ABSTRACT

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Allocation decisions in many natural resource markets are governed by mechanisms designed to alleviate information asymmetries and other types of market imperfections. For example, the crew in most commercial fisheries is remunerated via a lay system of payments designed to alleviate a potential team agency problem. The four essays in this dissertation explore the use of mechanisms in natural resource and environmental economics.

The first essay examines the lay system of payments in commercial fisheries. Under the lay system, the harvesting crew is remunerated via a share of total vessel revenues less a portion of trip expenditures. The essay has two goals. First, the essay provides an explanation for the lay system as an incentive mechanism to alleviate a potential team agency problem. This explanation of the lay system explains anomalies that are at odds with the theory of pure risk sharing. Second, the essay shows the implications of the lay system for econometric modeling of fisheries and for understanding firm behavior.

The second and third essay, examine bidder behavior in auctions for cutting rights of standing timber in British Columbia. The second essay provides an empirical

framework for estimating treatment *assignment* of observations given data on *outcomes*. The framework is used to explore whether bidder collusion was evident in a data set of nearly 3,000 auctions (over 10,000 individual bids) for cutting rights of standing timber in British Columbia from 1996-2000. The third essay examines the role of *ex ante* uncertainty over private values and *ex post* resale opportunities on bidder behavior. The essay extends the theoretical work of Haile (2003) by allowing for risk-averse bidders. The theoretical model is tested by examining both field data and experimental data from the lab.

The fourth essay provides a formal model of individual contribution decisions under a tontine mechanism. The essay analyzes the performance of tontines and compares them to another popular fundraising scheme: lotteries. Individual contribution decisions under the optimal tontine, an equivalent valued single-prize lottery, and the voluntary contribution mechanism are compared using a controlled laboratory experiment.

ESSAYS IN NATURAL RESOURCE ECONOMICS

by

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The Lay System in Commercial Fisheries: Origin and Implications[1]

I. Introduction

In most fisheries, the crew is rewarded by a lay system. Under the lay system, the crew is paid with a share of revenues or a share of revenues less costs, rather than a fixed wage per hour worked. This system is an integral part of fisheries, governing the remuneration and hence allocation of a crucial input in fisheries production. Labor costs are a substantial component of variable trip costs – often forty percent or more – so that the working of the system has a substantial impact on fishing firms' resource allocation. For example, Jin et al. calculate labor's share of total costs including fixed costs in the New England ground fish fleet constitute between 30 and 60 percent (Table II, p. 549). Consequently, it is important to understand of the allocation of labor inputs.

The presence and the functioning of the lay system are well known and accepted as one of fishery's important institutions. Compared with share contracts in agriculture, however, the lay in fisheries has received relatively little research.[2] This is true despite the fact that lay systems vary substantially among fisheries, and have changed over time. There is little evidence that the prevailing explanation of lays has any larger implication for modeling behavior in fisheries.

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[1] This essay was written with Ted McConnell. John Horowitz, Tigran Melkonyan, Daan van Sooest and two anonymous referees provided useful suggestions on an earlier version of this article. The authors are also grateful to participants at Camp Resources XI and the 2004 European Association of Environmental and Resource Economics meetings.

[2] See for example Stiglitz (1974); Braverman and Stiglitz (1982, 1986); Allen and Lueck (1992).

The lay system is typically explained as a means of sharing risk. Sutinen (1979) developed the standard model of risk sharing. Plourde and Smith (1989) extend the model to examine bioeconomic equilibrium when the crew is remunerated by a lay. However, there is reason to question the pure risk-sharing explanation. Production and price risk are pervasive in fisheries. In a pure wage system, asset owners would absorb all risk and so one sees the intuitive appeal of risk sharing. Under the lay system, the owner and crew share unanticipated fluctuations in prices and production.

Although the risk sharing model is widely accepted, several studies examine the incentive features of share contracts in specific situations. Matthiasson (1999) provides a model to explain the provisions of share contracts in Icelandic fisheries whereby captains are remunerated with a share of catch, subject to an agreed minimum, but do not share operating costs. In this model, the base wage and zero cost share is likely to coexist as an incentive contract when strategy-dependent captain-specific costs are important. In a study of the 19th century whaling contracts, Craig and Knoeber (1992) examine manager share holding in the U.S. fleet as an incentive to prevent shirking near the end of the captain's career. In their model, share contracts are optimal only as a means to resolve end period opportunism and are assumed to exist solely to prevent such behavior.

This paper has two goals. First, we propose an alternative explanation for the lay system: moral hazard and team agency. This explanation of the lay system explains anomalies, such as the presence of wages in some fisheries that are odds with the theory of pure risk sharing. Second we show the implications of the lay system for econometric modeling of fisheries and for understanding firm behavior. Models that fail to account explicitly for the incentive properties of shares may provide poor models of fishing firm.

The thrust of the paper is that the formation and working of the lay system are a fundamental force in fisheries influencing firm behavior and research results in a way that has been largely ignored.

Moral hazard is present in fisheries because the expense of monitoring workers is substantial. Individual effort is typically unobserved by a vessel owner, leaving harvest the only measure of crew effort. Team agency problems arise as the typical commercial fishing crew is comprised of multiple crew members individually and independently making decisions as to the allocation of costly and largely unobservable effort. In such a setting, crewmembers thatare paid via a fixed wage per hour worked are not likely to exert the same level of effort as those who are rewarded via an optimal incentive contract.

In the following section we argue that moral hazard plays a central role in crew effort allocation. We then show some neglected implications of lay systems that arise regardless of the explanation of lays. Finally we develop simple contracts in the spirit of Holmstrom (1979; 1982) that avert moral hazard when crew effort is not observable. The central motivation for paper is to further the understanding of the behavior of fishing firms.

II. Why Moral Hazard? Lays and Risk Sharing

In a production process such as commercial fisheries, with production or price risk, a variety of risk sharing agreements are feasible. At one extreme, vessel owners could rent their equipment to the captain and crew for a fixed payment, and let the renters absorb all the risk. At the other extreme, the crew could be paid a fixed wage, with the owner absorbing the risk. In practice we find a complex set of sharing arrangement in which the crew may get paid a share of the net returns, or may get one share of the gross returns and pay another share of the variable costs. These arrangements vary by industry and by vessel within industry.[3] Crew themselves often get different shares, depending on their experience, skills, and knowledge.

There are a number of reasons to view the pure risk sharing explanation for the development of the lay system as suspect. First, for the lay system to have emerged purely as a means to spread risk the vessel owners must be risk averse. Under the pure wage system the asset owner, not the crew, absorbs all production risk. A risk averse asset owner would like to share some of this risk with a hired crew. A risk averse crew would require additional compensation to relinquish a pure wage system. However, it is only optimal for asset owners to provide such reward if they are in fact risk averse. Otherwise labor will be remunerated via a fixed wage with the asset owner absorbing all production risk.[4]

Recent evidence weakens the notion that vessel owners are risk averse. In a location choice model, Eggert and Tveteras (2004) find that about 70% of the trips in a sample of Swedish trawlers demonstrate behavior inconsistent with risk aversion. In a stated preference study, Eggert and Martinsson estimate that about 50% of a sample of Swedish commercial fishermen responds inconsistently with risk aversion. Without risk averse owners, the pure risk-sharing argument for the lay system is not valid.

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[3] For example, in the Gulf of Mexico reef fishery, the lay systems seem to be different for each vessel, and researchers doing cost-earnings studies simply collect data on payments to labor (personal communication, Jim Waters, National Marine Fisheries Service).

[4] This result comes directly from Proposition 2 of Stiglitz (1974) which shows that in agricultural production a pure wage system will arise if and only if landlords are risk neutral. Under such assumption the asset owner absorbs all production risk and labor is paid its mean marginal product.

Second, risk is less convincing as an argument for the lay system in conjunction with the need to monitor labor. For the lay system to prevail simultaneously with efficient resource allocation crew effort would have to be carefully monitored and "contracts" completely enforced. Implicit to the theory of risk sharing is the existence of a recipe book that provides a menu of actions that dictate how factors of production are to be employed in every conceivable state of nature. All contingencies in the field would be accounted for and all effort observed. Contracts would be fully enforceable in the sense that if the agents adhere to this menu, they would be remunerated via a predetermined share of output (Stiglitz, 1974).

In practice, we do not observe such contracts in commercial fisheries. Given the vagaries of the production process in a commercial fishery – fluctuations in resource stock abundance, unexpected variations in weather conditions, the length of a typical fishing trip – developing and enforcing a contract that provide the crew with verifiable actions for every conceivable contingency is not feasible.

Third, the phenomenon that the shares paid to crew members within the same firm may differ according to experience, skill, knowledge, etc. implies that individual risk preference must differ systematically across these characteristics.[5] In the Atlantic sea-scallop fishery the captain and first-mate are remunerated via an ordinary crew share plus a bonus share of total harvests. For this outcome to be internally consistent with risk sharing, these agents must be more risk loving than an ordinary seaman. Not only does the base "wage" paid to such skilled fishermen depend upon an uncertain harvest but so does the bonus payment they accept as reward for demonstrated skill. Thus, vessel

[5] This observation is a direct application of Proposition 4 of Stiglitz (1974) which states that the group that is relatively risk averse assumes less than its proportion of the risk.

captains and mates accept a contract that is effectively more risky (relative to a certain outside wage) than unskilled, inexperienced crew. Under a pure risk-sharing explanation of the lay system, such an outcome constitutes an equilibrium only if one considers captains (mates) more risk loving than ordinary crew.

Finally, not all fisheries use lay systems and within a given lay system, some vessels may pay some crew wages. A key to understanding the choice of remuneration systems is to determine the circumstances under which a given type of institution is used. All fisheries are subject to some degree of uncertainty in harvests. Thus under the pure risk-sharing rationale for the development of the lay system, all fisheries should remunerate crew via a share system. The presence of wages for some crew is an anomaly within the pure risk-sharing explanation of the lay system.

Types of Remuneration Systems

Several fisheries operate with wages as well as shares. Such remuneration schemes are not necessarily consistent with risk sharing. We review a number of cases that shed light on the rationale for share contracting.[6] In the Chesapeake Bay blue crab fishery, vessels use a pure wage system in approximately 40% of the trips. Naturally in these cases the captain-owner absorbs the risk. In blue crab harvesting, the captain is able to observe crew effort, especially when there is only one crew. Vessels in the blue crab fishery set and tend a fixed number of pots per day. The operation is largely mechanized with wenches that pull pots. Crew pull pots and hook them up to the wenches, sort harvested crabs and re-bait pots. These actions are largely observable,

[6] Researchers studying costs and earnings in commercial fisheries typically do not collect information on contracts and simply record returns to crew, captain and boats. See, for example, Waters on the Gulf of Mexico reef fishery.

making monitoring costs low. Yet production in the blue crab fishery is no less stochastic than many fisheries with lay systems.[7]

Factory trawlers in the Bering Sea both harvest and process ground fish while at sea. Harvesting crews receive a share of revenues (typically 30-35% in aggregate). However, the processing crew and engineers are paid primarily via a daily wage. In a pure risk sharing arrangement, there is no reason to leave any member of the crew out of a sharing agreement. But in a remuneration system that uses to share system to provide incentives rather than share risk, the production crew would be most susceptible to a sharing arrangement.[8]

Third, in the Plourde and Smith model, revenue shares are determined solely by the underlying risk preference of the vessel owner and crew. Optimal revenue shares for a vessel owner take the form:

$$1-\beta = \frac{\alpha_c}{\alpha_c + \alpha_B},$$

where β is the optimal share of revenues for the crew and α_c and α_B are parameters on the variance of income in a quadratic utility function. Differences in the underlying composition of shares across or within fisheries can only be attributed to equivalent differences in individual risk posture. In the Chesapeake Bay blue crab fishery, when there is a share system, crew shares are about 10-25% of daily revenue. This implies considerable differences in risk preferences of owners across vessels.

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[7] We thank Doug Lipton for help with the blue crab data.

[8] We thank Ron Felthoven of the National Marine Fisheries Service for help with the Bering Sea fishery.

The floating trap fishery on Narragansett Bay provides another example of wages for crew. This fishery pays crew on skiffs via shares, but pays wages to crew chiefs and laborers on the mother vessel. This payment system is consistent with incentives that induce greater effort from harvesting crew.

Several studies have examined contractual relations in the nineteenth century U.S. whaling fleet (Davis, Gallman, and Hutchins, 1990; Craig and Knoeber, 1992; Craig and Fearn, 1993). The lays reported in these studies are inconsistent with both the pure risk-sharing story and Plourde and Smith's formulation of the optimal risk-sharing contract. Davis et al. report shares of total catch ranging from 6.58% for vessel captains, 4.31% (2.60%) for first (second) mates, and down to 0.60% (0.54%) for skilled (unskilled) seamen. Given that all crew members face the same risk per trip, there should not be such wide variations in the shares under a pure risk sharing story. Further, Davis et al. report wide variations in the relative lays for a given occupation depending upon the fishing ground visited during a given trip. E.g., captains (first mates) received an addition 7 (14) percent lay when a trip fished in the Pacific Ocean compared to the Atlantic. For skilled (unskilled) seamen these same differences represent a 17 (19) percent increase in lay for trips to the Pacific. Risk preferences would be unlikely to differ for trips to the Pacific Ocean as compared to those in the Atlantic by the magnitudes implied by these disparities. Hence it is difficult to view the Atlantic whaling lay as consistent with the Plourde and Smith formulation.

III. Owner-Skipper Contracts

Although many vessels are operated by captains who are not owners, there is only a limited literature on contracts between owners and captains. Craig and Knoeber (1992) and Matthiasson (1999) develop models of owner-captain contracts of the lay system as an incentive mechanism. Craig and Knoeber investigate the contractual relations between a whaling master (captain) and the vessel owner as a dynamic game. They argue that manager shareholding was a supplement to the market for managers designed to address end-of-career shirking. In their model, masters and vessel owners repeatedly interact in a well functioning labor market. As such, reputation and the threat of sanctions for poor performance are sufficient to regulate behavior (and prevent shirking) early in a captain's career. However, as a master nears the end of his career, both the threat of sanction and its realization are insufficient penalties to prevent defection and shirking. Owners therefore offer incentive contracts that reward hard work and effort as a master nears the end of his career.

While intuitively appealing to consider share contracts as an incentive mechanism, the Craig and Knoeber (1992) model and explanation for the lay system has limited appeal. The same economic logic that leads the authors to conclude that share contracts are optimal in the later periods of the game implies that in no stage of the game is it a "best" strategy for the owner to reward labor via a wage. Consider the choice of action by a master in period t, knowing that in period t+1 he will be rewarded via a lay regardless of period t's payoffs. Assuming that effort is costly, the master will choose a strategy (effort level) that is below that which optimizes joint payoffs. Therefore, the owner would want to institute the lay system in period t rather than period t+1. By a similar line of reasoning, we can conclude that in no period is it optimal to reward the master via a wage. Hence, viewing revenue sharing as the resolution to an end-period problem is unsatisfying.

Matthiasson's model pertains to the particular provisions of captain contracts in fisheries in Iceland. In these contracts, captains are rewarded the maximum of some predetermined fixed wage or share of revenues and do not share any of the operating costs. Given the optimality of a zero cost share, the generality of his results is limited. Almost all commercial fisheries in the United States include contractual provisions stating some degree of cost sharing for the captain and crew.

The results of Matthiasson stem from an assumption of strategy-dependent captain-specific costs. While this assumption may be valid for the particular fishery under study, it is hard to generalize to a wider array of fisheries. Operating costs may be strategy dependent but the link between strategy choice and captain costs is tenuous. Later in this paper, we provide an alternate representation of contract design that generates the Matthiasson structure as a special case under a much less stringent cost structure.

These contracts deal with the peculiar relationships between owners and captains, and pertain to a dynamic setting. They do not address the allocation of crew of most commercial vessels.

IV. Observability, Teams, and Incentive Contracts

We have argued that both the pure risk-sharing story and owner-captain contracts do not explain some significant aspects of remuneration in fisheries. As a competing hypothesis, we propose that remuneration systems in fisheries are consistent with incentive contracts designed to address the problem of moral hazard and team agency.

The examples of wages in fisheries are consistent with incentive-based contracts. In the blue crab fishery, the vessel is small, the crew few or simply one, and observability of effort high. Incentives for effort are not necessary. A risk-neutral owner would thus be indifferent between offering crew a share and fixed-wage contract. For Bering Sea factory trawlers, the effort of processing workers, who are compensated by wages, is more observable than effort by harvesting crew. Harvests are subject to vagaries outside the direct control and observation of vessel owners. It is difficult to separate the effects of individual shirking from those of a random shock. We would thus expect labor contracts to provide incentive for exerting effort in the harvesting crew whereas such provisions are less necessary for labor in processing, which is less random and more observable.

The lay system is also consistent with an industry with heterogeneous payment schemes that vary across vessels, geographic regions, and types of labor. All incentive contracts must satisfy an individual rationality constraint. In expectation, the labor contract must provide returns that are at least equivalent to some next best alternative while compensating the individual for undertaking a costly (but unobservable) action. Otherwise the individual would pursue the outside employment option. Testing the consistency of the moral hazard interpretation with this observation is equivalent to testing if such differences are correlated with differences in the cost of effort or payments from alternate employment.

Anecdotally, evidence of such correlation exists. Consider the example of differential wages across job types in the U.S. whaling fleet. Davis et al. report that on average captains of whaling vessels could earn approximately 23.3% (55.8%) more working in the merchant marines than could a ship's first (second) mate. These variations in outside wage alternates across employment types are positively correlated with the reported 53.7% (153.1%) differences in revenue share between whaling captains and first (second) mates.

V. The Implications of Share Systems

The share system has a variety of implications for modeling of fisheries. The implications depend on whether the shares differ for revenues and costs and whether shares vary across fishing firms within a fishery. We explore but do not exhaust the implications of share systems.

Risk aversion versus risk neutrality

One implication concerns the nature of risk preferences by owners and operators of fishing vessels. In models of risk sharing developed by Sutinen, risk naturally matters. Both the owner of the vessel and crew are assumed risk averse. Yet under the pure risk-sharing interpretation, the lay system is only optimal when the asset owner is risk averse. But when crew are paid fixed wages in the presence of uncertain returns, then one may argue that vessel owners are risk neutral and share contracts are designed as an incentive mechanism. Indeed, there is growing evidence in the empirical contract literature to support the risk-neutral transaction-cost view of sharing rather than the risk avoidance alternative.[9]

Researchers have begun to downplay risk aversion altogether in contract models (e.g., Holmstrom and Milgrom, 1991; Matthiasson 1999). The lay system can arise as a consequence of monitoring costs for crew and the difficulties in attributing output when there are multiple crewmembers. However, one would be foolish to argue that there is no

[9] See for example Hallagan (1978); Mulherin (1986); Leffler and Rucker (1991); and Allen and Lueck (1992).

risk in fisheries. Consequently, it is reasonable to believe that both moral hazard and risk aversion can be found in fisheries.

Duality models and share systems

Economists have estimated a variety of duality models for fisheries (Squires, 1987a, 1987b; Kirkley and Strand, 1988; DuPont 1990, 1991; Pradhan, Sharma, and Leung 2003; Sharma, Pradhan, and Leung, 2003). These models include profit, revenue and cost functions. They typically treat variable input prices as constants, and capital costs associated with vessels as fixed costs often excluded from econometric analysis. When firm behavior involves share systems, incorporating labor in duality models is problematic. One approach is to use the opportunity cost of labor, often a wage rate that could be earned in a similar industry. Squires (1987a) derives a Divisia index of the opportunity costs of the crew (mean annual income of total manufacturing), the mechanic (mean annual income of maintenance mechanic, machinery), and captain (20% higher than an ordinary seaman's). Differences in crew size and the vessel's port for a given trip are exploited to generate variations in the index across trips and vessels. DuPont (1991) proxies opportunity cost wages as an average of weekly earnings in the home port of a vessel. An alternative is to leave labor out of the modeling. Kirkley and Strand (1988) estimate a revenue function using only output prices and a composite input (the product of vessel gross registered tonnage and days absent from fishing per vessel), hence ignoring the allocation of labor. Each approach has advantages and drawbacks. Labor choices are obviously made. On the other hand, the opportunity cost of labor in other alternatives may not capture appropriately the costs of labor in a share system.[10] Given the high proportion of variable costs comprised by labor remuneration (see Jin et al.), the choice of how to treat labor can have a significant impact on estimation in dual models.

In some circumstances the share system undermines the interpretation of input demands and output supplies. One of the basic tenets underlying the use of dual based methods to recover vessel technology is that such representations are linearly homogeneous in factor and output prices. However, under a share system of remuneration, homogeneity assumptions may not hold as a global property. In fact, under the lay system both input demand and output supply are dependent upon the allocation of labor effort which may be discontinuous over certain ranges of price change. Such discontinuities in labor effort would map into similar discontinuities in factor demand and output supply functions.[11]

The share system may also lead to econometric problems. Consider an owner who remunerates labor via a lay system which includes a cost-sharing rule. The assumed objective of the owner in such an environment is to maximize his share of profits.[12] This

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[10] This is an assertion with casual observation but no hard empirical research behind it. A variety of forces separate the opportunity cost of labor in its next best use from the cost to the firm. Fringe payments available for onshore employment and the much greater risks of commercial fishing would likely raise the constant wage relative to the equivalent constant onshore wage for the skill group. Further, optimal share contracts are influenced by variations in factors correlated with the “disutility” of labor effort. To the extent that such measures are vessel specific, a constant onshore wage may fail to account for important differences in the labor decisions of vessels operating from the same port.

[11] The presence of the share system is not the only reason why the regularity conditions underlying duality results may be violated. For example, homogeneity assumptions may not hold due to corner solutions driven by regulatory conditions or other factors.

