Contract Enforcement, Social Efficiency, and Distribution:

Some Experimental Evidence

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

Key Words: Contracts; relational contracts; implicit contracts; market interaction; experimental economics; repeated transaction; opportunistic behavior.

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Recent controversies surrounding contract production in agriculture 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 1997). As an example of the latter, Schrader and Wilson (2001) 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. [1] Along similar lines, Hamilton (2001) points out that many broiler contracts contain a provision that allows a contractor to unilaterally change payment methods or rates, which provides integrators with the option of making discretionary *ex post* adjustments in pay. The lack of transparency can make it difficult for third-parties, such as courts or regulators, to enforce contracts. This means contracts may be rendered "incomplete" by limits to enforcement.

While enforcement problems appear to be key issues for policy makers, there have been few efforts to clarify the consequences of contractual enforcement on efficiency and distribution. Textbook principal-agent models are inadequate for studying enforcement problems because an underlying assumption of these models is that third-party

[1] 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 birds and feed are being weighed.

enforcement of contracts is perfect. Thus, most law and economics scholars focus on contracting environments where there are limits to third-party enforcement.

Incomplete contracts combined with the possibility of repeated trading, which is common in some agricultural subsectors[2], enables private parties to devise informal enforcement mechanisms (i.e. form *relational contracts*) to mitigate hold-ups and other contracting distortions (Telser 1980; Klein 1996; Macleod and Malcomson 1989; Levin 2003). Relational contracts also fit many of the stylized facts of agricultural contracting because even if explicit contracts exist to govern some obligations, some dimensions of performance, such as quantity commitments, timing of deliveries, scheduling of flock placements, and/or contract renewal policies are frequently omitted from explicit contracts, opening the door for the use of informal incentives.

Apart from theoretical issues, there is a paucity of adequate data for studying policy issues in vertically coordinated industries. 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. There is also 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 trading outcomes.

[2]In personal communication, senior members of the California Processing Tomato Growers Association suggested to one of the authors that, in any given year, processors will re-sign 90% of growers with whom they contracted with in the previous year. Thus, repeat trading is quite common. As another example, Hamilton (2001, pg. 85) suggests that, while most broiler contracts are flock-to-flock, some contracts implicitly make relationships continuous until terminated. This suggests that the same parties may trade repeatedly across many flocks. Moreover, the fact that growers must make relationship specific investments in long term assets such as new housing facilities that are specific to a particular processor implies that there is an expectation that trading will occur over multiple flocks or seasons. Additionally, the fact that there is physical specificity in the broiler industry (Vukina and Leegomonchai 2006) , which limits the number of trading partners in specific geographical regions, suggest that a certain amount of repeat trading with the same partner may unavoidable.

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 agricultural contracts would affect efficiency and distribution. 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. 2001). Others suggest that regulations and enforcement are necessary to enhance both fairness and competitiveness in contract agriculture which is becoming more one-sided in favor of large agribusinesses (Harl, et al. 2001; Taylor 2002).

This study uses experimental economics to investigate the effects of third-party enforcement on contractually based markets where there is concentration in favor of buyers. Our experimental setup is based on the design of Brown, Falk and Fehr (2004); two of our treatments are identical to theirs but we add a third treatment that allows us to examine issues pertinent to agricultural contracting. An experimental approach allows us to 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 third-party contractual enforcement are available. The insights generated in this study might inform policy makers about how policies that increase transparency and/or third-party enforcement of contracts can impact efficiency and distribution of rents.

Noussair and Plott (1995) 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 incorporate simplifying assumptions and abstractions.

One primary finding is that full third-party enforcement of contracts promotes social efficiency. However, we also find that a regime devoid of third-party 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 can be nearly perfectively substitutable with formal enforcement in terms of achieving high levels of social surplus. This finding is consistent with the case studies provided by Anderson (1999) and Gow, et al. (2000), which show that self-enforcing agreements can substitute for inefficient third party enforcement. However, we find that a negative side effect of informal enforcement is that it often exposes sellers (e.g. growers) to opportunistic behavior – a non-trivial fraction of trades resulted in sellers making *ex-post* profits that fell below reservation payoffs. An interesting result is that one-sided formal enforcement (buyers' obligations only) constrains subjects' ability to use informal enforcement, which resulted in significant efficiency losses, but increased seller earnings.

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

Motivation

Before describing our experiment in detail, we provide a discussion about some general features of agricultural contracts to motivate our design. Numerous agricultural economists have suggested that contracts are mechanisms for facilitating vertical coordination where processors can provide incentives for quality and/or other performance factors that enhance profits. In a typical contract, a grower agrees to sell or raise crops or animals in a manner that is consistent with conditions in a written or verbal agreement.[3] Growers are then paid according to a price established in the agreement.

At the heart of these contracts are the incentive contained within the agreement to motivate grower performance. In practice, a processor may care about a range of performance factors, including quality, on-time deliveries, product consistency, ability to use inputs efficiently, low levels of pathogens to ensure food safety, etc. While there is considerable heterogeneity in performance factors across contracts, the important point to note is that contracts are essentially incentive devices for increasing productivity and the value of trade. Thus, in choosing an experimental platform, we were primarily interested in a design that would allow us to assess the incentive effects of contracts.

There are two popular lines of investigation for studying contractual incentives. In "complete" contracting environments, where third-party enforcement is assumed to be perfect, the crucial issues of interest are how variations in explicit incentive structures might affect productivity. However, if we assume that there are limits to enforcement and/or other impediments for "complete" contracting, then parties may be constrained in their ability to structure explicit incentives and thus must also rely on implicit incentives,

[3] Under the Uniform Commercial Code, oral agreements are considered enforceable contracts if growers are deemed to be "merchants" (Hamilton (1995.

such as discretionary *ex post* adjustments in contract terms that can affect future trading equilibria. In practice, contractors can use discretionary adjustments in pay and/or contract termination or renewal policies to achieve this purpose.

Given that there are some barriers to third-party enforcement of agricultural contracts, it seems that implicit incentives cannot be ignored. Moreover, even if some explicit incentives exist in an incomplete contract, informal incentives will supplement explicit incentives in motivating performance. For example, although broiler growers typically face contracts that contain explicit incentives for efficient use of inputs, processors supplement these incentives with discretionary adjustments in payment terms and have discretion over the number and frequency of flock placements which affects future pay. Some economists have also suggested that when there are limits to enforcement, then it may be optimal to leave explicit incentives out entirely even if some dimensions of performance are enforceable (Bernheim and Whinston 1998). Thus, the focus of our study is on implicit incentives and how interventions (e.g. through new policy or new institutions) that impact third-party enforcement of contracts will affect trading efficiency and distribution.

Details of the Design

We follow the basic design of Brown, Falk and Fehr (BFF) and two of our treatments, to be discussed below, are identical to theirs. In addition, we add a third treatment which is unique to our study and allows us to capture certain features of agricultural contracts. In this treatment, we assume that buyers can make discretionary *ex post* adjustments in compensation in some treatments. Hamilton (2001) points out that some contractors can make unilateral adjustments in payments *ex post* so this allowance is consistent with real world aspects of agricultural contracting. Moreover, in light of our discussion in the introduction, we impose market concentration in favor of buyers (e.g. processors). Finally, we allow parties can track the reputations of past trading partners and have discretion to renew or dissolve existing business relationships based on past performance. This creates the possibility of relational contracting where the promise of future relationship specific gains from trade can be used to provide informal incentives to discipline current behavior.

