

**Contract Enforcement, Social Efficiency, and Distribution:
Some Experimental Evidence¹**

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Abstract

We use economic experiments to investigate how different contract enforcement regimes affect efficiency and the distribution of surplus in vertically coordinated markets where contractors have market power. We find that if a third party (e.g. government) provides full enforcement of contracts, social efficiency is enhanced. Surprisingly, we find when enforcement is completely absent, social efficiency will not necessarily decrease because trading partners find ways to self enforce contracts. However, opportunistic behavior by some traders widens the distribution of surplus generated and leaves some sellers (growers) with ex-post profits below reservation levels. Finally, partial or one-sided enforcement causes significant efficiency losses by constraining the ability of the regulated party to self enforce contracts.

Recent controversies surrounding contract production in the poultry industry have raised concerns among policy makers that vertically coordinated industries might favor large agribusiness at the expense of growers. Among the issues that have received increased attention include the apparent lack of bargaining power possessed by growers when negotiating and performing under contracts, and the lack of transparency concerning how performance and payments are determined (Pierce and Stewart). As an example of the latter, Schrader and Wilson analyze data from a survey of broiler growers and suggest that many growers were concerned about the quality of inputs they received from contractors and the timeliness and accuracy of bird weighing. Because both input quality and the accuracy of weighing can affect performance outcomes, which in turn, determine compensation, there might be a lack of transparency in the determination of pay.³ Along similar lines, Hamilton points out that many broiler contracts contain a provision that allows a contractor to unilaterally change payment methods or pay rates, which provides integrators with the option of making discretionary ex post adjustments in pay.

While the lack of transparency can make third-party enforcement costly, the problem is exacerbated by the Secretary of Agriculture's limited power to enforce the Packers and Stockyard Act of 1921⁴. While the Secretary may investigate alleged violations of the Act, the Secretary has only limited power to bring administrative action to resolve many violations of the Act; in fact, in the poultry industry, the Secretary only has authority to bring administration action for problems related to payment (Farmer's

³ Some states have sought to improve transparency of performance measures. For example, Georgia passed HB 648 on April 7, 2004. This bill requires processors to provide "any statistical information and data used to determine compensation paid" at a grower's request. The bill also allows growers to be present when their birds are being weighed after growout and when feed is being weighed prior to delivery.

⁴ This act is meant to ensure free competition and fair trade practices in livestock and poultry markets. USDA-GIPSA is responsible for enforcing the Packers and Stockyards Act.

Legal Action Group, Inc.). As such, many contract terms between growers and processors are unenforceable by a third party, which can lead to contracting distortions. This means contracts are rendered “incomplete” by the lack of third-party enforcement.

While concentration and enforcement problems appear to be key issues for policy makers, there have been few efforts to clarify the consequences of increased or relaxed contractual enforcement on efficiency and distribution. Because the economic theory of contracts, as well as a theory of contract regulation, are still evolving areas of scholarship (Schwartz), agricultural economists do not understand the workings of contractually linked industries in the same way that they understand the functioning of commodity markets. Textbook principal-agent models are inadequate for studying enforcement problems because an underlying assumption of these models is that institutions for third party enforcement exist and function perfectly. When contracts are complete, as in the standard principal-agent model, there is little justification for government intervention because such intervention will constrain allowable activity and, hence, efficiency. Thus, most law and economics scholars focus on regulation of *incomplete* contracts or contracts that have unenforceable components. However, the theory of incomplete contracts has its own conceptual difficulties (Tirole; Maskin). Hence, it has been difficult for economists to study these issues in a unified and logically consistent microeconomic framework. In recent years, however, advances in the theory of *relational contracts* (Levin; Malcomson and Macleod), which accommodates unenforceable components in contracts, provides a promising framework that is relatively free of conceptual difficulties for studying agricultural contracting relationships.

Apart from theoretical issues, there has been a paucity of data for studying policy issues in vertically coordinated industries. This is partly due to the difficulty of acquiring data from firms, who often treat data as proprietary. Additionally, even when data is available, it may not contain sufficient detail for conducting credible empirical analysis. Wu points out that most contracting relationships tend to be very situation specific and contracts are often “customized” to fit the economic realities of specific trading partners. Moreover, explicit terms in contracts are often buttressed by numerous unwritten rules, implicit incentives, and tacit expectations so that it is difficult find observational data that captures every important aspect of the contracting environment. Finally, there is a lack of historic precedent for many of the contracting issues that have emerged recently in U.S. agriculture. Thus, there is little data available for assessing the impact of alternative enforcement regimes on efficiency and distribution of gains from trade.