[12] Two alternate choice environments pointed out by an anonymous referee would be one where the asset owner makes all production decisions and remunerates the crew via a lay system but has no obligations for variable costs or one where a captain hired via the lay system makes all production decisions. In the former choice environment, the objective of the owner would be to maximize total vessel revenues subject to the satisfaction of the individual rationality and incentive compatibility constraints. In the latter choice

objective holds whether the vessel owner is also the vessel captain or hires a captain to operate the vessel. The vessel owner i's profit maximization is:[13]

$$\pi_i\left(p,w,x\right)=\max_{z}\hat{\beta}_i pf\left(z_i,E_i,x\right)-\hat{\gamma}_i wz_i$$

where E is the effort from the crew which we take as given for the time, z is a non-labor variable input such as fuel and bait, x is the resource stock, p is output price, w the input price, β_i and γ_i are the boat's share revenue and costs.[14] While we use only one input and one type of labor, the results generalize to a firm that employs multiple variable inputs and multiple types of labor.

We assume that labor is fixed, and only labor effort varies, depending on the contract. Sometimes labor requirements are fixed by vessel size and gear configuration. For trips lasting more than a single day, there is an upper limit on the number of crew determined by the number of berths on the vessel. The number of crew per vessel may also be influenced by social norms. Typically there is little variation within a fishery and region for a given type of vessel. While there are fisheries in which the number of crew is chosen, it is a reasonable starting point to suppose that crew size is fixed by the configuration of the vessel.

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environment, the behavioral objective of the firm would be the maximization of the crew share taking as given the share parameter.

[13] As a reviewer notes, the model ignores the distorting effects of taxes on wages that would be paid if there is a constant payment to the crew. In particular, wage payments would make the firm liable for its share of social security payments, giving another inducement to a share system relative to wage remuneration not based on risk aversion.

[14] For an absentee vessel owner, these shares will be $\hat{\beta}\equiv 1-,\beta\ \hat{\gamma}\equiv 1-\gamma$ where β and γ are labor's shares. For an owner-operated vessel, these share would be $\hat{\beta}\equiv 1-\frac{n-1}{n}\beta,\hat{\gamma}\equiv 1-\frac{n-1}{n}\gamma$ (where n is the number of crew) as the owner would receive both the owner's share of vessel profits plus a portion of the crew share for his role as vessel captain.

We look at the implications of the share system for the application of duality as if we had the true profit function. Then we look at the econometric implications of the share system. First duality cannot give a labor demand function because there is no wage. Second, input demands when they exist mix technology and shares. The demand for the variable input implied by profit maximization is

$$z = z\left(\frac{\hat{\beta}_i p}{\hat{\gamma}_i w}, E_i x\right),$$

so that only when the share of costs is the same as the share of revenues will the share system not distort input choice. With unequal shares, a higher share for revenue than for costs raises the *effective p/w* ratio, and leads to a higher than efficient level of variable inputs. The same result would hold for capital purchases also.

This result carries several econometric implications. In practice, when one estimates input demand equations or output supply equations, the prices *p* and *w* are used. However, as shown above, optimality conditions in a fishery operating under a lay system are defined in terms of a share weighted index of input and output prices rather than observed market prices. Consequently the parameter estimates have the shares embodied in them. Basic duality results for behavior may differ with shares. The supply function for vessel *i* would typically come from the envelope theorem:

$$\begin{aligned}\frac{\partial \pi_t(p,w,x,E)}{\partial p} &= \hat{\beta}_i f\left(z_i(p,w,x,E),E,x\right)\\ &= \hat{\beta}_i y\end{aligned}$$

Hence when the shares differ, the envelope theorem implies a supply response that is less than what one would expect without a share system. An analogous result holds for inputs.

We can examine the impact of shares on the estimation of a profit function.[15] Consider the generalized Leontief profit function:

$$\pi_i^{GL}\left(\hat{\beta}_i p_t, \hat{\gamma}_i w_t, x_t\right) = g_{11}\hat{\beta}_i p_t + g_{12}\left(\hat{\beta}_i p_t\right)^{0.5}\left(\hat{\gamma}_i w_t\right)^{0.5} + g_{22}\hat{\gamma}_i w_t$$
$$+ f_1 x_t \hat{\beta}_i p_t + f_2 x_t \hat{\gamma}_i w_t$$

where g_{ij} and f_i are parameters to be estimated. The impact of shares in this model will depend on whether the shares vary by observation. Without loss of generality we can write the ith firm's shares as $\hat{\beta}_i = \overline{\beta} + \theta_i, \hat{\gamma}_i = \overline{\gamma} + \varepsilon_i$ where θ_i and ε_i can be considered firm-specific random errors. The firm's supply function would be

$$\frac{\partial \pi_i^{GL}}{\partial p_t} = g_{11}\hat{\beta}_i + g_{12}(\beta\gamma)^{0.5}\left(\frac{w_t}{p_t}\right)^{0.5} + f_1 x_t \hat{\beta}_i$$

whereas the researcher would assume to be estimating

$$\frac{\partial \pi_i^{GL}}{\partial p_t} = g_{11} + g_{12}\left(\frac{w_t}{p_t}\right)^{0.5} + f_1 x_t$$

What would be the properties of parameters estimated from a model in which behavior is implied by share systems, but the research assumes that shares are not present? The answer depends on whether the shares vary by vessel. If constant, then the shares are simply incorporated into the parameters, marking them down by the proportion of the shares. If the shares vary across vessels, as they do within various fisheries, then the actual model estimated will have something like a multiplicative error-in-variables for some of the variables, with uncertain implications.

[15] To focus on the impact of the share system in estimation we represent the profit function and the random errors for estimation 'conventional' in the Pope-Just sense, based on the idea of a disturbance but without further attribution. A more sophisticated specification of the error would include a joint distribution for the input demands and output supply and would be more informed about whether the errors were errors in measurement or errors in specification.

The impacts on parameter estimates will depend on the dual model estimated. For example, when estimating a translog profit function, unaccounted for share parameters are factored as a constant shift in the overall model intercept. However, for other functional forms such as the normalized quadratic or generalized leontief, some weighted transformation of unaccounted for share parameters are embedded in estimated slope coefficients. In addition, the choice of whether to estimate a cost function, profit function or revenue function should be informed by the type of share system that prevails in the fishery. If the lays include separate shares for costs and revenues, then estimating a cost function, which takes output as predetermined, would seem inappropriate. Furthermore, the workings of the share system have direct implication for pooling data across vessels and time. The behavioral objectives of a hired captain remunerated by a share of total vessel revenue minus trip variable costs may differ from that of the same captain when remunerated by a share of net vessel profits. Incorrectly specifying either the behavioral objective of a given fishing firm or the relative prices underlying observed choice would impact the coherence of estimated parameters.

Such misspecification error may in part explain a number of empirical irregularities noted in literature. For example, when interpreting results from a translog model of behavior for the multispecies New England otter trawl fishery, Squires (1987a) indicates that, "The profit function is not convex at 81% of the sample points." DuPont (1991) encounters similar problems in estimating a normalized, restricted quadratic profit function for the British Columbia salmon fishery. Parameter estimates are inconsistent with the assumption of convexity of the profit function and generate a downward-sloping output supply function. Both authors attribute these problems to the need to aggregate prices and harvests over a number of different outputs and insufficient price variations.

There are several reasons why regularity conditions may not be satisfied in empirical work. The workings of the share system influence relative price ratios underlying choice which if left unaccounted for in econometric models would introduce measurement error and potential misspecifications. In addition, commercial fisheries are often subject to regulatory constraints that induce corner solutions for the allocation of some subset of factor demands and important factors of production such as resource stocks and the quality of physical capital are often unobserved or measured with error. Incorrectly specifying or measuring any of the factors underlying behavior and choice may impact parameter estimates.

Random utility models of location choice

Researchers have increasingly turned to location choice models for understanding fishery behavior.[16] The lay system affects how we set up and evaluate these models. Typically, researchers use some form of total vessel expected profits (and often a measure of its variability) as the primary determinant of fishing location choice. If the vessel is owner-operated, then this formulation of the problem is correct. The captain would select the site that maximizes total vessel profits. However, when the captain is not the owner of a vessel, his objective is no longer to maximize total vessel profits but rather to maximize his own payoffs. Under the lay system, the rank ordering of sites in terms of total vessel profits and the rank order of sites in terms of crew payoffs may not coincide.

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[16] See for example, Curtis and Hicks (2000) and Mistiaen and Strand (2000).

Consider the following example. A captain chooses between two sites, A and B. Fishing at site A generates revenue of $100 whereas fishing site B generates revenue $200. However, site B is further from shore than is site A and costs $100 to access whereas site A only costs $25 to access. If the lay for this captain is a 50-50 split of revenues and costs, then Site B is the preferred choice as doing so generates payoffs of $50 whereas choosing site A results in a payoff of only $37.50. However, if the captain share of revenues were to change to 60% and his cost share increase to 100%, then the rank ordering of sites for the captain will change. Under this new contract, he strictly prefers site A and its $35 payoff to site B and its $20 payoff. In general, we will tend to underestimate the impacts of costs on location choice when the relative ratio of revenue to cost shares is less than unity and overestimate its impacts when the relative ratio is greater than unity.

Production and efficiency under the share system

Efficient allocation of resources in fisheries depends on rules of capture and the dynamic structure of property rights. Given these more fundamental characteristics of the industry, we can ask whether the share system produces a given industry output more cheaply than other remuneration systems. Of course in a fishery output may not be efficient in a bioeconomic sense, regardless of shares.

Sutinen (1979) was interested in the efficiency of output under the share system. He argued that earlier research was incorrect in assuming that vessels were inefficient under the share system. Sutinen concluded that under risk aversion, firms operating under a share system would be efficient when compared with wage or rental contracts. However, he did not address the issue of the aggregate costs of production.

The issue has been explored in the context of bioeconomic equilibrium by Anderson (1982, 1986), and by Plourde and Smith (1989). Assuming risk neutral behavior, Anderson (1982) investigates industry equilibrium in terms of the number of vessels and output per vessel. He concludes that a share system will not change firm efficiencies or the bioeconomic equilibrium if revenues are shared at the same proportion as costs. Plourde and Smith (1989) analyze equilibrium conditions when risk averse captains harvest in a bioeconomic equilibrium. Using quadratic preferences, they show that under a share system the quantity of labor hired will be greater than that hired under an equivalent wage contract. However, aggregate stock size will be larger under a share system of remuneration than that corresponding to a wage based system.

The risk neutral result is easy to see. For firm *i*, profits after labor payments would be $\beta_i[pf(z_i,E_i,x)-wz_i]$. Assuming that aggregate fishing effort (E_i) is independent of the contractual shares (β_i), the skipper-owner would behave the same whether maximizing profits or a proportion of profits. Since agents that maximize profits always minimize the costs of a given level of output, no distortions in production decisions stem from sharing profits.

The effect of the share system has direct implications for efficiency in quota markets too, as shown by Hannesson (2000). In a market for individual quotas with risk neutral asset owners, Hannesson demonstrates how the decision to purchase quota is affected by the share system. Assuming that shares are over gross vessel revenues, the share system attenuates the incentive of an asset owner to invest in permits. As Hannesson notes, 'the gain from obtaining an additional quota for an existing boat must be shared with the crew (p. 185)'. All else equal, the owner will be more likely to invest in a new vessel rather than additional quota, leading to an equilibrium in which investment exceeds the optimal level.

One can complement the basic Hannesson analysis by allowing for vessels that operate with different shares on costs and revenues to examine the equilibrium allocation of quota across vessels. When all vessels receive the same price for harvests, the marginal profits from harvest for vessel *i* are given by

$$\hat{\beta}_i p - \hat{\gamma}_i c_i$$

where p is price and c_i is the marginal cost of harvest. Quota trade equilibrium requires marginal profits to be equal or vessels *i* and *j*: $\hat{\beta}_i p - \hat{\gamma}_i c_i = \hat{\beta}_j p - \hat{\gamma}_j c_j$. Rearranging this expression gives the equilibrium for quota trade:

$$c_i = \frac{\hat{\beta}_i - \hat{\beta}_j}{\hat{\gamma}_i} p + \frac{\hat{\gamma}_j}{\hat{\gamma}_i} c_j$$

Thus, there is no guarantee that the marginal costs will be equal across firms unless shares are equal for both revenues and costs. Whenever vessels operate with different shares on costs and revenues, the potential efficiency gains from quota trade diminish. Hence, two inefficiencies emerge share systems with ITQ's; (i) Hannesson's result that too many vessels remain active and (ii) a possible inefficient allocation of quota across firms.

VI. A Model with Multiple Crew

Consider a model of crew remuneration that follows the general framework of Sutinen (1979). There are two contracting parties: the owner of the vessel and the crew. The owner contracts with a captain/crew who determines the allocation of both the quality and quantity of labor effort. The owner plays the role of capitalist (budget breaker) in the spirit of Holmstrom (1982) and builds upon his observation that, "…budget breaking is the essential instrument in neutralizing externalities from joint production. The primary role of the principal is to administer schemes that police agents in a credible way…" The timing of decision making in our model is as follows. The parties enter a contract under which the owner agrees to provide the capital to the crew for use in the harvest of stochastic resource stocks. The contract consists of two parameters (β, γ) selected by the asset owner specifying the share of *ex post* revenues received by and the *ex ante* costs paid by the crew. In selecting the contract parameters, the asset owner takes as given the effect of these shares on the effort choice of the crew and the quantity of such labor chosen by the vessel captain. Contract parameters are selected to maximize the owner's share of total vessel profits.

Once the contract is agreed upon, the captain and crew agree to make a fixed payment of γVC to the owner – where VC is variable non-labor costs of production. By requiring the crew to cover a portion of the variable costs (assumed independent of labor effort), the owner is inducing an *ex ante* "fine" or punishment for shirking while at sea.[17] In equilibrium, the asset owner selects a level of γ such that *ex post* payments to the crew satisfy individual rationality and incentive compatibility constraints only if the crew exerts a target effort level, E*. Once these payments have been made, the crew allocates

[17] The perceived credibility of this "fine" is crucial. To maintain such credibility, ex post payments must be made in accordance with the contract parameters independent of cost and revenue realizations. Given the stochastic nature of harvests, we would thus expect to view some proportion of fishing trips where crew "earnings" are negative. In practice there is limited evidence of such. For example, approximately 2.2% of fishing trips (9 out of 414) for a sample of nine vessels operating in the Atlantic sea-scallop fisher during the period 1996-200 resulted in *ex post* payments being made by the crew to the vessel owner to cover cost liabilities in excess of their revenue share.

the capital provided by the owner and decides upon how much of its own effort to exert. *Ex post* rents are then reallocated between the parties according to γ and β.

Incomes under a sharing agreement for the owner are given by[18]

$$\pi^o = (1-\beta)p\alpha x\sum_i e_i - (1-\gamma)VC \tag{1}$$

and for the individual crew member

$$\pi^c = \frac{1}{N}\left[\beta p\alpha x\sum_i e_i - \gamma VC\right] \tag{2}$$

where, i = 1, N denotes the individual crew members, α is the catchability coefficient, and x is the flow of resource stocks. Production technology in the fishery is assumed to be given by $y = \alpha xE$ where $E = \Sigma e_i$. Non-labor inputs are used in fixed proportions so that the allocation of such inputs is independent of realized contract parameters.[19] The crew will exert low effort (e_L) or high effort (e_H) depending upon incentives, so that $e_i \in (e_L, e_H)$. Uncertainty enters directly through the flow of stocks which are random with $E[x] = \bar{x}$. Total variable costs are independent of labor effort so that $\pi_E > 0$, i.e., monetary incomes are strictly increasing for both parties in labor effort.

In our model, each crewmember allocates labor effort independently, selecting either a low or high level of effort at increasing personal cost. Given stochastic resource stocks, this suggests the possibility of a team agency problem. Without credible threat of detection and punishment, a crewmember lacks the incentive to exert full effort. Share systems as incentive contracts provide an alternate rationale to the pure risk-sharing

[18] Keeping in mind that $\hat{\beta} \equiv 1-\beta, \hat{\gamma} \equiv 1-\gamma$.

[19] This assumption does not imply that non-labor inputs are unproductive. For example, one could rewrite the catchability coefficient as a function of non-labor inputs such that $\alpha = \theta z$ where z is a vector of non-labor inputs.

arguments. We derive our results under the assumption of risk neutrality on the part of the asset owner. Under this assumption, we are able to show that in the case considered by Plourde and Smith (1989) – that of a single crewmember providing an ex ante known level of labor effort – there exists a wage contract that is strictly preferred to the optimal share contract by the crew and to which the asset owner is indifferent.

The preferences of the asset owner and the i^{th} crewmember are given by the following expected utility functions:

$$U^{o} = E\left[\pi^{o}\right] = (1-\beta)p\alpha\bar{x}\sum_{i} e_{i} - (1-\gamma)VC \tag{3}$$

$$U^{c} = \frac{1}{N}E\left[\pi^{c}\right] = \frac{1}{N}\left[\beta p\alpha\bar{x}\sum_{i} e_{i} - \gamma VC\right] - v(e_{i}) \tag{4}$$

where $v(e_i)$ is a strictly convex, non-monetary cost of effort for the ith crewmember. Equations (3) and (4) highlight two major differences between our model and those employed by Sutinen and Plourde and Smith. First, the asset owner and the crew are assumed risk neutral. Second, preference structures for the crew are not strictly increasing in effort level. Hence, any optimal contract must include incentive to invoke some desired level of individual effort and dissuade shirking whenever such effort is unobservable and/or unverifiable.

The asset owner guarantees a fixed level of utility to the crew, $\overline{U}_c$, and select the contract parameters (β and γ) that would induce a choice by the crew of individual effort (e_i) and total crew size (N) that would maximize his own welfare subject to an incentive compatibility and individual rationality constraint on crew behavior. In selecting the contract parameters, the owner takes as given the response of the captain and crew in determining both the quality and quantity of labor effort exerted. Formally, the problem faced by the owner is

$$
\begin{aligned}
&\max_{\beta,\gamma,\hat{e}} U^{o} = (1-\beta)p\alpha\bar{x}\sum_{i}\hat{e}_i - (1-\gamma)VC \\
&s.t.\ (i) U_i^C\Big|_{\hat{e}} \geq \overline{U}_i \\
&\quad (ii) U_i^C\Big|_{\hat{e}} \geq U_i^C\Big|_{e\neq\hat{e}}
\end{aligned}
\tag{5}
$$

where $\hat{e}$ is the level of individual effort that maximizes the asset owner's welfare. Constraint (i) is the individual rationality constraint evaluated at $\hat{e}$, and constraint (ii) is the incentive compatibility constraint evaluated at $\hat{e}$. Denote a solution to problem (5) by $[\beta^*, \gamma^*, e^*]$.

We restrict the choice of labor effort to the set (e_L, e_H) where e_L denotes a low effort level and e_H a high effort level.[20] This simplification eases the computational burden but has no impact on the general incentive properties of the optimal labor contract.[21] To examine solutions to equation (5), we use the individual rationality (i) and incentive compatibility constraints (ii) to solve for the optimal contract parameters β* and γ*. Substituting equation (4) into (ii) and noting that e* = e_H implies that for all crew members:

$$
\frac{1}{N}\left(\beta p\alpha\bar{x}E_H - \gamma VC\right) - v(e_H) \geq \frac{1}{N}\left(\beta p\alpha\bar{x}E_L - \gamma VC\right) - v(e_L) \tag{6}
$$

[20] In our setting, e_L does not have to be zero effort level. One can imagine a case where effort is imperfectly monitored over some range, but defections outside of this range easily detected and credibly punished. In this case, e_L would correspond to the lower bound of this range and e_H the upper bound.

[21] Our focus in this article is to examine labor contracts in commercial fisheries as a mechanism to resolve the team agency problem as opposed to the pure risk sharing argument set forth in the previous literature. We are thus concerned with the general structure/incentive properties of such contracts and not their exact derivation. The use of a discrete choice set for the agent's action space as opposed to a continuum of actions does not alter these basic features of the contract and are sufficient for our intentions.

Equation (6) has the straightforward interpretation that expected utility under high effort levels must be greater than or equal to that generated by exerting low effort. Hence, the problem of the asset owner is to select a revenue share β that satisfies equation (6) and maximizes his own expected utility. Solving for β, we have

$$\beta \geq \frac{N\left(v(e_H) - v(e_L)\right)}{p\alpha\bar{x}\left[e_H - e_L\right]} \tag{7}$$

Noting that for any given level of γ, the owner maximizes expected utility by minimizing subject to equation (3) we get

$$\beta^* = \frac{N\left(v(e_H) - v(e_L)\right)}{p\alpha\bar{x}\left[e_H - e_L\right]} \tag{8}$$

We now consider possible solutions for the cost share parameter, γ. To solve for γ, we substitute β* into the individual rationality constraint. By substitution we have that,

$$e_H \frac{v(e_H) - v(e_L)}{e_H - e_L} - \frac{1}{N}\gamma VC - v(e_H) \geq \overline{U} \tag{9}$$

This expression has the interpretation that for any potential crew member, the utility expected under the labor contract must not be less than the reservation utility. Solving equation (9) for γ and noting that the asset owner's utility is maximized by selecting the maximum possible value of γ that satisfies equation (5) we have that

$$\gamma^* = \frac{N}{VC}\left[e_H\left(\frac{v(e_H) - v(e_L)}{e_H - e_L}\right) - v(e_H) - \overline{U}\right] \tag{10}$$

In equation (10), the sign of the optimal cost share depends upon model parameters and is *a priori* unknown. For given parameter values, wages could include a flat payment independent of effort levels and harvests and no cost sharing. For other values, the owner and crew share both costs and revenues. If γ* < 0, then the optimal contract resembles that considered in Matthiasson (1999) where crew remuneration is based upon an internal wage rate plus a catch share. So the Matthiasson result emerges as a special case of the model with multiple crew. When $\gamma^* \geq 0$, we are in the more traditional setting for U.S. fisheries with a sharing of both catch and operating costs.