In each 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. 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 identify each other across rounds and develop reputation capital.

Within each trading round, buyers offer "contracts" to sellers specifying a price-quality combination for a unit of an abstract good.[4] While processors in the real world may care about different performance factors, for simplicity and economy of words, we will generically use "quality" as our main performance factor. Sellers can only accept or reject 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

[4] While most agricultural production contracts are not sales contracts, e.g., the buyer (processor) owns the crops or animals throughout the production process; processors are in a sense "buying" quality from the grower.

buyer and 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.

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 only he can choose to accept 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 (i.e. can earn relationship specific rents) and wants to establish a long-term relationship, the buyer can make a single, private offer to that seller in each round rather than venturing into the open market and hoping that that seller is the first to accept offer.[5] This is an essential feature of relational contracting as it implies that the promise of future relationship-specific gains from trade can sustain cooperation in the current period.

In every round there are five buyers and seven sellers. The fact that there are fewer buyers than sellers implies that there is market concentration in favor of buyers; i.e. two sellers in each round are left without contracts. This forces sellers to compete for a limited number of contracts which tilts bargaining power in favor of buyers because the demand for contracts is greater than the supply of contracts.[6] Excess demand for contracts is roughly consistent with stylized facts in some agricultural sectors. For example, according to Mitchell (2004), most chicken processors have waiting lists of growers who want to become growers and some existing growers would like to add capacity to expand contract

[5] 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.
[6] Camerer and Fehr (2006) report that in similar types of experiments with excess sellers, seller competition tends to drive buyers to make less favorable offers to sellers.

production. Our subjects maintained the same role (e.g., buyer) during both sessions in an experiment and no subject participated in more than one experiment (two sessions).

In order to test the impact of third party 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 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. 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 by a third party 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 formal enforcement of the contract is extremely costly. Our C and RC1 treatments are identical to BFF's. The third treatment is called "Relational Contract 2" (RC2) is unique to our study. This treatment is similar to RC1 except for 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. Our RC2 treatment allows for the 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 relative to the findings of BFF.

Round specific payouts are determined for buyers as follows:

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

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:

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

where r is a reservation payoff (=5 or =10 depending on session) in the absence of trade. We impose these reservation utilities for sellers to ensure that our seller subjects earned some money during the experiment. Varying the level of reservation payoffs would only induce buyers to change their price offers to ensure that sellers' reservation payoffs are covered, but efficiency would not be impacted. During the experiment, Q is restricted to be an integer in the set {1,2,…,10}, and P is restricted to be an integer in the set {0,1,2,…,100}. 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. An important point to note is that marginal cost never exceeds "3" and that marginal benefits to the buyer always exceeds "3" so that first best is achieved if the seller delivers maximum quality. At the end of each round, each subject is informed of her own payoffs and her trading partner's payoff.

We conducted thirteen experiments. Each experiment included two of the three treatments (C, RC1, or RC2). Each subject participated in both treatment sessions during an experiment, which means that order effects might arise.[7] Hence, the design

[7] At the end of an experiment, one session was randomly chosen via a public roll of a die to be the paying session. This is standard practice in the experimental economics literature. Potential subjects were informed

counterbalances the order of appearance for each of the three enforcement regime sessions.[8] 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 treatments, seven were RC1 treatments, and six were RC2 treatments. One hundred fifty-six subjects made 1,903 out of a total of 1,950 possible trades across all sessions. Table 1 provides a summary of treatments, sessions, participants, rounds, and trades. The experimental economy was programmed using Z-TREE software (Fischbacher 1999). For five C treatments, r=10, and for the others, r=5; r=5 for all RC1 and RC2 sessions. Additional details of the experiment, including instructions are available in the Appendix.

Predictions

In this section, we provide theoretical predictions under each contract enforcement treatment. Like BFF, we use the theory of repeated games to generate our baseline theoretical predictions. We focus on the case where the seller's reservation payoff r =5, but the case where r= 10 is analyzed similarly.

In treatment C, the sequence of events within a round is as follows. First the buyer offers a contract which specifies desired P and Q. If a seller accepts, payoffs are determined and the round ends. Note that Q and P specified in an agreement are third party enforceable, i.e., the seller and buyer cannot deviate from contracted Q and P. Thus, the buyer's profit-maximizing contract choice is determined by solving the problem:

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

Substituting the binding constraint into the objective function yields:

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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.

[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.

(4) $$\max_{Q}\left(10Q-5-c(Q)\right)$$

which gives the first-order condition:

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

However, one can see from the cost schedule above that marginal cost never exceeds 3 so that the buyer chooses to implement maximum quality, $Q^* = 10$. 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 is given by:

(6) $$S^C=\pi_b+\pi_s-r=77$$

Since perfect third party enforcement exists, parties have no incentive to deviate from this prediction in any round prior to round of a session.

Under RC1, the sequence of events within a round are the same as under C except after a seller accepts, Q is unenforceable by a third party so that the seller can deviate from quality specified in the contract. After Q is chosen by the seller, payoffs are determined and the round ends. To determine what Q 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, we must use backward induction and begin our analysis in round 15 of the finitely repeated game. Note that within a round, the interaction is sequential where the seller is the last mover. 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 sequence of events within a round is the same as RC1 except that after the seller chooses quality, the buyer 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 by a third party (RC1 or RC2), total surplus will be lower than the case when quality is enforceable (C).*

HYPOTHESIS 2: *In a buyers' market, whether quality is enforceable or not by a third party, all surplus goes to the buyer and trading sellers earn reservation payoffs.*

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

Certain aspects of Hypotheses 1 and 2 are similar to the equilibrium predictions of BFF in that only minimal quality trading is anticipated in RC1 and that all surplus goes to buyers. Our prediction concerning RC2 is even more stark as no trade is expected so the market collapses. However, these predictions rely on strong assumptions about the rationality or self-interested motivations of *all* subjects. When these assumptions are violated, such as when a subset of subjects have *social preferences* (Fehr and Gachter 2000; Charness and Rabin 2002; Engelmann and Strobel 2004, among others), then different outcomes may emerge. BFF show that when a subset of subjects have social preferences for fairness, then there is a perfect Bayesian equilibrium in which trade and high quality levels emerge even in a finitely repeated game. The key mechanism supporting this "cooperative" equilibrium is that subjects who care about equity are willing to choose strategies that are "fair" and support high rents even in the final period. The existence of rents in the final period will incentivize even selfish types to cooperate in earlier periods so that the perfect Bayesian equilibrium outcomes may resemble what might occur in an infinitely repeated game with strictly selfish types. Moreover, multiple equilibria are possible depending on subjects' beliefs and the nature of subjects' social preferences. 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 in an experimental setting, 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 provide an overview of trading outcomes from our three treatment sessions. Focusing on rows 8 and 9 of table 1, a first notable result is that, although hypothesis 3 suggests that the market will collapse under RC2, our subjects executed 449 out of the 450 possible trades. Of the three treatments, RC2 may most resemble agricultural markets given barriers to third party enforcement and the ability of processors to make *ex post* adjustments in contract terms. One can see that such markets can remain active even with limited third-party enforcement, as subjects are able to use informal enforcement mechanisms to replace third-party enforcement.