Given the paucity of theoretical and empirical research, it is not surprising that economists and policy makers hold numerous opinions about how government regulations of contracts would affect efficiency and distribution of rents in vertically coordinated industries. Some economists suggest that too much regulation may have unintended consequences that, for example, may cause livestock industries to relocate out of states in which regulations are binding (e.g. Boehlje, et. al.). Others suggest that regulations and enforcement are necessary to enhance both fairness and competitiveness in contract agriculture which is becoming more concentrated and one-sided in favor of large agribusinesses (Harl, Et al.; Taylor).

This study uses experimental economics to investigate the effects of alternative enforcement regimes on contractually based markets where buyers have market power.

An experimental approach allows us to control the trading environment and create exogenous variation in third-party enforcement, which allows the researcher to isolate and estimate causal relationships. We view this study as an initial step in understanding the microeconomic forces that shape behavioral and trading patterns in a contractually-based economy where buyers have bargaining power, and different levels of contractual enforcement are available. Moreover, we motivate our experimental design using the theory of relational contracting, which, we believe, is a coherent framework for conceptualizing contractual relationships between trading parties and is representative of the emerging business relationships that dominate vertical relationships in modern agricultural markets. The insights generated in this study might inform policy makers about how more rigorous enforcement of contracts by government agencies can impact contractual efficiency and the distribution of rents across parties.

Noussair and Plott argue that experiments need not replicate field situations and all institutional details to retain relevance for policy analysis. Instead, experiments are valuable in that they allow economists to examine general theories that should apply more broadly. If a theory does not apply in simple, controlled environments, one must question whether the theory is appropriate for explaining behavior or predicting responses in more complex environments. Moreover, abstracting from reality is not unique to experiments; indeed, most economic studies base conclusions on models with simplifying assumptions and abstractions.

The use of experiments to study relational contracting is even more critical than in other areas of microeconomics. Because relational contracts are based on the notion of a repeated game, there are typically many equilibria so that unique theoretical predictions

are generally not forthcoming (Brown, Falk and Fehr). Thus, conducting empirical studies in a controlled environment free of confounding factors can illuminate our understanding of relational contracts.

One primary finding, which is fully consistent with intuition, is that full enforcement of contracts promotes social efficiency. More surprising is that a regime devoid of formal enforcement can generate comparable social efficiency. In such regimes, many subjects use contractual renewal and discretionary adjustments in contract terms to provide informal enforcement that is nearly perfectly substitutable with formal enforcement in terms of achieving high levels of social welfare. However, a negative side effect of informal enforcement is that it often exposes sellers (e.g. growers) to opportunistic behavior – nearly one in five sellers make *ex-post* profits that fall below reservation payoffs. Such opportunism does not occur under full enforcement. Another surprising result is that one-sided formal enforcement (buyers' obligations only) constrains subjects' ability to use informal enforcement, which results in significant efficiency losses, but elevated average earnings of sellers.

One might question whether the use of students rather than farmers weakens our results. We regard the use of students as a strength because growers' attitudes toward contracting issues may be politicized by recent discussions about the “oppressive” nature of contracts. Most university students are less familiar with these political entanglements and may therefore respond in a more neutral manner.

Experimental Design

Our experimental design captures several important features of agricultural contracting relationships. First, it captures the idea that buyers (e.g. processors) have market power,

i.e., there are fewer buyers than sellers (growers), so that not all sellers who want contracts get them. Second performance, generically referred to as “quality,” is unenforceable by a third party due to transparency problems. Third, buyers can make discretionary *ex-post* adjustments in compensation. Fourth, parties can track the reputations of past trading partners and have discretion to renew or dissolve existing business relationships based on past performance. Each feature is present in an experimental economy based on the design of Brown, Falk, and Fehr (BFF).

In the experiment subjects are partitioned into two groups: buyers and sellers. All trading takes place on networked computers enclosed in cubicles to eliminate between-subject visual contact. Moreover, anonymity is preserved by assigning all buyers and sellers numeric ID numbers. We partition each experiment into two trading sessions. Each trading session features a unique contract enforcement regime described below. Each session has 17 trading rounds – two practice rounds and 15 ‘live’ rounds that may determine eventual cash payment. ID numbers are fixed across rounds, which allows buyers and sellers to establish long-term relationships by trading with the same partner over multiple rounds, and changed between sessions, so that there is no reputational carryover between contracting regimes.