Given the optimal revenue and cost shares, there are two empirical regularities that one ought to be able to explain. First, there are commercial fisheries where cost and revenue shares are constant across vessels. Is this regularity consistent with our model of incentive contracts and what type of economic conditions could generate such equilibration across distinct fishing vessels? In our model, the expressions defining the optimal revenue and cost shares are functions of the economic conditions – i.e., prices, production technology, stock flows – within a fishery, the reservation utility of labor, and the disutility of labor effort. Testing the consistency of our model with the observation of equal shares across firms within a given fishery is equivalent to determining whether the factors that determine an optimal contract are equivalent across these same firms. For a geographically concentrated fisher with relatively homogeneous firms we would expect such equivalence. Firms would face an identical distribution of resource stocks and would hire crew from localized labor markets where agents face similar outside options.

Second, there are instances where revenue and cost shares are equal for a given vessel. What types of industry structure and underlying biological/economic conditions would rationalize such an outcome as optimal? Conceptually one could derive a mathematical expression that would determine the structure necessary to rationalize equal cost and revenue shares but such an expression would have no clear economic interpretation. However, if one considers contractual parameters as the solution to a bargaining problem between a vessel owner and a pool of potential labor, then one could imagine such an outcome constituting an equilibrium. The expressions derived above for β^* and γ^* are the minimum possible incentive compatible revenue and cost shares. There are other possible equilibria for the contact shares one of which could set equal revenue and cost share ($\beta = \gamma$) provided that we have $\beta^* > \gamma^*$.

Single Crew with Observable Effort

In this section, we consider the solution to a special case of our more general model – that of a single crewmember with contractible and *ex post* verifiable effort. The case mirrors that considered by Plourde and Smith and highlights the major differences between the moral hazard and pure risk sharing interpretations of the lay system. First, the assumption of risk aversion on the part of the boat owner is fundamental to the pure risk sharing interpretation of the lay system. By relaxing this assumption, it is shown that there exists a wage contract that is strictly preferred by labor to the "optimal" share contract and to which the asset owner is indifferent. In such markets, we would expect labor to be paid a wage, not a revenue share. Second, given a risk neutral boat owner, revenue sharing is optimal only as an incentive mechanism to induce high effort levels when such effort is *ex post* unverifiable. Whenever such an asset owner can contract over effort levels and such contracts are verifiable, there exists a wage contract that strictly dominates any sharing arrangement.

We now consider potential solutions to the owner's maximization problem in this special case. The general structure of the analysis follows that presented in the previous section with two modifications. First, instead of considering multiple crew each providing an independently chosen level of fishing effort we limit crew size to a single agent. Second, we assume that effort is contractible and *ex post* verifiable. Hence, while the crew can still choose to provide either a high or a low level of effort, *ex ante* the parties can contract over this choice and *ex post* the choice is known to the asset owner. We maintain the assumption that $e^* = e_H$ and limit our analysis to contracts of the form

$$(\beta,\gamma)=\begin{cases}(\beta^*,\gamma^*) & if\ \ e=e^*\\ (0,0) & otherwise\end{cases}$$

in deriving potential solutions to equation (5).

The owner's maximization problem in this special case is given by the following version of equation (5):

$$\begin{aligned}&\max_{\beta,\gamma,\hat{e}} L=(1-\beta)\,p\alpha\bar{x}\hat{e}-(1-\gamma)VC\\ &s.t.\ \bar{U}\leq\beta p\alpha\bar{x}\hat{e}-\gamma VC-v(\hat{e})\end{aligned} \tag{11}$$

The maximization problem in equation (11) does not include an incentive compatibility constraint as the contract explicitly induces high effort given that the inequality is satisfied. Potential solutions to equation (11) are derived in a manner identical to that employed in the previous section. Solving the constraint for β, we get

$$\beta^*=\frac{\bar{U}+v(e_H)+\gamma VC}{p\alpha\bar{x}e_H} \tag{12}$$

where γ is exogenously determined by the vessel owner. Under this contract, the agent's expected utility is given by $\bar{U}$ when exerting high effort and $-v(e_L)$ when exerting low effort. The contract is thus an equilibrium solution to the principal's problem as it maximizes the principal's utility and the agent's best response is to exert high effort.

While equation (12) is an equilibrium contract, we can show that since effort is contractible and observable there exists a pure wage contract that weakly dominates this sharing arrangement for risk averse agents. Hence, in the basic Plourde and Smith situation with a risk neutral boat owner we would expect a pure wage contract. The proof is available in a separate appendix. We first show that there exists a wage contract under which it is the agent's best response to exert high effort. It is then shown that a risk averse agent strictly prefers a wage contract as payments under a wage contract second order stochastically dominate those earned under the optimal share contract. Finally we show that a risk neutral boat owner is indifferent between a wage and share contract as expected earnings are equal under both.

VII. Concluding Comments

This paper has explored the origin and implications of the lay system in fisheries. The received explanation for the lay system is the sharing of risk between the crew and the owner or captain. There are reasons to doubt risk sharing as the single explanation of the lay system. Risk sharing requires risk aversion to explain the lay system. Even if all agents are risk averse, it seems unlikely that it would be optimal for poorer crew to share risks with wealthier owners. Further, the share system is prominent in some fisheries with considerable uncertainty but do not have risk sharing among all crew. An additional explanation of the lay system is to avoid shirking among crew.

There are two sorts of implications of the lay system. In estimation, both random utility models and dual models of production may be subverted by the presence of shares. Concerning efficient resource use, differences in shares among vessels in an industry may mean that an ITQ system will not achieve full efficiencies. Further Hannesson shows that the presence of a share system will lead to investment in new boats rather than ITQ's.

Finally in a share system with different shares for revenues and costs, captain owner decisions about resource use will not be optimal.

There are fruitful directions for research. For example, we examine two types of remuneration systems offered in commercial fisheries – fixed wages and share contracts – and explain each in the context of individual incentives and the feasibility of monitoring and enforcing fisherman effort. However, there are two alternate incentive compatible contracts – fixed rental and rank-order tournaments – which arise in agriculture but are rarely observed in commercial fisheries. Why are such contract forms missing from fisheries?

Appendix

Consider the following wage contract structure where the principal offers the agent a wage

$$w = \begin{cases} \bar{U} + v\left(e_H\right) \; if \;\; e = e_H \\ 0 \;\; if \;\; otherwise \end{cases}$$

Under this contract structure, the agent's best response is to give high effort as he earns $\bar{U}$ when exerting high effort and -v(e_L) if exerting low effort. This constant wage contract second order stochastically dominates that derived in equation (12) which generates expected utility of $\bar{U}$ but has a positive variance as actual payments are dependent upon stochastic harvest levels. A risk averse agent thus strictly prefers a wage contract.

A risk neutral principal is indifferent between a wage and share contract as expected rents under both are equal. Under the optimal share contract the principle earns expected rents given by:

$$\begin{aligned} E\left(\pi^o\right) &= E\left[\left(1-\beta^*\right) p\alpha x e_H - \left(1-\gamma^*\right) VC\right] \\ &= p\alpha \bar{x} e_H - VC - v\left(e_H\right) - \bar{U} \end{aligned}$$

Under a wage contract, the principle earns expected rents given by:

$$\begin{aligned} E\left(\pi^o\right) &= E\left[p\alpha x e_H - VC - w\right] \\ &= p\alpha \bar{x} e_H - VC - v\left(e_H\right) - \bar{U} \end{aligned}$$

The risk neutral principal is thus indifferent between a pure wage and a pure sharing contract as both result in equal expected rents.

Chapter 2:

Inferring Treatment Status when Treatment Assignment is Unknown:

with an Application to Collusive Bidding Behavior in Timber Auctions[22]

I. Introduction

Several quite distinct empirical approaches have been developed to study the impact of public policies. Approaches as diverse as general equilibrium analyses to partial equilibrium research programs have lent important insights into fundamental issues such as the impact of taxes and subsidies on labor supply and demand (for examples, see Shoven and Whalley 1992; Ashenfelter 1978). Within this rich assortment of empirical methods, the program evaluation literature has witnessed rapid growth in the past several decades (see, e.g., Heckman and Smith 1995). The task confronting econometricians utilizing this framework is to identify treatment effects using non-experimental data on treatment assignment and outcomes conditional on treatment assignment. Intuitively, the crux of the approach relies on constructing the proper counterfactual since a given person cannot be observed simultaneously in both the treatment and control groups. With the increased focus on evidence-based policy in the U.S. and abroad, the methodology of program evaluation has expanded ubiquitously among both academicians and policymakers. And, it is fair to say that this area of research remains one of the most vibrant within empirical economics.

[22] This essay was written with Daniel Millimet and John List. Thanks to Bill Howard who provided the Canadian timber auction data. Also, numerous discussions with several Canadian officials, including but not limited to, Bruce McRae, Michael Stone, and Bill Howard, considerably enhanced this paper. Larry Ausubel, Patrick Bajari, Peter Cramton, John Horowitz, Esfandiar Maasoumi, Michael Margolis, Tigran Melkonyan, and Richard Woodward provided useful suggestions. The authors are also grateful to seminar participants at U. of Nevada – Reno, SMU, and Texas AM.

An interesting, and heretofore unexplored, aspect of the program evaluation method is that the underlying structure can be used as a starting point in the development of an empirical model to identify the treatment *assignment* of observations given data on *outcomes*. In this spirit, the major goal of this study is to provide a unified framework for estimating treatment assignment when the econometrician does not observe this piece of information. Such an approach has a myriad of economic applications. For example, in principal/agent settings determining whether agents have shirked conditional on output realizations remains an important issue in both a positive and normative sense. Further, detecting renegers of bilateral trade or environmental agreements based on trade and environmental quality patterns is an important policy consideration, as is detecting job search effort by the unemployed given data on the duration of unemployment spells.

After presenting our formal framework, we make use of economic theory to apply the empirical methods to a timely application concerning bidder behavior in Canadian softwood timber auctions. The long-standing softwood timber trade dispute between the U.S. and Canada is nearing resolution and the cornerstone of the agreement between the U.S. and the province of British Columbia (the largest player in Canada) is that auctions will be used to price cutting rights on non-auctioned federal lands. A necessary condition articulated in the agreement is that these auction markets must be viable and robust and not be influenced by collusion. We apply our empirical methods to examine whether these Canadian timber auctions meet this condition. Previous research related to our application – detection of corruption – has proceeded in a literature distinct from the treatment effects literature (see, e.g., Porter and Zona 1993, 1999; Baldwin et al. 1997; Pesendorfer 2000; Bajari and Ye 2003). Our approach clarifies many issues of importance in the collusion literature; it rejects the validity of some approaches commonplace in the literature, and highlights other approaches not previously considered. Making use of a unique data set that includes nearly 3,000 auctions (over 10,000 individual bids) for cutting rights of standing timber in British Columbia from 1996 – 2000, we find statistical evidence consistent with a model of collusive behavior by a nontrivial subset of bidders. From a policy perspective this finding merits serious consideration; in a normative sense, this application raises several issues that scholars should find useful.

The remainder of the paper is organized as follows. Section 2 presents our framework in which to infer unobserved treatment assignment. Section 3 presents the application to collusion in Canadian timber auctions. Section 4 concludes.

II. Empirical Framework

Our goal is to devise a coherent framework in which to infer the treatment status (i.e., membership in either the treatment or control group) of observations from non-experimental data when actual treatment assignment is unobserved by the econometrician. As stated, the problem represents a twist of the existing program evaluation methodology, where the goal is to identify the impact of a treatment on the outcome of interest, typically using non-experimental data (see, e.g., Heckman and Smith (1995) and the citations therein). Using standard notation from this literature, we let τ denote the average treatment effect (ATE), where $\tau = E[y_1 - y_0]$ and y_1 (y_0) is the outcome if treated (untreated), $\{y_{i1}\}_{i=1,\ldots,N1}$ denote the observed outcomes for the treatment sample (given by D_i=1), and $\{y_{i0}\}_{i=1,\ldots,N0}$ denote the observed outcomes for the control sample (given by D_i=0).[23] In this framework, the problem confronted in the program evaluation literature is how to consistently estimate τ given the observed sample $\{y_i, D_i\}_{i=1,\dots,N}$ obtained from non-experimental data, where $y_i = D_i y_{i1} + (1-D_i) y_{i0}$ and $N = N_0 + N_1$.[24]

In the current context, the problem is not how to identify the *treatment effect* given data on treatment assignment and outcomes conditional on treatment assignment for a sample of observations, but rather how to identify *treatment assignment* given data on outcomes for a sample of observations. Since some additional information is required for this to be feasible, we assume that the sign of τ is known. In other words, the goal is to estimate D_i – which is *unobserved* by the econometrician – for all *i* given the observed sample $\{y_i\}_{i=1}^{N}$ and the known sign of τ. Alternatively, one might focus on the less ambitious goal of detecting whether $D_i = 1$ for some *i* in the sample, i.e., detecting whether any observations in a given sample are treated. Many practical economic problems, of course, require this most basic piece of information.

To proceed, we take a multi-faceted approach to the identification of treatment assignment. Since we do not claim to know the underlying data generating process, we advocate analyzing several possibilities and, in the end, weighing the totality of the evidence. In our application (detection of collusion), this is consonant with the claim in Porter and Zona (1993, p. 519) that "finding a single test procedure to detect bid rigging is an impossible goal." Finally, we consider a slightly richer environment by assuming

[23] The treatment and control groups are mutually exclusive (i.e., the same individuals do not appear in both groups). Of course, if the same individual could be observed in both states the evaluation problem is solved.

[24] In the program evaluation literature, there are many parameters of interest (e.g., the average treatment effect, the average treatment effect on the treated or untreated, the local average treatment effect, etc.). For present purposes, we do not worry about such distinctions.

the presence of panel data on observations and outcomes. This approach not only maintains consistency with our application considered below, but also nests other important data configurations in that we consider (i) $D_{it} = D_i$ for all i (time-invariant treatment assignment) and (ii) $D_{it} \neq D_i$ for all i (time-varying treatment assignment).

Time-Invariant Treatment Assignment

To devise test(s) of whether in fact $D_i = 1$ for at least a subset of the sample, we begin by considering the 'true' data-generating process (DGP). Thus, the DGP gives the 'correct' specification that one would wish to estimate *if* treatment assignment were observed. As the true DGP is unknown in practice, we shall consider several potential specifications. Initially, we consider two cases given by:

$$y_{it} = X_{it}\beta + \tau D_i + \varepsilon_{it} \qquad \text{sgn}(\tau) \text{ is known} \tag{DGP1}$$

$$y_{it} = D_i\left[X_{it}\beta_1\right] + \left(1 - D_i\right)\left[X_{it}\beta_0\right] + \varepsilon_{it} \qquad \beta_0 \neq \beta_1 \tag{DGP2}$$

where y_{it} is the outcome for observation i at time t, X is a set of controls, and ε_{it} is a mean-zero, normally distributed, homoskedastic error term, which is independent of X and D.[25] Specifications (DGP1) and (DGP2) assume the 'true' model adheres to the standard linear approximation, where (DGP1) indicates that the treatment acts only as an intercept shift, while (DGP2) permits the treatment effect to enter both the intercept and slope terms. In addition, across both specifications, the idiosyncratic error term is presumed to be correlated among the treatment group (D = 1), but uncorrelated among the control

[25] Thus, if D were observed, we restrict attention to problems falling within the 'selection on observables' framework. Inferring treatment status in applications involving 'selection on unobservables' is left for future research.

group (D = 0), and between the treatment and control groups.[26] Finally, both specifications restrict the treatment assignment to be static over time. The question in this case becomes: if the underlying data is generated according to (DGP1) or (DGP2), how can one infer which (or, whether any) observations are treated given the data $\{y_{it}, X_{it}\}_{i=1,\ldots,N;t=1,\ldots,T}$.

Case I

To begin, we assume that the data is generated according to (DGP1). Accordingly, there are (at least) three methods for detecting whether D_i =1 for some subgroup of the sample. In each of the three methods considered, the null hypothesis being tested is that there are no treated observations (D_i =0 for all i); the alternative hypothesis is that some sample observations belong to the treatment group (D_i =1 for some i).

Our first detection algorithm notes that while this specification cannot be estimated on the observed sample (since D_i is unobserved); the model may be estimated via a standard fixed effects approach since D_i is time-invariant. Thus, one can estimate

$$y_{it} = X_{it}\beta + \tau_i + \varepsilon_{it} \tag{1}$$

where τ_i are observation fixed effects. Given the following set of assumptions:

(A1) (DGP1) represents the 'true' data-generating process

(A2) X_{it} in (DGP1) does not contain an intercept

(A3) X_{it} in (DGP1) does not contain any time invariant variables

(A4) there is no other source of time-invariant heterogeneity,

[26] In the application we consider, cross-section dependence within the treatment group is an important consideration. In other contexts, such correlation may seem unrealistic.

then $E\left[\hat{\beta}\right]=\beta$ and

$$E\left[\hat{\tau}_i\right]=\begin{cases}\tau & \text{if } D_i=1\\ 0 & \text{if } D_i=0\end{cases}$$

Consistency of the parameter estimates requires that T → ∞. As a result, if $\tau < 0$ (> 0) and τ_i is negative (positive) and statistically different from zero, then we reject the null that $D_i = 0$ for all i and infer that $D_i = 1$ for these observations. Note that under assumptions (A1) – (A4), this method also yields the identities of the treated observations.[27]

In the event that (A2) is unlikely to hold, an alternative detection method is available by estimating (1) after de-meaning the data. Specifically, it is clear that estimation of

$$\begin{aligned}\Delta y_{it} &= \Delta X_{it}\beta+\tau\Delta D_i+\Delta\varepsilon_{it}\\ &\equiv \Delta X_{it}\beta+\tilde{\tau}_i+\Delta\varepsilon_{it}\end{aligned} \tag{2}$$

where Δ in front of a variable indicates deviations from the overall sample mean, implies that $E\left[\hat{\beta}\right]=\beta$ and

$$E\left[\hat{\tilde{\tau}}_i\right]=\tau\Delta D_i=\begin{cases}\tau\left(1-\bar{D}\right) & \text{if } D_i=1\\ -\tau\bar{D} & \text{if } D_i=0\end{cases}$$

[27] If (A3) does not hold, it is still possible to estimate τ_i via a two-step estimation procedure. Specifically if (1) may be re-written as

$$\begin{aligned}y_{it} &= X_{it}\beta+\tau_i+\varepsilon_{it}\\ &= X_{0it}\beta_0+X_{1i}\beta_1+\tau_i+\varepsilon_{it}\\ &= X_{0it}\beta_0+\tilde{\tau}_i+\varepsilon_{it}\end{aligned}$$

where $X_{it} = [X_{0it}\ X_{1it}]$, then upon estimating $\tilde{\tau}_i$, $\hat{\tau}_i$ is given by the residuals from a second-stage regression of $\tilde{\tau}_i$ on X_{1i} (and no intercept).

under (A1), (A3), and (A4), where $\bar{D}$ is the sample proportion of treated observations. Moreover, if $\bar{D} \in (0,1)$, then

$$\operatorname{sgn}\left(\hat{\hat{\tau}}_i\right) = \begin{cases} \operatorname{sgn}(\tau) & \text{if } D_i = 1 \\ -\operatorname{sgn}(\tau) & \text{if } D_i = 0 \end{cases}$$

Thus, as above, if $\tau < 0$ (> 0) and $\hat{\hat{\tau}}_i$ is negative (positive) and statistically different from zero, then we reject the null that $D_i = 0$ for all i and infer that $D_i = 1$ for these observations. Once again, this method yields the identities of the treated observations.

A second detection method requires estimation of (1), obtaining the residuals, $\hat{\varepsilon}_{it}$, and testing for correlation among pairs of observations.[28] Under assumption (A1) only, pairs of observations i and j for which $\hat{\rho}_{ij}$, the estimated correlation coefficient between the residuals for observations i and j, is non-zero are inferred to belong to the treatment group. As in the previous case, this method consistently estimates the specific observations that belong to the treatment group as $T \to \infty$.

Formal statistical testing for non-zero correlation is accomplished using the Fisher-Z transformation given by

$$Z = \frac{1}{2}\ln\left(\frac{1+\hat{\rho}_{ij}}{1-\hat{\rho}_{ij}}\right)$$

where $z = Z\sqrt{(n-3)}$ follows a standard normal distribution under the null hypothesis of conditional independence and n is the number of observations. Rejection of the null hypothesis of zero correlation is equivalent to rejection of the null of belonging to the

[28] This test draws upon the *conditional independence* test of Bajari and Ye (2003) in the context of detecting collusion.

control group. Note that unlike the previous method based on the statistical significance of the fixed effects, this test does not require assumptions (A2) – (A4).

The final detection method calls for (i) guessing D_i, denoted by $\tilde{D}_i$ for each observation based on some *a priori* information, and (ii) estimating (DGP1) by replacing D_i with $\tilde{D}_i$.[29] This procedure almost certainly introduces measurement error into the estimation, but measurement error – even in the case of a mis-measured binary regressor – only attenuates $\hat{\tau}$ toward zero. Given the following assumptions:

(A5) $Cov\left(D_i, \tilde{D}_i\right) > 0$

(A6) $Cov\left(X_{it}, \xi_{it}\right) = 0,$ where ξ_{it} denotes the measurement error ,

in addition to (A1), $\hat{\tau}$ constitutes a lower bound in absolute value (Aigner 1973; Bollinger 1996; Black et al. 2000).[30] As a result, under (A1), (A5) – (A6), $\hat{\tau}$ should be of the correct sign if $D_i = 1$ for some i.[31] Further, if one uses the criterion that $\hat{\tau}$ should be statistically significant (and of the correct sign) in order to reject the null of no treated observations, then this is a conservative test as it will tend to under-reject the null (resulting in Type II error).

Black et al. (2000) show that the lower bound may be improved via estimation of

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[29] This algorithm builds on the proposed tests for collusion set forth in Porter and Zona (1993) and Bajari and Ye (2003).

[30] Bollinger (1996, 2001) and Black et al. (2000) derive measures of how to obtain various upper bounds (in absolute value) for $\hat{\tau}$ in fixed effects and cross-sectional models. Since we only are concerned with whether or not $\tau = 0$ in the current context, however, the upper bound is of little substantive interest.