One informal enforcement mechanism is private trading. Figure 1 provides a picture of the evolution of private trading across periods (averaged across all sessions) in the three enforcement regimes. It appears that the share of private trades under RC1 increases over time settling in between 60% to 70% of all trades by the 9th round and onward. The share of private trades conducted under session C is significantly lower than under RC1 and never exceeds 30%. These patterns are similar to BFF's results, which enhances the validity of our experimental procedures and results. In treatment RC2, the share of private trades is significantly lower than under RC1 but consistently higher than under C. Kruskal-Wallis (KW) tests indicate significant differences in private trading between RC1 and RC2 (p = 0.0001), C and RC2 (p=0.004), and C and RC1 (p=0.0001).

Our results are intuitive as they suggest that buyers rely more heavily on private trading as an informal enforcement mechanism when other enforcement instruments are missing.

Because buyers in RC2 appear to resort to private trading less, presumably due to the availability of discretionary *ex-post* adjustments in price as a second informal enforcement mechanism, it would be interesting to examine this instrument further. While the latitude to make *ex post* adjustments in payment terms is a controversial aspect of real world agricultural contracts as it can increase the probability of opportunistic behavior by buyers, our results suggest that these adjustments can also be used for incentive purposes. 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 (2003), which can be used to provide incentives in relational contracts. If buyers are indeed making discretionary adjustments to incentivize sellers, then we ought to observe a correlation between performance and price adjustments.

Table 2 reports price adjustments made by buyers cross tabulated against performance for all RC2 trades. The patterns indicate that buyers do seem to make adjustments contingent on performance. Note that conditional on good performance ($Q > Q^*$), rewards are used 33% (2%/6% ≈33%) of the time under public trading and 67% (4.4%/6.6%≈ 67%) under private trading whereas conditional on poor performance, sellers are rewarded only 1.1% (0.4%/35.2%) of the time under public trading and 2.5% (.2%/8%) under private trading. Conditional on good performance, deducts are observed 55% and 19.6% of the time under public and private trading respectively, but these shoot up to about 83.5% and 66.3% conditional on bad performance. Moreover, when sellers delivered quality that just meets agreed upon quality, no price adjustments is most frequently observed. These patterns suggest that (1) discretionary price adjustments do appear to be contingent on quality, which is consistent with incentive provision, and (2) that opportunistic behavior, independent of incentive provision, still occurs as evidenced by the fact that in trades where sellers met or exceed performance obligations, buyers still made downward adjustments some of the time. However, this opportunistic behavior appears much more common under public trading. For example, when sellers just met performance obligations, buyers still made downward adjustments 41% (11.4%/27.6%) of the time in public trading and 9.6% of the time in private trading.

Surplus and Profits

In this section, we discuss efficiency and the distribution of profits that resulted under our three treatments. Hypothesis 1 predicts that when third-party enforcement is imperfect, total surplus will drop relative to the case where there is perfect third-party enforcement. Figures 2a and 2b provides views of total surplus in each treatment under private and public trading. Under private trading (figure 2a), surplus under RC1 is consistently lower than under the other two treatments. In contrast, surplus under RC2 appears fairly close to surplus under C; we will show formally in subsequent econometric analysis that there is no significant difference in surplus generated under C and RC2 under private trading. The combination of private trading combined with *ex post* price adjustments appears to provide powerful productivity enhancing incentives even in the absence of third party enforcement. In contrast, under public trading (figure 2b), surplus under RC2 appears to be consistently lower than C and surplus under RC1 falls even further never exceeding 30.

Turning now to distributional issues, note that hypothesis 2 is a statement of about the distribution. The first part of the statement predicts that, in a concentrated market, all surplus will go to buyers, while sellers will only earn reservation payoffs. Figure 3 illustrates how sellers' share of the surplus evolves across periods. 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 less than reservation profits. At the other extreme, under RC1, sellers collectively never earned less than 50% of total surplus and in some cases earned more than 100% of surplus, especially in early and late rounds where self-enforcing relationships were either being established or were ending.

One possible explanation for high seller surplus shares in RC1 is that sellers have the discretionary power to select Q as Q is unenforceable; thus, the power to engage in opportunism belongs mainly to sellers. Figure 4a sheds more light on this by contrasting seller surplus shares under RC1 in private and public trading. Note that seller share tends to be lower under private trading. Private trades indicate that relational trading is taking place, making it less likely that parties will renege on their agreements for fear of breaching self enforcement constraints. If sellers or buyers shirk on their agreements for short term gains, they risk being terminated by the other party and foregoing long term relationship-specific gains. Nonetheless, one can see that even under private trading, seller shares increase dramatically in the final periods when relationships are about to end.

Figure 4b provides the same analysis for treatment RC2. Note that unlike RC1, seller shares are higher under private trading. Intuitively, in RC2, buyers also have opportunities to engage in opportunistic behavior so that private trading appears to discipline buyer opportunism thereby increasing sellers' share of surplus. Nonetheless, in the last few rounds where the ending date is imminent, buyer opportunism seems to increase so that sellers' share falls significantly even under private trading.

So far, we have suggested that opportunistic behavior may explain some of the patterns observed in figures 3 and 4. We now provide a more detailed analysis of when parties reneged on their agreements in the incomplete contract treatments. Figure 5 examines sellers' propensity to renege on quality across periods in the incomplete contract treatments. Note that the % of trades in which sellers reneged was lowest under private RC2 trading and highest under RC1 public trading. What is particularly interesting is that sellers reneged less on average in private RC1 trading relative to public RC1 trading, *except in the early and late rounds*. It appears that sellers were more likely to renege on quality before relationships were well established (early rounds) and when they were about to end (late rounds). This is consistent with what was observed in figures 3 and 4a which show that sellers extracted surplus share in early and late rounds. However, in RC2, sellers do not appear to renege in early and late rounds, presumably because buyers can still use discretionary bonuses and deducts to discipline sellers even when relationships are fragile.

Rows 1 through 4 of table 3 provide data on sellers' propensity to renege on quality after buyers retaliated; i.e. after being terminated. One might anticipate a priori that sellers would be less likely to renege on quality after being punished via termination but there was little evidence of this in the data. Under RC1, sellers appear to renege more if they were just terminated regardless of whether they were involved in public or private trades after termination. The evidence was more mixed for RC2. Under private trading, sellers reneged more if they were just terminated versus when they were not, but for public trades, it appears that sellers that were just terminated reneged less.