Within each trading round, buyers offer “contracts” to sellers specifying a price-quality combination for a unit of an abstract good.⁵ Sellers can accept or reject these offers. A buyer can make as many offers as desired in each round, but once one offer is accepted, all other offers are withdrawn and no additional offers can be made. Similarly, once a seller accepts an offer, no other offers can be entertained. In short, each buyer and

⁵ While most agricultural production contracts are not sales contracts, e.g., the buyer (processor) owns the chickens throughout the production process; processors are in a sense “buying” quality from the grower. That is, they purchase grower services to produce high-quality and cost-effective chickens.

seller can conclude at most one trade per round. No buyers are obligated to make offers and no sellers are obligated to accept offers in any round. When buyers or sellers do not enter contracts, a reservation payoffs is received (see below). At the start of each round, no buyer or seller has a contract; buyers must actively make offers to engage trade.

Buyers can extend two types of offers: public and private. Public offers are displayed on the computer screens of all sellers and buyers; any seller can accept any public offer. Private offers are extended by entering a specific seller's ID number into the computer. Only the seller identified is notified of the offer and he can chose to accept or reject it. Private offers enable long-term relationships, which lie at the core of the theory of relational contracts. For example, if a buyer predicts benefits from contracting with a specific seller and wants to establish a long-term relationship, the buyer can make a single, private offer to that seller at the beginning of a round rather than venturing into the open market and hoping that that seller is the first to accept the public offer.⁶ This is an essential feature of relational contracting as it implies that the promise of future *relationship-specific* gains from trade can sustain high performance in the current period.

In every round there are five buyers and seven sellers, which represents an unemployment rate of nearly 30 percent under full trading and means that buyers have significant bargaining power. Subjects maintained the same role (e.g., buyer) during both sessions and no subject participated in more than one experiment (two sessions).

In order to test the impact of enforcement on efficiency and surplus, we examine three enforcement regimes. First, in the “complete contract” (C) treatment, the computer fully and perfectly enforces all contractual components. That is, sellers must supply the

⁶ Firms often establish “private trades” by contacting specific suppliers with whom they have good relationships so they do not have endure costly public solicitations when the desired supplier is already known.

quality stipulated in the contract, and buyers must pay the agreed upon price. In the second treatment, which we call “Relational Contract 1” (RC1), the computer only enforces contractual price. Quality is unenforceable, i.e., sellers can supply quality that differs from contractual specifications. Thus, buyers must use implicit or “relational incentives” to enforce quality. In other words, the parties must use trust and the promise of future relationship-specific payoffs to sustain high quality in the current round. This treatment mimics real-world situations where neither input quality or the measurement of output (e.g. weighing of birds) is verifiable by a third party. In this case, measured performance is unenforceable because when quality is low, both parties can blame the other party (e.g., grower claims poor quality inputs or inaccurate weighing, and processor claims grower shirked) so that verifiability and enforcement of the contract is extremely costly. The third treatment is called “Relational Contract 2” (RC2), which is identical to RC1 with the added feature that the buyer can make *ex-post* adjustments to the promised price. In other words, after a buyer observes the seller’s delivered quality, the buyer can choose a price that deviates from contract specifications. This treatment is inspired by Hamilton’s claim that integrators can unilaterally change payment methods or rates. Our C and RC1 treatments are nearly identical to BFF’s experiments, with the exception that we had fewer buyers and sellers in each trading session (though the ratio is nearly identical). Our RC2 treatment is unique to our study and allows examination of a broader range of implicit incentive mechanisms that can be used to regulate relational trading. We show that our RC2 treatment can provide significant new insights into the nature of relational contracting. All rounds in a particular session feature the same treatment.

Round specific payouts are determined for buyers as follows:

$$(1) \pi_b = \begin{cases} 10Q - P & \text{if agreement reached} \\ 0 & \text{if no agreement reached} \end{cases}$$

where π_b is the buyer's payment, Q is the actual quality chosen by the seller, and P is the actual price received by the seller. All payments are given in experimental points where subjects earn one dollar for 70 points. The seller's profit is:

$$(2) \pi_s = \begin{cases} P - c(Q) & \text{if agreement reached} \\ r & \text{if no agreement reached} \end{cases}$$

where r is a reservation payoff (=5 or =10 depending on session) in the absence of trade. During the experiment, Q is restricted to be an integer in the set $\{1,2,\dots,10\}$, and P is restricted to be an integer from the set $\{0,1,2,\dots,100\}$.⁷ The cost function, $c(Q)$, is fully represented by the following schedule of quality-cost combinations: $\{1,0\}$, $\{2,1\}$, $\{3,2\}$, $\{4,4\}$, $\{5,6\}$, $\{6,8\}$, $\{7,10\}$, $\{8,12\}$, $\{9,15\}$, $\{10, 18\}$ so that $c(Q)$ is increasing and convex. At the end of each round, each subject learns the payoff for each subject (buyer and seller) involved in the transaction.