[31] In particular if (A1) fails because $\tilde{D}_i$ directly affects y, then the sign of the coefficient on $\tilde{D}_i$ contains no information about whether $D_i = 1$ for some i. To see this, suppose the 'true' DGP is given by $y_{it} = X_{it}\beta + \tau D_i + \delta \tilde{D}_i + \varepsilon_{it}, \tilde{D}_i = D_i + \xi_i$ and (A5) and (A6) hold. It follows then, that $y_{it} = X_{it}\beta + \left(\tau + \delta\right)\tilde{D}_i + \left(\varepsilon_{it} - \tau\xi_i\right)$ and $\operatorname{sgn}\left(\tau + \delta\right)$ reveals nothing about sgn (τ) and D_i.

$$y_{it} = X_{it}\beta + \tau_0 I\left[\tilde{D}_i = 0, \tilde{D}_i' = 1\right] + \tau_1 I\left[\tilde{D}_i = 1, \tilde{D}_i' = 0\right] + \tau_2 I\left[\tilde{D}_i = 1, \tilde{D}_i' = 1\right] + \eta_{it} \tag{3}$$

where I[·] is an indicator function equal to one if the condition is true, zero otherwise, and $\tilde{D}_i'$ is a second mis-measured indicator equal to one if observation *i* is suspected of belonging to the treatment group, zero otherwise. Black et al. (2000) prove that $0 < \left|E\left[\hat{\tau}\right]\right| < \left|E\left[\hat{\tau}_2\right]\right| < |\tau|$ if the measurement errors for $\tilde{D}_i$ and $\tilde{D}_i'$ are independent conditional on actual treatment assignment, D_i. Using this general framework, Black et al. (2000) also show how one may obtain a point estimate for τ via a method of moments estimator provided that the measurement errors are independent conditional on actual treatment assignment.[32] Given such independence, $\hat{\tau}$ should be of the correct sign and statistically significant if $D_i = 1$ for some *i*. Unfortunately, in many cases the assumption of conditionally independent measurement errors may not be reasonable.

Before continuing to the detection tests assuming the data are generated according to (DGP2), it is worth emphasizing that the tests for treatment assignment discussed thus far depend crucially on assumption (A1). Specifically, the validity of two aspects of the underlying model are worth emphasizing.[33] First, even if the structure of (DGP1) is correct (i.e., linear specification with the treatment affecting the intercept only), the tests may be invalid if there are omitted covariates. If only $\tilde{X}_{it} \subset X_{it}$ is included in (1), then the observation fixed effects will partially reflect the effect of the omitted covariates, even if the omitted variables vary across auctions. Moreover, such omissions will also

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[32] Unlike the classical measurement error model, an instrumental variable strategy only yields an upper bound (in absolute value) for τ; the point estimate is not identified given the correlation that exists between D_i and the measurement error (Black et al. 2000).

[33] Similar points are raised in Bajari and Ye (2003) in the context of detecting collusion.

affect the consistency of residual estimates, thereby invalidating the tests for conditional independence (e.g., residuals across observations may be correlated either because they belong to the treatment group or because of similar values of omitted covariates). Finally, such omissions will invalidate tests based on $\hat{\tau}$ $\left(and\ \hat{\tau}_2 \right)$ obtained using $\tilde{D}_i$ (or $\tilde{D}'_i$) if the omitted regressors are correlated with $\tilde{D}_i$ or $\tilde{D}'_i$. Second, the test based on correlation between the residuals presumes that the errors are uncorrelated across observations in the control group. Thus, the test relies on the fact that, absent treatment, there is no cross-sectional dependence.

Case II.

Next, we assume that the data are generated according to (DGP2). As before, the null hypothesis is that $D_i = 0$ for all i; the alternative is that $D_i = 1$ for some i. Under (DGP2), we consider three methods of detecting whether $D_i = 1$ for some subgroup of the sample. Our first detection algorithm makes use of the following assumptions:

(B1) (DGP2) represents the `true' data-generating process

(B2) $T \geq k$, where k is the rank of X_{it}.

Under assumptions (B1) – (B2), one may estimate the following model separately for each observation in the sample:

$$y_{it} = X_{it}\beta_i + \varepsilon_{it} \tag{4}$$

where

$$E\left[\hat{\beta}_i\right] = \begin{cases} \beta_1 & if\ D_i = 1 \\ \beta_0 & if\ D_i = 0 \end{cases}$$

and consistency requires T → ∞.Upon estimating $\left\{\hat{\beta}_i\right\}_{i=1,\ldots,N}$,the null of *either* no treated or no control observations is equivalent to the null $H_0 : \hat{\beta}_i = \beta \ \forall i$, which may be tested using an F-test, as in a standard Chow test. Rejection of the null implies that different observations are operating under different regimes, a result inconsistent with the notion that all observations belong to the control group.[34]

Two remarks are worth noting prior to extending the model. First, this detection algorithm does not identify the specific observations belonging to the treatment group. Second, if (DGP1) represents the underlying data structure, only the intercepts should differ across the treatment and control groups. Thus, if the null $H_0 : \hat{\tilde{\beta}}_i = \tilde{\beta} \ \forall i$ cannot be rejected, but $H_0 : \hat{\alpha}_i = \alpha \ \forall i$ is rejected, where $\beta_i = \left[\alpha_i \ \tilde{\beta}_i\right]$ and α_i is the intercept, then this is consonant with (DGP1) being the `true' model, as opposed to (DGP2).

If assumptions (B1) – (B2) are met, then a second detection algorithm is available based on correlation of the residuals (conditional independence). As in Case I, one may obtain consistent estimates of the residuals, $\hat{\varepsilon}_{it}$, as T → ∞ by estimating (4) separately for each *i* and then testing for correlation among pairs *i*, *j*, $i \neq j$. Pairs of observations *i* and *j* for which $\hat{\rho}_{ij}$ is non-zero are inferred to belong to the treatment group. Note that unlike the previous detection algorithm, this method identifies the specific observations belonging to the treatment group.

Given that the rank condition (B2) may fail in practice, a third detection method calls for replacing D_i with $\tilde{D}_i$, estimating

[34] This test builds on the test of *exchangeability* outlined in Bajari and Ye (2003) in the collusion context.

$$y_{it} = \tilde{D}_i \left[X_{it} \beta_1 \right] + \left(1 - \tilde{D}_i \right) \left[X_{it} \beta_0 \right] + \varepsilon_{it} \quad (5)$$

and testing the null $H_0 : \hat{\beta}_1 = \hat{\beta}_0$. As before, this procedure introduces non-trivial measurement error into the estimation, making this a conservative test as it will tend to under-reject the null of no treated observations.[35] Moreover, as noted above (see footnote 32), an instrumental variables strategy will not correct this problem, as it will only produce upper bounds (in absolute value) for β_1 and β_0 (since D_iX_{it} and $(1- D_i)X_{it}$ remain negatively correlated with the measurement error). Furthermore, to our best knowledge there exists no method for deriving more accurate lower bounds that would improve upon the power and performance of the test procedures.[36]

Prior to continuing, two comments pertaining to Case II are necessary. First, (DGP2) specifies that the errors are correlated (uncorrelated) among treated (untreated) observations. However, the measurement error introduced via the use of $\tilde{D}_i$ implies that the residuals will not be consistent estimates of the errors; thus, estimates of the pair wise correlations are also inconsistent. Thus, assumption (B2) is necessary for any valid test based on error correlation within (DGP2). Second, as in Case I, it is worth emphasizing that the three detection algorithms considered in Case II depend on assumption (B1). The presence of omitted, relevant covariates in practice will invalidate both tests if the omitted variables are correlated with some of the included regressors. In addition, correlation of the residuals between control observations (e.g., due to common, omitted

[35] Unlike in Case I, the presence of a second mismeasured indicator is of little help as inference would still be based on bower bounds for β_1 and β_0 as opposed to actual point estimates.

[36] The only possible solution we envision is an expanded version of the method of moments estimator for obtaining actual point estimates as proposed in Black et al. (2000).

variables or cross-sectional dependence arising for other reasons) will invalidate tests based on the independence of the errors.

Time-Varying Treatment Assignment

(DGP1) and (DGP2) both suppose that treatment assignment is time-invariant. In many applications, however, this may not be the case. For example, countries may renege on bilateral trade or environmental agreements at some points in time and not others; similarly, firms may engage in price-fixing only at certain times. In light of this possibility, we redefine the treatment assignment of interest as

$$D_{it} = \begin{cases} 1 & \textit{if treated at time } t \\ 0 & \textit{otherwise} \end{cases} \tag{6}$$

which is still unobserved by the econometrician. Consequently, we consider two additional specifications of the underlying DGP:

$$y_{it} = X_{it}\beta + \tau D_{it} + \varepsilon_{it} \qquad \operatorname{sgn}(\tau) \text{ is known} \tag{DGP3}$$

$$y_{it} = D_{it}\left[X_{it}\beta_1\right] + \left(1 - D_{it}\right)\left[X_{it}\beta_0\right] + \varepsilon_{it} \qquad \beta_0 \neq \beta_1 \tag{DGP4}$$

where all notation has been previously defined. As before, we consider each specification in turn, and devise tests of the null that $D_{it} = 0$ for all *i, t* versus the alternative that $D_{it} = 1$ for some *i, t*.

<u>Case III.</u>

Assume the data are generated according to (DGP3). Under this specification, we consider two methods of detecting whether $D_{it} = 1$ for some subgroup of the sample. The first detection algorithm replaces D_{it} with $\tilde{D}_{it}$, the hypothesized value of D_{it}, as in the previous section. Estimation of

$$y_{it} = X_{it}\beta + \tau\tilde{D}_{it} + \varepsilon_{it} \tag{7}$$

introduces measurement error, but still allows for a conservative test based on the sign and statistical significance of $\hat{\tau}$. In addition, as in Case I, a less conservative test may be devised if a second mis-measured indicator of $D_{it}, \tilde{D}'_{it}$ – with uncorrelated measurement errors conditional on treatment assignment – is available. Such a test is based on the sign and significance of τ_2 from the regression

$$y_{it} = X_{it}\beta + \tau_0 I\left[\tilde{D}_{it} = 0, \tilde{D}'_{it} = 1\right] + \tau_1 I\left[\tilde{D}_{it} = 1, \tilde{D}'_{it} = 0\right] + \tau_2 I\left[\tilde{D}_{it} = 1, \tilde{D}'_{it} = 1\right] + \eta_{it} \tag{8}$$

Finally, a point estimate for τ is available via the method of moments estimator outlined in Black et al. (2000) if the measurement error associated with the two indicators, $\tilde{D}_{it}, \tilde{D}'_{it}$, is uncorrelated conditional on treatment assignment. In all cases, if $\hat{\tau}$ (or $\hat{\tau}_2$) is of the correct sign and statistically significant, one rejects the null of no treated observations.

A second detection algorithm, based on correlation of the errors among the treatment group, is possible only if one pursues the Black et al. (2000) method of moments approach to derive point estimates of β and τ in (7). Given consistent estimates of the parameters in (7), one may test for correlation of the residuals, $\hat{\varepsilon}_{it}$, among pairs of observations *i* and *j*. This will also be a conservative test, however, as even when *i* and *j* both belong to the treatment group for some *t* and *t'*, respectively, the residuals may be uncorrelated if $t \neq t'$. Moreover, if one suspects cross-sectional dependence of the errors even absent treatment, then this test will be invalid.

Case IV.

Lastly, assume that the underlying data structure is given by (DGP4). Under this specification, there continues to be (at least) two feasible detection algorithms; both, however, require replacing D_{it} with $\tilde{D}_{it}$. First, given the following assumptions:

(D1) (DGP4) represents the `true' data-generating process

(D2) T_{0i}, $T_{1i} \geq k$ for all i, where k is the rank of X_{it} and T_{0i} (T_{1i}) is the number of periods in which observation i is assumed to be untreated (treated),

then the following regression equation is estimable separately for each observation:

$$y_{it} = D_{it}\left[X_{it}\beta_{1i}\right] + \left(1 - D_{it}\right)\left[X_{it}\beta_{0i}\right] + \varepsilon_{it}$$

Given (D1) – (D2), $E\left[\hat{\beta}_{0i}\right] = \beta_0$ and $E\left[\hat{\beta}_{1i}\right] = \beta_1$ and consistency requires T → ∞. Upon estimating $\left\{\hat{\beta}_{0i}, \hat{\beta}_{1i}\right\}_{i=1,\ldots,N}$, the null $H_0 : \hat{\beta}_{0i} = \hat{\beta}_{1i}\ \forall i$ may be tested using an F-test, as in a standard Chow test. Rejection of the null implies that at least some observations are operating under different regimes during different periods, which is inconsistent with the observation belonging to *either* the treatment or control group throughout the sample period.

A few comments are warranted. First, as in Case II, this procedure introduces non-trivial measurement error into the estimation, making this a conservative test as it will tend to under-reject the null of no 'structural break' for any particular observation (Type II error). Second, this method identifies particular observations that are treated for at least some periods (although not the specific periods). Finally, as in Case II, if (DGP3) is the 'true' model, only the intercepts should differ across the periods for which a particular observation is treated and not treated.

Given that (D2) may fail in practice, a second detection method is available based on pooling the sample and estimating the single regression model

$$y_{it} = \tilde{D}_{it}\left[X_{it}\beta_1\right] + \left(1 - \tilde{D}_{it}\right)\left[X_{it}\beta_0\right] + \varepsilon_{it} \tag{10}$$

and testing the null $H_0 : \hat{\beta}_1 = \hat{\beta}_0$. Once again, this procedure introduces measurement error into the estimation, making this a conservative test. Furthermore, as previously stated (see footnote 32), an instrumental variables strategy will not correct this problem, as it will only produce upper bounds (in absolute value) for β_1 and β_0, and improved lower bounds also will not remedy the situation (see footnote 36). Finally, it is worth noting that, as in Case II, a test based on correlation of the errors will not be valid unless consistent point estimates of the parameters of (10) are obtainable.

Distributional Analysis

Up to this point, the analysis has been couched within a standard linear regression framework. Of course this is quite restrictive; more robust evidence of treatment assignment can potentially be gleaned from comparisons of the *distribution* of outcomes of suspected treated and untreated observations. In general terms, then, the underlying DGP may take the following form:

$$F_1\left(y_{it} \middle| X_{it}\right) \neq F_0\left(y_{it} \middle| X_{it}\right) \tag{DGP5}$$

$F_1(\cdot)$ and $F_0(\cdot)$ are the cumulative distribution functions (CDFs) of the outcome for the treatment and control group, respectively. Note that (DGP5) nests all of the previous models (DGP1) – (DGP4).

To formally test for differences in the distributions across the two groups, we utilize a two-sample Kolmogorov-Smirnov (KS) statistic (see, e.g., Abadie 2002). To implement such tests, we once again require an initial guess for D_i or D_{it}, $\tilde{D}_i$ or $\tilde{D}_{it}$, depending on whether or not treatment assignment is assumed static. While measurement error is introduced, the distribution-based tests will remain valid, although conservative, provided $\tilde{D}_i$ $\left(\text{or } \tilde{D}_{it}\right)$ are positively correlated with the true values.

To proceed, let W and V denote two variables, where W (V) represents the outcomes for the hypothesized treatment (control) group. $\{w_i\}_{i=1}^{N_1}$ is a vector of N_1 observations of W; $\{v_i\}_{i=1}^{N_0}$ is an analogous vector of realizations of V. Let $F_1(w)$ and $F_0(v)$ represent the CDF of W and V, respectively. Thus, the null hypothesis of interest – the absence of *either* control or treated observations – is equivalently expressed as H_0: $F_1=F_0$. To test this null hypothesis, we define the empirical CDF for W as

$$\hat{F}_{1N_1}\left(w\right)=\frac{1}{N_1}\sum_{i=1}^{N_1} I\left(W\leq w\right)$$

where $I(\cdot)$ is an indicator function. $\hat{F}_{0N_0}\left(v\right)$ is defined similarly for V. Next, we define the following KS statistic:

$$d^{eq}=\sqrt{\frac{N_0N_1}{N_0+N_1}}\sup\left|F_1-F_0\right| \tag{11}$$

Our test of $F_1 = F_0$ is based on the empirical counterpart of d^{eq} using the empirical CDFs. Specifically, the test requires:

(i) computing the values of $\hat{F}_1\left(z_q\right)$ and $\hat{F}_0\left(z_q\right)$ for where z_q, $q = 1, \ldots, Q$ denotes points in the support Z that are utilized (Q=500 in the application), and

(ii) computing $\hat{d}^{eq}=\sqrt{\frac{N_0N_1}{N_0+N_1}}\max\left\{\left|\hat{F}_1\left(z_q\right)-\hat{F}_0\left(z_q\right)\right|\right\}$.

Inference is conducted using the bootstrap procedure outlined in Abadie (2002). Specifically, we pool the two samples, resample (with replacement) from the combined sample, split the new sample into two samples, where the first N_1 observations are placed in the treatment group and the remainder are placed in the control group, and compute the KS statistic, $\hat{d}^{eq*}$. This process is repeated *B* times, and the p-value is given by

$$p-value = \frac{1}{B}\sum_{b=1}^{B} I\left(\hat{d}^{eq*} > \hat{d}^{eq}\right) \tag{12}$$

The null hypothesis is rejected if the p-value is less than the desired significance level, say 0.10.

To this point *W* and *V* have represented two *unconditional* variables. Yet (DGP5) indicates that it is differences in the *conditional* distributions that matter. Thus, to control for differences in observed attributes, we perform our test of equality on the *residual* distribution of outcomes.[37] To obtain the relevant distributions, we estimate

$$y_{it} = X_{it}\beta_1 + v_{it}^1, \qquad i = 1,\ldots,N \tag{13}$$

by ordinary least squares (OLS) using the hypothesized treatment sample and obtain the distribution of residuals $\hat{v}_{it}^1 = y_{it} - X_{it}\hat{\beta}_1$. One could proceed similarly and obtain the residual distribution, $\hat{v}_{it}^0$, for the control group as well. However, because differences in the coefficients also suggest that observations belong to different regimes – the treatment and control group – as discussed in the context of the exchangeability tests in the previous section, we avoid simply testing for the equality of the distributions of

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[37] A similar strategy is followed in Maasoumi and Millimet (2003), who analyze the distributions of pollution conditional on income at various points in time, and Pesendorfer (2000), who analyzes the distribution of low order cartel and all non-cartel bids conditioned on auction- and time-specific variables to test for anti-competitive bidding.

$\hat{v}_{it}^{k}, k = 0,1$. Instead, we compare the distribution of $\hat{v}_{it}^{1}$ with the distribution of

$\hat{\hat{v}}_{it}^{0} = y_{it} - X_{it}\hat{\beta}_1, i = 1,..., N_0$; it is straightforward to show that

$\hat{\hat{v}}_{it}^{0} = y_{it} - X_{it}\hat{\beta}_1 = \hat{v}_{it}^{0} + X_{it}\left(\hat{\beta}_0 - \hat{\beta}_1\right)$. As a result, differences in $\hat{v}_{it}^{1}$ and $\hat{\hat{v}}_{it}^{0}$ arise due to differences in the coefficients as well as differences in the residuals, and not differences in the observed covariates, thus constituting the proper empirical test of (DGP5).

For these residual tests, inference is conducted using the same bootstrap procedure outlined above. The only difference is that the first-stage regression, (13), is estimated anew during each bootstrap repetition. As discussed in Maasoumi et al. (2004) and Millimet et al. (2004), this (i) accounts for parameter uncertainty in the estimation of the residual distributions, and (ii) maintains the between-sample dependence that arises from a common set of coefficients being used to obtain the residuals $\hat{v}_{it}^{1}$ and $\hat{\hat{v}}_{it}^{0}$ within each bootstrap repetition.

III. Application: Detecting Collusion in Canadian Softwood Timber Auctions

U.S.-Canada Softwood Lumber Dispute

To highlight the economic relevance of the application at hand, we begin by providing a brief historical account of U.S.-Canadian relations regarding timber trade. The U.S.-Canada softwood lumber dispute dates back to at least the 1820s when disputes occurred between New Brunswick and Maine. In the 1840s, the U.S. imposed duties of 20 to 30 percent on imports of Canadian lumber. Since that time the U.S. has repeatedly imposed and dropped duties on softwood lumber imported from Canada. Canada retaliated by placing an export duty on saw logs which was raised and lowered until the Wilson Bill in 1894 removed all U.S. tariffs on lumber. This hiatus of trade restrictions lasted a mere three years. The ups and downs of tariffs and duties on both sides of the border have continued until today.

While the tariff rates on different lumber products have changed over time, the legal issues behind the tariffs have not. The two main underlying issues are (i) the downward pressure on U.S. lumber prices due to imports of Canadian lumber and (ii) the ownership structure of Canadian forests (94 percent are publicly owned, resulting in U.S. accusations of public subsidies; specifically, cutting rights for standing timber on federal lands are priced below fair market value). While the depressing nature of trade on domestic prices benefits U.S. consumers, U.S. lumber producers favor import restrictions and have been fairly effective at garnering such protections. From the Canadian perspective, exports to the U.S. are a primary contributor to the economic strength of its timber industry, as nearly 80 percent of all Canadian timber exports are shipped to the U.S.

During the past two decades, U.S. producers have repeatedly filed countervailing duty (CVD) petitions with the U.S. International Trade Commission (ITC). The most recent dispute began in 2002 when the Coalition for Fair Lumber Imports (CFLI) filed a CVD petition and an anti-dumping (AD) petition. In April 2002, the Department of Commerce (DoC) released a final determination in the subsidy and antidumping case, setting a combined CVD/AD rate of 27.22 percent.[38] Since the DoC's ruling, negotiators have discussed various measures to be undertaken on a province-by-province basis to lift the CVD/AD.

[38] The final determination is subject to ongoing appeals and a final rate for the CVD/AD has yet to be settled.

