + \delta E_q[\alpha\{m(q)\}|e](\tilde{\pi} - \bar{\pi})$$
$$\tilde{\pi}_{e_L} = (1-\delta)\{lq_H + (1-l)q_L - [p + lb(q_H) + (1-l)b(q_L)]\} + \delta\bar{\pi} + \delta[l\alpha_H + (1-l)\alpha_L](\tilde{\pi}_{e_L} - \bar{\pi})$$
$$\tilde{\pi}_{e_H} = (1-\delta)\{hq_H + (1-h)q_L - [p + hb(q_H) + (1-h)b(q_L)]\} + \delta\bar{\pi} + \delta[h\alpha_H + (1-h)\alpha_L](\tilde{\pi}_{e_H} - \bar{\pi})$$

Solving the recursive payoffs gives:

$$\tilde{\pi}_{e_L} = \bar{\pi} + \frac{(1-\delta)\{lq_H + (1-l)q_L - [p + lb(q_H) + (1-l)b(q_L)] - \bar{\pi}\}}{1 - \delta[l\alpha_H + (1-l)\alpha_L]} = \bar{\pi} + \frac{(1-\delta)\{\pi_{e_H} - \bar{\pi}\}}{1 - \delta[l\alpha_H + (1-l)\alpha_L]}$$
$$\tilde{\pi}_{e_H} = \bar{\pi} + \frac{(1-\delta)\{hq_H + (1-h)q_L - [p + hb(q_H) + (1-h)b(q_L)] - \bar{\pi}\}}{1 - \delta[h\alpha_H + (1-h)\alpha_L]} = \bar{\pi} + \frac{(1-\delta)\{\pi_{e_H} - \bar{\pi}\}}{1 - \delta[h\alpha_H + (1-h)\alpha_L]}$$

Similarly, for the agent define the payoffs as:

$$\tilde{u} = (1-\delta)E_q[p + b(q) - c(e)|e] + \delta\bar{u} + \delta E_q[\alpha\{m(q)\}|e](\tilde{u} - \bar{u})$$
$$\tilde{u}_{e_L} = (1-\delta)[p + lb(q_H) + (1-l)b(q_L)] + \delta\bar{u} + \delta[l\alpha_H + (1-l)\alpha_L](\tilde{u}_{e_L} - \bar{u})$$
$$\tilde{u}_{e_H} = (1-\delta)[p + hb(q_H) + (1-h)b(q_L) - c] + \delta\bar{u} + \delta[h\alpha_H + (1-h)\alpha_L](\tilde{u}_{e_H} - \bar{u})$$

Solving the recursive payoffs gives:

$$\tilde{u}_{e_L} = \bar{u} + \frac{(1-\delta)[p + lb(q_H) + (1-l)b(q_L) - \bar{u}]}{1 - \delta[l\alpha_H + (1-l)\alpha_L]} = \bar{u} + \frac{(1-\delta)[u_{e_L} - \bar{u}]}{1 - \delta[l\alpha_H + (1-l)\alpha_L]}$$
$$\tilde{u}_{e_H} = \bar{u} + \frac{(1-\delta)[p + hb(q_H) + (1-h)b(q_L) - c - \bar{u}]}{1 - \delta[h\alpha_H + (1-h)\alpha_L]} = \bar{u} + \frac{(1-\delta)[u_{e_H} - \bar{u}]}{1 - \delta[h\alpha_H + (1-h)\alpha_L]}$$

Adding the principal and agent's payoffs give the corresponding formulas for surplus:

$$\tilde{s} = (1-\delta)E_q[q - c(e)|e] + \delta\bar{s} + \delta E_q[\alpha\{m(q)\}|e](\tilde{s} - \bar{\pi})$$
$$\tilde{s}_{e_L} = (1-\delta)\{lq_H + (1-l)q_L\} + \delta\bar{s} + \delta[l\alpha_H + (1-l)\alpha_L](\tilde{s}_{e_L} - \bar{s})$$
$$\tilde{s}_{e_H} = (1-\delta)\{hq_H + (1-h)q_L - c\} + \delta\bar{s} + \delta[h\alpha_H + (1-h)\alpha_L]](\tilde{s}_{e_H} - \bar{s})$$

$\tilde{s}_{e_L}$ and $\tilde{s}_{e_H}$ are solved in a similar manner.

$$\tilde{\pi}_{e_L} = \bar{\pi} + \frac{(1-\delta)(\pi_{e_L} - \bar{\pi})}{1-\delta[l\alpha_H + (1-l)\alpha_L]} \qquad \tilde{\pi}_{e_H} = \bar{\pi} + \frac{(1-\delta)(\pi_{e_H} - \bar{\pi})}{1-\delta[h\alpha_H + (1-h)\alpha_L]}$$
$$\tilde{u}_{e_L} = \bar{u} + \frac{(1-\delta)(u_{e_L} - \bar{u})}{1-\delta[l\alpha_H + (1-l)\alpha_L]} \qquad \tilde{u}_{e_H} = \bar{u} + \frac{(1-\delta)(u_{e_H} - \bar{u})}{1-\delta[h\alpha_H + (1-h)\alpha_L]}$$
$$\tilde{s}_{e_L} = \bar{s} + \frac{(1-\delta)(s_{e_L} - \bar{s})}{1-\delta[l\alpha_H + (1-l)\alpha_L]} \qquad \tilde{s}_{e_H} = \bar{s} + \frac{(1-\delta)(s_{e_H} - \bar{s})}{1-\delta[h\alpha_H + (1-h)\alpha_L]}$$

An optimal contract provides the incentive for the agent to choose e_H. Furthermore, self-enforcement implies that the principal has the incentive to provide honest performance feedback and to pay the corresponding performance bonus $b(q)$. Therefore, the following six constraints must be satisfied:

1. The principal's participation constraint makes it worthwhile to enter the contract:

$$\tilde{\pi}_{e_H} \geq \bar{\pi} \tag{$\tilde{PC_P}$}$$

2. The agent's participation constraint makes it worthwhile to enter the contract:

$$\tilde{u}_{e_H} \geq \bar{u} \tag{$\tilde{PC_A}$}$$

3. The agent's incentive compatibility constraint prevents the agent from shirking:

$$hb(q_H) + (1-h)b(q_L) - c + \frac{\delta}{1-\delta}[h\alpha_H + (1-h)\alpha_L](\tilde{u}_{e_H} - \bar{u}) \geq$$
$$lb(q_H) + (1-l)b(q_L) + \frac{\delta}{1-\delta}[l\alpha_H + (1-l)\alpha_L](\tilde{u}_{e_L} - \bar{u}) \qquad (\tilde{IC})$$

4. The principal's truth telling constraint provides her with the incentive to report feedback honestly:

$$\frac{\delta}{(1-\delta)}\alpha_H(\tilde{\pi}_{e_H} - \bar{\pi}) - b(q_H) = \frac{\delta}{(1-\delta)}\alpha_L(\tilde{\pi}_{e_H} - \bar{\pi}) - b(q_L) \qquad (\tilde{TT})$$

5. Conditional on observing the high performance value, q_H, the principal's promise keeping constraint ensures that she will honor the discretionary bonus, $b(q_H)$:

$$\frac{\delta}{(1-\delta)}\alpha_H(\tilde{\pi}_{e_H} - \bar{\pi}) \geq b(q_H) \qquad (P\tilde{K}_H)$$

6. Conditional on observing the low performance value, q_L, the principal's promise keeping constraint ensures that she will honor the discretionary bonus, $b(q_L)$:

$$\frac{\delta}{(1-\delta)}\alpha_L(\tilde{\pi}_{e_H} - \bar{\pi}) \geq b(q_L) \qquad (P\tilde{K}_L)$$

Appendix: Instructions for the Private Performance Signal Treatment

You can earn money during this experiment, with the exact amount depending on the decisions you make during the experiment. Your experimental income is calculated in points, which will be converted into cash at the rate of: $1 = 30 points. We will start you off with a balance of 150 points ($5).

All written information you received from us is for your private use only. You are not allowed to pass over any information to other participants in the experiment. Talking during the experiment is not permitted. Violations of these rules may force us to stop the experiment.

General Information

This experiment is about how people buy and sell goods for which quality matters. Participants are divided into two groups: half will be buyers and the other half sellers. Then a trading period will start in which a buyer and seller will trade one unit of a good that can vary in quality. The price agreed upon between the buyer and seller and the quality of the good traded will determine how much money each party makes in that period. There will many trading periods throughout the course of this experiment.

Who will you trade with? The computer will randomly match each participant in the room with another participant to form a buyer-seller pairing. You will be informed whether you are the buyer or seller in your pairing. You will trade with your pair-member. You will not be informed of the actual identity of the other person (and s/he will not be informed of your identity). All sellers and buyers are assigned a numeric ID which is not associated with their real identity. You will also retain your ID and role (e.g. buyer or seller) through the entire experiment.

For how many periods will you trade with the same person? All participants will remain matched with their pair-member for a random number of periods. How is this determined? At the end of each period, the computer will determine randomly whether the same pairings will continue for the next period or whether new pairings will be formed. In any given period, there is an 80% chance that the same pairings will continue for the next period. In other words, there is an 80% chance that you will continue to trade with the same person in the next period. To help you understand this, imagine the computer has been programmed to spin a roulette wheel. If it lands on 1,2,3,4, 5, 6, 7, or 8 then you will continue to trade with the same person the next period. But if it lands on 9 or 10 the current pairings are immediately terminated. And then for the next period, the computer will randomly match you with a different person in the room to form a new pairing. This process will repeat for every new pairing. At the beginning of each period, you will be notified on-screen whether the random matching process has kept you with the same person or matched you with a new person.

When does the entire experiment end? If one of two conditions holds: (1) The experiment will end if all participants have already been matched with all possible trading partners. This is because no participant will be matched with the same person more than once during this experiment. For example, if there are 4 buyers and 4 sellers, then no buyer or seller will have more than 4 unique pairings. After 4 unique pairings, the experiment ends. (2) Even if all unique pairings have not been exhausted, the last pairing will occur once the experiment has lasted at least 18 periods. In other words, if you have traded at least 18 periods for the experiment, then your current pairing is your last one. This does not mean the experiment stops at 18 rounds exactly; it only means that when your last pairing randomly ends, you will not be paired with a new partner.

To summarize, if you have had less than 4 different trading partners during the experiment, and the experiment has not lasted at least 18 total periods, then when your current match is randomly terminated, the computer will match you with a new person and the experiment would continue. However, if the experiment has lasted at least 18 total periods, then the experiment will end once your current pairing is randomly terminated.

Conducting Trades

Each trade occurs within a trading period and each trading period is divided into three phases:

1. During the proposal phase, the buyer can make a proposal on the terms of trade to the seller. The seller can either accept or reject the proposal.

2. If the seller accepts the proposal, then during the effort determination phase, the seller privately chooses an effort level to supply.

3. After the seller makes an effort choice comes the quality realization & payment determination phase. During this phase, the buyer privately observes the realized quality and then can make final adjustments in payment depending on the initial terms of the proposal.

Specific details of each phase are given below:

1. The Proposal Phase

Each period starts with a proposal phase. A proposal allows the parties to agree to the terms of trade by including a list of promises and obligations of both parties (see below for details). The buyer can submit a single proposal during the proposal phase. Once a proposal is submitted, the seller will decide to accept or reject the proposal.

How does a buyer make a proposal? A proposal screen will appear that will require the buyer to enter values for the following terms: price, two quality-contingent bonuses, and a requested effort level. The buyer must also specify whether to include an optional ending condition for the trading relationship. These four components are described below:

- **Price** This allows the buyer to state the price s/he will pay for the good traded. The buyer enters a price in the "Price" field. The price ranges from -250 to 250 (whole numbers).

  Important: The price the buyer specifies will be *binding*. This is similar to an upfront payment or a legally binding obligation once the proposal is agreed upon, the computer will ensure that the price is paid to the seller.

- **Quality-contingent bonus** Two possible quality levels for the good traded can be observed by the buyer: 30 or 120. The quality level realized depends partially on a decision made by the seller in the effort determination phase (see below for explanation).

  In the proposal, the buyer states that s/he will pay a bonus linked to each possible quality level. That is, a bonus level must be specified for two cases: when the realized quality is 30 and when realized quality is 120.

  To enter a bonus for the case when quality is 30, enter a number into the "Bonus amount for quality 30" field to specify the size of the bonus (whole numbers from 0 to 250).

  Similarly, for the case when quality is 120, enter a number into the "Bonus amount for quality 120" field to specify the size of the bonus (whole numbers from 0 to 250).

  Important: The stated bonuses are *not binding*. During the payment determination phase, to come later, the buyer can choose any bonus level s/he wishes. Thus, the stated bonuses, one for each possible quality level, are discretionary. Only the price is ensured to be paid to the seller.

  The total offered payment to the seller is: price + quality-contingent bonus.

- **Requested effort level** In the next phase, sellers who accept an offer must choose an effort level of either 0 or 1 to supply. The seller's effort choice helps determine whether the quality level will be 30 or 120 for that period. Buyers must request an effort level that they want their seller to choose. To indicate the requested effort level, click the corresponding button next to the question, "What effort level do you request the seller to provide?"

  Note:If the offer is accepted, the seller is *not* obligated to choose the effort level requested by the buyer.

- **Optional ending condition** The buyer may also decide to include an ending condition for the trading relationship in the proposal. To include the condition, the buyer clicks the “Yes” button next to the question, “Would you like to include the condition that the relationship will end if you report a quality of 30?” If the buyer does not wish to include the ending condition, he/she clicks the “No” button.

  Note: If included and the offer is accepted by the seller, then the buyer and seller will *not* trade in future rounds if the buyer reports that quality 30 was realized in the *current* trading period (see phase #3 below). This does *not* mean that the experiment will end if the condition is satisfied. It does, however, mean that the two parties will not be able to make or accept offers in their pairing until randomly rematched with a new trading partner.

After the buyer has specified these terms, s/he needs to click the “Commit Decision” button to submit it. Then comes the effort determination phase.

2. Effort Determination Phase

Following the proposal phase, sellers can accept or decline the buyer’s proposal. Sellers who accept the agreement will choose between the two possible effort levels: 0 or 1. The seller clicks the button (0 or 1) corresponding to his or her effort choice. **Important:** Only the seller observes his/her effort choice. The buyer does not observe effort but can make inferences about the chosen effort based on the quality realized in that period.

3. Quality Realization and Payment Determination Phase

During this phase, the realized quality level is privately observed by the buyer. Then, the buyer provides feedback to the seller concerning the quality level. The buyer does this by choosing to send one of two possible messages: “Quality was 30” or “Quality was 120”. The buyer will click the button next to the corresponding message he/she wishes to send to the seller.