We conducted thirteen experiments. Each experiment included two of the three treatments (C, RC1, or RC2). Each treatment session lasted 17 rounds (two practice and 15 live) and each subject participated in both treatment sessions during an experiment, which means that order effects might arise. Hence, the design counterbalances the order of appearance for each of the three enforcement regime sessions.⁸ At the end of an experiment, one session was randomly chosen via a public roll of a die to be the paying session. Before subjects were paid, they provided demographic information. There were a total of 26 sessions (two in each of the thirteen experiments) where 13 were C

⁷ Note that the P and Q offered in the contract are also restricted to these sets.

⁸ For example, if C was the first session of the night and RC1 was the second session, we made sure that in the next experiment RC1 was first followed by C. We did this for all of our treatments.

treatments, seven were RC1 treatments, and six were RC2 treatments. One hundred fifty-six subjects made 1,885 trades. The experimental economy was programmed using Z-TREE software (Fischbacher). For five C treatments, $r=10$, and for the others, $r=5$; $r=5$ for all RC1 and RC2 sessions.

Subjects were recruited from existing e-mail lists from the various departments throughout a Midwestern university, and through paid advertisements in campus newspapers. Potential subjects were informed that actual earnings depend upon the rules of the game and the participant's and other participants' actions. Average earnings were \$23 per subject and ranged from \$13 to \$41. Additional details of the experiment, including instructions are available in the Appendix.

Predictions

Under C, quality specified in an agreement is enforceable, i.e., the seller cannot deviate from contracted Q . Thus, the buyer's profit-maximizing contract choice is determined by solving the problem:

$$(3) \quad \max_{Q,P} (10Q - P) \quad \text{s.t.} \quad P - c(Q) \geq 5 \quad (\text{Participation constraint})$$

If the constraint holds with equality (which it must for a profit-maximizing buyer), solving for P , and substituting into the objective function yields:

$$(4) \quad \max_Q (10Q - 5 - c(Q))$$

which gives the first-order condition:

$$(5) \quad 10 - c'(Q) = 0$$

However, one can see from the cost schedule above that marginal cost never exceeds 3 so that the buyer chooses to implement the maximum quality level, $Q^* = 10$. With Q^* in hand, it is easy to solve for $P^* = 23$ from the participation constraint to ensure that the

seller will accept the contract. Because buyer and seller can earn at least as much when trading rather than not trading, we predict all five buyers make offers and five sellers accept these offers; hence, in equilibrium, five trades take place in the round and joint surplus per trade is given by:

$$(6) \quad S^C = \pi_b + \pi_s - r$$

The above prediction holds in the final round of a “live” session (round 15). But it is straightforward to show, by backward induction, that the same outcome occurs in every round prior to round 15.

Under RC1, quality is unenforceable so that the seller can deliver quality different than contract specifications. To determine what quality the seller will choose and what contract offer the buyer will make, note that it was common knowledge that each live session ends at the end of period 15. Thus, focusing on round 15, we must analyze the interaction between the buyer and seller by using backward induction. Because the seller is the last mover, consider what the seller would do given that she has accepted a contract and is guaranteed some payment, P . Note from her objective function (2) that profit is maximized when $Q = 1$ so that production cost is zero. Hence, equilibrium quality is $Q^{RC1} = 1$. The buyer, anticipating that the seller will deviate from agreements specifying $Q > 1$, will offer just enough to ensure participation; that is $P^{RC1} = 5$. Again, both buyers and sellers earn at least as much under trade as under no trade. Thus, in equilibrium, five trades take place during the round and joint surplus is given by $S^{RC1} = 5$. The unenforceability of quality leads to substantially lower joint surplus and quality. Using backward induction, it is easy to verify the same outcome in all previous rounds.

In RC2, the buyer first offers a contract, which specifies desired price and quality. If a seller accepts, the seller then chooses actual quality, i.e, the seller may deviate from desired quality. After the buyer observes delivered quality, she chooses a payment at her discretion, which can deviate from the price specified in the contract.