The proposed solution with respect to British Columbia (BC) – the largest player on the Canadian side – revolves around a market-based system.[39] Under the proposed market-based pricing system, cutting rights on a portion of the federal lands would be auctioned in a first-price sealed bid auction to enable estimation of a simple hedonic function of the form

$$b = X\beta + \varepsilon \tag{14}$$

where b is the winning auction bid, X is a vector of plot attributes, and ε is an error term. After estimating (14), results are to be used to price cutting rights on other public lands not subject to an auction according to

$$P = X\hat{\beta} \tag{15}$$

where $\hat{\beta}$ is the OLS estimate of β in (14). Provided that the initial market signal from the auctions is reliable, the cutting rights on public lands are priced via a robust market mechanism, a necessary condition for lifting the CVD/AD. However, concerns exist that some of the Canadian firms may have colluded (i.e., engaged in bid-rigging), which could invalidate the use of the auction's results to set prices for non-auctioned public lands. Thus, the salient question becomes: Is there evidence that Canadian firms engaged in behavior inconsistent with competitive bidding models when submitting bids in these auctions? Prior to discussing the data, we provide additional details on the auction mechanism itself.

[39] The timber industry in BC employs more than 80,000 people directly and generates shipments exceeding $15 billion annually.

The SBFEP Auction Market

Timber auctions are conducted in BC under the Small Business Forest Enterprise Program (SBFEP). SBFEP timber auctions are publicly advertised by the Ministry of Forests (MoF). These advertisements include the geographic location of the plot, an announced *upset rate* (reserve price), and an estimated net cruise volume (NCV) of the standing timber on the plot. Interested bidders typically conduct an independent evaluation of the plot to determine the accuracy of the MoF estimates. Based upon these evaluations, bidders determine their value of the timber rights and submit a sealed tender indicating a *bonus bid* (a fixed amount per m^3 to be paid in addition to the announced upset rate) to the MoF. At a designated time, the MoF announces the identity of *all* bidders and their bid. Finally, the MoF awards the cutting rights to the highest bidder.

BC is currently using data from SBFEP Category 1 timber auctions that occurred in 1996 – 2000 as the basis for a preliminary testing of their hedonic pricing scheme; thus, we focus on these data.[40] Our objective is to determine if the SBFEP auctions offer a legitimate environment from which to price the timber cutting rights on non-auctioned land. For this to be the case, the auctions should operate as intended: without collusion. Our definition of collusion in this context, then, is either an explicit or implicit arrangement among a group of Category 1 bidders designed to limit competition and increase joint profits. There is some anecdotal evidence to suggest previous attempts at bid-rigging in Category 1 auctions. For instance, according to MoF officials, a Category

[40] SBFEP auctions are subdivided into two types: Category 1 and Category 2. Category 1 auctions are sealed bid tenders by registered market loggers who do not own or lease a processing facility. Category 2 auctions are open to both registered market loggers and registered owners of small "historic" processing facilities. In Category 1 auctions, bidders vie for timber cutting rights in order to sell the harvested timber to end users. In the interior of BC, almost all harvested timber is sold to either major forest license holders or local sawmills. Category 1 bidders typically enter into an agreement in principle to sell the timber to a prospective buyer prior to the auction. The winning bidder then consummates the agreement in principle.

1 bidder was convicted of attempting to rig bids in an SBFEP auction; he was subsequently fined and sentenced to serve jail time.[41] While this particular auction predates our sample, it may signal a more pervasive underlying pattern of behavior amongst loggers.

There a number of characteristics of the SBFEP auction market – similar to those reported in Porter and Zona (1993) for Department of Transportation sealant contracts in Nassau and Suffolk counties – that could help sustain cooperative agreements among bidders.[42] First, the supply and location of SBFEP timber auctions is exogenously determined by the MoF and is relatively insensitive to the price received for cutting rights. By contract, the MoF designates the location (and hence the associated volume, species mix, and quality) of timber that must be removed from any auctioned plot. Second, bidders compete solely on price. Winning bidders supply labor and materials and harvest a pre-determined quantity of timber over some fixed time horizon; output is thereby constant across all potential bidders. The resulting product homogeneity helps to facilitate collusion, as it reduces the dimensions upon which firms must coordinate action (Porter and Zona 1993).

Third, the MoF's policy of publicly announcing the identity of all bidders along with their bid enables firms to perfectly detect deviations from cooperative agreements. This could enable bidders to detect and credibly punish deviations from previously

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[41] A regional MoF employee overheard the convicted bidder attempting to bribe fellow loggers to either withhold bids entirely from an upcoming auction or to submit "phantom" bids below a predetermined price.

[42] It should be noted that there are other characteristics of the SBFEP auction market that serve to deter collusion. For example, the use of an upset rate (reserve price) purportedly set at 70% of the estimated value of an auctioned tract limits the potential gains from bid-rigging. Furthermore, there are a large number of registered loggers (and hence potential bidders) in some districts which may serve to limit the expected profitability of any bidding-ring.

agreed upon bidding strategies. Fourth, the nature of the timber market in BC serves to aggregate information and coordinate behavior, which as the existing theoretical and empirical literature suggests will help sustain anti-competitive outcomes. Insofar as localized bidders interact with the same set of mills, such information aggregation is likely to occur. A bidder thus has a great deal of information on the expected profitability of a plot for competitors, which provides greater incentive and prospects for coordinating actions. Furthermore, the pre-contracting of timber prices with local mills could serve as a coordination mechanism.[43]

Fifth, the restrictions on participation in SBFEP Category 1 auctions (participants must have at least one year's harvesting experience and be registered loggers) ensure that the set of potential competitors in any auction is common knowledge. Furthermore, participation in SBFEP auctions is largely localized, with bidders participating within confined geographic areas. These factors serve to reduce uncertainty over the level of competition in a given market, which could enhance the stability of any cartel arrangements (Porter and Zona 1993). Sixth, the same set of loggers encounter one another in multiple markets, which may serve to limit competition. Repeated contact between competitors in multiple markets (forest districts and plots) has been shown to facilitate collusion by relaxing incentive constraints (Bernheim and Whinston 1990). Finally, the timing and spacing of SBFEP auctions – which tend to be spread out over time – may serve to facilitate collusion. This regularity increases the discounted stream

[43] Intuitively, mills have the incentive to provide low quotes in order to obtain raw materials at minimal cost. If localized bidders contract primarily with the same subset of mills, this would give the mills a degree of monopsonist power. By appropriately adjusting their price quotes, the mills could coordinate bidder behavior around an anti-competitive outcome.

of collusive payouts relative to an optimal deviation since it limits the potential profits a deviating bidder can earn before other cartel members retaliate (Porter and Zona 1993).[44]

Theoretical Background

Prior to discussing the data and the empirical estimates, it is useful to present a simple theoretical framework for examining the impact of collusion. As the empirical methods outlined in the previous section presume either that the sign of the treatment effect is known (Cases I and III) or that the return to exogenous covariates differs amongst collusive and competitive firms (Cases II and IV), the theoretical model provides the basis upon which we derive this information.

Following Bajari and Ye (2003), we begin by defining a bidding strategy for firm i in a particular auction as a mapping, $B_i(\cdot):[\underline{r},\overline{r}]\rightarrow\Re_{+}$, where r_i is the revenue estimate of the project if undertaken by firm i with probability and cumulative distribution functions $g_i(r)$ and $G_i(r)$; $[\underline{r},\overline{r}]$ is the support of r, which is identical for all firms. The auction is a first-price sealed-bid auction: firms submit sealed bids, and the highest bidder is awarded the timber rights at a price equal to the winning bid. As in Bajari and Ye (2003), we assume risk neutrality and that the distributions g_i and G_i are common knowledge, but that the actual revenue estimate, r_i, is known only to firm i. Suppose there exists an increasing equilibrium such that $B_i(\cdot)$ is strictly increasing and differentiable on the support of r_i for all i. It follows, then, that there exists an inverse bid function, $\varphi_i(\cdot)$, that is also strictly increasing and differentiable on the support of the bids,

[44] Although multiple contracts were offered on some days, these contracts were often for smaller plots. Further, even when multiple larger contracts were awarded on a given day, the profitability of winning and holding several such contracts at one time is limited. Capacity constraints and legal obligations binding winning firms to remove a set volume of timber over some pre-determined period imply a substantial opportunity cost of capital utilization.

b_i. Denoting the set of strategies followed by other firms bidding in the auction as B_{-i}, then the probability that firm i wins the auction is

$$P_i(b_i) \equiv \Pr\left[r_j < \varphi_j(b_i)\, \forall j \neq i\right] = \prod_{j \neq i} G_j\left[\varphi_j(b_i)\right].$$

The expected profits for firm i from participating in the auction are given by:

$$E\left[\pi_i; b_i, B_{-i}\right] = (r_i - b_i)\, P_i(b_i) \tag{16}$$

Equation (16) indicates that expected profits for a given firm depend only on its private information.

A competitive bidder derives an optimal bid, b_i^*, to maximize (16) conditional upon own revenues from the project and some probability distribution of the revenues of all competitors. If revenues from the project depend on a vector of project- and/or firm-specific characteristics, Z, then there will be a common relationship between the bid by each firm and these attributes (i.e., $b_i^* = \varphi(Z)$ for all i, where $\varphi(\cdot)$ is some function). Alternatively, if firms (or a subset of firms) are engaged in bid-rigging, it need not be the case that low valuation cartel members submit bids that adhere to these equilibrium conditions. In fact, such bidders are likely to submit 'phantom' bids that are more likely to be correlated and are much less aggressive than non-cartel members (Porter and Zona 1993, 1999; Pesendorfer 1996, 2000; Bajari and Ye 2003). Consequently, based on this prior evidence we would expect that collusion – to the extent that it exists in our application – has a negative impact on bids *ceteris paribus*.[45] Thus, in the notation of the previous section, we assume sign(τ) < 0. Moreover, this framework suggests that the bids

[45] This assumption, and hence our identification strategy, is true in the context of a first-price IPV auction. The interested reader should see Graham and Marshall (1987) who outline a model of collusion in second-price or English auctions and Baldwin et al. (1997) who outline empirical strategies to detect collusion in such settings.

of low valuation cartel members will not follow the same relationship, namely $\varphi(Z)$, as competitive firms, and that the bids of cartel members are likely to be correlated conditional on Z.

The Data

We observe 2,671 SBFEP sealed-bid tender first-price auctions conducted in the interior of BC between January 1996 and December 2000. These auctions provide more than 10,000 individual bids. To generate the data for the empirical model, we combine information from a number of sources. First, a list of all bidders currently registered to participate in SBFEP timber auctions was provided by the MoF in BC. This list was used to generate unique identification codes for each bidder in the data set. Second, the MoF provided the raw bid sheets for each of the 2,671 auctions. The bid sheets provide information on (i) the regional office holding and date of the auction, (ii) the estimated NCV of timber on the plot, (iii) the upset rate for the auction, and (iv) the identity and bonus bid per m^3 for each participant in the auction. Finally, the MoF provided information on the characteristics of each plot and the required deadline to complete the harvest of the specified timber. We were careful to follow the Canadian hedonic specification when choosing auction-specific covariates:

- UPSET RATE: announced reservation price per m^3
- NCV: estimated net cruise volume (divided by 1000)
- VPH: estimated volume of trees per hectare (divided by 1000)
- LNVPT: log of estimated volume per tree
- LSPI: the average selling price index for timber harvested
- DC: deflated development costs (divided by NCV)

- SLOPE: weighted average slope
- BWDN: estimated percent of volume blown down
- BURN: estimated percent of volume burned
- CY: estimated percent of volume to be extracted via cable
- HP: estimated percent of volume to be extracted via helicopter
- HORSE: estimated percent of volume to be extracted via horse
- UTIL: estimated capacity utilization for firm *i* – ratio of current backlog of timber contracts in m^3 to maximum backlog of timber contracts in m_3
- CYCLE: estimated cycle time for harvested timber
- LNB: natural log of the number of bidders.

For the interior region of BC, SBFEP auctions are conducted at 31 regional offices. Table 1 provides the mean bid and upset rate per m^3 by district and auction type in the data set. In determining which bids to employ in the empirical analysis, we eliminated all auctions for which (i) the designation was Category 2, (ii) the NCV of the plot was less than 1,000 m^3, (iii) there were fewer than three bids placed, and (iv) the sale method was other than a sealed-bid tender. This approach is consistent with the criteria the BC government plans to use to estimate the hedonic pricing function given in (14). Furthermore, we eliminate any firm that places only one bid in the sample, as we would be unable to identify the individual fixed effects for any such bidder. These selection criteria result in a sample of 6,353 observed bids placed by 847 different firms.

Summary statistics of the relevant variables are provided in Table 2. The statistics are provided for the full sample of 6,353 bids, as well as by sub-sample depending on whether the bid is assumed to be part of a bid-rigging scheme or not. We utilize two means of classifying bids as collusive or competitive at this point. For purposes of utilizing the methods set forth in Case I and Case III, we postulate an auction-invariant and auction-varying indicator of collusion, respectively.

As a starting point for identifying potentially collusive *firms* in the auction-invariant models, we draw upon previous theoretical and empirical literature identifying conditions likely to foster and sustain collusive outcomes. First, we examine the number of potential competitors in a given market and the frequency with which these competitors bid. It is well understood that both the likelihood of firms entering a collusive arrangement and stability of anti-competitive behavior conditional upon such agreements are directly related to the level of concentration in a given market (Chamberlain 1929; Bain 1951; Dolbear et al. 1968). More recently, authors have considered not only the level of concentration in a given market, but also the frequency with which pairs of firms interact in this market. Benoit and Krishna (1985) and Fudenberg and Maskin (1986) show the importance of repeated interaction between individuals for the stability of trigger strategies and anti-competitive pricing.

Using the theoretical studies outlined above as a guide, we settled on the following set of decision rules for selecting pairs of firms whose bidding behavior warrant further examination: (i) the pair must have jointly submitted bids in at least six auctions during the sample period; (ii) each firm must have won at least one of these auctions; and (iii) there must be a fairly even balance in the pairwise rank of submitted bids between the two firms.[46] Based upon these selection criteria, we classify 130 firms

[46] Our selection criteria implicitly assume that the cartel strategy followed is one whereby all but a single, predetermined cartel member submits "phantom" bids. There are alternate cartel strategies whereby only a single cartel firm participates in a given auction. Such a cartel would go undetected via our selection

(2,617 total bids) as belonging to the treatment group in the auction-invariant specification.

To identify suspected collusive *bids* in the auction-varying models, we denote a bid as collusive if it is placed by a firm suspected of engaging in bid-rigging under the auction-invariant classification in an auction in which at least one other firm also suspected of being engaged in bid-rigging participated. Based upon this criteria, we classify 1,475 total bids (distributed among 127 firms) as belonging to the treatment group in the auction-varying specification.

Lastly, throughout the empirical analysis, we assume that our bid data are drawn from IPV auctions. This is intuitively appealing for these data considering that bidders face different capacity constraints (and possibly possess different information about the composition of species on any given plot), suggesting that idiosyncratic, firm-specific cost factors are more important than plot-specific uncertain costs. Furthermore, bidders contract with different mills to sell harvested timber at some predetermined price. To the extent that these mills face different demand conditions and/or capacity constraints, the price quotes generated will differ. This implies independent and private expectations over revenues across loggers.

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criteria. We argue that such omission would serve to make the Bajari and Ye (2003) identification strategy a conservative indicator of collusion-provided that there is not a systematic selection issue. On the other hand, if there is some unaccounted for deterministic process by which firms organize bids into competitive and collusive auctions, then not only ours, but any identification strategy that relies upon a comparison of estimates from reduced form bid functions across competitive and cartel designations is potentially biased.

Empirical Results

Auction-Invariant Treatment Assignment

Empirical results from a variety of tests assuming that the underlying data are generated according to (DGP1) are presented in Tables 3 – 6. The first set of results is displayed in Table 3. The "level specification" refers to results obtained from estimating equation (1), omitting a constant term. The first specification uses the total bid as the dependent variable. Because there appears to be a selection issue whereby suspected bid-rigging firms tend to participate in auctions with higher upset rates, we utilize a second specification that includes the upset rate as a covariate. The final specification uses the bonus bid as the dependent variable. Assuming that $\text{sgn}(\tau) < 0$, under assumptions (A1) – A(4), a negative and statistically significant firm effect is evidence of collusive behavior by that particular firm. Only one firm effect is negative and statistically significant in the first specification at the $p<0.10$ level; two (15) firm effects are negative and statistically significant in the (second) third specification. While this represents minimal evidence of collusion (at best less than two percent of the sample of firms), assumption (A2) in particular – the absence of a constant term – seems implausible for these data.

Given the restrictiveness of (A2), the final three columns in Table 3 – labeled "deviation specification" – present the results obtained from estimating equation (2). Again, three cases are considered. Now, 247 (226) of the 847 firm effects are negative and statistically significant at the $p < 0.10$ level using the total bid as the dependent variable and not conditioning (conditioning) on the upset rate; 259 firm effects are negative and statistically significant using the bonus bid as the dependent variable. As a result, replacing (A2) with the much less restrictive assumption that $\bar{D}$ lies in the open interval bounded by zero and unity gives rise to much stronger empirical evidence consistent with anti-competitive behavior by some subset of bidders.[47]

Table 4 contains results from the conditional independence tests – requiring only assumption (A1) – using the three prior specifications from Table 3.[48] Across all model specifications, we reject the null of conditional independence for 23.4 to 29.6 percent of all bidder pairs that submit bids in at least four common auctions. For pairs of firms that submit bids in fewer than eight common auctions, these percentages range from 4.1 percent (for firms that jointly participate in four auctions in our model of the bonus bid) to 30.5 percent (for firms that jointly participate in five auctions in our model of the bonus bid). For firms that participate in eight or more common auctions, the fraction of bidder pairs for which we reject conditional independence ranges from 33.3 percent (for the bonus bids of firm pairs with eleven repetitions) to a high of 80 percent (for the total bids of firms with ten repetitions). Thus, relaxing (A2) – (A4) provides empirical evidence consistent with collusive behavior.

Table 5 contains the next set of results, obtained from estimating (DGP1) replacing D_i with $\tilde{D}_i$, where $\tilde{D}_i$ is the auction-invariant classification defined in the previous section and utilized in Table 2. As in Table 3, we estimate three specifications.

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[47] Since our fixed effects estimates are random variables, one would expect approximately five percent of the sample (42 of the estimated individual effects) to be negative and significant at the $p < 0.10$ level in the absence of treatment. However, we observe negative and significant effects for approximately one-third of our sample, providing evidence consistent with some non-negligable portion of bidders receiving treatment.

[48] Note, Bajari and Ye (2003) test for conditional independence after estimating a regression model equivalent to ours except omitting firm specific effects. In other words, the authors test for residual correlation conditional only on the observables, X, in (1). We believe that testing for independence conditional on firm effects and X represents a more conservative test of collusion as correlation between firm pairs that bid together in the same auctions that may otherwise show up in the residuals may instead be captured by the fixed effects (Type II error given the null of no collusion). On the other hand, omission of the fixed effects risks mistaking similar firm unobservables for collusion (Type I error given the null).

In all cases, a negative and statistically significant coefficient on $\tilde{D}_i$ is evidence of collusion. In the two specifications using the total bid as the dependent variable, the coefficient on $\tilde{D}_i$ is positive, and is statistically significant even when the upset rate is excluded from the covariate vector. Yet when we examine the bonus bid in the third specification, the coefficient of $\tilde{D}_i$ is not statistically significant at conventional levels. Nevertheless, the fact that the final specification focusing solely on the bonus bid yields the strongest evidence of collusion is consonant with our suspicion that suspected non-competitive firms tend to participate in higher stakes auctions with larger upset rates.

While the third specification yields a coefficient on $\tilde{D}_i$ of the 'correct' sign if collusion is present in the sample, the estimate is not statistically significant at conventional levels. However, as noted in Section 2, because $\tilde{D}_i$ is measured with error, the coefficient is attenuated toward zero. To (potentially) improve on the estimate of τ, we define a second indicator of collusive firms, $\tilde{D}_i'$ and provide estimates of equation (3) in Table 6. In creating $\tilde{D}_i'$, we draw upon the theoretical literature that examines the effect of multimarket contact on firms' ability to sustain collusion (Bernheim and Whinston 1990; Spagnolo 1999; Matsushima 2001). Formally, these models show how the strategic linking of markets can increase individual firm profits by relaxing incentive constraints that would otherwise limit the ability of firms to sustain cooperative agreements in a repeated game setting. Intuitively, multi-market contact expands the set and severity of credible punishment strategies that deter deviations from a collusive arrangement.

In BC, loggers (Category 1 bidders) interact in the SBFEP auction market and as contract laborers for firms that hold long-term tenure licenses. As such, we would expect that bidders who interact in both of these markets are more likely to collude. Unfortunately, our data do not permit us to identify which bidders contract with any given tenure holder. Instead, we create an indicator variable that proxies for the likelihood that any subset of firms would interact in this labor market and use this proxy as a second potential indicator of collusion.

In creating this proxy, we note that the annual allowable cuts (AAC) for tenure contracts are heavily concentrated across four major firms: Canadian Forest Products Ltd, Slocan Forest Products Ltd, Weyerhaeuser Company Ltd, and West Fraser Mills Ltd. While these firms employ a number of full-time loggers, they also outsource for a large percentage of any harvest labor needs. As such, we hypothesize that bidders located in a district where these firms have a 'large' total AAC would be more likely to engage in the type of multi-market contact that has been shown to facilitate collusion. Making use of GIS tools, we constructed detailed information about the geographic location and AAC volume for tenure holdings of these firms. We were able to identify twelve (out of 31) districts in the interior region of BC where these four firms have combined AACs of over 400,000 m^3.

To generate data on the geographic location of bidders, we combined information from several sources. First, a list of all bidders currently registered to participate in SBFEP timber auctions was provided by the MoF. The registration data contained home localities for approximately two-thirds of the 1,771 bidders in the initial sample. We hand coded the location of all remaining bidders by searching the BC yellow pages (logging firm/companies) or white pages (individual loggers). From these we were able to acquire geographic locations for all but roughly 350 bidders. Making use of GIS tools, we identified 13 cities located in these 12 districts where there were more than 15 registered bidders. Thus, our second indicator is a binary variable that equals one for any firm located in one of these cities; zero otherwise.