Note: The message that the buyer decides to send *may or may not* reflect the realized quality level (i.e. the buyer is not required to report the actual quality he observes).

After choosing the message to send, the buyer must also decide the actual bonus to be paid to the seller. The buyer will enter his/her bonus choice in the bonus payment field. *Nothing restricts the buyer from choosing a bonus level that is different from the bonus that was specified in the proposal.* The actual bonus can range from 0 to 250 (whole numbers) at the buyer's discretion.

Finally, buyers will also be asked to make a guess about the seller's effort choice in that period. Click the button corresponding to your guess (0 or 1).

Viewing Messages: The seller then observes the message and actual bonus payment sent from the buyer. Sellers will also be asked to make a guess about the realized quality level that they believe the buyer actually observed in that period. Click the button corresponding to your guess ("Realized quality was 30" or "Realized quality was 120").

Note: If the proposal included the optional ending condition and the message "Quality was 30" was sent to the seller from the buyer, then the buyer and seller will not trade after the current period. That is, if the ending condition is satisfied, offers cannot be made or accepted in the pairing until the buyer and seller are randomly rematched with a new trading partner.

At the end of each period, the buyer and seller will be shown a summary screen. The following information is displayed on this screen:

- the points that you individually earned (or lost) in this period.

Note: **Your points are private.** Your partner cannot observe your earnings. Furthermore, note that buyers and sellers can incur losses in each period. These losses are subtracted from your points balance.

If an agreement was formed, the following information will also be displayed:

- the Price the buyer offered.
- the Proposed Bonuses (for the two possible quality levels)
- the Actual bonus granted

Please enter all the information on the screen in the documentation sheet supplied to you. This will help you keep track of your performance across periods so that you can learn from your past results.

At the beginning of the next period, the computer will inform you if you have been randomly matched with the same trading partner or with a different partner.

How do Buyers Make Money?

- If the **buyer does not make an offer or the seller rejects the offer**, the buyer will receive 30 points for that period. If the trading relationship has ended as a result of the ending condition, the buyer will receive 30 points in each subsequent period **until randomly rematched with a new seller**.
- If the buyer's offer is accepted, the buyer's points for the period depend on the realized quality, the price, and the actual bonus paid. That is,

Buyer Points = Realized Quality - Price - Actual Bonus

- As you can see, the higher the realized quality, the more points the buyer earns. At the same time, the lower total payments (price plus actual bonus), the more points the buyer earns.
- In summary, higher quality and lower payments means more points for the buyer.

How is realized quality determined? The realized quality depends, in part, on the seller's effort choice. The table below summarizes the likelihood of observing quality levels 30 or 120 given the seller's effort choice:

After the seller makes his/her effort choice, the actual quality is randomly determined by the computer based on the above probabilities. That is, for either effort choice, there is a chance observing a quality level of 30 or a quality level of 120. An

	Quality = 30	Quality = 120
Effort = 0	0.75	0.25
Effort = 1	0.25	0.75

effort choice of 1, however, increases the chance of observing a quality level of 120. To help you understand this, imagine we randomly pick one ball from a bingo cage containing 4 balls numbered 1 through 4. If the seller's effort choice was "0" then drawing the ball labeled 1 would result in the quality level of 120 while balls labeled 2, 3, or 4 result in a level of 30. If the seller's effort choice was "1" instead, then drawing balls labeled 1, 2, or 3 would result in a quality level of 120 while the ball labeled 4 would result in a quality level of 30.

How do Sellers Make Money?

- If **the seller rejects the proposal or the buyer does not make an offer**, the seller will receive 30 points for that period. If the trading relationship has ended as a result of the ending condition, the seller will receive 30 points in each subsequent period **until randomly rematched with a new buyer**.
- If the seller has accepted an offer, then the seller's points depends on the price, actual bonus, and effort costs s/he incurs. The points of a seller are determined as follows:

$$\textbf{Seller Points} = \textbf{Price} + \textbf{Actual Bonus - Effort Costs}$$

How are effort costs calculated? Effort costs depend on the seller's private effort choice of 0 or 1. The table below summarizes the seller's costs:

Effort:	0	1
Cost:	**0**	**10**

An effort choice of "1" costs the seller 10 points while a choice of "0" costs the seller no points.

- As you can see, the higher the actual payments, the more points a seller earns. At the same time, the higher effort cost reduces points.

4. HOW DOES THE PRINCIPAL STRUCTURE PERFORMANCE PAY ACROSS AGENTS WITH VERIFIABLE OUTPUT?: EXPERIMENTAL EVIDENCE

In this paper, I conduct an experiment exploring how the principal structures performance pay across two agents with independent yet verifiable production technologies. An agent's output realization is a noisy function of his effort choice, which the principal cannot observe. His output, however, is unaffected by the other agent's effort choice. Although the agents produce independently, the principal can structure an agent's performance pay to depend on both output realizations. As a result, the agent's expected compensation may depend on how much effort the other agent chooses to exert. Based on the relative magnitude of the performance bonuses specified in the contract, the principal endogenously creates cooperative, independent, or competitive performance schemes. For instance, the principal may prefer to reward the agents jointly by paying them the most when they both perform well. This contrasts with a relative performance mechanism where an agent is rewarded the most when he performs well while the other agent performs poorly. One key question explored in this study is how the length of interaction alters the types of contracts that principals prefer to design. In contrast to a one-shot environment, agents have the ability to monitor each other's effort choices across time in a repeated relationship, which may affect both the types of evaluation schemes that principals implement and the amount of rent left to the agents.

The importance of this research stems from recent trends in how companies organize and compensate their employees. First, organizing employees into teams of workers has become more common. Lawler et al. (2001) surveyed Fortune 1000 companies about their workplace practices between 1987 and 1999. They found that the percentage of firms with more than one-fifth of its employees working in teams rose from 37% in 1987 to 61% in 1999. A similar trend occurred in Great Britain where an employment survey found that the fraction of respondents with employees working in a team increased from 47% in 1992 to 59% in 2006 (Gallie et al. (2012)). Second, more workplaces have linked employee pay to performance. Lawler and Mohrman (2003) discuss updated survey results from Lawler et al. (2001) which includes new data from 2002. Between 1987 and 2002, the percentage of Fortune 1000 companies offering individual incentive plans to over three-fifths of its workforce rose from 5% to 33%. A similar statistic for group incentive schemes documented an increase from 5% to 20% of firms. More recent evidence also highlights the frequent use of performance incentives. A 2014 survey of 1,452 American firms notes that 58% of the respondents used some form of individual performance pay, such as a performance bonus, while 26% used some form of group performance pay (Bruce (2014)).

The experimental design also addresses important aspects that have not been explored within the experimental literatures on contract design and group incentive systems. First, most experiments on contract design focus on bilateral relationships between a single agent and principal. Those that do contain two or more agents have mainly focused on fixed wage, or gift exchange, contracts. As a result, this experiment provides some insight into the effectiveness of performance-contingent pay in contracting environments with multiple agents. Second, the experimental literature on team incentives has not allowed subjects, acting as principals, to design the terms of compensation. The experimenter, instead, imposes the specific terms of compensation and compares the efficiency of different incentive systems in environments where subjects only act as agents. The novelty of this experimental design is that, via the contract offer, principals endogenously specify both the types of compensation schemes they prefer to use to motivate the agents and the manner in which the group's surplus is divided between the principal and the agents. Therefore, important strategic considerations can now be accounted for when studying group incentives. More specifically, the experiment documents whether principals 1) have a preference for implementing certain types of group compensation schemes, 2) design optimal schemes in line with the equilibrium predictions, and 3) divide the surplus equitably or disproportionately with the agents.

The main results of the experiment suggest that predictions under the standard assumption of self-interest fail to explain the observed contracts for both treatments. Theoretically, one-shot interactions provide the principal with flexibility in terms of implementing contracts that are competitive, cooperative, or independent. Although roughly three-quarters of contracts in equilibrium predict competition among the agents, where an agent is paid more when outperforming his peer, over 80% of the observed contract offers favor cooperation where an agent is rewarded the most when both agents perform well. When the relationship is repeated, the equilibrium is unique and takes an extreme form of cooperation where agents are only rewarded when they both perform well. The data, however, suggest that principals tend to favor competition relatively *more* in the repeated environment. Now, only 50% of contracts foster cooperation while 25% support competition. Therefore, counter to the comparative static prediction, the proportion of cooperative compensation schemes declines in the repeated environment. Analyzing the data more closely, however, the types of team incentives that principals design across treatments are not significantly different although contract offers are significantly more variable in the repeated environment as measured by absolute deviations from the treatment's average contract offer. This finding contrasts with the unique equilibrium in the repeated environment and the wide range of feasible equilibria in one-shot interactions.

Furthermore, the theory predicts that agents can expect to be compensated less in the repeated environment since agents can monitor each other's effort choices across time and, therefore, punish their partners when they shirk. Earnings for agents, however, are not significantly different across treatments and tend to be higher than required in equilibrium if all parties are self-interested. When aggregating data across the two treatments, over 79% of the offers made give the agents larger expected earnings while more than 97% of efficient trades give the agents higher expected earnings. This result supports previous findings where payoff distributions are less extreme than predicted and, as a result, the agents earn some rent. Additionally, the data from the repeated treatment suggests that agents, to some extent, engage in peer monitoring. That is, agents are more likely to choose low effort after observing their peer shirk in the previous period. Finally, I illustrate how Fehr and Schmidt (1999)'s theory of inequity aversion is a relatively better predictor of the efficient outcomes for one-shot interactions compared to the standard assumption of self-interest. More specifically, agents who are averse to earnings differences with the principal, but not to differences with the other agent, can explain why contracts favor cooperation in the static setting and why the payoff difference between the principal and the agents is less extreme.

4.1 Related Literature

4.1.1 Theoretical

Economists have proposed several incentive systems that encourage agents to work efficiently when organized in teams. These mechanisms can foster competition among the agents such as a tournament where agents are ranked based on their productivity and are rewarded when they outperform their peers (Lazear and Rosen (1981); Green and Stokey (1983)). In contrast, other models have emphasized cooperation among the agents by making pay contingent on the group's overall level of performance meeting some target (Holmstrom (1982)). Finally, some models have incorporated concepts of peer monitoring as one method to mitigate shirking (Kandel and Lazear (1992)).

The optimal performance scheme that efficiently motivates the agents depends, in part, on the nature of the agents' production technology. In some environments, the agents produce output jointly and the principal can only measure the group's aggregate output. Here, however, it is assumed that the agents produce independently observable outputs. Furthermore, the principal can link an agent's pay not only to his output realization, but to the other agent's realization as well. The main implications to be tested were derived by Che and Yoo (2001) who assume that each agent's performance realization is verifiable by a third party such as a court. Kvaløy and Olsen (2006) extend their model by allowing output to potentially be unverifiable, which gives the principal the discretion to renege on the agents' promised performance pay. They also remove a common productivity shock from Che and Yoo (2001)'s model. With this revision, the main results of Che and Yoo (2001) are not altered significantly, which is why I focus on the latter model with verifiable output since the environment is slightly less complex and, therefore, easier for subjects to comprehend.

4.1.2 Empirical

For conciseness, I summarize relevant laboratory experiments on group incentives as they are the most closely related to this study. Therefore, I do not highlight studies using observational data collected in the workplace. These studies, however, usually focus on how one type of group incentive scheme affects the workers' productivity.[1] Experiments have also analyzed the effectiveness of different types of incentive systems within a group of agents. Nalbantian and Schotter (1997) study several performance schemes in groups of 6 agents. These include a tournament where the subjects are split into groups of 3 and the team with the higher output receives a prize that is shared among the teammates. They also study a revenue sharing scheme where all 6 agents split the group's total output equally. Van Dijk et al. (2001) compare individual piece rates, group revenue sharing, and a rank-order tournament using a real-effort task among two agents. Both of these studies find that the average effort of the agents is larger under the tournament. Furthermore, Harbring (2006) compares revenue sharing and a tournament among two agents in an environment where agents

[1] Workplace studies can be characterized based on the type of incentive systems analyzed. For group piece rates, see Weiss (1987) (electronic manufacturing) and Hamilton et al. (2003) (garment production). For group piece rates and profit-sharing, see Boning et al. (2007) (steel minimills). For gainsharing, see Hansen (1997) (call center). For group or firm-wide targets, see Knez and Simester (2001) (airline departures), Reilly et al. (2005) (health services), and Burgess et al. (2010) (tax collection). For tournaments, see Eriksson (1999) (Danish corporations), Bognanno (2001) (American corporations), and Bandiera et al. (2013) (fruit production).

can engage in pre-play communication before making their effort choices. Average effort choices are higher under the revenue sharing scheme since subjects are able to coordinate their effort choices through communication.

In these experiments, the specific terms of the compensation scheme were designed by the experimenter. Although this allows us to determine which mechanisms effectively motivate the agents, it does not analyze several important decisions made by the principal. First, principals may have a preference for implementing one type of performance system over another. In previous experiments on group incentives, the performance schemes are analyzed individually with subjects participating in one scheme at a time. This study can capture a preference because the principal makes performance cooperative or competitive via her contract offer, which gives her flexibility in terms of the types of incentives she can implement. Furthermore, this study tests whether principals design performance mechanisms that are in accordance with the theoretical predictions, which creates feedback for future theoretical work on how group incentives are implemented. Finally, the principal now has a claim to a portion of the group's surplus instead of earnings being allocated solely among the agents. Such a division is more realistic within the principal-agent framework.

These novelties are commonly present in the experimental literature on contract design. Contracting experiments with multiple agents, however, have mainly focused on gift exchange contracts. Therefore, each agent's pay is not contingent on his performance level.[2] Two studies have allowed the principal to tie pay to performance in an environment with multiple agents. Whitford and Ochs (2006) analyze a contracting game where the production technology is not independent, but is a function of both agents' effort choices. They find that performance bonuses are not sufficient at inducing the agents to exert high effort although subjects are unable to gain experience since the stage game is played only once. Königstein and Lünser (2007) study a game where the principal offers a linear contract to the agents who can self-select into either an individual or group performance task. Like in Whitford and Ochs (2006),

[2]See Maximiano et al. (2007), Charness and Kuhn (2007), Gächter and Thöni (2010), Abeler et al. (2010), and Gächter et al. (2012).

however, individuals selecting into the group task do not produce individually observable outputs so that pay is not based on one's relative performance to the other agents.

4.2 Theoretical Framework

4.2.1 Environment

The framework follows Kvaløy and Olsen (2006)'s model with verifiable output. A principal can interact with two agents in either a static or infinitely-repeated environment. For now, all parties are assumed to be risk neutral, self-interested, and they share a common discount factor δ if the relationship is repeated.