In order to determine equilibrium in RC2, first consider how the buyer, who is the last mover, will respond to a given quality, Q^0 , chosen by the seller. The buyer's payoff is $\pi_b = 10Q^0 - P$ and therefore profit is maximized by setting $P^{RC2} = 0$. The seller, anticipating that the buyer will renege on any contracting agreement with positive payment, will expect to earn $\pi_s = P^{RC2} - c(Q^0) = 0 - c(Q^0) < 5$ from the contract, which is lower than reservation earnings. Thus, the seller accepts no contract. In equilibrium, no trade takes place and no surplus is earned in round 15. A similar logic follows for round 14 and earlier. With two-sided discretion, the contracting market is completely destroyed by opportunism.

The above analysis provides us with several hypotheses:

HYPOTHESIS 1: *When Q is unenforceable (RC1 or RC2):*

- a) Equilibrium quality will be lower than the case when quality is enforceable (C).*
- b) Total surplus will be lower than the case when quality is enforceable (C).*

HYPOTHESIS 2: *When buyers have bargaining power, whether quality is enforceable or not, all surplus goes to the buyer and trading sellers earn reservation payoffs.*

HYPOTHESIS 3: *If both quality and payment are unenforceable (RC2), the contracting market collapses and no trade occurs in equilibrium.*

A key point to emphasize is that, due to market power, sellers never earn rents above reservation payoffs. Hence, enforcement is predicted not to help growers. Sellers

can only be helped by raising reservation payoffs. Moreover, enforcement is predicted only to raise efficiency. One can see that quality (and hence total surplus) improves as one moves from no enforcement (RC2), where no trade occurs, to one sided enforcement (RC1), where the market is restored but only delivers minimal quality and surplus, to full enforcement (C), where socially efficient quality and surplus emerges.

There are two complicating factors that may prevent the above predictions from being manifested in actual trading. First, as mentioned earlier, multiple equilibria are not uncommon in repeated games, so it should not be surprising if actual behavior deviates from that predicted above. Second, emerging economic research on *social preferences* suggest that the population is not comprised of only rational, self-regarding profit-maximizing individuals (Fehr and Gächter; Charness and Rabin; Engelmann and Strobel, among others) on which the above predictions depend. Charness and Rabin point out that a non-trivial fraction of the population cares about social efficiency, but withdraw willingness to sacrifice (i.e. *concern withdrawal*) for the sake of social efficiency when others are unwilling to sacrifice. The presence of these types of individuals will allow for reputation building, which can fundamentally alter theoretical predictions as individuals concerned about social welfare can sustain quality and surplus levels that exceed predicted levels in RC1 and RC2, even in period 15. Moreover, if sellers manifest *concern withdrawal* then buyers have an incentive to offer sellers a non-trivial share of the surplus to prevent concern withdrawal. The presence of relationship-specific rents in period 15 implies that in period 14 and earlier, buyers and sellers have an incentive to stick to the terms of the contract to facilitate future trades. Thus, the promise of future payoffs may discipline trading partners in any given round and provide an informal

enforcement mechanism to sustain high quality, even in the absence of formal enforcement. Given the possibility of multiple equilibria and the existence of types that are not purely self interested, it becomes even more important to study subjects' actual behavior, rather than to rely purely on theory. Relying only on theory may lead to conclusions that are misleading and policy prescriptions that are counter-productive. Nonetheless, our theoretical predictions are useful for heuristic purposes as they provide an organizing framework.

Results

Trading

In this section, we analyze trading data (table 1) from our three treatment sessions. A striking result is that the data does not support hypothesis 3, which predicts that no trade should take place under RC2. In fact more trade takes place under RC2 (nearly 100 percent) than under C or RC1. Of the three treatments, RC2 may most resemble agricultural markets; one can see that such markets can remain active even if no formal enforcement is possible. Two possible explanations for this result are that (1) subjects are simply playing a different equilibrium than the one our model predicts, which is not unexpected in repeated games, and (2) subjects have heterogeneous social preferences as discussed earlier. Indeed, BFF ran experiments that are similar to our C and RC1 and find that actual play under RC1 significantly differs from predicted levels. BFF postulate that social preferences, which can make it possible for buyers to credibly promise sellers positive relationship specific rents even in period 15, are used by buyers to discipline sellers in earlier periods. If we carry this rationale over to our RC2, then it is possible that buyers who care about social welfare can credibly promise not to shirk on payments

to sellers after observing quality. If enough of these buyers exist in the economy and sellers know this, then sellers may be willing to sign contracts and the market does not collapse. This may be a mechanism through which relationship-specific rents can be generated even in the last round of the finitely repeated game, which allows relational contracting to form in earlier periods.