Rows 5-8 compares sellers' promised profit, which is the amount they would earn if all parties honor the terms of the contract, to actual profits earned. On average, sellers earned more than promised in RC1 in both public and private trading, which is no surprise considering that sellers can engage in opportunistic behavior to extract rents from buyers. In contrast, in RC2, sellers earned significantly less profits than they were promised, although the gap is smaller under private trading than it is under public trading. This could be because buyers can behave opportunistically by making downward adjustments in price even when their sellers performed (recall that conditional on good performance, deducts were still observed 55% of the time in public trades). This is consistent with claims made by some growers that they earn less than expected under contract production. For instance, Hamilton (2001) reports that, in a survey of broiler growers, 43% of growers indicated that they earned less than they had anticipated under contract production.

Rows 9 and 10 reveal that 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. However, in RC2, the possibility of opportunism by buyers resulted in sellers earning less than reservation payoffs, *ex post*, in nearly 24% of trades in public trading and 7.9% in private trading. Thus, although sellers in RC2 accepted the best contracts *ex ante*, in nearly one in four public trades, sellers ended up with profits that fell below reservation payoffs. This was mitigated by private trading but sellers still earned profits that fell below reservation payoffs in 8% trades.

We now turn to an econometric investigation of our data thereby allowing us to undertake a more complete analysis of surplus and profits. We are interested in examining three regressions where the three dependent variables are surplus, buyer profit and seller profit. We are also interested in the impact of RC1 and RC2 on each of the dependent variables so we include dummy variables for these two regimes as explanatory variables in each regression. Moreover, because our experiment is a repeated game where subjects can establish cooperative self-enforcing agreements by making the continuation equilibrium conditional on past play, the observations are history dependent. Thus, surplus/profits in a given period may depend on the underlying equilibrium, which in turn depends on strategies/actions adopted in the previous period. We account for this history dependence by including variables that capture previous strategic interactions, including one period lagged quality chosen by sellers, lagged discretionary price deviations by buyers, and a measure of the length private relationships to account for the history of each trade. A number of additional control variables were used including demographic information obtained from post experiment surveys, and period and $period^2$ time trend variables to capture possible nonlinear trends due to learning and search. 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. There may also be unobserved experiment effects that cause composite error term of observations within an experiment to be correlated due to experiment specific group compositions. Thus, we ran our regression with robust standard errors adjusted for clustering on experiments.

While our main explanatory variables of interest are RC1 and RC2, we would ideally like to interact these treatment variables 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 offers. However, the decision to trade privately may be endogenous so a two-stage endogenous switching model is used to conduct the regressions. The first stage probit model is used to predict the probability of private trading and then the inverse Mill's ratio calculated from this probit is included as a regressor in the second stage regressions. In the second stage, the sample is split into public and private trades and then the regression equation is estimated for each subsample. The exclusion restrictions used for the first stage probit are variables for "positive surprise" and "negative surprise" lagged one period. To explain these variables, in each period of the incomplete contracting treatments, and prior to the time when sellers had chosen quality, we asked the buyer to state the quality level she *expected* the seller to supply, denoted by $E(Q)$. If actual quality chosen exceeds the buyer's expected quality, then there was a "positive surprise". On the other hand, if quality is lower than expectation, then there was a "negative surprise." These variables are attractive exclusion restrictions because they should affect the probability that a buyer will continue to contract privately in the following period, but should have no *direct* impact on surplus and profits in a future period; i.e. these variables should only affect future surplus and profits through the private variable.

Table 4 presents the results of the first stage regression.[9] This regression reveals a strong and positive relationship between RC1 and the probability that private trading is used which is consistent the data presented in figure 1. The previous length of a private relationship between a buyer and seller also significantly increases the probability of

[9]Our probit is similar to BFF's probit in their Table III. However, our results differ dramatically because BFF estimated contract renewal where the dependent variable is probability of private contracting with *same* seller as previous period. We estimate only the probability of private regardless of whether it is with same seller or not. This is because we are interesting in what explains private trading, not just what explains contract renewal. Moreover, BFF use data only from RC1 sessions whereas we pool RC1 and RC2 data.

private trading. Thus, buyers/sellers locked in relationships are highly likely to use private trading. These results provide econometric validation of earlier discussions.

Table 5a reports the second stage regression with surplus as the dependent variable. The list of explanatory variables is the same as the list used in the first stage probit except the exclusion restrictions, positive and negative surprise, are omitted. A dummy variable for treatment C was omitted so that C represents the base treatment against which comparisons are made. The coefficients for RC1 are negative and significantly different from zero for both subsamples suggesting that surplus under RC1 is lower than surplus under C regardless of whether trades were conducted publicly or privately. However, the difference in coefficients across the subsamples is estimated to be 26.48 and significant at the 5% level according to a Chow test, suggesting that private trading reduced efficiency loss (relative to C) by an average of 77% . Turning now to RC2, it is interesting to note that its coefficient for the private sample is not significantly different from zero so that 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.

Tables 5b and 5c report results for the second stage regressions where buyer (5b) and seller (5c) profits are the dependent variables. In table 5b, the coefficients for RC1 are negative and significantly different from zero across both subsamples so that buyers clearly lose profits under the RC1 relative to C. The same coefficients are positive, although only significant for the private subsample in table 5c. Thus, there is some mild evidence that sellers benefited from RC1 (relative to C) under private trading. While this appears to contrast figure 4a, which shows that sellers earn a larger share of surplus under RC1 in public trading, recall from figures 2a and 2b that surplus is much higher under private trading; thus, higher shares may not necessarily mean higher absolute profits for sellers.

Our results indicate that, on average, the impact of RC1 under public trade is estimated to reduce buyer profit by 45.18, but would not impact seller profits. Under private trade, both buyers and sellers benefit as buyer losses drop from 45.18 to 16.20, while seller profits are 7.97 higher than they are under C. Hence, even under private trade, RC1 reduces buyer profit and increases seller profit, which contradicts Hypothesis 2. In contrast, RC2 has very different effects on buyer and seller profits. None of the coefficients for RC2 in tables 5b or 5c are significantly different from zero. The only evidence that profits might decrease under RC2 is that, in table 5b, the difference in RC2 coefficients across subsamples is 9.12, but it is significant at only the 10% level.

Overall, it appears that neither social efficiency and/or profits will necessarily change in the absence of formal enforcement of contracts provided that the trading parties have a sufficient range of informal enforcement mechanisms available. If the number of informal instruments is limited (e.g. RC1) so that buyers are constrained in their ability to use informal mechanisms to enforce quality, then significant surplus losses can occur.

Conclusion and Implications

In this article we report the results of an economic experiment that investigates contracting relationships between buyers and sellers in an environment that featured different degrees of formal enforcement of contracts and market concentration in favor of buyers. Because market concentration and constraints to third party enforcement of contracts are both common features of real world agricultural contracting, we believe that our results can help policy makers understand the basic microeconomic forces that shape contracting relationships between processors and growers. Moreover, by exogenously varying our enforcement regimes, we provide some experimental counterfactuals that can help economists and policy makers understand how government involvement in contracting markets might affect social efficiency and the distribution of surplus.