In Table 6, the parameter of interest is the coefficient on the variable indicating $\tilde{D}_i = \tilde{D}_i' = 1$. In this case, all three estimates are positive, with the coefficients in the first two specifications gaining statistical significance. These estimates suggest two conclusions. First, there is little evidence of collusion. Second, while utilizing $\tilde{D}_i'$ did improve the lower bounds in the first two specifications, the fact that the estimated treatment effect switched sign in the third specification indicates that the measurement errors associated with $\tilde{D}_i$ and $\tilde{D}_i'$ are correlated even conditional on treatment assignment. This fact precludes the use of the method of moments estimator of Black et al. (2000) to derive consistent point estimates of τ.

In sum, then, if the data are generated according to (DGP1), the methods attempting to estimate τ provide empirical evidence consistent with collusion by some subset of bidders. Specifically, there is compelling evidence of anti-competitive behavior in that a approximately one-third of the firm effects are consistent with collusion in the "deviations specification" in Table 3, and the lower bound (in absolute value) estimate of τ is negative, albeit statistically insignificant, in Table 5 when analyzing the bonus bid using our main hypothesized indicator of anti-competitive firms. Moreover, inference based on the notion of conditional independence strongly suggests the presence of anti-competitive behavior by an economically meaningful sub-sample of firms.

<u>Case II.</u>

Under (DGP2) the only detection algorithm that is available in the current application is the test of exchangeability based on estimation of (5). Since assumption (B2) is not met with our data, tests based on estimation of (4) are not feasible. Results using the total bid as the dependent variable and excluding the upset rate from the conditioning set are presented in Table 7. The primary result of interest is the test for the constancy of the coefficient vector across the samples with $\tilde{D}_i = 1$ and $\tilde{D}_i = 0$ (i.e., suspected colluding and non-colluding firms). Here, we easily reject the null that $\beta_{non\text{-}collude} = \beta_{collude}$ at the p<0.01 confidence level. Moreover, if we condition on the upset rate or utilize the bonus bid as the dependent variable, we continue to reject the null of equal coefficients at the p<0.05 level.[49] Furthermore, the average predicted markup in bids - conditioned on observed covariates - is greater for competitively designated firms which is consistent with an assumption of "phantom" bidding on the part of some subset of cartel bids. Given the theoretical framework used to conceptualize the bidding behavior of firms provided earlier, and assuming assumption (B1) holds, this constitutes further evidence of collusion.

Auction-Varying Treatment Assignment

Under (DGP3) the only test for collusion available is to estimate (7) using $\tilde{D}_{it}$, the auction-varying classification of collusive bids, which was defined previously in the data section and used in Table 2. Results are provided in Table 8 for the three specifications

[49] All results not reported are available from the authors upon request.

used in the earlier tables. In all three specifications, the coefficient on $\tilde{D}_{it}$ is positive and statistically significant. Since measurement error only attenuates the estimates, this provides evidence against the presence of collusion in the data.

To (potentially) reduce the measurement error, Table 9 provides estimates of (8), which utilize two mis-measured indicators of auction-varying collusion. Our second indicator is a binary variable that equals one for any bid placed in an auction held in one of the twelve districts in the interior of British Columbia where Canadian Forest Products Ltd, Slocan Forest Products Ltd, Weyerhaeuser Company Ltd, and West Fraser Mills Ltd have combined AACs of over 400,000 m^3; zero otherwise. Since at least some firms place bids in multiple districts, this indicator is not constant across auctions for all firms.

In Table 9, the parameter of interest corresponds to the coefficient on the variable indicating $\tilde{D}_{it} = \tilde{D}'_{it} = 1$. These estimates again suggest two conclusions. First, there is some, albeit weak, evidence of collusion. The estimated coefficient on our indicator of collusion is negative for our second and third model specifications - with the later estimate statistically significant at the $p < 0.01$ level. Second, the fact that utilizing $\tilde{D}'_{it}$ did not improve the lower bounds reported in Table 8 (and even caused the estimated treatment effects to switch sign for two of the three specifications) indicates that the measurement errors associated with $\tilde{D}_{it}$ and $\tilde{D}'_{it}$ are once again correlated conditional on treatment assignment.

In sum, if the data are generated according to (DGP3), the methods utilized provide only modest evidence of collusion in the data. Nonetheless, there is some evidence of anti-competitive behavior in that the lower bound (in absolute value) estimate of τ is negative in two of our three specifications and statistically significant in a model of bonus bids.

Case IV.

As in Case II, if (DGP4) is assumed to characterize the underlying data, the only detection algorithm that is available in the current application is the test of exchangeability based on estimation of (10). Since assumption (D2) is not met in our data, tests based on estimation of (9) are not feasible. Results using the total bid as the dependent variable and excluding the upset rate from the conditioning set are presented in Table 10. The primary result of interest is the test for the constancy of the coefficient vector across the samples with $\tilde{D}_{it} = 1$ and $\tilde{D}_{it} = 0$ (i.e., suspected colluding and non-colluding bids). Here, we easily reject the null that $\beta_{\text{non-collude}} = \beta_{\text{collude}}$ at the $p<0.01$ confidence level; we continue to reject the null of equal coefficients at the $p<0.01$ level if we condition on the upset rate or utilize the bonus bid as the dependent variable. Furthermore, the predicted mark-ups of suspected collusive bids are lower on average than for the non-collusive sample of bids which is consistent with a model of "phantom" bidding on the part of some subset of cartel bidders. Given the theoretical framework used to conceptualize the bidding behavior of firms provided earlier, and assuming that (D1) holds, this is evidence of collusion, as in Case II.

Distributional Analysis

The final set of results is provided in Table 11. We report the value of d^{eq} and the corresponding p-value associated with the null of equality of the distributions for many comparisons. Panels I and II compare unconditional and residual distributions of the total bid, where the residual distributions in Panel II are obtained by including the upset rate in the conditioning set in equation (13). Panel III examines the unconditional and residual distributions of the bonus bid. In all three panels, we divide the sample into collusive and competitive bids via four methods. First, we compare the distribution of bids by firms with $\tilde{D}_i = 1$ versus firms with $\tilde{D}_i = 0$ (see Figure 1). Second, we compare the distribution of bids by firms with $\tilde{D}_i = \tilde{D}'_i = 1$ versus all other firms (see Figure 2). Third, we compare the distribution of bids for which $\tilde{D}_{it} = 1$ versus bids for which $\tilde{D}_{it} = 0$ (see Figure 3). Finally, we compare the distribution of bids for which $\tilde{D}_{it} = \tilde{D}'_{it} = 1$ versus all other bids (see Figure 4).

The basic results are easily summarized: in 16 of the 20 unique comparisons, we reject the null of equality of the distributions at the $p < 0.05$ confidence level.[50] However, two of the four exceptions are for comparison of residual distributions of the bonus bid using both auction-invariant collusion indicators, $\tilde{D}_i$ and $\tilde{D}'_i$, (p=0.434), and both auction-varying collusion indicators, $\tilde{D}_{it}$ and $\tilde{D}'_{it}$, (p=0.160). On the other hand, when we utilize only our preferred single auction-invariant and auction-varying indicators of collusion, we easily reject the null of equal distributions (p=0.014 and p=0.028, respectively). Moreover, analyzing the figures, especially Figure 1 (bottom panel), reveals that the distribution of suspected collusive bids lies to the left of the corresponding distribution of presumed competitive bids, especially in the upper tail of the distribution. This provides some evidence at the distributional level that 'phantom' bids are present in the data.

[50] The unconditional tests in Panels I and II are identical.

Summation

Given the difficult nature of the problem – identifying treatment assignment when it is unobserved – we pursue several tests based on different underlying structures of the data. Tests based on fixed effects models (Case I), conditional independence (Case I), and exchangeability (Cases II and IV) provide empirical evidence consistent with collusion by some subset of bidders. The distributional tests (Case V) are also in line with these parametric tests, providing further evidence suggestive of collusive behavior. While these tests are perhaps most consonant with the underlying theoretical framework, their validity clearly hinges on the assumption that the assumed DGP is correctly specified. Tests based on estimating the treatment effect and comparing its sign to the assumed negative effect of collusion utilizing mis-measured treatment indicators (Cases I and III) are much weaker. We obtain a mixed set of results – some positive and some negative point estimates – with the negative point estimates being statistically significant in only a single case (although the estimates do represent lower bounds). We conclude from our application that there is reason to question claims that the SBFEP auctions provide an accurate market signal to price timber on other federal lands.

That being the case, we wish to avoid making normative claims concerning the current state of the softwood lumber trade dispute. For example, it is not the purpose of this study to assess whether collusion renders the proposed timber pricing system less than satisfactory. Future work should determine whether the evidence of collusion is sufficient to undermine the integrity of the proposed timber pricing system and assess the performance of this system relative to alternative mechanisms for pricing standing timber.

IV. Conclusion

In light of the increased focus on the cost effectiveness of public policies in both the U.S. and abroad, the practical importance of program evaluation has expanded rapidly. In this spirit, program evaluation based on a potential outcomes framework has matured to the point that it is now considered an important public policy tool alongside more ambitious general equilibrium analyses. In this study we make use of the program evaluation structure, but depart from the traditional approach in order to provide a framework to identify the treatment assignment of observations given data on outcomes. We view this empirical modeling approach as having numerous economic applications. For example, principal-agent problems and detection problems of the sort commonly confronting microeconometricians are included in this broad set of applications.

We showcase our methodology by examining bidder behavior in SBFEP timber auctions in British Columbia for the period 1996 – 2000. Understanding bidder behavior in these auctions is integral to the resolution of the U.S.-Canadian softwood lumber dispute. As part of this resolution, British Columbia is to auction off cutting rights on a portion of its federal land and then estimate a hedonic price function to determine the shadow values of the plot's characteristics. These shadow values are then to be used to price cutting rights on the non-auctioned federal lands. For this solution to lead to an agreement of "changed circumstance" (lifting of the CVD/AD), British Columbia must show that this method provides a viable and robust market. In this sense, it is important that collusion does not undermine the integrity of the auctions. In sum, our findings suggest that the observed bidding patterns do not arise from a model of perfectly competitive bidding.

Table 1. Average Bids by District and Category

District	All Auctions			Category 1 Only			Category 2 Only		
	# Bids	Avg. Total Bid	Upset Rate	# Bids	Avg. Total Bid	Upset Rate	# Bids	Avg. Total Bid	Upset Rate
1	59	$31.27	$27.61	34	$27.20	19.31	21	$37.81	34.16
2	368	43.37	36.03	341	43.48	35.81	18	46.81	44.28
3	494	44.20	33.24	470	44.58	33.29	17	42.78	36.11
4	440	47.75	33.36	402	47.57	32.72	38	49.73	37.30
5	157	57.65	30.81	108	58.77	28.56	49	55.19	33.81
6	225	40.79	25.25	190	41.04	25.73	25	34.30	25.37
7	32	18.71	22.11	22	14.93	14.14	5	23.08	27.44
8	300	20.07	8.89	279	20.25	8.66	14	16.55	11.70
9	176	45.93	40.61	154	46.39	40.65	22	42.78	40.41
10	27	8.67	7.50	13	3.68	4.10	2	2.10	0.25
11	220	51.74	36.66	173	52.40	36.06	22	50.79	38.75
12	341	35.23	18.08	293	32.77	16.71	6	41.37	31.51
13	211	46.48	29.16	139	44.51	26.61	68	50.28	32.71
14	168	43.70	27.01	133	45.32	27.19	31	36.77	26.54
15	268	44.04	28.06	188	45.38	27.07	63	44.15	31.41
16	487	51.59	42.56	434	51.85	43.12	46	50.01	39.67
17	134	28.21	22.29	121	26.73	20.07	10	39.77	35.97
18	225	39.21	21.08	187	39.90	20.43	33	35.90	24.75
19	301	46.23	28.59	255	48.75	29.96	13	35.89	22.61
20	518	34.55	24.19	469	34.32	23.66	43	36.61	26.66
21	357	26.70	21.86	268	26.16	21.56	4	17.02	20.24
22	273	29.69	20.32	250	30.19	21.01	4	26.48	26.87
23	381	35.57	26.66	339	39.48	28.84	11	42.92	40.43
24	251	36.55	26.58	222	36.91	26.06	25	33.12	28.17
25	58	36.15	26.74	58	36.15	26.74	0	-	-
26	977	51.05	40.90	846	50.60	39.14	121	54.61	49.80
27	650	52.85	37.50	578	52.57	37.34	72	55.08	38.22
28	453	38.04	14.02	378	37.56	13.33	75	40.45	17.50
29	482	47.67	37.31	438	47.77	36.61	44	46.66	41.30
30	955	47.63	34.52	713	48.53	35.39	114	43.63	31.35
31	370	30.09	19.37	362	30.00	19.20	6	36.66	25.85
Total		10358			8857			997	

NOTES: Figures in the table represent the total number of bids, average total bid (upset rate plus bonus bid), and average upset rate (reservation price) for each of the 31 regional districts in the interior of British Columbia. Category 1 auctions are limited to loggers; Category 2 auctions are designated for small, historic sawmills.

Table 2. Summary Statistics by Hypothesized Treatment Status

Variable	All Bids	Auction-Invariant Classification		Auction-Varying Classification	
		Control	Treatment	Control	Treatment
Total Bid ($/m^3)	46.416	44.456	49.214	44.922	51.359
	(17.455)	(17.809)	(16.544)	(17.683)	(15.703)
Upset Rate ($/m^3)	33.863	31.844	36.747	32.600	38.041
	(14.923)	(14.982)	(14.356)	(15.033)	(13.755)
Bonus Bid ($/m^3)	12.200	12.256	12.121	11.971	12.957
	(9.540)	(9.822)	(9.124)	(9.608)	(9.277)
UTIL: Firm's Capacity	0.271	0.227	0.334	0.249	0.343
Utilization (%)	(0.342)	(0.345)	(0.327)	(0.346)	(0.320)
Fringe Firm (1 = Yes)	0.225	0.378	0.007	0.292	0.005
	(0.418)	(0.485)	(0.083)	(0.455)	(0.069)
NCV: Net Cruise Volume	8.949	8.710	9.291	8.805	9.427
(1000s m^3)	(6.841)	(7.023)	(6.561)	(7.016)	(6.209)
VPH: Volume of Trees (1000s	0.272	0.271	0.273	0.270	0.277
m^3 per hectare)	(0.131)	(0.43)	(0.112)	(0.137)	(0.110)
LSPI: Average Selling Price	114.884	115.030	114.676	114.766	115.274
Index for Harvested Timber	(17.952)	(18.145)	(17.673)	(18.151)	(17.277)
DC: Development Costs	1.352	1.446	1.217	1.404	1.179
($/NCV)	(2.313)	(2.504)	(2.002)	(2.383)	(2.056)
SLOPE: Weighted Average	15.761	16.674	14.456	16.517	13.260
Slope	(11.813)	(12.979)	(9.770)	(12.556)	(8.460)
LNVPT: Log (Estimated Volume	-0.811	-0.781	-0.854	-0.798	-0.855
Per Tree)	(0.538)	(0.549)	(0.520)	(0.548)	(0.502)
BWDN: Estimated Volume	0.018	0.022	0.013	0.020	0.013
Blown Down (%)	(0.099)	(0.108)	(0.083)	(0.104)	(0.079)
BURN: Estimated Volume	0.011	0.017	0.003	0.014	0.001
Damaged By Fire (%)	(0.102)	(0.127)	(0.048)	(0.116)	(0.010)
CY: Estimated Volume to be	0.056	0.077	0.027	0.069	0.015
Harvested by Cable (%)	(0.207)	(0.241)	(0.140)	(0.228)	(0.100)
HP: Estimated Volume to be	0.013	0.019	0.004	0.017	0.0004
Harvested by Helicopter (%)	(0.108)	(0.132)	(0.058)	(0.113)	(0.011)
HORSE: Estimated Volume to	0.063	0.084	0.034	0.074	0.029
be Harvested by Horse (%)	(0.241)	(0.274)	(0.179)	(0.259)	(0.165)
CYCLE: Estimated Cycle Time	3.915	3.880	3.965	3.906	3.947
for Harvested Timber	(1.790)	(1.843)	(1.709)	(1.854)	(1.558)
LNB: Log (No. of Bidders)	1.726	1.689	1.778	1.681	1.874
	(0.518)	(0.525)	(0.504)	(0.527)	(0.459)
Number of Bids	6353	3736	2617	4878	1475
Number of Firms	847	717	130	720	127

NOTES: Figures represent sample means; standard deviations in parentheses.

Table 3. Auction-Invariant Treatment Model (Fixed Effects Estimation)

	Dependent Variable					
	Level Specification			Deviations Specification		
	Total Bid	**Total Bid**	**Bonus Bid**	**Total Bid**	**Total Bid**	**Bonus Bid**
LSPI	0.252**	0.065**	-0.009	0.252**	0.065**	-0.009
	(0.011)	(0.009)	(0.009)	(0.011)	(0.009)	(0.009)
DC	-0.546**	-0.172**	-0.019	-0.546**	-0.172**	-0.019
	(0.063)	(0.052)	(0.053)	(0.063)	(0.052)	(0.053)
VPH	15.545**	5.889**	1.909	15.545**	5.889**	1.909
	(1.230)	(1.029)	(1.045)	(1.230)	(1.029)	(1.045)
NCV	0.091**	0.018	-0.013	0.091**	0.018	-0.013
	(0.023)	(0.186)	(0.019)	(0.023)	(0.186)	(0.019)
SLOPE	-0.165**	-0.010	0.055**	-0.165**	-0.010	0.055**
	(0.018)	(0.015)	(0.015)	(0.018)	(0.015)	(0.015)
LNVPT	9.587**	3.853**	1.459**	9.587**	3.853**	1.459**
	(0.349)	(0.308)	(0.297)	(0.349)	(0.308)	(0.297)
BWDN	-6.057**	-0.632	1.364	-6.057**	-0.632	1.364
	(1.344)	(1.110)	(1.142)	(1.344)	(1.110)	(1.142)
BURN	-19.492**	-3.133**	3.524**	-19.492**	-3.133**	3.524**
	(1.579)	(1.337)	(1.341)	(1.579)	(1.337)	(1.341)
CY	-11.591**	-5.546**	-3.143**	-11.591**	-5.546**	-3.143**
	(0.919)	(0.765)	(0.781)	(0.919)	(0.765)	(0.781)
HP	-43.146**	-17.667**	-7.258**	-43.146**	-17.667**	-7.258**
	(1.599)	(1.406)	(1.359)	(1.599)	(1.406)	(1.359)
HORSE	-15.543**	-4.866**	-0.486	-15.543**	-4.866**	-0.486
	(0.916)	(0.782)	(0.779)	(0.916)	(0.782)	(0.779)
CYCLE	-2.293**	-0.783**	-0.181*	-2.293**	-0.783**	-0.181*
	(0.091)	(0.080)	(0.077)	(0.091)	(0.080)	(0.077)
LNB	4.119**	5.147**	5.606**	4.119**	5.147**	5.606**
	(0.279)	(0.230)	(0.237)	(0.279)	(0.230)	(0.237)
UTIL	-2.119**	-1.945**	-1.973**	-2.119**	-1.945**	-1.973**
	(0.418)	(0.344)	(0.355)	(0.418)	(0.344)	(0.355)
Control for Upset Rate	No	Yes	No	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6353	6353	6353	6353	6353	6353
# of Firms	847	847	847	847	847	847
Average # of Bids per Firm	7.5	7.5	7.5	7.5	7.5	7.5
# Effects Neg. & Statistically Significant ($p < 0.10$)	1	2	15	247	226	259
R^2	0.606	0.769	0.185	0.726	0.815	0.338

NOTES: Standard errors in parentheses. *(**) indicates significant at the $p < 0.05$ (0.01) level.

Table 4. Percentage of Bidder Pairs Failing Tests of Conditional Independence

	Total Bid (No Upset Rate)	Total Bid (Control for Upset)	Bonus Bid
4 Repetitions	6.9%	6.9%	4.1%
	(5 out of 72)	(5 out of 72)	(3 out of 72)
5 Repetitions	27.8%	22.2%	30.5%
	(10 out of 36)	(8 out of 36)	(11 out of 36)
6 Repetitions	29.0%	16.1%	19.3%
	(9 out of 31)	(5 out of 31)	(6 out of 31)
7 Repetitions	23.1%	23.1%	26.9%
	(6 out of 26)	(6 out of 26)	(7 out of 26)
8 Repetitions	47.6%	42.9%	47.6%
	(10 out of 21)	(9 out of 21)	(10 out of 21)
9 Repetitions	66.7%	58.3%	58.3%
	(8 out of 12)	(7 out of 12)	(7 out of 12)
10 Repetitions	80%	50%	50%
	(8 out of 10)	(5 out of 10)	(5 out of 10)
11 Repetitions	50%	33.3%	33.3%
	(3 out of 6)	(2 out of 6)	(2 out of 6)
12+ Repetitions	66.7%	50%	58.3%
	(8 out of 12)	(6 out of 12)	(7 out of 12)
All Firm Pairs	29.6%	23.4%	25.7%
	(67 out of 226)	(53 out of 226)	(58 out of 226)

NOTES: Cell entries report the percentage of firm pairings that submit bids which fail to demonstrate conditional independence at the $p < 0.05$ level of significance. Tests for conditional independence use the Fisher transformation on the estimated residuals from the fixed effects auction invariant treatment model (Table 3). For example, 4.1% of the unique bidder pairs that compete against one another in four auctions submit bonus bids that are not conditionally independent.

Table 5. Auction-Invariant Treatment Model (Random Effects Estimation): Single Hypothesized Treatment Indicator

	Dependent Variable		
	Total Bid	**Total Bid**	**Bonus Bid**
Constant	22.371**	5.492**	-1.176
	(1.462)	(1.188)	(1.187)
LNB	4.448**	5.593**	6.011**
	(0.265)	(0.214)	(0.219)
UTIL	-1.806**	-1.617**	-1.644**
	(0.397)	(0.319)	(0.329)
Control for Fringe Firms	-1.480**	-0.953**	-0.750*
	(0.572)	(0.402)	(0.404)
Collude (1 = Yes)	1.617**	0.021	-0.465
	(0.711)	(0.481)	(0.479)
Plot Characteristics	Yes	Yes	Yes
Upset Rate	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes
Total Observations	6353	6353	6353
Average # of Bids per Firm	7.5	7.5	7.5
R^2	0.623	0.774	0.207

NOTES: See Table 3.