When the parties agree to trade in a given period, each agent independently produces output $q \in \{L, H\}$ where L denotes the low output value and H denotes the high output value with $H > L$. The value of each agent's output accrues directly to the principal. An agent's output realization is a stochastic function of the agent's effort choice, which is also high, e_H, or low, e_L. An agent incurs a cost of c when choosing high effort while low effort is costless. Unlike output realizations, which are publicly observable, only the agents can observe each other's effort choices.

The probability of an agent realizing H after choosing e_H is denoted as p_H. Similarly, p_L represents the probability of realizing H when an agent chooses e_L. It is assumed that $0 \leq p_L < p_H < 1$, which implies that high effort increases an agent's likelihood of realizing high output.

In order to engage in trade, the principal offers the agents a take-it-or-leave-it contract specifying the terms of compensation. These terms depend on the output realizations of agents i and j, denoted $q_i \in \{L, H\}$ and $q_j \in \{L, H\}$ respectively. Both realizations are verifiable, which implies that the principal cannot renege on any payments stated in the contract. Although the agents' output realizations are independent, the principal structures compensation to depend on both values. More specifically, the principal offers agent i a bonus vector, $\boldsymbol{\beta_i} \equiv (\beta_{HH}, \beta_{HL}, \beta_{LH}, \beta_{LL})$, where the first subscript represents the value of agent i's output realization, q_i, and the second subscript represents the value of agent j's output realization, q_j, in the current period. Therefore, the bonus vector $\boldsymbol{\beta_i}$ specifies agent i's compensation for each of the four possible outcomes that could occur if the parties engage in trade. Each component of the bonus vector is restricted to be nonnegative as the agents are subject to limited liability. As a result, the principal cannot extract payments from the agents. Finally, since the agents are identical, the terms of compensation will not vary across the agents. Therefore, the subscript is dropped on the bonus vector, $\boldsymbol{\beta} \equiv \boldsymbol{\beta_i} \equiv \boldsymbol{\beta_j}$, because agents are offered a symmetric contract.

After defining $p_i \equiv p(H|e_i)$ and $p_j \equiv p(H|e_j)$, we can express agent i's expected compensation as:

$$u(e_i, e_j, \boldsymbol{\beta}) = p_i p_j \beta_{HH} + p_i(1 - p_j)\beta_{HL} + (1 - p_i)p_j\beta_{LH} + (1 - p_i)(1 - p_j)\beta_{LL}$$

Agent j's expected compensation is defined similarly. Depending on $\boldsymbol{\beta}$, the principal can implement three types of compensation schemes:

1. If $u(e_i, e_H, \boldsymbol{\beta}) > u(e_i, e_L, \boldsymbol{\beta})$ the principal practices *joint performance evaluation*. In this case, $(\beta_{HH}, \beta_{LH}) > (\beta_{HL}, \beta_{LL})$ and agent i's expected compensation increases when agent j chooses high effort.[3]

2. If $u(e_i, e_H, \boldsymbol{\beta}) = u(e_i, e_L, \boldsymbol{\beta})$ the principal practices *independent performance evaluation*. In this case, $(\beta_{HH}, \beta_{LH}) = (\beta_{HL}, \beta_{LL})$ and agent i's expected compensation does not depend on agent j's effort choice.

3. If $u(e_i, e_H, \boldsymbol{\beta}) < u(e_i, e_L, \boldsymbol{\beta})$ the principal practices *relative performance evaluation*. In this case, $(\beta_{HH}, \beta_{LH}) < (\beta_{HL}, \beta_{LL})$ and agent i's expected compensation decreases when agent j chooses high effort.

The timing of play is as follows. If the principal does not offer a contract to the agents or at least one of the agents rejects the offer, the principal receives an outside

[3]The strict inequality implies that at least one of the components of the vector is strict.

option of $\bar{\pi} = 0$ while each agent receives $\bar{u} = 0$ for the given period. If both agents accept the offer, they simultaneously make their effort choices. After this, all parties observe q_i and q_j. Each agent also observes the other agent's effort choice, which allows the agents to monitor each other's effort choices in the repeated environment. Finally, since performance is verifiable, the appropriate bonuses stated in the contract are transferred from the principal to each agent and payoffs are realized.[4]

4.2.2 Optimal Contracts in the Static Setting

When designing the contract, the principal's goal is to maximize her expected net earnings. Assume that the principal always prefers that both agents choose high effort, e_H, in order to fulfill this goal.

The principal's optimization problem in the one-shot setting solves for the least-cost contract, which induces both agents to exert high effort:

$$\min_{\boldsymbol{\beta}} \; u(e_H, e_H, \boldsymbol{\beta}) \;\; \text{subject to:}$$

$$u(e_H, e_H, \boldsymbol{\beta}) - c \geq \bar{u} = 0 \tag{PC}$$

$$u(e_H, e_H, \boldsymbol{\beta}) - c \geq u(e_L, e_H, \boldsymbol{\beta}) \tag{IC_S}$$

$$\boldsymbol{\beta} \equiv (\beta_{HH}, \beta_{HL}, \beta_{LH}, \beta_{LL}) \geq 0 \tag{LL}$$

The participation constraint (PC) ensures that each agent accepts the contract offer. In order to prevent an agent from shirking and choosing low effort, the static incentive

[4]It is also assumed that the agents cannot not engage in side contracting and thus are prevented from making transfer payments to one another.

compatibility (IC_S) must also be satisfied.[5] Finally, the limited liability constraint (LL) ensures that the bonuses are nonnegative.

The agent's outside option, $\bar{u} = 0$, is sufficiently small so that (PC) does not bind. (IC_S) can be rewritten as:

$$p_H\beta_{HH} + (1 - p_H)\beta_{HL} - p_H\beta_{LH} - (1 - p_H)\beta_{LL} \geq \frac{c}{p_H - p_L}$$

Kvaløy and Olsen (2006) prove that the principal's optimal static contract $\boldsymbol{\beta}^*_{\boldsymbol{S}} = (\beta_{HH}, \beta_{HL}, 0, 0)$ satisfies:

$$p_H\beta_{HH} + (1 - p_H)\beta_{HL} = \frac{c}{p_H - p_L} \tag{1}$$

The principal has flexibility in terms of the values of β_{HH} and β_{HL} that she can specify. More specifically, the equilibrium is not unique and the set of contracts making (1) bind can exhibit joint ($\beta_{HH} > \beta_{HL}$), independent ($\beta_{HH} = \beta_{HL}$), or relative ($\beta_{HH} < \beta_{HL}$) performance evaluation.

Each agent's expected compensation under $\boldsymbol{\beta}^*_{\boldsymbol{S}}$ is $u(e_H, e_H, \boldsymbol{\beta}^*_{\boldsymbol{S}}) = \frac{p_H c}{p_H - p_L}$. Therefore, an agent's expected payoff is $\frac{p_H c}{p_H - p_L} - c = \frac{p_L c}{p_H - p_L}$. Likewise, the principal's expected payoff is $2(p_H H + (1 - p_H)L - \frac{p_H c}{p_H - p_L})$.

4.2.3 Optimal Contracts in the Infinitely Repeated Setting

When the relationship is infinitely repeated, the principal still wishes to design the least-cost contract that induces both agents to choose e_H. The contract, however, must induce high effort as a subgame perfect equilibrium.

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[5]To ensure that high effort from both agents is the unique equilibrium, we must also satisfy a second incentive compatibility constraint: $u(e_H, e_L, \boldsymbol{\beta}) - c \geq u(e_L, e_L, \boldsymbol{\beta})$. That is, an agent would prefer to choose high effort when the other agent shirks. Kvaløy and Olsen (2006) show that it is without loss of generality to ignore this constraint. This is because the agents have the same expected payoff when choosing (e_H, e_H) under the optimal contracting scheme (described below) as when choosing (e_L, e_L) under the cost-minimizing contract when this second incentive compatibility constraint binds. Therefore, the agents are indifferent between the two outcomes when the additional incentive compatibility constraint is violated under the optimal contract.

Repeated interaction benefits the principal because the agents are able to observe and monitor each other's effort choices across time. Specifically, both Che and Yoo (2001) and Kvaløy and Olsen (2006) show that under joint performance evaluation (JPE), the agents can credibly commit to a punishment scheme that discourages the other agent from shirking. Intuitively, an agent's expected compensation increases when the other agent chooses e_H under JPE only. JPE, therefore, allows an agent to effectively punish the other agent by playing e_L in all subsequent periods after observing the other agent shirk in the current period. This threat creates an implicit incentive for each agent to choose e_H, which lowers the amount of explicit incentives that the principal must provide to the agents in order to induce high effort as the unique equilibrium during each period.

Formally, assume that an agent's effort choice in the current period depends upon the entire history of past effort choices made by both agents. Furthermore, assume that $\boldsymbol{\beta}$ is stationary across periods, which allows us to easily specify each agent's continuation payoff.[6] If an agent shirks in the current period by choosing e_L, the lowest payoff that he can guarantee for himself in future periods equals $\min[u(e_L, e_L, \boldsymbol{\beta}), u(e_L, e_H, \boldsymbol{\beta})]$. This expression therefore represents the worst payoff that can be sustained in equilibrium given $\boldsymbol{\beta}$.

Assuming that the agents have an incentive to enforce the punishment, the necessary incentive compatibility constraint for both agents to choose e_H in the current period is:

$$\frac{1}{1-\delta}[u(e_H, e_H, \boldsymbol{\beta}) - c] \geq u(e_L, e_H, \boldsymbol{\beta}) + \frac{\delta}{1-\delta}\min[u(e_L, e_L, \boldsymbol{\beta}), u(e_L, e_H, \boldsymbol{\beta})] \quad (IC_R)$$

This constraint says that the agent will play e_H as long as the expected present value from doing so is greater than shirking in the current period and then receiving the worst sustainable payoff in all subsequent periods of interaction.

[6]The stationary assumption is commonly used in the dynamic contracting literature when agents are risk neutral (see Levin (2003)).

Under relative (RPE) or independent (IPE) performance evaluation, $u(e_L, e_L, \boldsymbol{\beta}) \geq u(e_L, e_H, \boldsymbol{\beta})$. Therefore, (IC_R) reduces to $u(e_H, e_H, \boldsymbol{\beta}) - c \geq u(e_L, e_H, \boldsymbol{\beta})$, which is the static incentive constraint (IC_S). This illustrates that the punishment provides no advantage in the repeated setting under RPE or IPE. The principal must provide the same incentives from the static environment to induce both agents to exert high effort.

With JPE, however, (IC_R) reduces to:

$$\frac{1}{1-\delta}[u(e_H, e_H, \boldsymbol{\beta}) - c] \geq u(e_L, e_H, \boldsymbol{\beta}) + \frac{\delta}{1-\delta} u(e_L, e_L, \boldsymbol{\beta}) \tag{2}$$

Since $u(e_L, e_L, \boldsymbol{\beta}) < u(e_L, e_H, \boldsymbol{\beta})$ under JPE, the right hand side of the static incentive constraint (IC_S) is now relaxed. Therefore, the principal can lower the amount of expected compensation she must provide to the agents in the repeated environment. Intuitively, this results from the agents engaging in peer monitoring, which creates slack in the (IC_S) constraint.

The principal's relaxed optimization problem is:

$$\min_{\boldsymbol{\beta}} \; u(e_H, e_H, \boldsymbol{\beta}) \;\; \text{subject to:}$$

$$u(e_H, e_H, \boldsymbol{\beta}) - c \geq \bar{u} = 0 \tag{PC}$$

$$\frac{1}{1-\delta}[u(e_H, e_H, \boldsymbol{\beta}) - c] \geq u(e_L, e_H, \boldsymbol{\beta}) + \frac{\delta}{1-\delta} \min[u(e_L, e_L, \boldsymbol{\beta}), u(e_L, e_H, \boldsymbol{\beta})] \tag{IC_R}$$

$$\boldsymbol{\beta} \equiv (\beta_{HH}, \beta_{HL}, \beta_{LH}, \beta_{LL}) \geq 0 \tag{LL}$$

Kvaløy and Olsen (2006) show that the optimal contract in the repeated setting, $\boldsymbol{\beta_R^*} = (\beta_{HH}, \beta_{HL} = 0, 0, 0)$, satisfies (2) with equality. This is an extreme form of JPE where the agents are only rewarded when they both perform well. Under the optimal scheme $\beta_{HH} = \frac{c}{(p_H + \delta p_L)(p_H - p_L)}$.

Furthermore, two important points must be stated. First, Kvaløy and Olsen (2006) prove that the punishment scheme of low effort, e_L, from both agents is self-enforcing given $\boldsymbol{\beta_R^*}$. That is, an agent has an incentive to play e_L when being punished by the other agent. Second, the equilibrium is collusion-proof from an outcome where at least one of the agents chooses e_L, which guarantees that e_H from both agents is the unique subgame perfect equilibrium in the repeated setting. These points illustrate that the above analysis is sufficient in solving the principal's optimization problem.

Under $\boldsymbol{\beta}^*_{\boldsymbol{R}}$, each agent's expected payment is $u(e_H, e_H, \boldsymbol{\beta}^*_{\boldsymbol{R}}) = \frac{p_H^2 c}{(p_H+\delta p_L)(p_H-p_L)}$. Therefore, an agent's expected payoff is $\frac{p_H^2 c}{(p_H+\delta p_L)(p_H-p_L)} - c$. The principal's expected payoff is $2(p_H H + (1-p_H)L - \frac{p_H^2 c}{(p_H+\delta p_L)(p_H-p_L)})$.

4.3 Experimental Design

4.3.1 Procedures

The experiment was framed as a buyer (the principal) interacting with two sellers (the agents). Sellers were informed that their individual effort choices helped determine the quality level of a fictitious good that they individually produced. After instructions were read aloud, subjects were randomly assigned roles to be either a buyer or a seller, which remained fixed throughout the experiment. During each period subjects interacted in groups of three, each consisting of one buyer and two sellers, via computers and individuals remained anonymous.

At the beginning of each round, the buyer could make at most one offer to the sellers. In the offer, the buyer specified a nonnegative integer value for β_{HH} and β_{HL}. The values of β_{LH} and β_{LL} were fixed at 0, the optimal contract value, to help make the experimental environment less complex for subjects and, therefore, easier to comprehend. First, it became less demanding for buyers to specify the terms of the contract offer as they only had to specify two bonuses instead of four. Second, the simplification made it easier for sellers to understand how they are compensated since they only received a bonus in that case that their output was $q = H$. Furthermore, $L = 0$ so it may seem natural that a seller receives no bonus when failing to generate additional surplus for the group.[7] To complete the offer, the buyer also stated the effort level that she wanted each seller to choose in order to signal her preferred effort choice. This request, however, was nonbinding.