Quality and Price

Average quality (table 1) was highest in the C treatment at 9.20 followed by RC2 and RC1. This is consistent with hypothesis 1a, which predicts that the highest quality level would emerge under C. A Kruskal-Wallis (KW) test conducted across all three treatments strongly suggests that the three treatments differ in average quality delivered ($p < 0.001$). Surprisingly, RC2, which represents a lower degree of enforceability than RC1 delivered higher quality than RC1. A possible explanation for this result is that the ability of buyers to make discretionary *ex-post* adjustments in price provides a mechanism for providing quality incentives even in the absence of formal enforcement. That is, if sellers anticipate that buyers will reward for high performance or deduct for poor performance, then sellers may be more reluctant to perform poorly. These sorts of price adjustments are consistent with “discretionary bonuses” of the sort discussed by Levin, which can be used to provide incentives in relational contracts. Lee and Wu model a relational contracting problem in which buyers have market power and suggest that *negative* bonuses (i.e. price deducts) will be used by buyers to incentivize sellers. If buyers are indeed making discretionary adjustments to reward or punish seller performance, then we ought to observe a correlation between performance and price adjustments.

Table 2 reports the types of adjustments made by buyers cross tabulated against performance for all RC2 trades. The patterns indicate that buyers do make adjustments contingent on performance which provides a mechanism for buyers to deliver incentives. Note that conditional on good performance (quality at least meets agreed upon quality), rewards (upward adjustments in price) are used 19% (10.9%/56.8% \approx 19%) of the time whereas conditional on poor performance, sellers are rewarded only 1.5% (0.6%/43.2%) of the time. Conditional on good performance, deducts (downward price adjustments) are observed 31% of the time, but this shoots up to about 80% conditional on bad performance. These patterns suggest that (1) discretionary price adjustments do appear to be dependent on quality which is consistent with our intuition that these adjustments are informal means of providing incentives just as Levin predicts, and (2) that opportunistic behavior, independent of incentive provision, still occurs as evidenced by the fact that, in 31% of trades where sellers met performance obligations, buyers still made downward adjustments. A final interesting observation is that deducts are used much more frequently than rewards; specifically, while 56.8% of total trades in RC2 involved sellers meeting obligations, rewards were observed only 11.5% of the time. Thus pattern is consistent with Lee and Wu's prediction that deducts, not rewards, will be used when buyers have market power.⁹

Table 3 allows us to examine the role of discretionary price adjustments further. This table reports the results of a probit regression that regresses the probability of a deduct on both positive and negative differences between quality supplied and quality specified in the contract. A number of additional regressors were included as control

⁹ Lee and Wu's prediction is not driven by social preferences as their relational contracting model makes neoclassical rationality assumptions about the preferences of agents. Instead, it is driven purely by market power.

variables. The reported results are in terms of changes in probability for small changes in the independent variables. Note that when suppliers exceed quality obligations, it does not help them avoid deducts. However, when suppliers underperformed relative to the contractually specified level of quality, this increased the probability of a deduct, as the marginal effect is 0.167 and significant at the 5% level. It is interesting to note that the probability of a deduct falls significantly if a trade was based on a private offer suggesting that when buyers hand picked sellers, the buyer is less likely to punish or behave opportunistically toward these sellers. One possible explanation is that, because private trades indicate that relational trading is taking place, it is unlikely that the parties will renege on their agreements for fear of violating self enforcement constraints. In other words, in relational contracting, it is the promise of future payoffs that disciplines behavior in a current period. If sellers or buyers shirk on their agreements for short term gains, they risk being terminated by the other party and foregoing long term gains. Self enforcement therefore place bounds on the degree to which parties can deviate from the agreement without causing the relationship to unravel. Indeed, when we calculated summary statistics (not shown in table), our data showed that average quality shortfall was -0.29 under private trade whereas it was -1.46 under public trade. Similarly, average price adjustment was -4.66 under private trade and -11.9 under public trade. This is broadly consistent with the notion that subjects tend not to deviate from agreements as much under private trade suggesting that the threat of separation can impose discipline on the parties.

Surplus and Profits

We begin by investigating the distribution of surplus across buyers and sellers. Table 1 indicates that average surplus differs across the three treatments, which is not surprising considering that average surplus differences should track quality differences. What is most interesting is that sellers earn the highest *average* profits under RC1; in fact, their actual profit exceeds promised profit from the contract. One possible reason for this is that, in RC1, sellers have the discretionary power to select Q as Q is unenforceable; thus, this discretionary power appears to allow sellers to offset some of the market power of buyers. Buyers only have informal means of enforcing quality either via termination or by appealing to sellers' social preferences. That is, if some sellers have a concern for social efficiency, but have a tendency toward concern withdrawal, then these sellers will exert high quality so long as buyers are willing to share rents with these sellers. Indeed, 23% of trades (not reported in table 1) in RC1 involved sellers choosing $Q = 10$ and these sellers earned an average of 41.6. On the other hand, in the 77% of trades where sellers supplied $Q < 10$, average seller profit was 21.4. This is broadly consistent with the notion that some sellers are willing to supply the socially efficient amount of quality, and that their buyers are also willing to share the gains from trade leading to efficient and mutually beneficial trade.