Table 6. Auction-Invariant Treatment Model (Random Effects Estimation): Two Hypothesized Treatment Indicators

	Dependent Variable		
	Total Bid	**Total Bid**	**Bonus Bid**
Constant	21.591**	5.599**	-0.749
	(1.489)	(1.206)	(1.210)
LNB	4.468**	5.590**	6.001**
	(0.264)	(0.214)	(0.220)
UTIL	-1.842**	-1.627**	-1.637**
	(0.397)	(0.320)	(0.329)
Control for Fringe Firms	-1.331**	-0.973*	-0.832*
	(0.572)	(0.405)	(0.407)
Collude1 = 1, Collude2 = 0	0.202	-0.765	-1.054
	(0.887)	(0.603)	(0.601)
Collude1 = 0, Collude2 = 1	1.506*	-0.263	-0.930
	(0.675)	(0.481)	(0.482)
Collude1 = 1, Collude2 = 1	4.459**	0.849	-0.356
	(0.976)	(0.665)	(0.660)
Plot Characteristics	Yes	Yes	Yes
Upset Rate	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes
Total Observations	6353	6353	6353
Average # of Bids per Firm	7.5	7.5	7.5
R^2	0.628	0.775	0.207

NOTES: See Table 3 and text for further details.

Table 7. Auction-Invariant Treatment Model (Random Effects Estimation): Exchangeability Test

	Sample		
	All Firms	Non-Colluding Firms	Colluding Firms
Constant	22.986**	23.058**	20.521**
	(1.438)	(1.837)	(2.275)
LSPI	0.275**	0.269**	0.292**
	(0.009)	(0.013)	(0.015)
DC	-0.658**	-0.746**	-0.474**
	(0.059)	(0.071)	(0.105)
VPH	16.324**	13.309**	25.635**
	(1.183)	(1.364)	(2.341)
NCV	0.103**	0.107**	0.082**
	(0.021)	(0.027)	(0.035)
SLOPE	-0.163**	-0.117**	-0.234**
	(0.017)	(0.022)	(0.027)
LNVPT	8.462**	7.532*	8.656**
	(0.325)	(0.418)	(0.517)
BWDN	-7.610**	-5.504*	-13.683**
	(1.285)	(1.525)	(2.381)
BURN	-20.316**	-19.545**	-23.626**
	(1.376)	(1.480)	(3.918)
CY	-12.867**	-12.846**	-14.432**
	(0.854)	(1.011)	(1.666)
HP	-43.804**	-43.094**	-43.586**
	(1.350)	(1.499)	(3.316)
HORSE	-16.407**	-15.294**	-16.973**
	(0.749)	(0.896)	(1.327)
CYCLE	-2.350**	-2.187**	-2.662**
	(0.084)	(0.110)	(0.129)
LNB	4.447**	4.089**	4.995**
	(0.265)	(0.346)	(0.410)
UTIL	-1.770**	-1.700**	-2.200**
	(0.397)	(0.519)	(0.616)
Control for Fringe Firms	-2.039**	-1.578**	-1.999
	(0.517)	(0.599)	(2.739)
Year Fixed Effects	Yes	Yes	Yes
Observations	6353	3736	2617
Number of Firms	847	717	130
Average Bids per Firm	7.5	5.2	20.1
R^2	0.620	0.622	0.620
Predicted Markup: $\left. x\hat{\beta}+\hat{u} \middle/ upset \right.$	2.953	3.605	2.009
H_0: $\beta_{\text{non-collude}} = \beta_{\text{collude}}$		p = 0.00	

NOTES: Dependent variable is total bid. When the upset rate is included as a regressor, the p-value for the test of exchangeability is p=0.02. When the bonus bid is the dependent variable, the p-value is p=0.02 as well. See Table 3 for further details.

Table 8. Auction-Varying Treatment Model (Random Effects Estimation): Single Hypothesized Treatment Indicator

	Dependent Variable		
	Total Bid	**Total Bid**	**Bonus Bid**
Constant	23.076**	5.581**	-1.313
	(1.436)	(1.173)	(1.171)
LNB	4.299**	5.473**	5.905**
	(0.267)	(0.216)	(0.221)
UTIL	-1.822**	-1.667**	-1.706**
	(0.396)	(0.319)	(0.329)
Control for Fringe Firms	-1.748**	-0.719**	-0.366
	(0.521)	(0.369)	(0.372)
Collude (1 = Yes)	1.576**	1.283**	1.149**
	(0.386)	(0.309)	(0.318)
Plot Characteristics	Yes	Yes	Yes
Upset Rate	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes
Total Observations	6353	6353	6353
Average # of Bids per Firm	7.5	7.5	7.5
R^2	0.623	0.775	0.205

NOTES: See Table 3 and text for further details.

Table 9. Auction-Varying Treatment Model (Random Effects Estimation): Two Hypothesized Treatment Indicators

	Dependent Variable		
	Total Bid	**Total Bid**	**Bonus Bid**
Constant	20.456**	6.210**	0.812
	(1.459)	(1.188)	(1.192)
LNB	4.278**	5.515**	5.944**
	(0.266)	(0.216)	(0.220)
UTIL	-1.832**	-1.657**	-1.685**
	(0.394)	(0.319)	(0.327)
Control for Fringe Firms	-1.668**	-0.730*	-0.428
	(0.509)	(0.369)	(0.370)
Collude1 = 1, Collude2 = 0	1.961**	1.519**	1.329**
	(0.601)	(0.484)	(0.495)
Collude1 = 0, Collude2 = 1	3.139**	-1.220**	-2.649**
	(0.375)	(0.306)	(0.301)
Collude1 = 1, Collude2 = 1	4.323**	-0.008	-1.432**
	(0.537)	(0.434)	(0.435)
Plot Characteristics	Yes	Yes	Yes
Upset Rate	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes
Total Observations	6353	6353	6353
Average # of Bids per Firm	7.5	7.5	7.5
R^2	0.632	0.776	0.220

NOTES: See Table 3 and text for further details.

Table 10. Auction-Varying Treatment Model (Random Effects Estimation): Exchangeability Tests

	Sample		
	All Bids	Non- Colluding Bids	Colluding Bids
Constant	22.986**	23.162**	19.942**
	(1.438)	(1.608)	(3.144)
LSPI	0.275**	0.277**	0.282**
	(0.009)	(0.011)	(0.020)
DC	-0.658**	-0.706**	-0.545**
	(0.059)	(0.066)	(0.130)
VPH	16.324**	15.768**	20.411**
	(1.183)	(1.297)	(2.945)
NCV	0.103**	0.095**	0.168**
	(0.021)	(0.024)	(0.047)
SLOPE	-0.163**	-0.156**	-0.179**
	(0.017)	(0.019)	(0.037)
LNVPT	8.462**	7.753**	8.942**
	(0.325)	(0.375)	(0.646)
BWDN	-7.610**	-6.942**	-12.862**
	(1.285)	(1.415)	(3.164)
BURN	-20.316**	-20.153**	-45.255*
	(1.376)	(1.406)	(23.897)
CY	-12.867**	-12.861**	-13.208**
	(0.854)	(0.918)	(2.746)
HP	-43.804**	-43.673**	-69.830**
	(1.350)	(1.392)	(20.737)
HORSE	-16.407**	-16.155**	-15.937**
	(0.749)	(0.808)	(1.909)
CYCLE	-2.350**	-2.351**	-2.418**
	(0.084)	(0.095)	(0.176)
LNB	4.447**	4.145**	5.350**
	(0.265)	(0.304)	(0.571)
UTIL	-1.770**	-1.493**	-3.094**
	(0.397)	(0.462)	(0.782)
Control for Fringe Firms	-2.039**	-1.806**	-0.908
	(0.517)	(0.532)	(4.110)
Year Fixed Effects	Yes	Yes	Yes
Total Observations	6353	4878	1475
Number of Firms	847	846	127
Average Bids per Firm	7.5	5.8	11.6
R^2	0.620	0.616	0.631
Predicted Markup: $x\hat{\beta}+\hat{u}\big/ upset$	2.953	3.313	1.754
H_0: $\beta_{non\text{-}collude} = \beta_{collude}$		p=0.00	

NOTES: Dependent variable is total bid. When the upset rate is included as a regressor, the p-value for the test of exchangeability is p=0.00. When the bonus bid is the dependent variable, the p-value is p=0.00 as well. See Table 3 for further details.

Table 11. Distribution-Based Treatment Model: Tests of Equality of Bid Distributions

Treatment Definition	Unconditional Distributions		Residual Distributions	
	d^{eq}	p-value	d^{eq}	p-value
I. Total Bid				
Collude = 1 (Auction-Invariant Indicator)	4.245	0.000	2.612	0.000
Collude1 = 1, Collude2 = 1 (Auction-Invariant Indicators)	3.665	0.000	4.410	0.000
Collude = 1 (Auction-Varying Indicator)	5.301	0.000	2.565	0.000
Collude1 = 1, Collude2 = 1 (Auction-Varying Indicators)	6.109	0.000	2.913	0.000
II. Total Bid				
Collude = 1 (Auction-Invariant Indicator)	4.245	0.000	1.028	0.104
Collude1 = 1, Collude2 = 1 (Auction-Invariant Indicators)	3.665	0.000	1.850	0.008
Collude = 1 (Auction-Varying Indicator)	5.301	0.000	2.561	0.006
Collude1 = 1, Collude2 = 1 (Auction-Varying Indicators)	6.109	0.000	1.932	0.028
III. Bonus Bid				
Collude = 1 (Auction-Invariant Indicator)	1.231	0.036	1.326	0.014
Collude1 = 1, Collude2 = 1 (Auction-Invariant Indicators)	0.946	0.394	0.934	0.434
Collude = 1 (Auction-Varying Indicator)	2.234	0.000	2.615	0.028
Collude1 = 1, Collude2 = 1 (Auction-Varying Indicators)	2.496	0.000	1.574	0.160

NOTES: Residual distributions condition on the same set of control variables as in Tables 3 – 9, where Panel I (II) does not (does) condition on the upset rate as well.

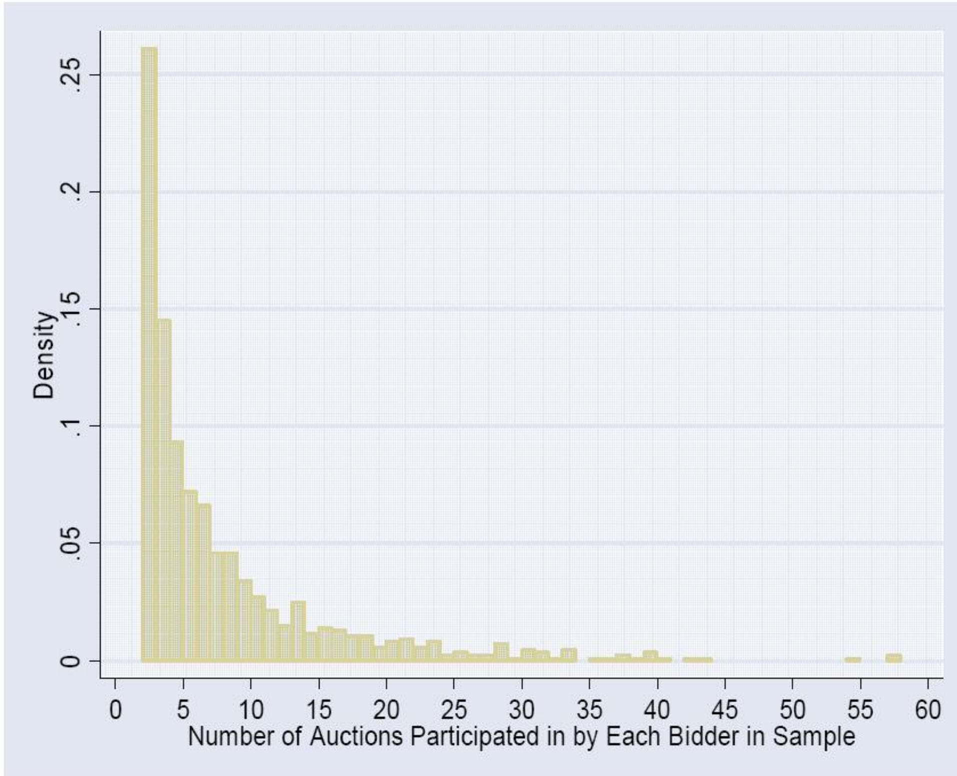


Figure 1. Distribution of Number of Bids Placed by Each Bidder in Sample.

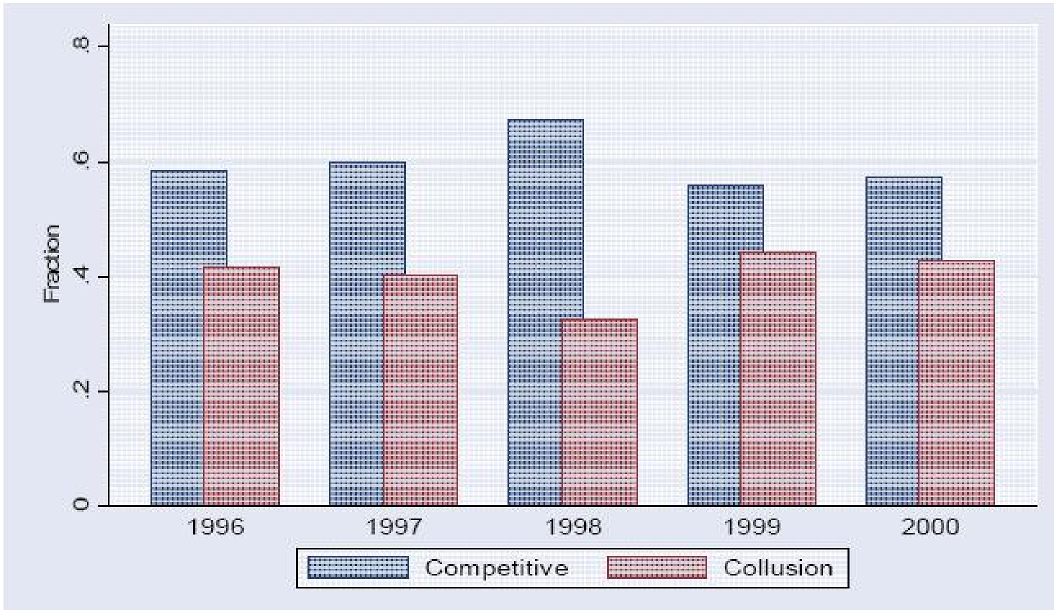


Figure 2. Fraction of Observed Bids Suspected of Being Anti-Competitive by Year.
NOTES: Status of bids as competitive or collusive is determined by sign and significance of bidder fixed effects obtained from Table 3, "deviations specification," using the bonus bid as the dependent variable.

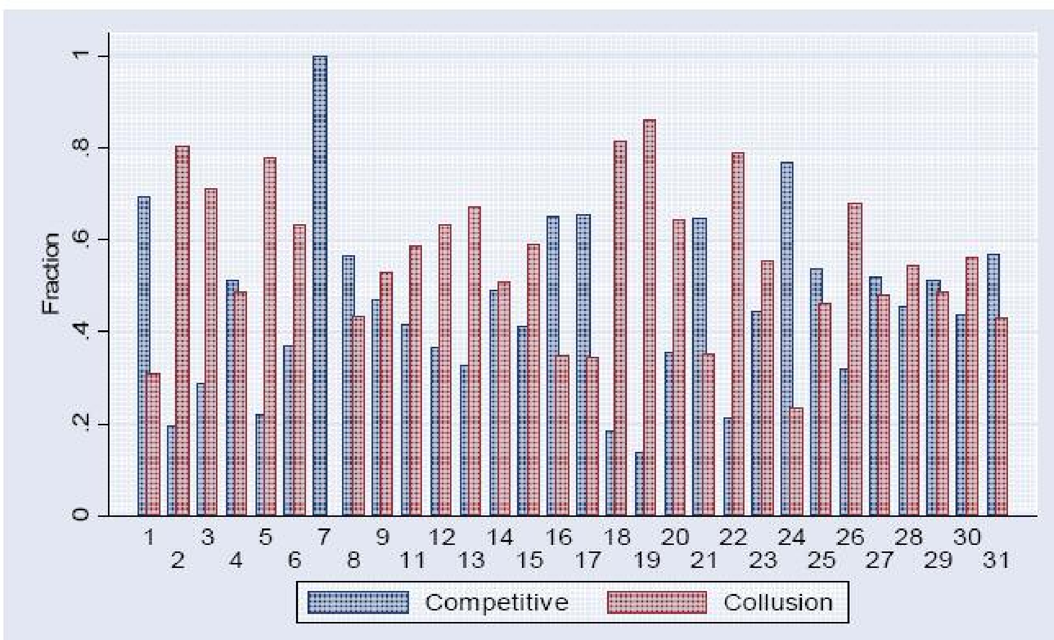


Figure 3. Fraction of Observed Bids Suspected of Being Anti-Competitive by District.
NOTES: Status of bids as competitive or collusive is determined by sign and significance of bidder fixed effects obtained from Table 3, "deviations specification," using the bonus bid as the dependent variable.

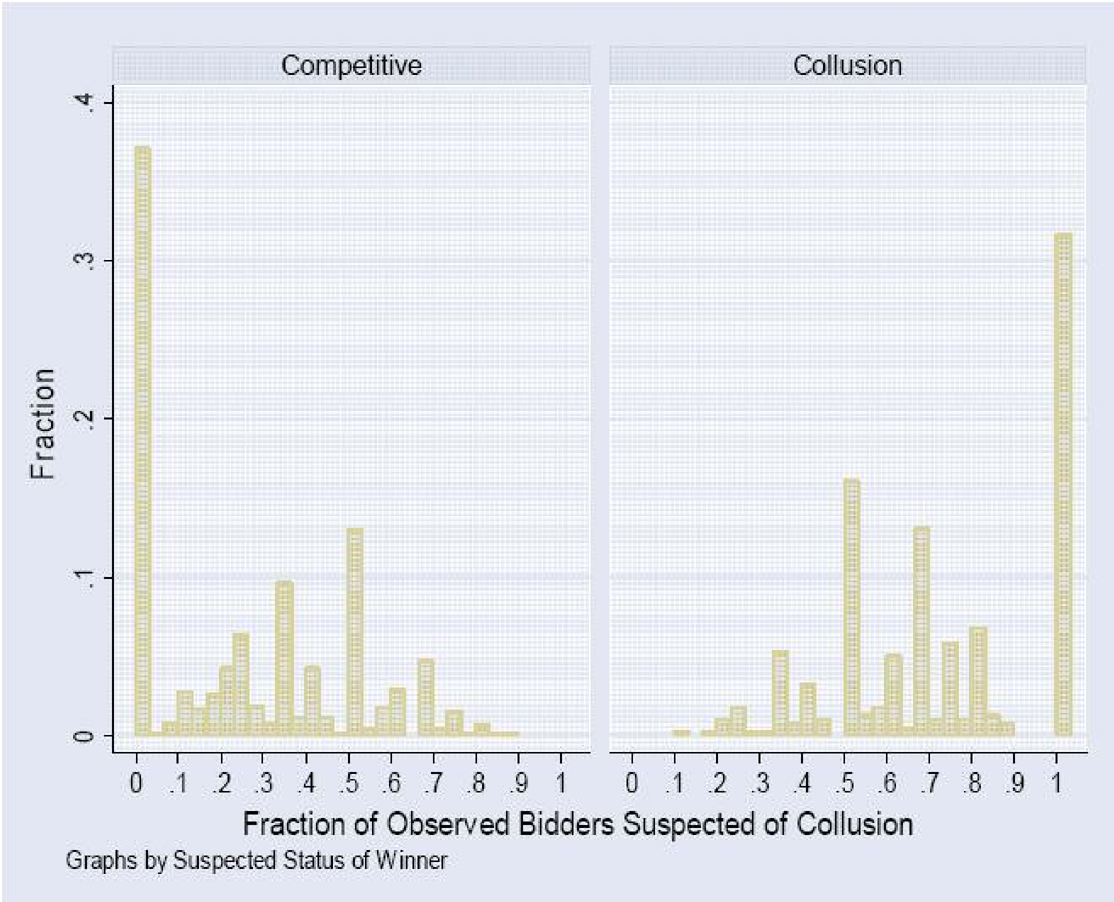
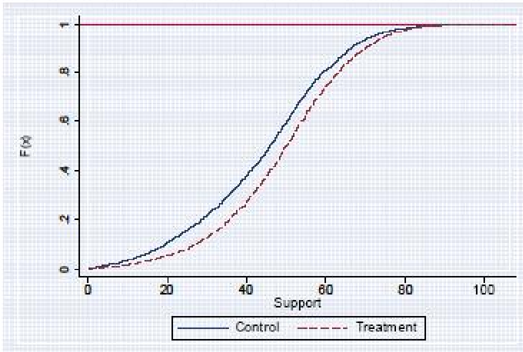
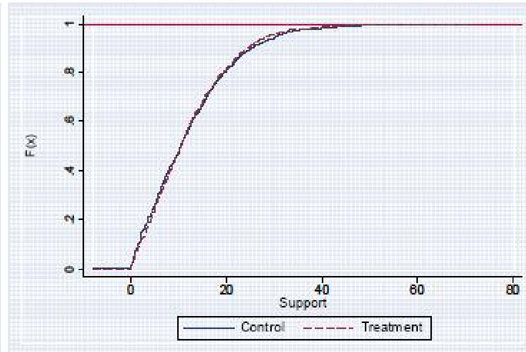


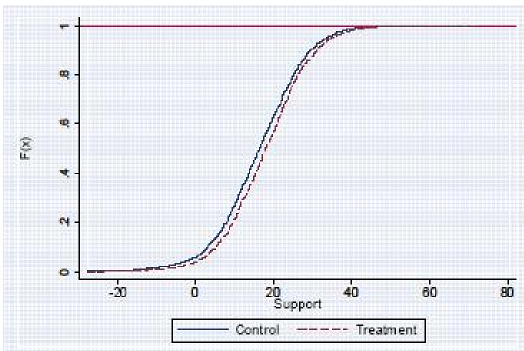
Figure 4. Fraction of Observed Bidders in each Auction Suspected of Collusion.
NOTES: Status of winning and losing bidders determined by sign and significance of bidder fixed effects obtained from Table 3, "deviations specification," using the bonus bid as the dependent variable.