If the two sellers received an offer, they independently decided to either accept or reject it. If both sellers accepted the offer, they then privately made their respective effort choices, framed as letters with $e_L = X$ and $e_H = Y$. After this, all three subjects observed the quality level of the two goods produced. Sellers then observed the effort choice made by the other seller since effort choices were made simultaneously. At the end of the round, the appropriate payment(s) were transferred from the buyer to each seller. Subjects were also shown a summary screen listing the relevant information and choices they observed during the round.

If the buyer did not make an offer or at least one of the sellers rejected the offer, all parties received their outside option of 0 points for that period. Earnings were framed as points, which accumulated over the duration of the experiment. At the end of the experiment, points were converted into cash and subjects were immediately paid in private.

The experiment uses a 2x1 design, where the length of each group's interaction varies across treatments. More specifically, the manner in which the subjects were rematched into groups of three at the start of each round depended on the treatment. In the static environment, subjects were randomly rematched after each period to form new groups consisting of one buyer and two sellers. In contrast, subjects faced a 75% (i.e. $\delta = 0.75$) probability of being rematched with the same group members in the next round when the relationship was repeated. This method is commonly used to test infinitely-repeated horizon models in the laboratory (Roth and Murnighan (1978); Dal Bó (2005)). This was conveyed to the subjects with a randomly drawn integer between 1 and 4 being displayed on their screens at the start of each period. If a 4 was drawn, subjects were randomly rematched to form new groups.

[7] In future work, this restriction could also be relaxed by allowing the buyer to specify a bonus paid to a seller whenever his output is $L = 0$. Such a design could avoid situations where sellers earn negative earnings, but would also create the potential for the buyer's earnings to be negative.

Sessions were conducted during Spring 2016 at Purdue University's Vernon Smith Experimental Economics Laboratory, which has a subject pool containing mainly undergraduate students. Subjects who had previously participated in an experiment on contract design were excluded from participating. Each session lasted roughly 75 minutes consisting of instruction reading, two practice periods, and a quiz to test familiarity with the procedures before actual trading began. In the one-shot treatment sessions lasted for 15 periods. In the repeated matchings treatment subjects were informed that the experiment would last at least 12 periods with the last round occurring once the last supergame randomly ended. That is, if a 4 was randomly drawn at the start of a given period after round 12, which gives an expected session length of 15 periods, the session was finished. To keep the number of rounds consistent across sessions in the repeated matchings treatment, the random draws were predetermined according to δ. Subjects were made aware of this before trading began and could verify the draws were predetermined by opening a sealed envelope containing the draws once the session was finished. Random rematchings occurred at the beginning of rounds 5, 8, and 10 with the experiment ending after round 14. zTree, an experimental software, was used to conduct the experiment (Fischbacher (2007)). Four sessions were conducted for each treatment with each containing 9 subjects. Average earnings, including show-up fees, were equal to \$20.75.

4.3.2 Parameters and Comparative Statics

Table 4.1 displays the parameter values used in the experiment, which will be used to numerically illustrate the comparative statics. The first prediction concerns the type of contracts that the principals design across the two treatments. Figure 4.1 displays the set of cost-minimizing contracts for both environments. The dashed line represents the 45 degree line and, therefore, indicates points where the principal practices IPE ($\beta_{HH} = \beta_{HL}$). Points below the dashed line represent cases of JPE ($\beta_{HH} > \beta_{HL}$) while points above the line represent cases of RPE ($\beta_{HH} < \beta_{HL}$). In

Variable	Value	Description
δ	0.75	discount factor/probability of continuation
H	80	high performance value
L	0	low performance value
p_L	0.25	probability of H given e_L
p_H	0.75	probability of H given e_H
c	15	cost of e_H
β_{HH}, β_{HL}	$\{0, \cdots, 200\}$	set of possible performance bonuses

Table 4.1.: Parameter values used in the experiment

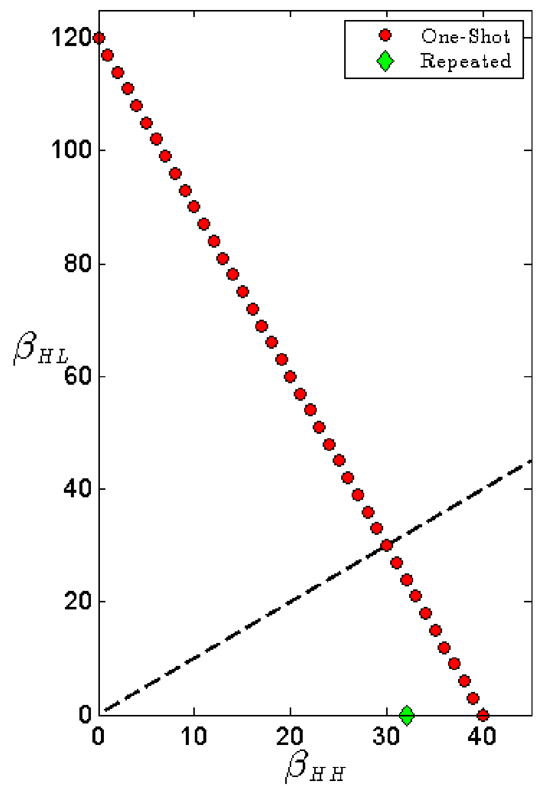


Fig. 4.1.: Set of cost-minimizing contracts by treatment

the static environment, 30 out of the 41 possible contracts practice RPE. The contract that exhibits IPE has $\beta_{HH} = \beta_{HL} = 30$. This leaves 10 contracts where the principal implements JPE. In contrast, the optimal contract in the repeated environment is unique with $\beta_{HH} = 32$ and $\beta_{HL} = 0$. These equilibrium predictions suggest that the proportion of contracts practicing JPE should not decrease in the repeated matchings treatment.

Prediction 1: *When moving from the static to the repeated environment, principals are no less likely to implement contracts that practice joint performance evaluation.*

Given the set of cost-minimizing contracts, each party's expected payoff in a given period can also be calculated. In the static case, the agent's expected compensation for all feasible contracts in equilibrium equals $u(e_H, e_H, \boldsymbol{\beta_S^*}) = 22.5$. This leaves each agent with a net payoff of 7.5 experimental points in a given period. Therefore, the principal's expected payoff equals 75 points. In the repeated environment, the principal is able to capture a larger share of the surplus due to peer monitoring. The principal's expected payoff is now 84 points while $u(e_H, e_H, \boldsymbol{\beta_R^*}) = 18$ leaving agents with a net payoff of 3 points.

Prediction 2: *When moving from the static to the repeated environment, principals capture a larger share of the group's surplus, thereby reducing the net payoff to agents.*

For both treatments, the extreme payoff distribution between the principal and agents relies critically on the assumption that parties are self-interested. Previous experiments, however, illustrate that parties tend to split the surplus more equitably, which implies that agents typically earn more than their reservation values.[8] Therefore, prediction 2 fails to take these common findings into account. After presenting the main results, I consider cases of non-standard preferences and illustrate how these preferences alter the predicted surplus distribution between the principal and the agents.

4.4 Results

4.4.1 Contractual Form

In this section, I compare the types of contracts that principals design across the two treatments and see how well contracts correspond to the theoretical predictions. Figure 4.2 displays the set of contract offers for each treatment. Within each treatment, I also highlight the principal's requested effort choice stated in the offer by using blue circles to represent e_H requests and orange triangles to represent e_L requests. Larger triangles or circles indicate more observations for that value of (β_{HH}, β_{HL}). For the one-shot treatment, 79.4% of the offers display JPE. As a result, relatively few offers display RPE (6.1%). The average value for β_{HH} is 41.2 while the average value

[8]See Fehr et al. (2007) for evidence from a contracting experiment with performance bonuses and Güth et al. (1982) for evidence from the ultimatum game.

for β_{HL} is 25.8, which provides further evidence of a preference for JPE. Furthermore, principals tend to offer bonuses that are larger than required in equilibrium. 82% of the offers contain some combination of (β_{HH}, β_{HL}) that make the static incentive compatibility (IC_S) constraint non-binding.[9] Furthermore, principals request e_H in 90% of the offers. 88% of the requests for high effort satisfy (IC_S), which contrasts with the offers that request e_L where only 33% of these observations satisfy (IC_S).

Compared to the one-shot treatment, the distribution of contract offers is more variable in the repeated environment. Although principals are predicted to implement an extreme form of JPE with $\beta_{HL} = 0$, 50.6% offers exhibit JPE in this treatment while 26.8% exhibit RPE. Therefore, a lower proportion of contract offers favor JPE in the repeated matchings treatment, which conflicts with the first comparative static prediction. Additionally, the average value for β_{HH} is relatively lower in the repeated treatment at 35.8 while the average value for β_{HL} is relatively higher at 28.8. This provides further evidence that contract offers favor JPE relatively less in the repeated treatment although JPE continues to be the most commonly used. Like the one-shot treatment, however, a majority of the offers, 75%, make the (IC_R) constraint non-binding.[10] Therefore, in both treatments most agents are offered payments higher than expected in equilibrium. In this treatment, however, a smaller proportion of offers request that e_H is chosen at 67%. Although the proportion of high effort requests is smaller in the repeated treatment, these offers satisfy (IC_R) more frequently relative to the offers that request e_L. The proportion that satisfy (IC_R) is 87% and 51% respectively for e_H and e_L requests.

Figure 4.3 is similar to figure 4.2 except it focuses on the offers accepted by the agents. Acceptance rates were 89% and 82% for the one-shot and repeated treatments respectively. Given these high acceptance rates, the characteristics of accepted

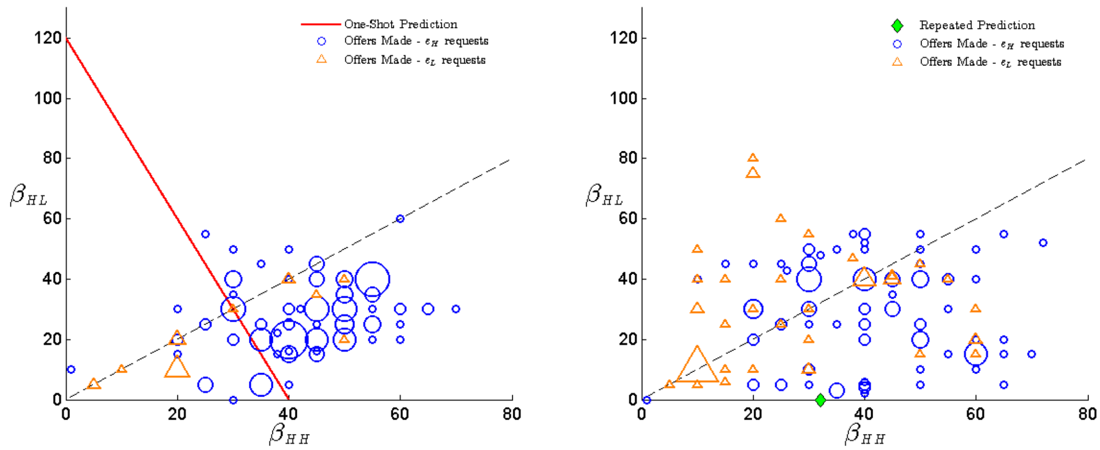
[9]This statistic was computed by comparing the agent's expected payment assuming both agents choose high effort, $p_H p_H \beta_{HH} + p_H(1-p_H)\beta_{HL}$, with the expected payment in the static equilibrium, $\frac{p_H c}{p_H - p_L}$.

[10]This statistic was computed by comparing the agent's expected payment assuming both agents choose high effort, $p_H p_H \beta_{HH} + p_H(1-p_H)\beta_{HL}$, with the expected payment in the repeated equilibrium, $\frac{p_H^2 c}{(p_H + \delta p_L)(p_H - p_L)}$.

contracts are similar to the set of contract offers. A majority of the contracts practice JPE although JPE is more prevalent in the one-shot treatment. Furthermore, most contracts request that e_H is chosen although the proportion is higher in the one-shot treatment.

Finally, figure 4.4 focuses on the set of completed trades within each treatment where both agents chose high effort, e_H. Therefore, these are the contracts that generated the efficient outcome in the given period. 51% of the accepted offers were efficient in the one-shot treatment compared to 44% in the repeated treatment. Several notable differences occur when comparing the set of efficient contracts to the set of contract offers. First, in the repeated matchings treatment, a larger proportion of efficient offers exhibit JPE. 68.9% of the efficient contracts exhibit JPE in this treatment although 55.1% of the those accepted exhibit JPE. Second, within both treatments a large percentage of efficient contracts give the agents larger earnings than expected in their respective equilibriums. Over 97% have payments higher than necessary in equilibrium. This suggests that agents require larger rents in order to reach efficiency. Finally, a majority of efficient contracts request that e_H is chosen. Only one efficient trade in the one-shot treatment requests low effort while 18% of the efficient trades request e_L in the repeated treatment. Out of these low effort requests in the repeated treatment, however, only one observation violates (IC_R) which indicates that subjects, at least theoretically, had the incentive to choose e_H.

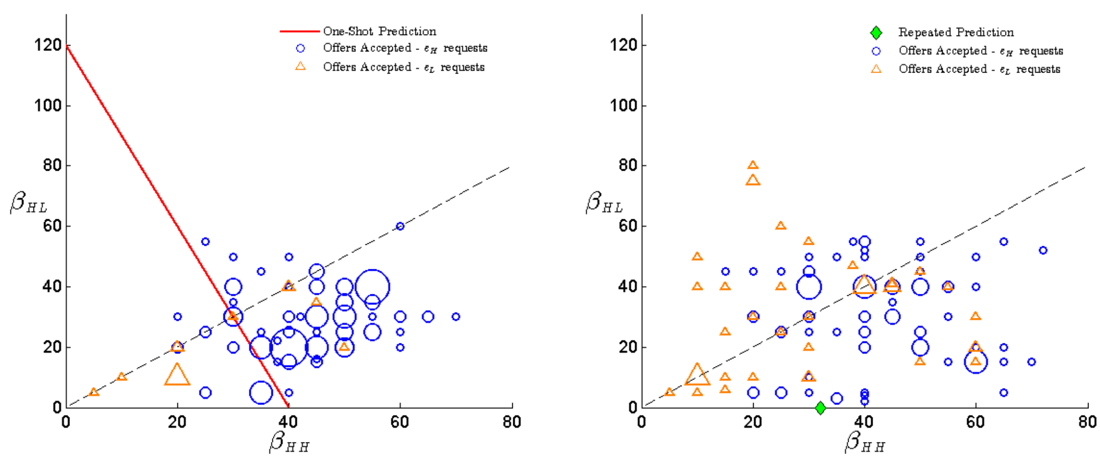
The first comparative static prediction focuses on how the set of optimal contracts changes across treatments. More specifically, the fraction of contracts that exhibit JPE should not decrease in the repeated treatment. The previous analysis, however, suggests that the results do not support this prediction. In order to more precisely describe how the degree of JPE changes across treatments, I analyze the fraction of compensation coming from β_{HH}. That is, I focus on the ratio $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$. Although this fraction ignores the magnitude of the agents' expected compensation, it provides an intuitive way to measure the extent to which JPE is used in a given contract. The value is bounded between 0 and 1, with 1 being the extreme form of JPE ($\beta_{HH} >$



(a) One-shot treatment (b) Repeated treatment

For one-shot treatment, $N = 180$. For repeated treatment, $N = 168$. Larger bubbles indicate more observations. One observation not displayed for repeated treatment with $\beta_{HH} = 125$ and $\beta_{HL} = 100$. The dashed line is the 45° line.