We also investigate how much of the surplus is shared with sellers across the three treatments. Note that hypothesis 2 suggests that, with buyer market power, sellers will always receive rents equal to reservation payoffs. However, the data suggests that average earnings are higher than reservation payoffs across all three treatments. For all three treatments, Wilcoxon sign rank tests yielded *p-values* of 0.00 strongly rejecting the equality of average profits and reservation payoffs for sellers. Also, average profits of

sellers differ across the three treatments as verified by a Kruskal-Wallis test (p -value=0.00).

Two specific observations caught our attention. First, in RC1, sellers' average profit exceeds promised profit (25.98 vs. 21.29). We conjecture that sellers' discretionary power to deviate from contracted quality gives them the ability to take back some of the monopsony rents buyers had captured. In contrast, in RC2, sellers earned significantly less ex post rents than they were promised (16.04 vs. 23.21). This could be because some buyers behave opportunistically by making downward adjustments in price even when their sellers performed (recall that conditional on good performance, deducts were still observed 31% of the time). Second, in the C and RC1 treatments, sellers rarely earned profits that fell below reservation levels so that, on average, sellers made good decisions in accepting and rejecting contracts. This is verified by the fact that average promised profits exceed reservation payoffs in all three treatments.¹⁰ However, in RC2, the possibility of opportunism by buyers resulted in sellers earning less than reservation payoffs, ex post, in nearly 19% of trades. Thus, although sellers in RC2 accepted the best contracts, on average, nearly one in five sellers ended up with profits that fell below reservation payoffs. This is consistent with anecdotal evidence in the real world where some growers complain that they did not earn as much as they were promised.

Figure 1 offers another perspective on how sellers did in terms of the proportion of total surplus they captured across rounds. Note that in both C and RC2, sellers consistently captured less than 50% of total surplus; in fact, in round 15, sellers earned a negative fraction of total surplus, which is possible when surplus is positive but sellers earn negative profits. At the other extreme, under RC1, sellers rarely earned less than

¹⁰ Reservation pay was 5 for RC1 and RC2 and either 5 or 10 in C.

50% of total surplus and in some cases earned more than 100% of surplus (surplus positive, buyers earn negative profits, and sellers' profits exceed surplus).

Table 4 allows for a more complete examination of surplus and profits. This table reports the results from three regressions where the three dependent variables are surplus, buyer profit and seller profit. These regressions allow us to determine the impact of RC1 and RC2 on each of the dependent variables. Moreover, a number of control variables were used including demographic information obtained from post experiment surveys, and fixed effects for experiments, rounds, buyers, and sellers. Note that a dummy for C was omitted so that C represents the base treatment against which comparisons are made. The "reservation" variable represents the seller's reservation payoff to account for the fact that reservation payoffs varied from 5 to 10 in some C sessions. While our main variables of interest are RC1 and RC2, they were also interacted with a "private" dummy because it is very likely that the incremental impact of RC1 and RC2 will depend on whether trades were based on private or public offers. This is because sellers who receive private offers may have established long term relational contracts with buyers, which may affect the offers and responses they receive from buyers.

Focusing on regression (1) first, an F-test for the joint significances of RC1 and RC1×Private yielded a *p-value* of 0.00 (not reported in table). This suggests that the conditional mean of surplus differs between the C and RC1 treatments. To examine this further, the incremental impact of RC1 on surplus given that trades are public is estimated to be -47.04 and significant at the 5% level. Under private trading, the incremental impact of RC1 on surplus is given by the sum of the coefficients for RC1 and Private×RC1, which is estimated to be -21.33 (not reported in table) with a standard error

of 1.65 (p -value=0.00). Thus, irrespective of whether private or public trading occurs, surplus decreases under RC1 relative to C, which suggests that the absence of quality enforcement has significant efficiency costs. However, given that the estimated coefficient for Private \times RC1 is positive (25.72) and significant at the 5% level, private trading appears to significantly mitigate efficiency losses. Sellers who do not trade privately may have trouble establishing relational contracts; thus, these sellers conduct most of their trading outside of relationships where no relational incentives exist to enforce their behavior. These sellers appear to impose significant costs on society. To put this into perspective, social surplus is maximized when $Q=10$, which results in a surplus of 77. The loss of 47.04 in surplus represents a 60% reduction in social efficiency relative to first best. The fact that the coefficient Private \times RC1 is 25.72 and significant at the 5% level may imply that relational contracting can provide powerful informal enforcement of quality. These results support Hypothesis 1b which predicts that the absence of enforcement will lower total surplus.