We find that full formal enforcement of contracts promotes social efficiency. We also find, however, that if formal enforcement is missing (i.e. the government is "hands off" and/or performance is not verifiable by a third party due to lack of information or institutions), trading parties can use discretionary adjustments to contract terms combined with contract renewal to create a level of social surplus and profits that nearly matches the level obtained under full formal enforcement. This finding is consistent the claims of Telser (1980) and Klein (1996) who suggest that the ability of trading partners to self enforce contracts can enhance economic productivity. However, our experiments show that if formal enforcement is one-sided or incomplete, this can constrain the range of self enforcement mechanisms leading to significant losses in social efficiency. This finding is consistent with the theoretical work of Bernheim and Whinston (1998) who show that, given limits to third party enforcement, it may be optimal to *increase* the level of incompleteness of a contract. The policy implication is that if the government chooses to monitor and enforce contracts more vigorously, doing so in a one-sided or *ad hoc* manner can crowd out informal incentives and introduce inefficiencies. Government attempts to enforce contracts should not ignore informal mechanisms that are already in place.

With regard to distribution, a very complex pattern emerges under our three enforcement regimes. On the surface, it would be reasonable to assume that, with buyer concentration, most of the gains from trade would be captured by buyers. We find that this is generally true under the extreme cases of full formal enforcement and no formal enforcement. However, in the intermediate case, when there is only partial or one-sided enforcement - where buyers' contractual obligation are rigorously enforced but sellers' obligations are not - sellers can capture a significant share of social surplus. However, the tradeoff is that social efficiency is reduced considerably. The practical implication of this result is that, while government enforcement of processor obligations would help growers, it may have significant costs in social efficiency.

We close our article with a caveat. While efficient trading can still occur under a "hands of policy" of no third party enforcement, there was evidence of opportunistic behavior on the part of buyers. Specifically, while buyers' ability to make discretionary adjustments in price *ex post* can provide informal incentives to discipline sellers, a non-trivial fraction of trades also exposed sellers to discretionary downward adjustments even when they performed. Because of this opportunistic behavior, sellers, on average, earned less than what they were promised in their contracts and a non-trivial fraction of sellers even earned profits that fell below their reservation profits. However, it is not clear whether this opportunism depends solely on the enforcement regime. For example, if the market were concentrated in favor of sellers, would such opportunism exist? If processors were forced to use discretionary bonuses rather than deducts, would this limit opportunism? If sellers can bargain collectively, how might this affect efficiency and opportunism? It would be interesting to tackle these questions in future research.

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Table 1. Summary of Treatments, Sessions, Rounds, Participants, and Trades

	C	*RC1*	*RC2*
1. Third party enforcement of price?	Yes	Yes	No
2. Third party enforcement of quality?	Yes	No	No
3. Total No. of sessions	13	7	6
4. No. of buyers per Session[A]	5	5	5
5. No. of sellers per Session[A]	7	7	7
6. Rounds per session	15	15	15
7. No. of possible trades per session (Row 4 × Row 6)	75	75	75
8. Total No. of possible trades across all sessions (Row 3 × Row 7)	975	525	450
9. Total No. of actual trades executed by subjects (% of total possible)	942 (97%)	512 (98%)	449 (99.8%)

[A]Note that there were two sessions in each experiment. Thus, each group of 5 buyers and 7 sellers (12 subjects) who were recruited for an experiment actually participated in two sessions. Hence, there were a total of 156 subjects (12 subjects per experiment) who participated in the 26 sessions.

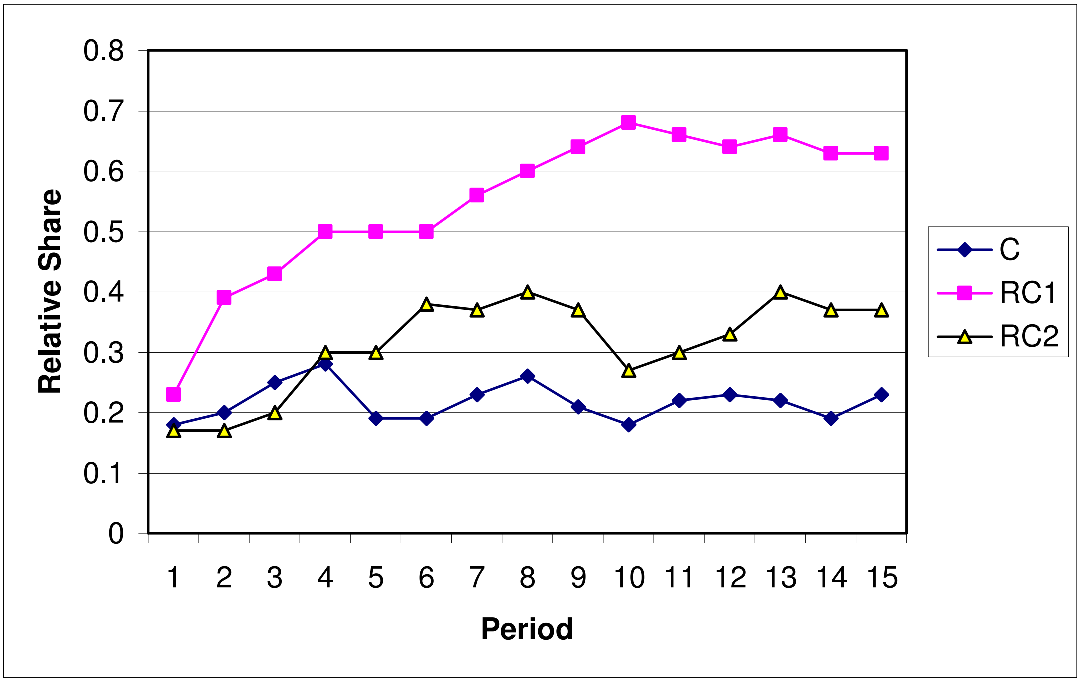

Figure 1. Relative share of trades conducted via private offers.

Table 2. Discretionary Adjustments Made by Buyers According to Performance in Treatment RC1 (Bold = Private, Standard Font = Public)

	Discretionary Price Adjustment by Buyer			
	Reward	*No Adjust.*	*Deduct*	*Overall*
$Q > Q^*$	2%	0.7%	3.3%	6%
	4.4%	**0.9%**	**1.3%**	**6.6%**
$Q = Q^*$	3.3%	12.9%	11.4%	27.6%
	1.1%	**13.8%**	**1.6%**	**16.5%**
$Q < Q^*$	0.4%	5.3%	29.4%	35.2%
	0.2%	**2.5%**	**5.3%**	**8%**
Overall	5.7%	18.9%	44.1%	68.9
	5.7%	**17.2%**	**8.2%**	**31.1**

Note 1: Q^* = quality supplier agreed to deliver in the contract.
Note 2: Reported in % of total number of transactions.
Note 3: Total number of trades was 449.