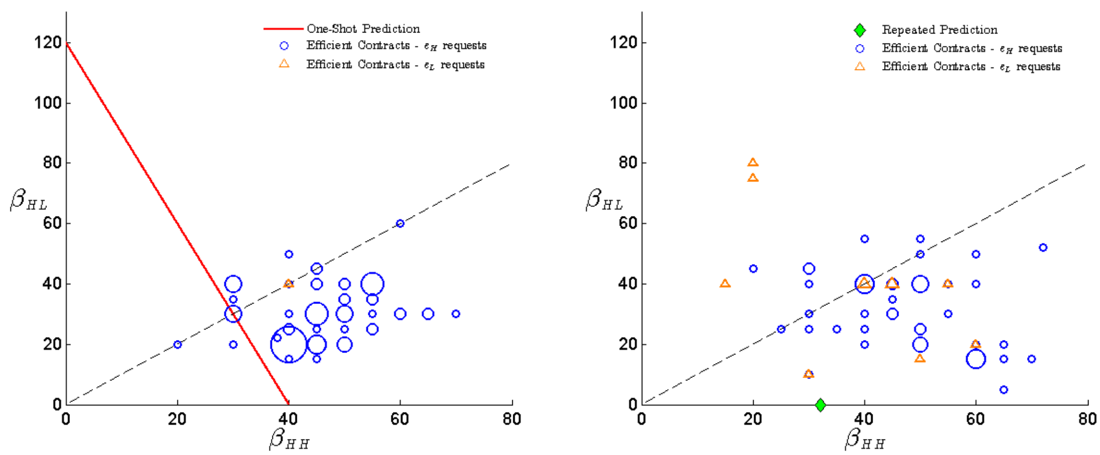
Fig. 4.2.: Offers made by treatment



(a) One-shot treatment (b) Repeated treatment

For one-shot treatment, $N = 160$. For repeated treatment, $N = 138$. Larger bubbles indicate more observations. One observation not displayed for repeated treatment with $\beta_{HH} = 125$ and $\beta_{HL} = 100$. The dashed line is the 45° line.

Fig. 4.3.: Offers accepted by treatment



(a) One-shot treatment

(b) Repeated treatment

Only includes offers where both agents chose high effort, e_H. For one-shot treatment, $N = 81$. For repeated treatment, $N = 61$. Larger bubbles indicate more observations. One observation not displayed for repeated treatment with $\beta_{HH} = 125$ and $\beta_{HL} = 100$. The dashed line is the 45° line.

Fig. 4.4.: Efficient offers by treatment

$\beta_{HL} = 0$), 0.5 being IPE ($\beta_{HH} = \beta_{HL}$), and 0 being the extreme form of RPE ($\beta_{HL} > \beta_{HH} = 0$). Therefore, values closer to 1 highlight contracts that favor JPE while values closer to 0 favor RPE.

Figure 4.5 compares box plots displaying the values of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$ for each treatment. Once again, I focus on the set of offers made, accepted offers, and efficient trades separately. In all three cases, the median values for the ratio are lower in the repeated treatment, which contradicts prediction 1. Furthermore, for offers made and accepted offers the average value for the repeated treatment is 0.57, which is less than 0.62, the average value for the one-shot treatment. The average values are closer for efficient offers, however, with values of 0.61 and 0.59 for the one-shot and repeated treatments respectively. The box plots also illustrate how the ratio is consistently more variable in the repeated treatment despite the unique equilibrium prediction in the repeated environment and the multiplicity of equilibria in the one-shot environment.

To test for across-treatment differences concerning the value $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$ and its variability, table 4.2 presents the results of multi-level random effects regressions (at the session and principal levels). Columns (1) through (3) present the results of regressions where the dependent variable equals $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$. Although the estimate on the repeated treatment dummy is negative in sign, the magnitudes of the estimates are fairly small and insignificant. Therefore, the degree of JPE is not significantly different across treatments. Columns (4) through (6) present the results of regressions where the dependent variable equals the absolute value of the difference between the observed value of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$ and the treatment mean value of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$. For offers made, the repeated treatment dummy variable has a significant estimate of 0.068 which indicates that there are significantly higher deviations from the mean in the repeated treatment. The same result holds true for accepted offers but not for efficient trades. Therefore, contract offers and accepted contracts are significantly more variable in the repeated treatment in terms of the types of performance evaluation used.

Main Result 1: *When comparing the one-shot and repeated environments, no significant differences in the degree of joint performance evaluation (*$\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$*) exist across*

Table 4.2.: Degree of joint performance evaluation ($\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$) regressions

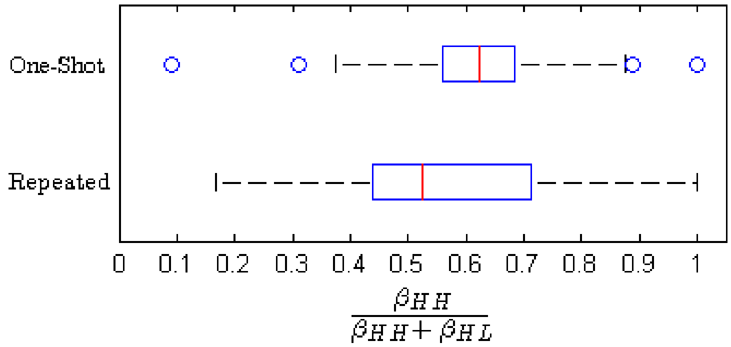
	$\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$			*Absolute Deviation from Treatment Mean*		
	(1) All Offers	(2) Accepted Offers	(3) Efficient Trades	(4) All Offers	(5) Accepted Offers	(6) Efficient Trades
Repeated treatment dummy	-0.0622	-0.0672	-0.0262	0.0681***	0.0597**	0.0570
	(0.0583)	(0.0602)	(0.0412)	(0.0251)	(0.0284)	(0.0403)
$\frac{1}{\text{Period}}$	-0.0384	-0.0454	0.0303	0.0218*	0.00945	-0.00510
	(0.0526)	(0.0408)	(0.0471)	(0.0117)	(0.0165)	(0.0245)
Length of Interaction	0.00472	0.00706	0.00735	-0.00551	-0.00439	-0.00257
	(0.00677)	(0.00831)	(0.0115)	(0.00498)	(0.00742)	(0.0143)
Constant	0.626***	0.625***	0.594***	0.0865***	0.0861***	0.0722***
	(0.0226)	(0.0238)	(0.0162)	(0.0189)	(0.0185)	(0.0186)
Observations	348	298	142	348	298	142

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regressions estimated using multi-level random effects (at the session-principal levels) with robust standard errors clustered at the session level. Regressions (1), (2) and (3) have a dependent variable of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$. Regressions (4), (5) and (6) have an dependent variable equal to the absolute value of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$ - treatment mean value. *Repeated treatment dummy* takes a value of 1 if the observation belongs to the repeated treatment. *Length of Interaction* indicates the number of periods that the same principal-agent-agent group of subjects have interacted with each other.

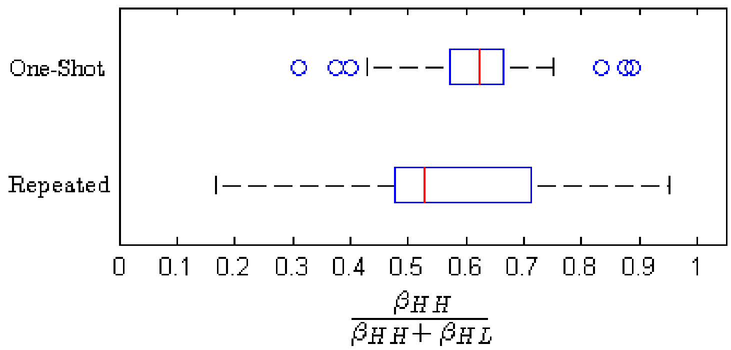
treatments. For all contract offers and offers accepted, however, absolute deviations from the treatment mean of $\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$ *are significantly higher in the repeated treatment. Therefore, the types of incentive systems used are more variable when the relationship is repeated.*

4.4.2 Expected Payoffs to Agents

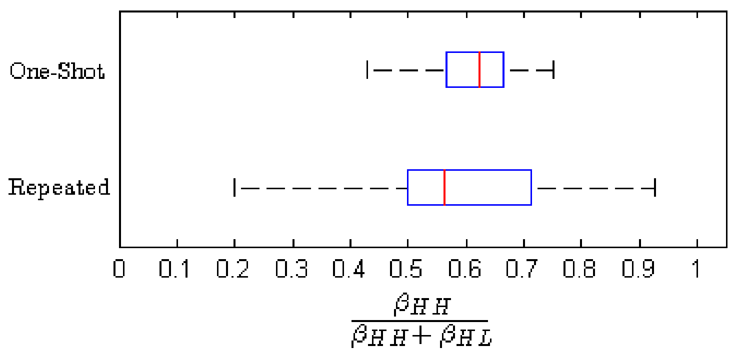
The second comparative static prediction concerns how the agent's expected payoff changes across treatments. According to the standard prediction, an agent's net payoff in a given period is expected to decrease from 7.5 points in the one-shot treatment to 3 points in the repeated treatment. Figure 4.2, however, illustrates that roughly 79% of the contracts offer payments higher than necessary in equilibrium,



(a) Offers Made



(b) Accepted Offers



(c) Efficient Offers

The box plots show the relative degree of joint performance evaluation exhibited in the contracts. A value of 1 indicates extreme joint performance evaluation (JPE) with $\beta_{HL} = 0$. In contrast, a value of 0 indicates extreme relative performance evaluation (RPE) with $\beta_{HH} = 0$. A value of 0.5 indicates independent performance evaluation (IPE), $\beta_{HH} = \beta_{HL}$. $N = 348$, 298, and 142 for figures (a), (b), and (c) respectively. Circles correspond to observations that fall outside 1.5*interquartile range.

Fig. 4.5.: Degree of joint performance evaluation ($\frac{\beta_{HH}}{\beta_{HH}+\beta_{HL}}$) by treatment

which suggests that expected payoffs are higher than predicted in both treatments. To explore this issue further, I analyze the agent's expected per-period payoff across treatments. To calculate each agent's expected payoff, I assume that both agents pick the principal's requested effort level stated in the contract offer if the contract offer is rejected by at least one agent.[11] Furthermore, for accepted offers I use the realized effort choices of the two agents to calculate each agent's expected payoff. That is, the expected payoff for agent i is calculated as $p_i p_j \beta_{HH} + p_i(1-p_j)\beta_{HL} - c(e_i)$ where $p_i \equiv p(H|e_i)$ and $p_j \equiv p(H|e_j)$.

Figure 4.6 presents histograms for each treatment of the agent's expected payoff given the bonuses stated in the contract. All offers, accepted offers, and efficient trades are presented separately. The shapes of the histograms are fairly similar across treatments. On average, the expected payoffs across treatments are fairly close to one another. The average value for all offers made is slightly higher in the one-shot treatment as predicted. For accepted offers and for efficient trades only, however, the average value is slightly higher in the repeated treatment. Finally, both treatments see average values that are higher than their respective equilibrium predictions confirming that agents expect to earn some rent. The distribution of expected payoffs, however, suggests that across-treatment differences are minor. Table 4.3 confirms these results by presenting multi-level random effects regressions (at the session and principal levels) where the dependent variable is an agent's net expected payoff under these assumptions. The estimate on the repeated treatment dummy is insignificant and close to zero for all subsets of the data and it is only negative for the entire set of offers.

Main Result 2: *The agent's expected payoff does not significantly differ across treatments. For both treatments, however, agents on average expect to earn rents above the standard equilibrium prediction.*

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[11] The results are robust to different specifications of the expected payoffs for rejected offers such as assuming that the agents choose e_H if the appropriate incentive compatibility constraint is satisfied.

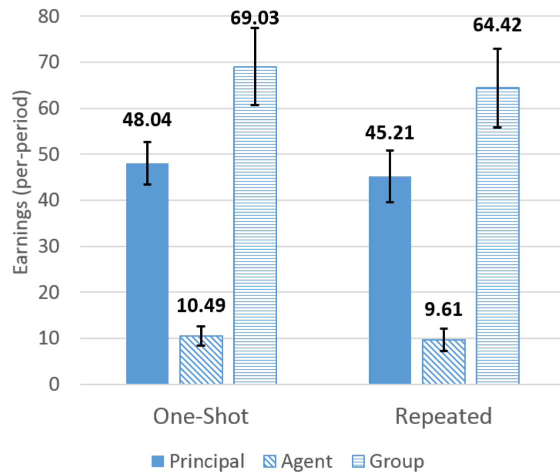
Fig. 4.6.: Distribution of the agents' expected payoffs

For accepted offers, each agent's expected payoff is calculated using the two agent's realized effort choices for that round. For rejected offers, the agent's expected payoff assumes that the agent chooses the principal's requested effort level stated in the contract offer. Two observations for the repeated treatment are omitted with an expected value of 74.06.

Table 4.3.: Regression estimates for impact of repeated matching on the agent's expected payoff

	(1) All Offers	(2) Accepted Offers	(3) Efficient Trades
Repeated treatment dummy	-0.710	0.188	0.0245
	(2.839)	(3.053)	(2.518)
$\frac{1}{\text{Period}}$	-1.027	1.529	6.213
	(2.741)	(4.131)	(9.624)
Length of Interaction	0.233	-0.0632	-0.0693
	(0.533)	(0.739)	(0.862)
Constant	11.29***	11.19***	15.35***
	(2.288)	(2.287)	(3.035)
Observations	696	596	284

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For accepted offers, each agent's expected payoff is calculated using the two agent's realized effort choices for that round. For rejected offers, the agent's expected payoff assumes that the agent chooses the principal's requested effort level stated in the contract offer. The dependent variable equals the agent's expected payoff under these assumptions. Regressions estimated using multi-level random effects (at the session and principal levels) with robust standard errors clustered at the session level. *Repeated treatment dummy* takes a value of 1 if the observation belongs to the market power treatment. *Length of Interaction* indicates the number of periods that the same principal-agent-agent group of subjects have interacted with each other.