Turning now to RC2, an F-test for the joint significance of RC2 and Private \times RC2 yields a p -value of 0.00 (not reported in table). This suggests that the conditional mean of surplus differs between C and RC2. The incremental impact of RC2 on surplus under public trade is estimated to be -6.86 and significant at the 5% level. The incremental impact of RC2 on surplus under private trade is given by the sum of the coefficients of RC2 and Private \times RC2, which was estimated to be 4.13 (not reported in table) with a standard error of 3.44. Because the p -value is 0.34, this estimate is not significantly different from zero; thus, RC2 does not appear to reduce surplus relative to C, provided that subjects exploit private trades to facilitate relational contracting. This result is in

stark contrast to RC1 as it appears that relational contracting combined with the ability of buyers to make discretionary adjustments in price provides informal enforcement that can nearly perfectly substitute for formal enforcement in producing high levels of social efficiency. Discretionary *ex post* adjustments in price are not unlike the discretionary bonuses (deducts) discussed by Levin in reference to relational contracting. While these discretionary price adjustments can clearly deliver incentives, they are less effective when they are used outside of relational trading. Parties who trade privately, and therefore are involved in relational contracts, tend to increase surplus by 10.99 over those who are not involved in relational trading (public trade). These results suggest that Hypothesis 1b will not hold when parties have a sufficient range of informal enforcement mechanisms at their disposal.

Because relational contracts involve long term relationships which can only be established through private trading, we have an indirect way of estimating the returns to relational contracting. Note that the returns to private trading under RC1 (RC2 =0) is the sum of the coefficients for Private and Private \times RC1, which is estimated to be 22.08 (not reported in table) with a standard error of 2.04 (*p-value*=0.00). Consequently, returns to relational trading appears to be significant under RC1 yielding an average surplus gain of 22.08. For RC2, the estimated return to relational trading is 7.35 with a standard error of 2.13 (*p-value*=0.001). Again, relational trading appears to be important for enhancing surplus, although the magnitude is smaller than in the case of RC1. In contrast, relational trading appears to be counter-productive under C, as evidenced by the fact that the Private coefficient is -3.64 and significantly different from zero at the 5% level of significance. Thus, there appears to be no benefit to relational contracting in complete

Table 1. Key Summary Statistics

		Quality				
	#Trades/#Possible	Mean	STD	Min	Max	
C	942/975 (97%)	9.20	1.49	2	10	
RC1	512/525 (98%)	5.13	3.55	1	10	
RC2	449/450 (99.8%)	7.68	2.51	1	10	

		Surplus				
	#Trades/#Possible	Mean	STD	Min	Max	
C	“	69.13	11.63	14	77	
RC1	“	38.90	28.42	5	77	
RC2	“	59.54	19.66	5	77	

		Buyer Profit				
	#Trades/#Possible	Mean	STD	Min	Max	
C	“	59.10	16.20	-10	85	
RC1	“	17.88	23.96	-90	99	
RC2	“	48.50	21.65	-25	100	

		Seller Profit					
	#Trades/#Possible	Promised Profit	Mean Actual Profit	STD	Min	Max	% of Trades where Profits Fall Below Reservation
C	“	17.01	17.01	11.62	-5	82	5%
RC1	“	21.29	25.98	18.44	-17	100	1.8%
RC2	“	23.21	16.04	13.88	-18	55	18.7%

Note. Promised profit is the amount the seller earns if both parties stick to the terms of the contract.

Table 2. Discretionary Adjustments Made by Buyers According to Performance.

	Discretionary Price Adjustment by Buyer			
	<i>Reward</i>	<i>No Adjust.</i>	<i>Deduct</i>	<i>Overall</i>
$Q \geq Q^*$	10.9%	29.3%	17.6%	56.8%
$Q < Q^*$	0.6%	7.8%	34.7%	43.2%
<i>Overall</i>	11.5%	36.1%	52.3%	100%

Note 1: Q^* = quality supplier agreed to deliver in the contract.

Note 2: Reported in % of total number of transactions.