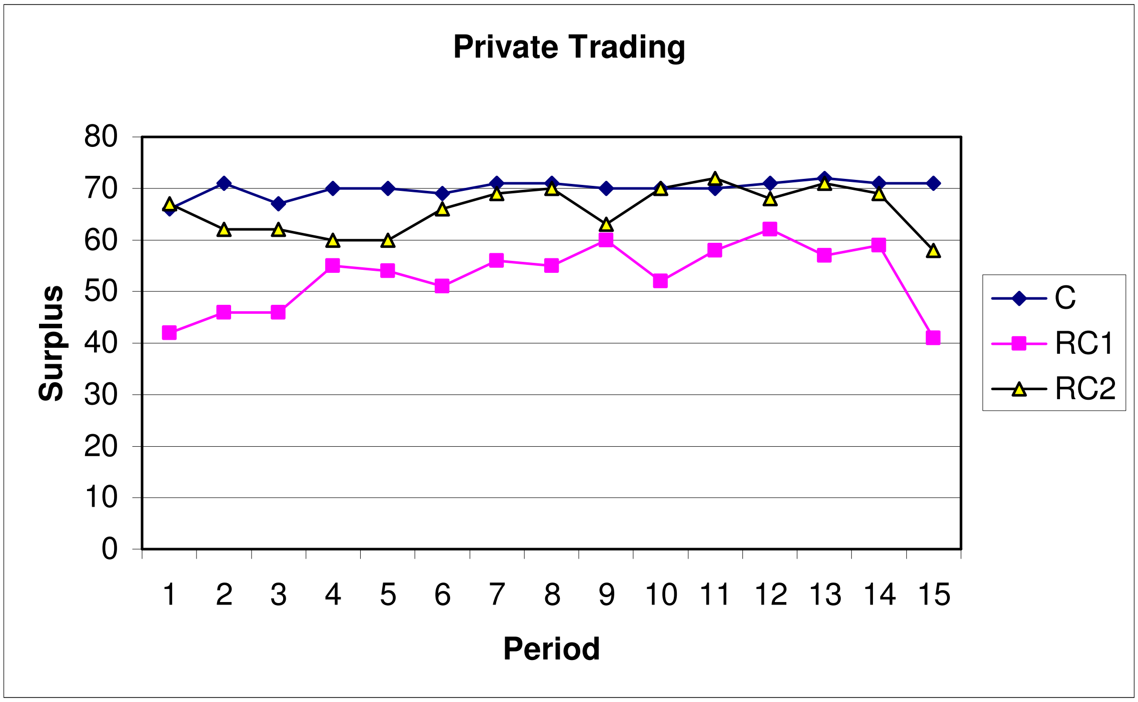

Figure 2a. Trading surplus under private trades.

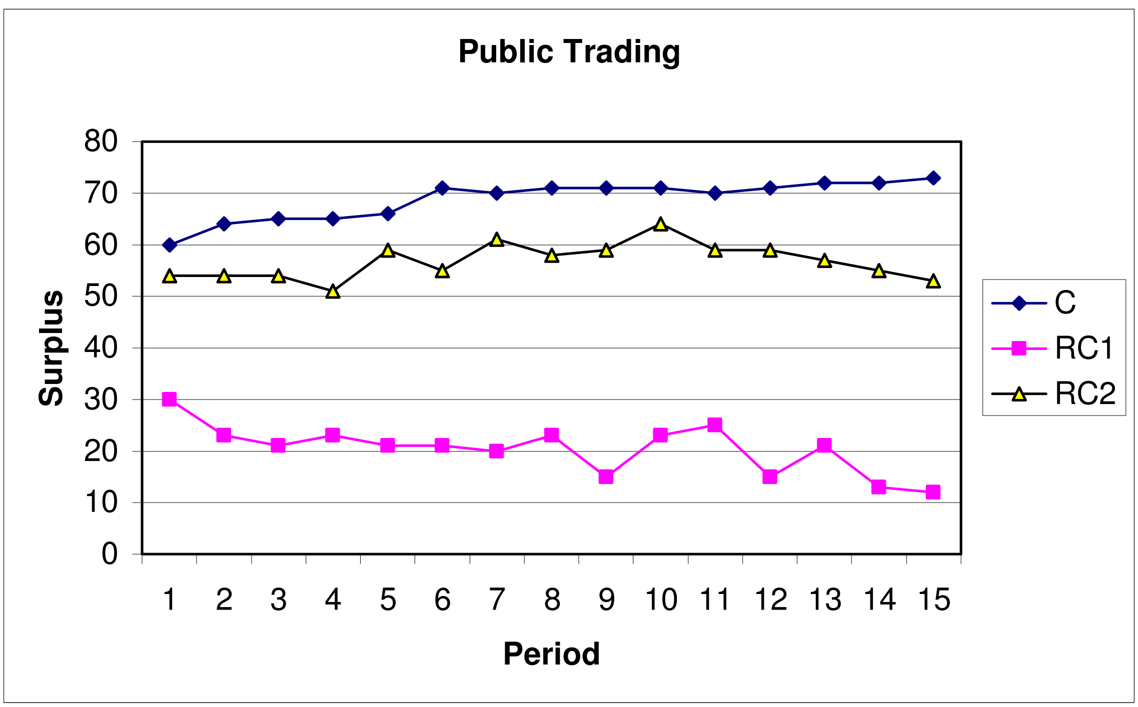

Figure 2b. Trading surplus under private trades.

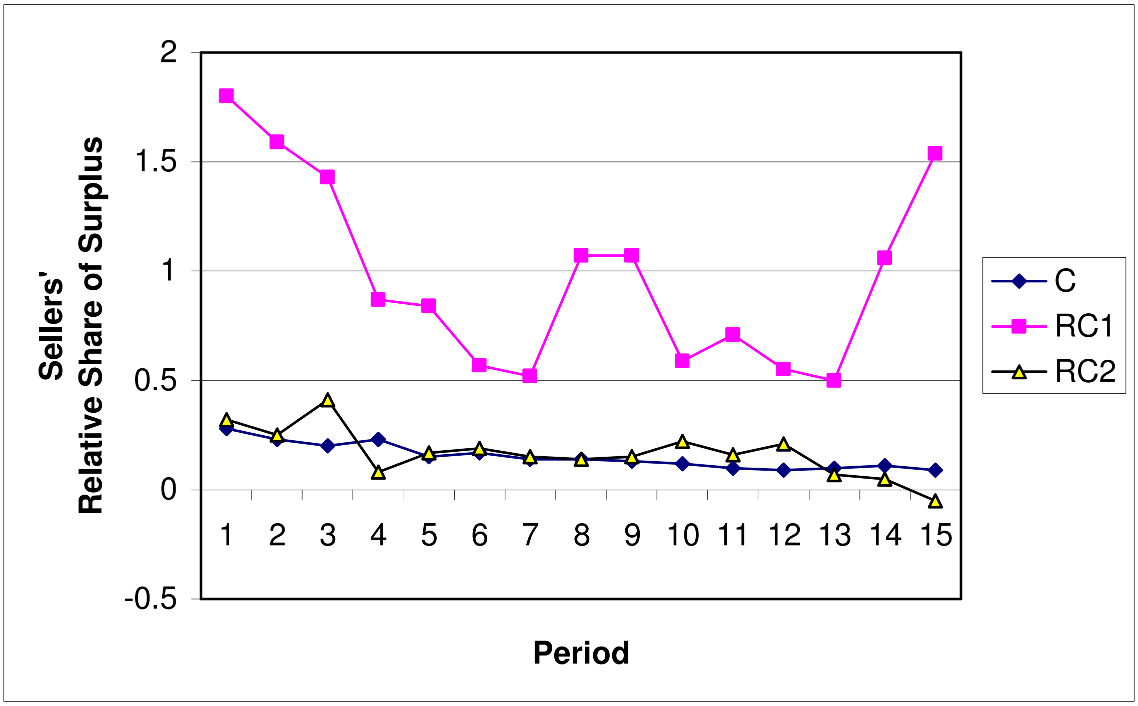

Figure 3. Sellers' relative share of total surplus. Defined as seller profits minus reservation payoffs divided by sum of seller and buyer profits minus reservation payoffs; i.e. $(\pi_S - r)/(\pi_S + \pi_b - r)$ **.**