$N = 160$ for principal and groups in the one-shot treatment and $N = 138$ for principal and groups in the repeated treatment. Since there are two agents in each group, the number of observations is double for agents in each treatment. 95% confidence intervals for the mean are also reported.

Fig. 4.7.: Average per-period earnings for completed trades by treatment

4.4.3 Per-Period Earnings

Now I analyze actual payoffs for completed trades to test for any earnings differences across treatments.[12] Figure 4.7 presents the average per-period earnings of principals, agents, and the groups of three subjects for each treatment. Average earnings are fairly similar across treatments. In both environments, principals capture roughly 70% of the group's surplus leaving each agent with 15% of the surplus on average. The surplus, however, is slightly higher in the one-shot treatment, which is expected since the fraction of observations where both agents choose e_H rises from 44% in the repeated treatment to 51% in the one-shot treatment. Although agents on average capture 15% of the group's surplus, their earnings are slightly higher than predicted in equilibrium. On average, agents in a given period earn 3 and 6.6 points higher than expected in the one-shot and repeated treatments respectively. Table 4.4 presents multi-level random effect estimates of actual earnings for completed trades and confirms that no significant differences in earnings occur across treatments.

[12]The qualitative results are similar when analyzing all offers or efficient trades only.

Table 4.4.: Regression estimates of the impact of repeated matching on per-period earnings for completed trades

	(1) Principal Payoffs	(2) Agent Payoffs	(3) Group Payoffs
Repeated treatment dummy	-3.540	-1.387	-5.498
	(8.173)	(3.124)	(9.232)
$\frac{1}{\text{Period}}$	-6.924	-1.678	-10.19
	(6.983)	(3.988)	(10.28)
Constant	49.99***	10.98***	71.72***
	(6.404)	(2.845)	(8.110)
Observations	298	596	298

* $p < 0.10$, ** $p < 0.05$, $^{***}p < 0.01$. Regressions (1) and (3) estimated using multi-level random effects (at the session and principal levels) with robust standard errors clustered at the session level. Regression (2) is similar but also includes random effects at the agent level. *Repeated treatment dummy* takes a value of 1 if the observation belongs to the market power treatment. Only completed trades considered.

4.4.4 Agent Effort Response in the Repeated Treatment

In this section I briefly explore the factors that help determine an agent's propensity to shirk and choose low effort in the repeated treatment. More specifically, Kvaløy and Olsen (2006)'s model argues that agents have an incentive to punish their peers under the optimal contract, $\boldsymbol{\beta_R^*}$, by playing low effort in all future periods after observing the other agent shirk in the previous period. To study these dynamics, table 4.5 presents the results of a multi-level random effects regression (at the session-principal-agent levels) and the marginal effects of a Probit regression where the dependent variable takes a value of "1" if the agent chooses e_L in the current period and a value of "0" if e_H is chosen. Violating the repeated incentive compatibility constraint (IC_R) does not have a significant effect on the agent choosing e_L and, although the estimates are positive as expected, they are fairly close to 0. Therefore, the specific punishment scheme prescribed in the model does not seem to be driving the agent's effort choice. Agents, however, do respond to the other agent's effort choice in the previous period. When their peer shirks in the last round, the propensity of the agent to choose e_L in the current round significantly increases by 16-19%. Furthermore, this propensity to shirk is not significantly affected by the other agent's performance realization in the previous round. Therefore, some aspects of the punishment scheme prescribed in the model fall in line with the observed dynamics. Specifically, agents respond to their peer's previous effort choice but not to their performance realization. Finally, agents are also significantly influenced by the effort choice requested by the principal in the contract offer. If the principal requests that the agent picks e_H, this significantly reduces the agent's propensity to choose e_L by 17-20%.

4.5 Inequity Averse Preferences

The standard predictions assume that all parties are self-interested. As a result, the static equilibrium is not unique with cost-minimizing contracts that exhibit RPE,

Table 4.5.: Regression estimates of the determinants of the agent to choose low effort (e_L) in the repeated treatment

	(1)	*Probit* (2)
(IC_R) violated dummy	0.0118	0.0332
	(0.127)	(0.109)
e_H requested dummy	-0.208***	-0.172***
	(0.0576)	(0.0532)
Other agent chose e_L in the last period dummy	0.161***	0.192***
	(0.0601)	(0.0504)
Other agent realized q_L in the last period dummy	0.0430	0.0440
	(0.0312)	(0.0429)
Constant	0.401***	
	(0.0658)	
Observations	196	196

* $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$. Regression (1) is a linear probability model estimated using multi-level random effects (at the session-principal-agent levels) with robust standard errors clustered at the session level. Regression (2) presents the marginal effects of a Probit regression with robust standard errors clustered at the session level. The dependent takes a value of 1 if the agent chose e_L in the current period and a value of 0 if they chose e_H. *Repeated treatment dummy* takes a value of 1 if the observation belongs to the market power treatment. Only completed trades considered. (IC_R) *violated dummy* takes a value of 1 if the bonuses stated in the contract offer violate incentive compatibility. e_H *requested dummy* takes a value of 1 if the principal requested that the agent picks high effort in the current period.

IPE, or JPE. Results for the one-shot treatment, however, suggest that principals have a preference for designing contracts that 1) exhibit JPE and 2) give agents payments larger than necessary in equilibrium. In this section, I now consider situations in which the agents are other-regarding in the static environment to illustrate how this alters the equilibrium predictions and how this provides one feasible explanation for the observed deviations from the standard prediction. More specifically, each agent may experience inequity (or inequality) aversion. Fehr and Schmidt (1999) develops this theory, which assumes that individuals make payoff comparisons with others and differences in earnings may generate some level of disutility for the agent.

Within this context, I continue to assume that the principal remains risk neutral and self-interested. Therefore, the principal is not averse to inequity.[13] Each agent, however, makes inequity comparisons to an exogenous reference group. Itoh (2004) considers an agent's reference group to be the other agent. Under this assumption he derives the optimal contract within two different contexts. In the first, the agent is only averse to income differences. Therefore, the agent disregards the cost of effort c and compares only the relative payments that the agents receive from the principal. Itoh (2004) then assumes that each agent compares his net payoff to the other agent. Therefore, c is now important when making inequity comparisons to the other agent. For the sake of brevity, I will only focus on the latter case where the agent makes net payoff comparisons with his reference group.[14]

The utility of agent i, u_i, now depends not only on his material payoff but also on comparisons to the other agent's income or net payoff. As before, define β_{ij} as the bonus payment made to agent i depending on the output realizations of agents i and j, denoted as $q_i \in \{L, H\}$ and $q_j \in \{L, H\}$ respectively. The i subscript therefore represents the value of q_i while the j subscript represents the value of q_j. Since the agents are offered a symmetric contract, β_{ji} therefore represents the bonus payment

[13]This simplification, although restrictive and ad hoc, makes it easier to solve for optimal contracts assuming that the agents are inequity averse.

[14]The theoretical results are only slightly altered when assuming that the agent disregards c when making inequity comparisons.

made to agent j as a function of q_j and q_i respectively. Finally let $c_i \in \{0, c\}$ denote the cost of agent i's effort choice and c_j denote the cost of agent j's effort choice.

If agent i is averse to differences in net payoffs with the other agent his utility can be expressed as:

$$u_i = \beta_{ij} - c_i - \alpha \max\{(\beta_{ji} - c_j) - (\beta_{ij} - c_i), 0\} - \beta \max\{(\beta_{ij} - c_i) - (\beta_{ji} - c_j), 0\}$$

The term α represents agent i's level of disadvantageous inequity aversion. This occurs when agent i's net payoff is lower than that of agent j. Similarly, β represents agent i's level of advantageous inequity aversion, which occurs when his net payoff is higher than that of agent j.[15] Two restrictions are commonly placed on α and β. First, $\alpha \geq 0$ implying that an agent's utility cannot increase when earning less than the other agent. Second, $|\beta| \leq \alpha$ so that the effect of disadvantageous inequity on the agent's utility is at least as large as the effect of advantageous inequity.

Instead of summarizing the theoretical results of Itoh (2004), figure 4.8 presents the numerical predictions based on the experimental parameters for varying levels of α and β. In contrast to the standard prediction, the optimal contract is now unique for the specific values of α and β, and for most cases agents are paid positive amounts for both β_{HH} and β_{HL}. In the case that $\alpha = 2.5$, or when α is fairly large, the expected payment is larger than required under the standard equilibrium. For the majority of cases, however, the expected payment is lower relative to the standard prediction providing the principal with a cost-saving benefit. Furthermore, note that when β is sufficiently large, the optimal contract now takes a unique form with $\beta_{HL} = 0$, the extreme form of JPE.

Prediction 3: *If agents are inequity averse to other agent: 1) the contractual form is unique and can be RPE, IPE, or JPE depending on the values of α and β and 2) the agent's expected compensation tends to be less than what is required under the standard prediction to ensure efficiency.*

[15] β is commonly used in the literature to denote the level of advantageous inequity. Do not confuse this with β_{ij} which is used to indicate bonus payment made to agent i.

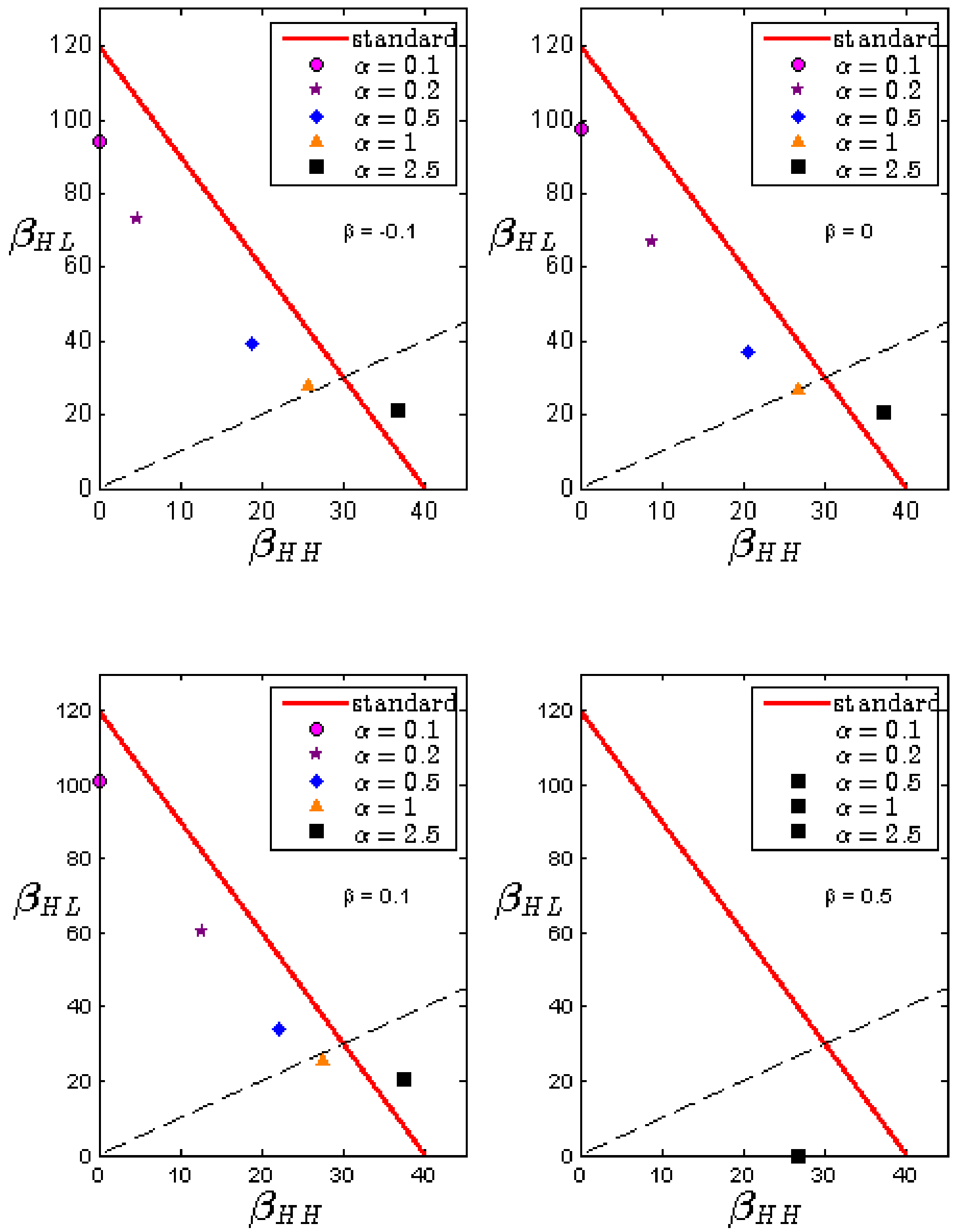


Fig. 4.8.: Cost minimizing contracts: agent makes inequity comparisons to the other agent's payoff

Although comparisons with the other agent are important to consider, the principal also remains a valid reference group for the agent. For example, the difference in payoffs is quite large under the standard prediction with the principal expecting to earn ten times more than each agent. Therefore, earnings differences between the principal and each agent are also important to consider when discussing inequity aversion. Using the framework of Itoh (2004), I derive the set of optimal contracts assuming that the agent now makes inequity comparisons with the principal. Once again, I will only present the numerical results under the experimental parameters.[16] Furthermore, I focus on situations where α is sufficiently small so that an agent's earnings are always less than the principal for all possible output realizations. Since the principal seeks to minimize her costs, such an outcome would be expected if the payments are sufficient in motivating agents to choose e_H.[17] Therefore, the expression for the utility function below excludes situations of advantageous inequity (β) due to this restriction.

If agent i is averse to differences in net payoffs with the principal, his utility can be expressed as:

$$u_i = \beta_{ij} - c_i - \alpha\{(q_i + q_j - \beta_{ij} - \beta_{ji}) - (\beta_{ij} - c_i)\}$$

Figure 4.9 presents the set of optimal contracts for varying levels of α. Compared to the standard prediction, the agent's expected payment is generally larger when the principal serves as his reference group. Furthermore, the set of optimal contracts is usually not unique for a given value of α but tends to favor JPE.

Prediction 4*: If agents are inequity averse to the principal: 1) contracts tend to favor JPE although they are generally not unique and 2) the agent's expected compensation is greater than what is required under the standard prediction to ensure efficiency.*

[16]See the appendix for details.

[17]Under the experimental parameters, $\alpha \leq 2.71$ for this assumption to remain valid. Since this value is relatively large it is not an unrealistic restriction for most agents.

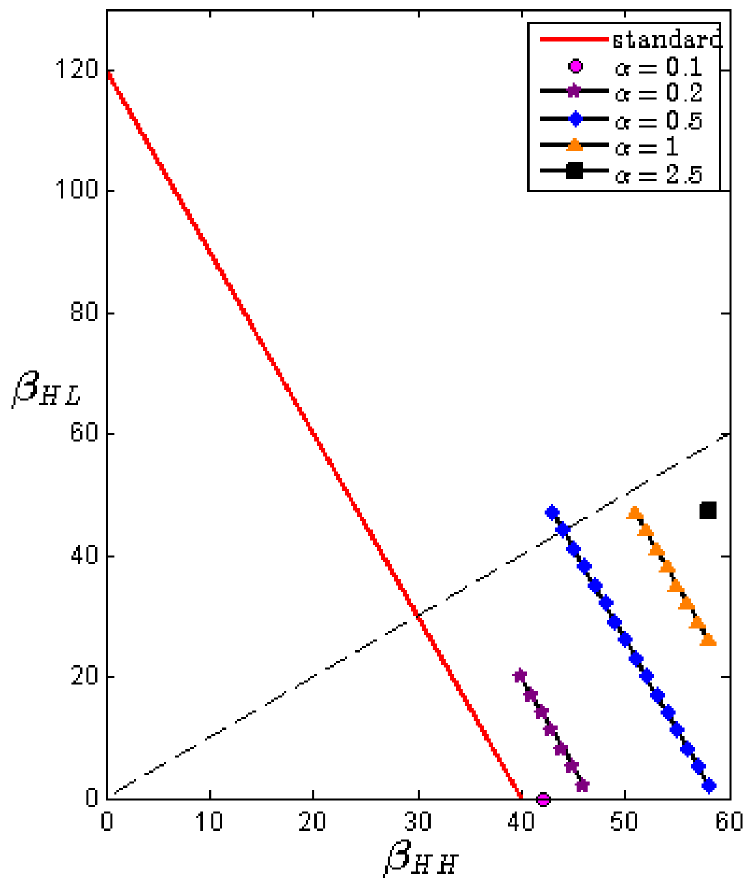


Fig. 4.9.: Cost minimizing contracts: agent makes inequity comparisons to the principal's payoff

4.5.1 Comments

The equilibrium predictions under inequity aversion suggest one interesting story that can explain why efficient contracts, and contracts more generally, in the one-shot treatment favor JPE. Figure 4.4 illustrates this preference where 81% of the efficient trades practice JPE in the one-shot treatment. Furthermore, 97% of these trades give the agents higher expected earnings than predicted under the standard equilibrium. These facts support the notion that agents are inequity averse to the principal but not to the other agent. Inequity aversion to the other agent provides the principal with a cost saving advantage. In contrast, the predictions under inequity aversion to the principal, found in figure 4.9, highlight both the bias towards JPE and the higher earnings agents expect to receive for a wide range of feasible α values.

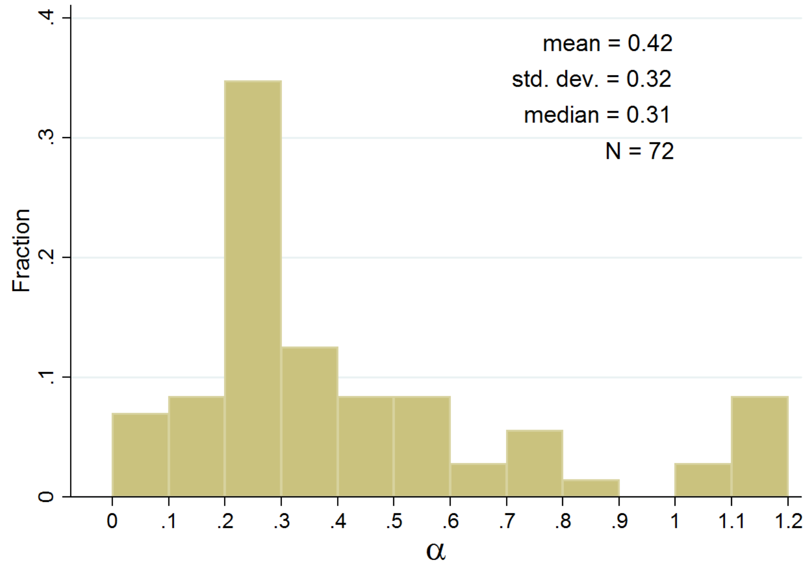
Main Result 3: *Efficient contracts in the one-shot treatment suggest that agents are inequity averse to the principal's earnings.*

4.5.2 Estimating α

I now attempt to estimate the α parameter given the set of efficient contracts observed in the one-shot treatment. To do so, I use the values of β_{HH} and β_{HL} observed in an efficient contract to derive the corresponding value of α under these assumed preferences.[18] In the appendix, I illustrate that the value for α depends on whether the relevant (PC) or (IC) constraint binds. Under the experimental parameters, the value can be calculated as:

$$\alpha = \begin{cases} \frac{30 - .75\beta_{HH} - 0.25\beta_{HL}}{2.25\beta_{HH} + .75\beta_{HL} - \beta_{HL} - 110} & \text{if } 0 < \alpha \leq 0.14 \\ \frac{20 - .75\beta_{HH} - 0.25\beta_{HL}}{2.25\beta_{HH} + .75\beta_{HL} - 180} & \text{if } 0.14 < \alpha \leq 2.71 \end{cases}$$

[18] This analysis assumes that 1) the two agents have the same α value, 2) the principal can observe α, and 3) principals always construct optimal contracts. Although these facts are fairly unrealistic, it provides a benchmark for estimating the distribution of α.



9 observations omitted. For 2 observations, the agent's expected compensation fell below the amount required under standard preferences (i.e. $\alpha = 0$) causing the estimates to be negative. For the other 7 observations, the value of $\beta_{HH} \geq 60$, which implies that the agent would experience advantageous inequity in the case that both output realizations are $q = H$. Therefore, α would not apply to these observations.

Fig. 4.10.: Estimates of α for efficient contracts in one-shot treatment

Figure 4.10 presents the distribution of estimated α values for efficient contracts in the one-shot treatment. Note that 9 observations are excluded as they either 1) imply that the agent may experience advantageous inequity so that β instead of α applies or 2) the agent's expected compensation falls below the value under standard preferences so that the α estimates are negative. Roughly one-third of the estimates have $0.2 \leq \alpha < 0.3$ with an average value of 0.42.

How do these estimates compare to other experiments concerning the distribution of α? Fehr and Schmidt (1999) calibrate α using data from an ultimatum game and find that 30% of observations have an α equal to 0, 0.5, or 1, while 10% have an α of 4. Blanco et al. (2011) also analyze ultimatum game data and find that 31% of the observations have $\alpha < 0.4$ while 36% have $\alpha \geq 0.92$. In contrast, my estimates favor lower values of α. For instance, 63% have $\alpha < 0.4$ while only 11% have $\alpha \geq 0.92$. Two other studies also have estimates that support low α values. Yang et al. (2016) conduct a menu choice experiment where the subject's decision helps determine his own payoff as well as his partner's payoff. They estimate that over 98% of the observations have $\alpha < 0.25$. Finally, Hoppe and Schmitz (2013) study a contracting game where the principal has incomplete information about the agent's effort cost. Their quantal response equilibrium estimates for α range between 0.07 and 0.25. The literature, therefore, currently has a wide range of distributions when estimating α.

4.6 Conclusion

The purpose of this experiment is to explore the types of performance incentives that principals endogenously design across two agents and how the length of the relationship affects the types of incentives that principals use. With regard to performance evaluation, principals favor joint performance evaluation in both the static and repeated settings. The use of relative performance evaluation, however, is less prevalent in the one-shot treatment, which contradicts the comparative static prediction assuming that parties are self-interested. Furthermore, agent's earnings on average are similar across treatments. For completed trades, principals on average capture roughly 70% of the group's surplus although this amount is less than what is predicted in equilibrium.

Although the standard predictions are not in line with the observed contracts designed by principals, I illustrate that agents who experience inequity aversion to payoff differences with the principal can explain why joint performance evaluation is favored in the one-shot treatment. This finding provides one feasible explanation as to why contracts are not significantly restructured across treatments as cooperation can be supported as the unique equilibrium outcome in both settings. One caveat, however, is that other behavioral theories may also predict the observed contracting behavior. Kőszegi (2014) provides an extensive survey of work that incorporates behavioral motivations into contracting models. Other motivations that should be considered more carefully include loss aversion, risk aversion, overestimating the return to effort, and reciprocity. Future empirical work, in particular studies that can separately identify the implications of these behavioral aspects, is needed to understand the specific factors that 1) effectively motivate agents working in teams and 2) influence the types of incentive systems that principals prefer to use when employing multiple agents.

Furthermore, many team environments incorporate explicit sanctions by team members when a peer is caught shirking or free-riding. The theoretical framework of this paper uses an implicit punishment scheme where an agent withholds effort after observing the other agent shirking. Although the results of this study suggest that agents are more likely to withhold effort after their peer shirks, future work could incorporate explicit punishment schemes such as administering a costly punishment to team members after they shirk. Alternatively, one could incorporate rewards where agents can award other team members after they work hard. Such research could illustrate whether the carrot or the stick is more effective at encouraging peers to work hard in contracting environments with multiple agents and whether this alters the types of team incentives that principals design.

Appendix: Inequity Comparisons to the Principal

I now assume that the agent makes inequity comparisons to the principal, who remains risk neutral and self-interested. For simplification and in accordance with the parameters of the experiment, $L = \bar{u} = \beta_{LH} = \beta_{LL} = 0$ in the following analysis. Furthermore, I restrict my attention to situations in which the agent always experiences disadvantageous inequity aversion relative to the principal. That is, for all possible contingencies, the principal earns at least as much as the agent. If possible, such an outcome would be expected when the principal seeks to minimize her costs. The following results, however, illustrate that this restriction only remains valid if α is sufficiently small. Therefore, I ignore the potential cases where the agent may experience advantageous inequity aversion (β) as this only applies when α is fairly large under the experimental parameterization.

Net Payoff Comparisons with the Principal

The table below highlights the net payoff for each party for all possible output realizations when each agent chooses the efficient effort, e_H. Note that each agent earns less than the principal when $L = 0$. In the case where both outputs are H, the principal earns more than each agent if $\beta_{HH} < \frac{2}{3}H + \frac{1}{3}c$. Likewise, in the case where only one realization is H, the principal earns more than the agent with output realization H if $\beta_{HL} < \frac{1}{2}(H + c)$.

Agent i's q	Agent j's q	Principal's Payoff	Agent i's Payoff	Agent j's Payoff
H	H	$2(H - \beta_{HH})$	$\beta_{HH} - c$	$\beta_{HH} - c$
H	$L = 0$	$H - \beta_{HL}$	$\beta_{HL} - c$	$-c$
$L = 0$	H	$H - \beta_{HL}$	$-c$	$\beta_{HL} - c$
$L = 0$	$L = 0$	0	$-c$	$-c$

Table 4.6.: Set of possible net payoffs in a given period assuming agents choose e_H

The (PC) and (IC) constraints must be slightly revised to account for α. Now, the participation constraint is:

$$\begin{aligned}
&p_H p_H(\beta_{HH} - \alpha[2(H - \beta_{HH}) - (\beta_{HH} - c)]) \\
&+ p_H(1 - p_H)(\beta_{HL} - \alpha[(H - \beta_{HL}) - (\beta_{HL} - c)]) \\
&+ (1 - p_H)p_H(0 - \alpha[H - \beta_{HL} - (0 - c)]) \\
&+ (1 - p_H)(1 - p_H)(0 - \alpha[0 - (0 - c)]) - c \; \geq 0
\end{aligned} \tag{PC}$$

Rearranging:

$$p_H(1 + 3\alpha)\beta_{HH} + (1 - p_H)(1 + 3\alpha)\beta_{HL} \geq \frac{c}{p_H}(1 + \alpha) + 2\alpha H \tag{PC}$$

Similarly, the incentive compatibility constraint is:

$$\begin{aligned}
&p_H p_H(\beta_{HH} - \alpha[2(H - \beta_{HH}) - (\beta_{HH} - c)]) \\
&+ p_H(1 - p_H)(\beta_{HL} - \alpha[(H - \beta_{HL}) - (\beta_{HL} - c)]) \\
&+ (1 - p_H)p_H(0 - \alpha[H - \beta_{HL} - (0 - c)]) \\
&+ (1 - p_H)(1 - p_H)(0 - \alpha[0 - (0 - c)]) - c \geq \\
&p_L p_H(\beta_{HH} - \alpha[2(H - \beta_{HH}) - \beta_{HH}]) \\
&+ p_L(1 - p_H)(\beta_{HL} - \alpha[(H - \beta_{HL}) - \beta_{HL}]) \\
&+ (1 - p_L)p_H(0 - \alpha[H - \beta_{HL}]) \\
&+ (1 - p_L)(1 - p_H)(0 - \alpha[0 - 0])
\end{aligned} \tag{IC}$$

Rearranging:

$$p_H(1 + 3\alpha)\beta_{HH} + [(1 - p_H)(1 + 3\alpha) - \alpha]\beta_{HL} \geq \frac{c}{(p_H - p_L)}(1 + \alpha) + \alpha H \tag{IC}$$

Now, the principal's static optimization problem is:

$$\min_{\beta_{HH},\beta_{HL}\geq 0} p_H p_H \beta_{HH} + p_H(1-p_H)\beta_{HL} \text{ subject to:}$$

$$p_H(1+3\alpha)\beta_{HH} + (1-p_H)(1+3\alpha)\beta_{HL} \geq \frac{c}{p_H}(1+\alpha) + 2\alpha H \tag{PC}$$

$$p_H(1+3\alpha)\beta_{HH} + [(1-p_H)(1+3\alpha) - \alpha]\beta_{HL} \geq \frac{c}{(p_H-p_L)}(1+\alpha) + \alpha H \tag{IC}$$

$$\beta_{HH} \leq \frac{2}{3}H + \frac{1}{3}c,\ \beta_{HL} \leq \frac{1}{2}(H+c)$$

Case One

Suppose that the incentive compatibility (IC) binds. Since $\alpha > 0$, the cost-minimizing contract offer sets $\beta_{HL} = 0$ with:

$$\beta^*_{HH} = \frac{1}{p_H(1+3\alpha)}\left[\frac{c}{p_H-p_L}(1+\alpha) + \alpha H\right]$$

Note that when you plug this into (PC), this result holds only if:

$$\left[\frac{c}{p_H-p_L}(1+\alpha) + \alpha H\right] \geq \left[\frac{c}{p_H}(1+\alpha) + 2\alpha H\right]$$

As long as $Hp_H(p_H-p_L) - p_L c > 0$, this becomes:

$$\alpha \leq \frac{p_L c}{Hp_H(p_H-p_L) - p_L c}$$

Therefore, if $\alpha \leq \frac{p_L c}{Hp_H(p_H-p_L)-p_L c}$ the incentive compatibility (IC) is the binding constraint. Therefore, (IC) is the relevant constraint when the agents do not suffer from a high level of disadvantageous inequity aversion. One must also ensure that $\beta^*_{HH} < \frac{2}{3}H + \frac{1}{3}c$.