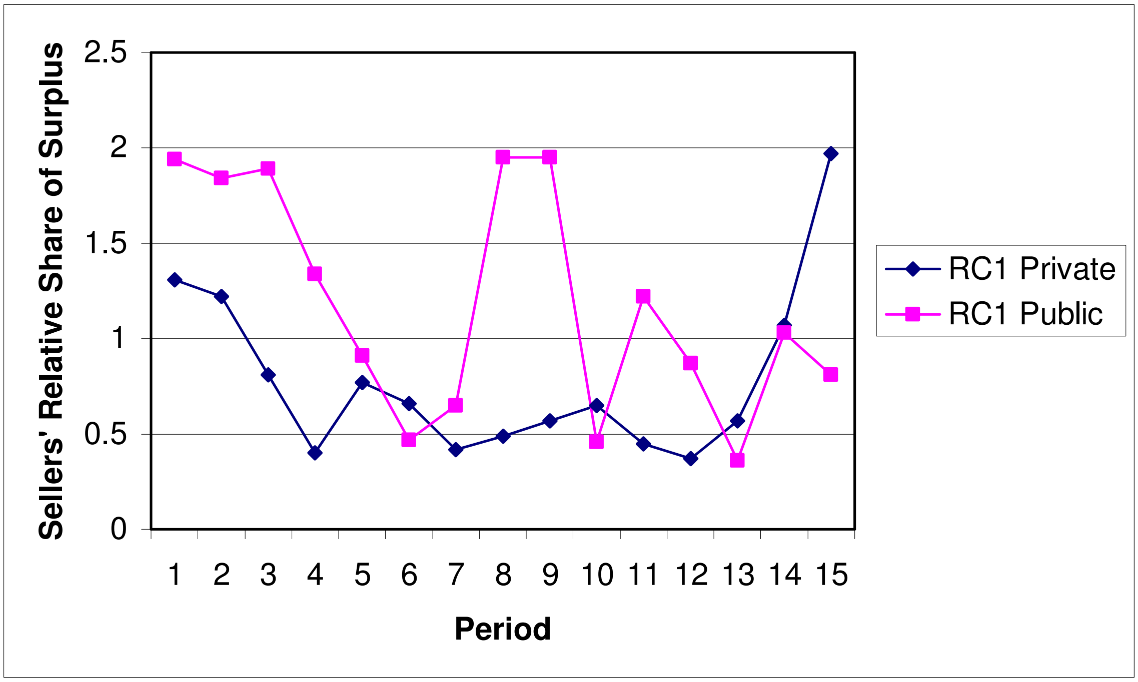

Figure 4a. Sellers' relative share of total surplus under RC1 – Public vs. Private.

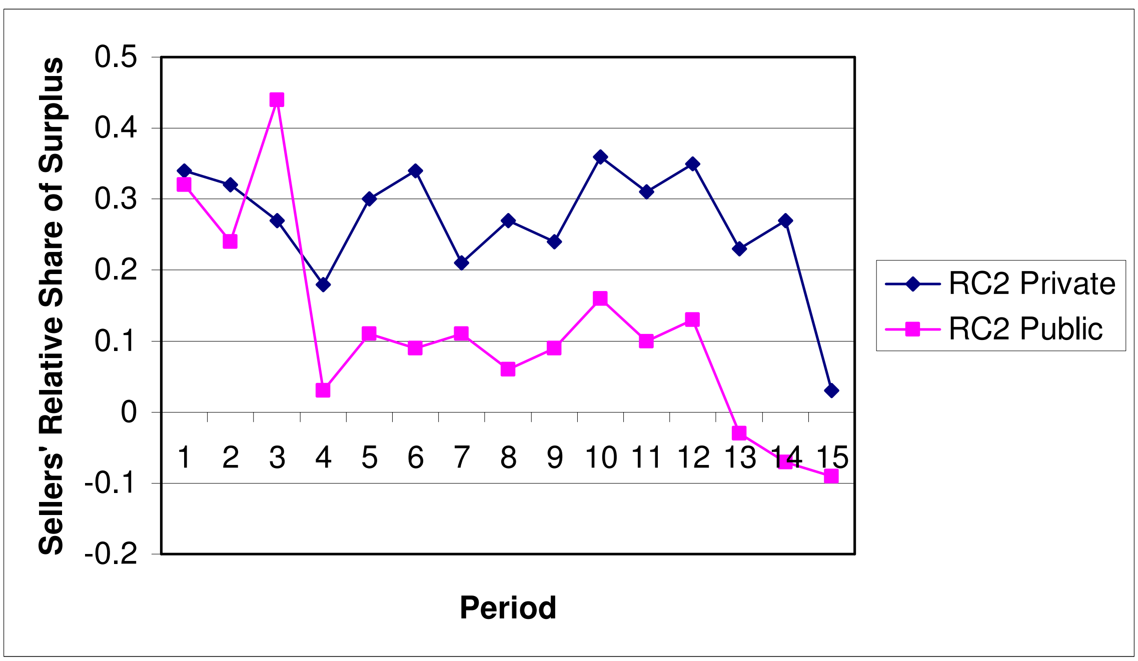

Figure 4b. Sellers' relative share of total surplus under RC2 – Public vs. Private.

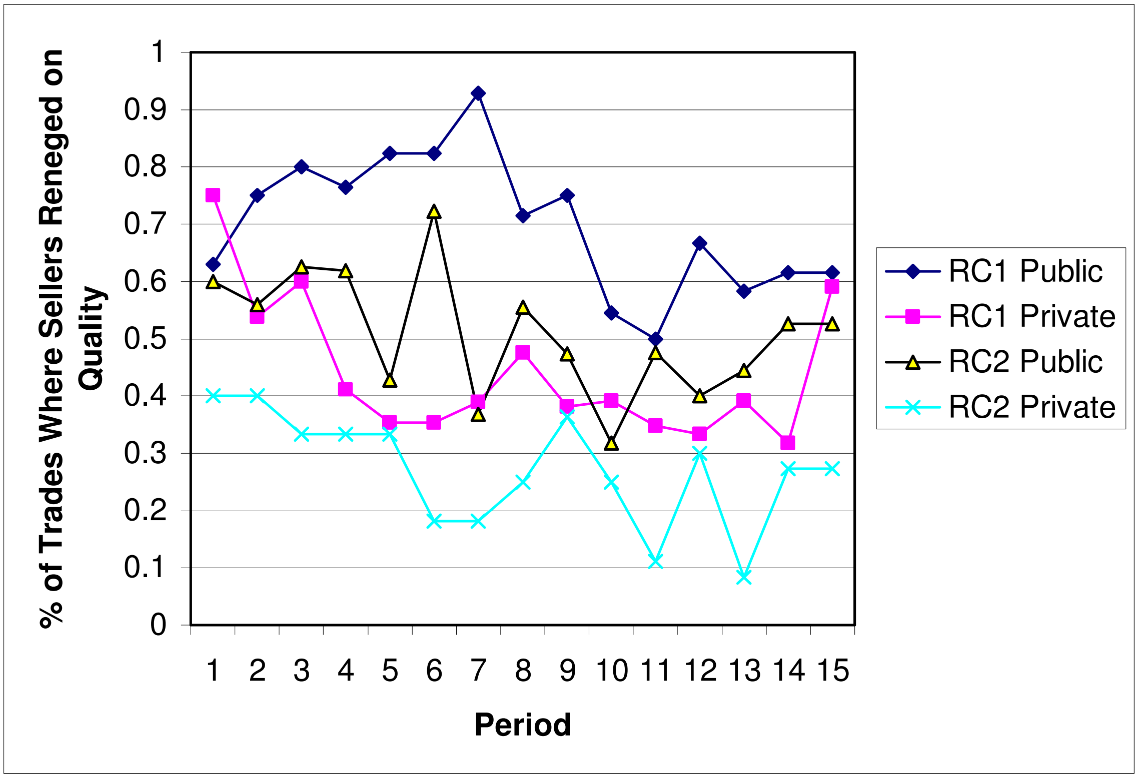

Figure 5. Percentage of trades where seller reneged on quality (i.e. supplied lower quality than what was agreed upon in contract).

Table 3. Summary Statistics – Evidence of Opportunistic Behavior

	C	RC1	RC2
1. % of public trades where $Q < Q^*$ in period following termination	--	81%	39%
2. % of public trades trades where $Q < Q^*$ in all other periods.	--	69%	52%
3. % of private trades where $Q < Q^*$ in period following termination	--	73%	43%
4. % of private trades where $Q < Q^*$ in all other periods.	--	32%	23%
5. Avg. promised profit to seller in public trading	14.4	13.5	21.5
6. Actual mean seller profit in public trading	14.4	19.8	12.9
7. Avg. promised profit to seller in private trading	26.5	27.7	26.9
8. Actual mean seller profit in private trading	26.5	31	22.9
9. % public trades where seller profit fell below reservation	6%	3%	24%
10. % private trades where seller profit fell below reservation	1.9%	0.7%	7.9%

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

Table 4. First Stage Regression: Probability of Private Trade.

Regressors	Coefficients
RC1	0.96**
	(0.21)
RC2	0.24
	(0.17)
Reservation	-0.06
	(0.05)
Lagged positive surprise (Max{Q – E(Q), 0})	-0.025
	(0.05)
Lagged negative surprise (Min{Q-E(Q),0})	0.020
	(0.03)
Lagged Quality	0.06
	(0.04)
Length of private relationship prior to current period (in periods)	0.51**
	(0.13)
Lagged price deviation (P* - P)	-0.004
	(0.008)
Period	-0.001
	(0.05)
Period^2	-0.001
	(0.003)

N = 1720
Log Pseudo Likelihood = -693.36
Pseudo R^2 = 0.37

**Significant at 5% level
*Significant at 10% level
Note 1. The estimation procedure is a probit regression with standard errors adjusted for clustering on experiments (in parentheses).
Note 2. Demographic variables for gender, age, GPA, and employment status are included as control variables but their coefficients were not reported.

Table 5a. Second Stage Regression (Dependent Var. = Surplus)

Regressors	*Private*	*Public*	*Chow Tests for differences in β*
RC1 (dummy)	-8.23**	-34.35**	26.48**
	(2.32)	(6.86)	(7.83)
RC2 (dummy)	-3.55	-9.08**	5.76
	(2.04)	(3.45)	(3.77)
Lagged Quality	4.21**	2.89**	
	(0.62)	(0.73)	
Length of private relationship.	0.403	-0.14	
	(0.28)	(0.60)	
Lagged price deviation	-0.30*	0.02	
	(0.148)	(0.18)	
Inverse Mills Ratio	0.48	0.04	
	(0.41)	(0.19)	
Period	1.31	0.83	
	(1.03)	(0.57)	
$Period^2$	-0.10*	-0.04	
	(0.057)	(0.03)	
Constant	30.79**	35.61**	
	(12.23)	(7.73)	
N	590	1130	
R^2	0.48	0.66	

Note 1. Standard errors are robust standard errors adjusted for clustering on experiments.
Note 2. Demographic variables for gender, age, GPA, and employment status are included but their coefficients were not reported.

Table 5b. Second Stage Regression (Dependent Var. = Buyer Profits)

Regressors	*Private*	*Public*	*Chow Tests for differences in β*
RC1 (dummy)	-16.20**	-45.18**	29.39**
	(2.96)	(9.58)	(8.92)
RC2 (dummy)	-0.69	-9.03	9.12*
	(3.88)	(5.91)	(5.09)
Lagged Quality	3.14**	1.96**	
	(0.55)	(0.84)	
Length of private relationship.	-0.85	-1.33	
	(0.54)	(1.28)	
Lagged price deviation	-0.23**	-0.005	
	(0.11)	(0.13)	
Inverse Mills Ratio	1.00	-0.09	
	(0.65)	(0.35)	
Period	2.06*	2.01**	
	(1.08)	(0.58)	
$Period^2$	-0.10*	-0.07**	
	(0.05)	(0.03)	
Constant	14.38	17.25	
	(24.38)	(13.76)	
N	590	1130	
R^2	0.41	0.64	

Note 1. Standard errors are robust standard errors adjusted for clustering on sessions.
Note 2. Demographic variables for gender, age, GPA, and employment status are included but their coefficients were not reported.

Table 5c. Second Stage Regression (Dependent Var. = Seller Profits)

Regressors	*Private*	*Public*	***Chow Tests for differences in β***
RC1 (dummy)	7.97**	10.83	-2.91
	(1.75)	(8.49)	(8.26)
RC2 (dummy)	-2.87	-0.06	-3.36
	(3.77)	(3.05)	(4.12)
Lagged Quality	1.08**	0.93	
	(0.30)	(0.75)	
Length of private relationship.	1.25**	1.19	
	(0.36)	(0.96)	
Lagged price deviation	-0.07	0.03	
	(0.09)	(0.11)	
Inverse Mills Ratio	-0.53	0.12	
	(0.39)	(0.23)	
Period	-0.76	-1.18**	
	(0.85)	(0.47)	
Period^2	-0.003	0.03	
	(0.05)	(0.03)	
Constant	16.41	18.36	
	(23.30)	(10.63)	
N	590	1130	
R^2	0.27	0.64	

Note 1. Standard errors are robust standard errors adjusted for clustering on sessions.
Note 2. Demographic variables for gender, age, GPA, and employment status are included but their coefficients were not reported.