Under β^*_{HH}, the principal's expected payment is:

$$p_H p_H \beta^*_{HH} = \frac{p_H}{(1+3\alpha)}\left[\frac{c}{p_H-p_L}(1+\alpha) + \alpha H\right]$$

The expected payment is increasing in α if $H > \frac{2c}{(p_H-p_L)}$.

Case Two

If $\alpha > \frac{p_L c}{H p_H (p_H - p_L) - p_L c}$ then (PC) will bind. The cost-minimizing contract $(\beta^*_{HH}, \beta^*_{HL})$ is no longer unique for the range of α that satisfy all of the necessary constraints. Therefore, β^*_{HH} and β^*_{HL} make (PC) bind subject to some restrictions. First, one must ensure that (IC) is not violated. Plugging the (PC) into (IC):

$$\frac{c}{p_H}(1+\alpha) + 2\alpha H - \alpha\beta_{HL} \geq \frac{c}{(p_H - p_L)}(1+\alpha) + \alpha H$$

$$\Rightarrow$$

$$\beta_{HL} \leq H - \frac{(1+\alpha)}{\alpha}\left[\frac{p_L c}{p_H(p_H - p_L)}\right]$$

Furthermore note that $\beta_{HL} \leq \frac{1}{2}(H+c)$. Therefore, the set of contracts that minimizes the principal's expected payment satisfies (PC) with equality, requires $\beta_{HL} \leq \min\{H - \frac{(1+\alpha)}{\alpha}\left[\frac{p_L c}{p_H(p_H - p_L)}\right], \frac{1}{2}(H+c)\}$, and requires $\beta_{HH} \leq \frac{2}{3}H + \frac{1}{3}c$. Under this contracting scheme, the principal's expected payment is:

$$p_H p_H \beta^*_{HH} + p_H(1-p_H)\beta^*_{HL} = \frac{p_H}{(1+3\alpha)}\left[\frac{c}{p_H}(1+\alpha) + 2\alpha H\right]$$

The expected payment is increasing in α if $H > \frac{c}{p_H}$. This also implies that α is not too large. If $\frac{c(1+\alpha)+2p_H\alpha H}{p_H(1+3\alpha)} > \frac{1}{3}p_H(2H+c) + \frac{1}{2}(1-p_H)(H+c)$, the principal must then increase β_{HH} or β_{HL} so that the agent's net payoff is sometimes higher than the principal's net payoff.

Estimating α

In the paper, I estimate α by using the observed values of β_{HH} and β_{HL} from the efficient contract offers in the one-shot treatment. Assuming that the agent makes net payoff comparisons to the principal, the value of α depends on whether the (IC) or (PC) constraint binds. In the set of equations below, the first equation solves for α when (IC) binds while the second equation solves for α when (PC) binds.

$$\alpha = \begin{cases} \frac{\frac{c}{p_H - p_L} - p_H\beta_{HH} - (1-p_H)\beta_{HL}}{3p_H\beta_{HH} + 3(1-p_H)\beta_{HL} - \beta_{HL} - \frac{c}{p_H - p_L} - H} & \text{if } 0 < \alpha \leq \frac{p_L c}{H p_H (p_H - p_L) - p_L c} \\ \frac{\frac{c}{p_H} - p_H\beta_{HH} - (1-p_H)\beta_{HL}}{3p_H\beta_{HH} + 3(1-p_H)\beta_{HL} - \frac{c}{p_H} - 2H} & \text{if } \frac{p_L c}{H p_H (p_H - p_L) - p_L c} < \alpha \\ & \text{and } \frac{c(1+\alpha) + 2p_H \alpha H}{p_H(1+3\alpha)} < \\ & \frac{1}{3}p_H(2H + c) + \frac{1}{2}(1 - p_H)(H + c) \end{cases}$$

Appendix: Instructions for the One-Shot Matchings Treatment

You can earn money during this experiment, with the exact amount depending on the decisions you and others make during the experiment. Your experimental income is calculated in points, which will be converted into cash at the rate of: $1 = 25 points. We will start you off with a balance of 125 points ($5).

All written information you received from us is for your private use only. You are not allowed to pass over any information to other participants in the experiment. Talking and cell phone use during the experiment is not permitted. Violation of these rules may force us to stop the experiment.

General Information

This experiment is about how people trade goods, which can vary in quality level. Participants are divided into two roles: $\frac{1}{3}$ will be buyers and the other $\frac{2}{3}$ will be sellers. These roles are determined randomly by the computer and remained fixed throughout the experiment.

Who will you trade with? The computer will randomly match each buyer in the room with two sellers to form a group of three participants. You will trade within this group of three. You will not be informed of the actual identity of the other participants you are matched with (and they will not be informed of your actual identity).

For how many periods will you trade with the same participants? All participants will remain matched with their group for one period. At the beginning of each period, the computer will randomly rematch all participants in the room to form new groups, each consisting of 1 buyer and 2 sellers.

When does the experiment end? The last grouping will occur once the experiment reaches the 15th period. Therefore, the experiment will end after period 15.

Important: It is very important that you pay careful attention to these instructions as it will help you understand the experiment. Furthermore, all participants must complete a quiz with 5 questions which will test your familiarity with the procedures. As an added incentive, you will receive $0.25 for each question answered correctly on the quiz. Exceptionally poor performance on the quiz may prevent you from participating in today's experiment.

Finally, as we read through the instructions, we will conduct 2 trial periods so that you can get accustomed to the computer's interface. During the trial periods, no money can be earned since you are simply practicing. Please follow along and do not enter things on your computer screen until I instruct you to do so.

During each period, the buyer has an opportunity to purchase two fictitious goods from the sellers in his/her group. Each seller is responsible for producing one of these goods. A seller's good will be one of two potential quality levels, which will be described as "quality level 0" and "quality level 80" for convenience. The seller makes an effort choice, either effort choice 'X" or effort choice "Y," which helps determine the quality level of the good that he/she produces.

At the end of each period, the earnings of each party depends on: 1) the quality level of the good produced by each seller and 2) the payments specified in the buyer's proposal, which must be accepted by both sellers before they produce the goods.

The outline below illustrates that each period is divided into several phases if both sellers accept the buyer's proposal:

1. Buyer makes proposal

2. Each seller makes an effort choice

3. The quality level of each seller's good is determined

4. Payments, outline in the proposal, are transferred from the buyer to each seller

1. The Proposal

Each period starts with the buyer having an option to make a proposal to the sellers. The buyer can submit a single proposal to the sellers. If submitted, the sellers will then decide to accept or reject the proposal.

To make a proposal, the buyer must click the "Make Proposal" button on the computer screen followed by the "Update" button. The buyer then specifies values for several components, which will now be described.

First, the buyer must specify two payments he/she agrees to pay to each seller. These payments depend upon the quality levels of the two goods produced in the current period.

- "Bonus (80/80)" is the bonus paid to a seller when both of the goods produced have a quality level of 80. Both of the sellers receive this bonus payment. Therefore, "Bonus (80/80)" is paid out twice by the buyer, with each seller receiving one of these bonus payments (of equal value).

- "Bonus (80/0)" is the bonus paid when only one of the goods has a quality level of 80 while the other good has a quality level of 0. Only the seller with a quality level of 80 receives this bonus payment, so in this case the buyer pays out this bonus only once.

- When a seller's quality level is 0, he/she does not receive a bonus payment from the buyer.

The bonus values must be whole numbers ranging from 0 to 200. Furthermore, the stated bonuses are binding, which means that the buyer must pay the corresponding bonus(es) in the case that at least one of the goods has a quality level of 80.

To help the buyer formulate bonus values, a Calculation Toolbox is also available on screen. Here, buyers can enter hypothetical values for "Bonus (80/80)" and "Bonus (80/0)." The toolbox will then display the buyer's earnings for all potential scenarios that could occur later in the period.

Second, the buyer must state the effort level, either "X" or "Y," that he/she wants each seller to choose. To indicate the desired effort level, click the corresponding button next to the Desired Effort Level text. Sellers, however, are not obligated to choose the buyer's desired effort level. It is simply a request.

After the buyer has specified the bonuses and the desired effort level, he/she must click the "Commit Decision" button to submit the proposal. Then, both sellers will observe the terms of the proposal independently and must decide to accept or reject the proposal. The sellers cannot communicate with one another and must make this decision on their own. A seller must click the OK button to confirm his/her decision.

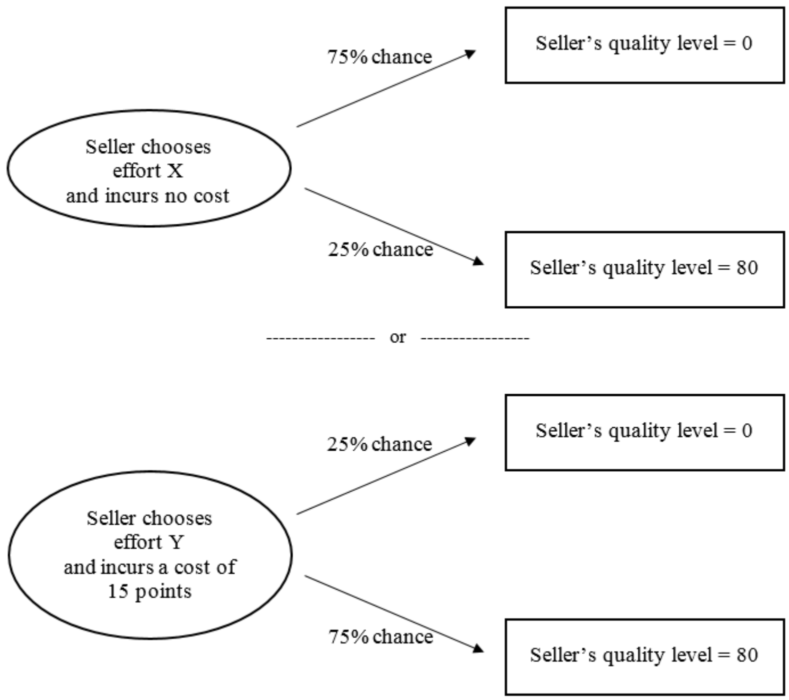
2. Sellers Make Their Effort Choices if They Both Accept the Proposal

If both of the sellers accept the proposal, they must then make their effort choices, which helps each seller determine the quality level of the good that he/she produces. Each seller clicks the button next to his/her corresponding effort choice (either "X" or "Y"). To confirm the choice, click the OK button.

Important: The buyer cannot observe the effort choices of the sellers. Furthermore, the two sellers make their effort choices simultaneously and independently. That is, they cannot communicate about the effort levels they intend to choose and, at this time, they cannot observe the effort choice made by the other seller.

3. Each Seller's Quality Level is Determined after Effort Choices Are Made

The diagram below illustrates how a seller's effort choice helps determine his/her quality level:



We wish to emphasize several points based on this information:

- Effort choice "X" gives a seller a one-in-four chance of realizing a quality level of 80 (and a three-in-four chance of realizing a quality level of 0).

- Choosing effort "Y" raises a seller's effort cost by 15 points. It also increases the likelihood that the seller's quality level is 80. Now, the seller has a three-in-four chance of realizing a quality level of 80.

- Each seller's quality level is not affected by the effort choice made by the other seller. That is, a seller's quality level is independent of any decision made by the other seller in his/her group. A seller's quality level is determined only by: 1) his/her individual effort choice and 2) by chance.

After the sellers make their effort choices, the computer determines the quality level of each good based on the corresponding probabilities listed above. Then, the buyer and the two sellers will jointly observe the quality level of the good produced by each seller. That is, everyone in the group observes the quality level of the two goods produced. Furthermore, each seller can now observe the effort choice made by the other seller in the current period.

4. Appropriate payments, outlined in the proposal, are transferred from the buyer to each seller.

Each person's payoff depends on: 1) the quality levels of the two goods produced, 2) the bonuses stated in the proposal, and 3) for each seller, his/her individual effort choice.

The following table describes the four different possible earnings for each group member when the group engages in trade. The sellers are denoted as Seller #1 and Seller #2 arbitrarily.

Seller #1's Quality Level	Seller #2's Quality Level	Buyer's Points	Seller #1's Points	Seller #2's Points
80	80	80+80 -2×"Bonus(80/80)"	"Bonus(80/80)" - Effort Cost	"Bonus(80/80)" - Effort Cost
80	0	80 -"Bonus(80/0)"	"Bonus(80/0)" - Effort Cost	0 - Effort Cost
0	80	80 -"Bonus(80/0)"	0 - Effort Cost	"Bonus(80/0)" - Effort Cost
0	0	0	0 - Effort Cost	0 - Effort Cost

In summary:

- The value of each good to the buyer depends on its quality level. A quality level of 0 generates no value for the buyer, and hence no earnings. A quality level of 80 generates 80 points for the buyer.
- The stated bonuses are only applicable in certain cases:
- A seller receives no bonus if their quality level is 0.

- If one of the quality levels is 0 while the other is 80, the buyer pays "Bonus (80/0)" to the seller with a quality level of 80.
- If both of the quality levels are 80, the buyer pays "Bonus (80/80)" to both of the sellers.

What if the buyer does not make a proposal or at least one of the sellers rejects the buyer's proposal?

- If the buyer does not make a proposal or at least one of the sellers rejects the proposal, the buyer and the two sellers will receive 0 points each for that period.

5. End of the Period Summary Screen

At the end of each period, the buyer and sellers will be shown a summary screen. The following information is displayed on this screen:

- The points that you individually earned (or lost) in this period.
- If a proposal is made, the specific terms stated the buyer's proposal
- If trade occurs:
  - each seller's quality level
  - sellers also observe both effort choices (the buyer does not observe effort choices)

Note: Your group members' points will *not* be displayed on your screen. Similarly, your points will *not* be displayed on their computer screens.

Furthermore, it is possible that buyers and sellers can incur losses in a given period. These losses are subtracted from your points balance, which accumulates across all periods throughout the duration of the experiment.

Please enter all the information on the summary screen in the record sheet supplied to you. This will help you keep track of your performance across periods so that you can learn from your past results.

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