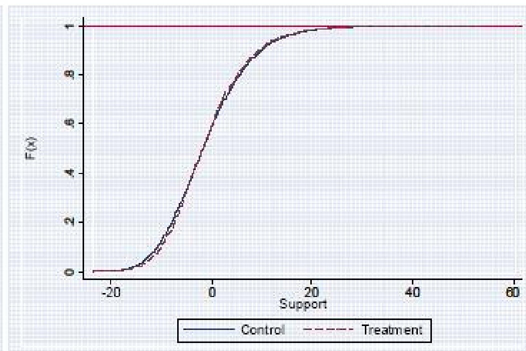
Total Bid (Unconditional)



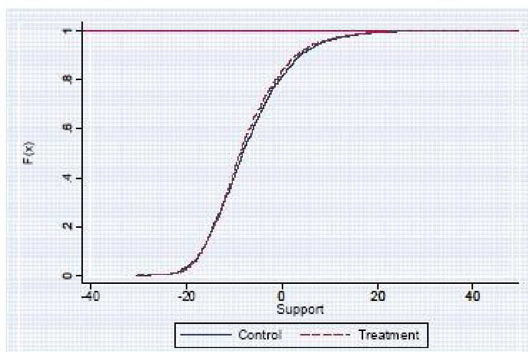
Bonus Bid (Unconditional)



Total Bid I (Residual)



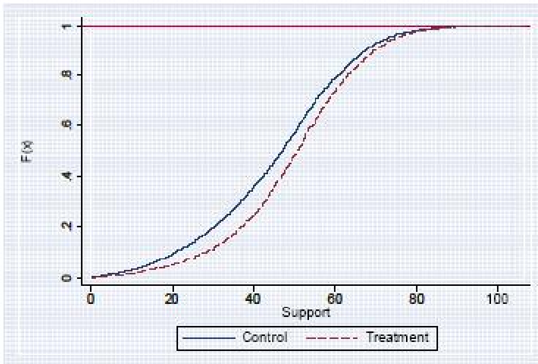
Total Bid II (Residual)



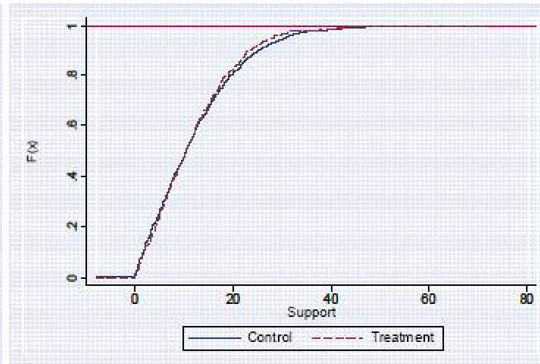
Bonus Bid (Residual)

Figure 5. CDFs of Bids Differentiated by Single Hypothesized Auction-Invariant Treatment Indicator.

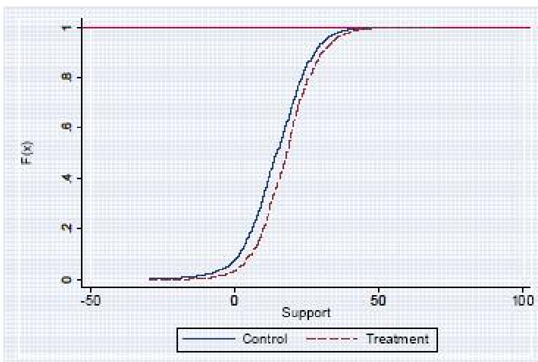
NOTES: Treatment (Control) group includes firms hypothesized to be (not be) colluding. Total Bid I (II) does not (does) condition on the upset rate. See text for further details.



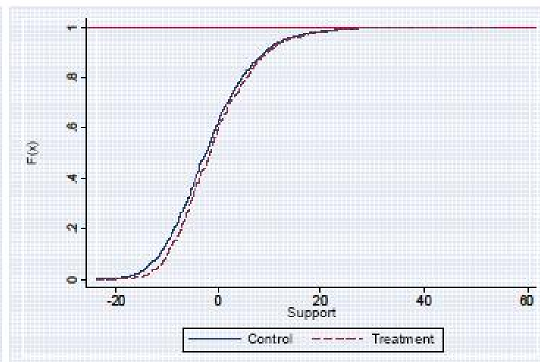
Total Bid (Unconditional)



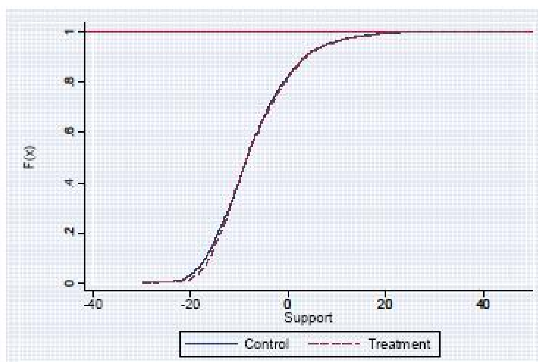
Bonus Bid (Unconditional)



Total Bid I (Residual)



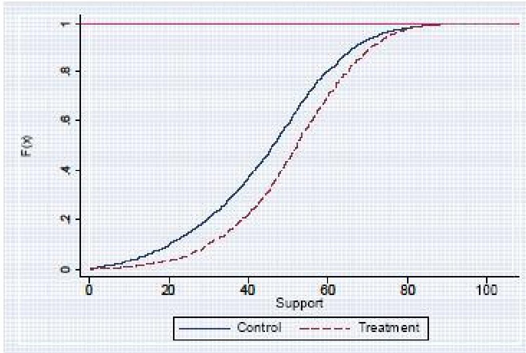
Total Bid II (Residual)



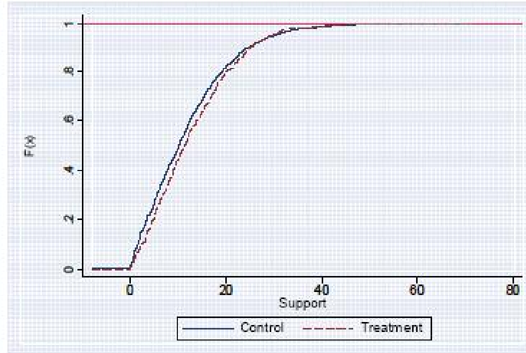
Bonus Bid (Residual)

Figure 6. CDFs of Bids Differentiated by Two Hypothesized Auction-Invariant Treatment Indicators.

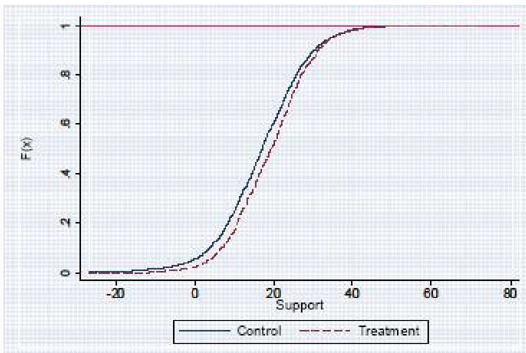
NOTES: Treatment group includes firms hypothesized to be colluding according to both indicators; control group includes all remaining firms. See Figure 5 and text for further details.



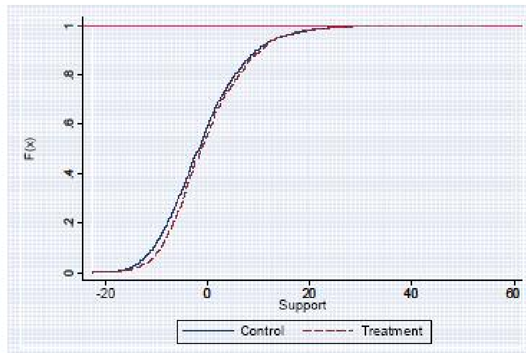
Total Bid (Unconditional)



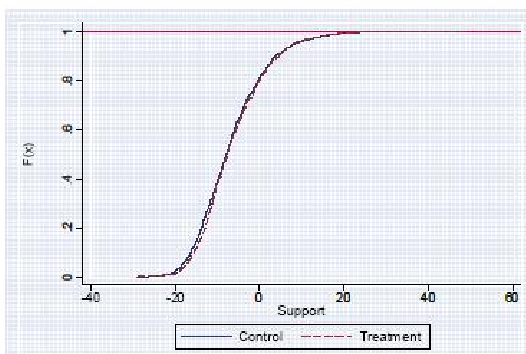
Bonus Bid (Unconditional)



Total Bid I (Residual)



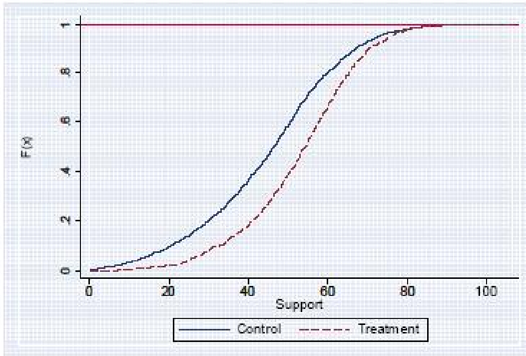
Total Bid II (Residual)



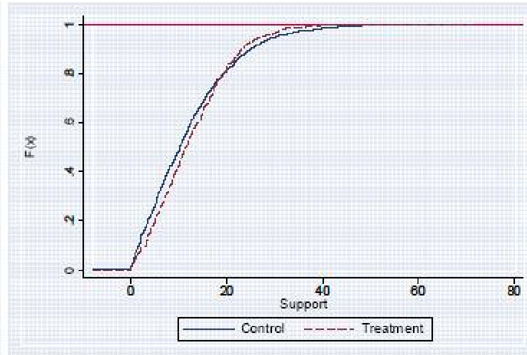
Bonus Bid (Residual)

Figure 7. CDFs of Bids Differentiated by Single Hypothesized Auction-Varying Treatment Indicator.

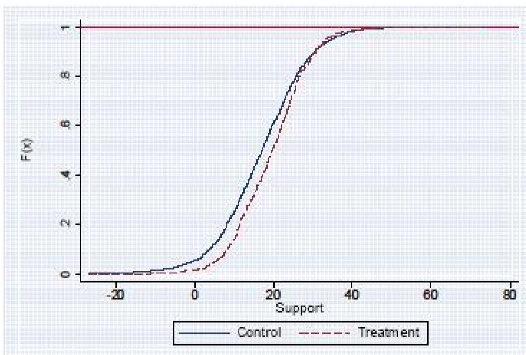
NOTES: Treatment (Control) group includes bids hypothesized to be (not be) made under collusion. See Figure 5 and text for further details.



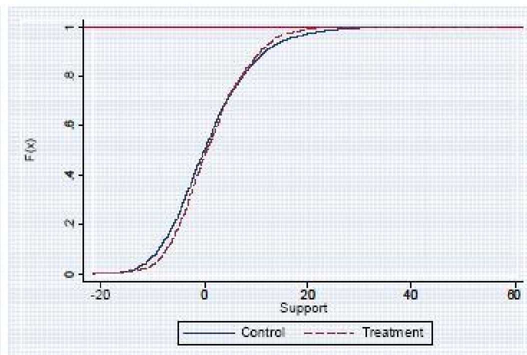
Total Bid (Unconditional)



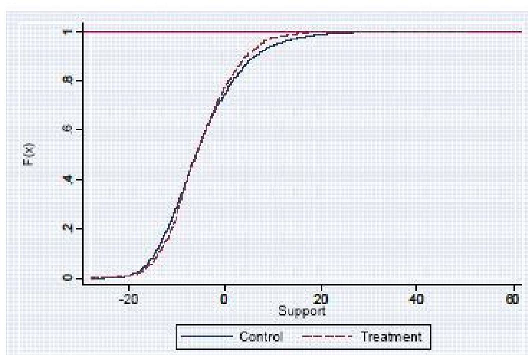
Bonus Bid (Unconditional)



Total Bid I (Residual)



Total Bid II (Residual)



Bonus Bid (Residual)

Figure 8. CDFs of Bids Differentiated by Two Hypothesized Auction-Varying Treatment Indicators.

NOTES: Treatment group includes bids hypothesized to be made under collusion according to both indicators; control group includes all remaining bids. See Figure 5 and text for further details.

Chapter 3:

Auctions with Resale When Private Values are Uncertain:

Theory and Empirical Evidence[51]

I. Introduction

Auctions have a long and storied past. From the human slave auctions carried out in ancient Egypt to the marriage auctions for brides in Asia Minor to the Praetorian Guard auctioning off the Roman Empire in A.D. 193, auctions have been used to allocate goods and services. While auctions have certainly served an important purpose throughout history and are now used to sell almost anything one can imagine – vintage wines, Treasury bills, pollution permits, baseball cards, etc. – economists have only recently begun to explore rigorously the theoretical underpinnings of various auction formats. The seminal work is due to Vickrey, who made several contributions – deriving the Nash equilibrium bidding strategy for first-price auctions, demonstrating revenue equivalence, and proposing the second-price auction as strategically equivalent to the English auction – in his 1961 study.[52]

An extensive literature examining the optimal design and application of auctions has since developed. Our point of departure in this study is to relax the maintained assumption that individual valuations are known with certainty at the time of the first-

[51] This essay was written with Andreas Lange and John List. Thanks to Bill Howard who provided the Canadian timber auction data. Also, numerous discussions with several Canadian officials, including, but not limited to, Bruce McRae, Michael Stone, and Bill Howard, considerably enhanced this paper. Larry Ausubel, Peter Cramton, Glenn Harrison, Liesl Koch, and Tigran Melkonyan provided useful suggestions during the discovery process. Seminar participants at several universities provided comments that improved the manuscript.

[52] This third contribution has recently been called into question by Lucking-Reiley (2000), who argues that stamp auctioneers were using second price auctions some 65 years before Vickrey's seminal work.

price sealed bid auction.[53] By relaxing the assumption of known use values and allowing secondary (resale) markets, we find ourselves in an environment that is quite common in practice. U.S. Forest Service timber auctions, the procurement of governmental contracts, estate auctions, art auctions, FCC auctions and the like all fit in this general class of allocation mechanisms.[54] Unlike the traditional auction literature that assumes independent private values (IPV) that are known with certainty, when bidders have *ex ante* uncertainty about independent private values and anticipate resale opportunities, equilibrium bidding strategies are dependent upon option values conveyed from the secondary market. Intuitively, bidder behavior in this case is fundamentally linked to the existence and structure of potential resale markets.

Our study attempts to make both theoretical and empirical advances in this area. Theoretically, we advance Haile (2001, 2003) by relaxing the maintained assumption of risk-neutral preferences. With known valuations in the context of a symmetric, IPV first price auction, it is well documented that risk-averse agents will submit bids that first-order stochastically dominate those of risk-neutral counterparts. In the context of a symmetric, common-value auction, it is well documented that risk-averse agents submit bids that are first-order stochastically dominated by a risk-neutral counterpart. Since the auction markets considered herein contain features of both common and independent

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[53] It is well established in the literature that when bidders receive multi-dimensional or uncertain signals, auctions may generate inefficient allocations (Pesendorfer and Swinkels (2000), Jehiel and Moldovanu (2001), Goeree and Offerman (2003)). Efficiency and bidding strategies in such an environment are dependent upon the weight individual bidders assign to both the private and common value components of a signal and upon the number of participants in a given market. However, this literature has not considered the effects of secondary markets.

[54] There is a growing theoretical literature that examines the impacts of such resale opportunities on bidder behavior and a seller's optimal choice of auction format (see, for example, Bikhchandani and Huang (1989), Gupta and Lebrun (1999), Haile (2000, 2001, 2003), Troger (2003), and Garratt and Troger (2003)).

private values, we are a priori unable to predict the effects of risk aversion on bidder behavior without first developing an extension of extant theory. By allowing symmetric agents with CARA preferences, we derive several testable implications.

Our main empirical objectives are to (i) evaluate the validity of our theoretical model of auctions with resale, and (ii) provide empirical evidence of behavior in such markets that can aid in the design and implementation of efficient mechanisms for the allocation of goods and services. To achieve these objectives, we combine insights from naturally occurring data with insights gained from a controlled laboratory experiment. One benefit of our approach is that it enables a comparison of behavior across two different environments with varying levels of control and realism.

Our naturally occurring data are drawn from nearly 3,000 timber auctions (over 10,000 individual bids) from the Small Business Forest Enterprise Program (SBFEP) for the interior region of British Columbia (BC) for the period 1996-2000. These data can be viewed as extending the empirical findings in Haile (2001), who used U.S. timber auction data to explore bidding behavior before and after a federal regulation that allowed resale. Unlike his temporal identification strategy, our identification rests on static comparisons between bidding patterns of two very different bidder groups: loggers and mills located in the BC interior. While we find evidence consonant with our theoretical predictions and in line with Haile's (2001) findings, we are cautious to make strong inference because exact comparisons cannot be unequivocally made. As in Haile's (2001) study, where several identification assumptions are necessarily imposed, in our case variations in the underlying valuations, risk posture, and structure (nature) of secondary markets are largely unobserved and therefore may frustrate appropriate inference. This fact highlights the difficulty of evaluating the impacts of resale on bidder behavior using uncontrolled field data.

One way to approach this quandary is to make use of a laboratory experiment. By studying artificial markets that differ only in whether a secondary market is available, we are permitted a unique insight into whether the resale market by itself can lead to such predicted consequences. Experimental methods thus allow us to study the effects of resale possibilities that would be difficult to identify in naturally occurring data. Keeping an eye toward designing a laboratory setting that resembles naturally occurring markets while maintaining a strong theoretical link, we designed an experiment using the first-price auction with both a second stage optimal auction (OA) as well as an English auction (EA) continuation game of complete information. This particular design choice allows a controlled test of existing theory and a useful benchmark for making inference from field data, since the division of surplus on the secondary market in BC most likely lies within these two market extremes.

The lab results are broadly in line with theoretical expectations. We find that experimental subjects submit bids that are significantly higher in markets with resale organized by an optimal auction than in those without such opportunity (or with secondary markets organized by an EA). An interesting data pattern not anticipated by extant theory is that over lower ranges of the signal space, realized bids are less than the risk-neutral theoretical predictions, while over higher ranges of the signal space, realized bids are greater than the risk-neutral theoretical predictions. Yet these tendencies are consonant with our theory of bidding by agents with CARA preferences.

The remainder of the paper is crafted as follows. Section II provides an overview of the SBFEP auction market and our strategy for identifying resale differences using reduced-form bid functions. Section III develops a theory of bidding by agents with CARA preferences in auction markets that parallel our laboratory setting. Section IV discusses the laboratory experiment and results. Section V concludes.

II. The SBFEP Auction Market

The SBFEP Auction – Background and Predictions

Our naturally occurring data are drawn from nearly 3,000 timber auctions (over 12,000 individual bids) from the British Columbia SBFEP for the period 1996-2000 – the identical data set that BC is using to begin its new pricing approach under the changed circumstance agreement for the U.S.-Canadian softwood lumber dispute.[55] To examine the effects of ex post resale opportunities on bidder behavior, we compare reduced-form bid functions across distinct subsets of bidders that face different market conditions.

SBFEP auctions in BC allocate standing timber of less than 50,000 metric board feet cubed (m^3) to small logging companies and contractors. SBFEP timber sales account for approximately 13 percent of the harvested timber in the province. About half of this timber is allocated via sealed bid tenders to the highest bidder under section 20 of the province's Forest Act. These auctions are subdivided into two types: Category 1 and Category 2, where Category 1 auctions include only market loggers. Category 2 auctions are open to both registered market loggers and registered owners of processing facilities.

Category 1 bidders purchase timber cutting rights and sell harvested timber to end users. In the interior of BC almost all harvested timber is sold to either major forest

[55] See Price and List (2004) for a discussion of the solution to the trade dispute.

license holders or local sawmills. *Ex ante*, bidders contract with a prospective buyer to arrange an agreement in principle to sell/buy if they win the auction. The bidders then submit bids and the winner consummates the agreement in principle and chooses to lock in the stumpage price he bid. Category 2 bidders purchase timber cutting rights to obtain raw materials for their processing operations. Bidders either process harvested timber or trade it to obtain needed materials. Since processing facilities are actively engaged in the *ex post* buying and selling of harvested logs whereas loggers in the interior contract *ex ante* to deliver all harvest to a given buyer, we believe that resale might enter into the bidding strategies of the former but not the latter. Intuitively, since mills have an outside option to sell logs on a spot market whereas loggers do not have such an option, one would expect that processing facilities would provide an upper envelope on the observed bids of loggers from the interior. To identify whether this effect holds, we rely upon cross sectional variation.[56]

Identifying Resale Effects from Reduced-Form Bid Functions

We define a bidding strategy for firm *i* as a mapping, $B_i(\cdot):[\underline{t},\bar{t}]\rightarrow\Re_+$, where t_i is firm *i*'s expected value with probability and cumulative distribution functions $g_i(t)$ and $G_i(t)$. We assume that the distributions g_i and G_i are common knowledge, but that the expected value t_i is known only to firm *i*. Further, we assume that all bidders are risk neutral and draw values from an identical support, $[\underline{t},\bar{t}]$. Suppose there exists an increasing equilibrium such that $B_i(\cdot)$ is strictly increasing and differentiable on the

[56] In this sense, our identification strategy is much different from Haile (2001), who analyzes individual bids from the U.S. timber auctions and makes use of the temporal variation in the imposition of federal regulations by examining bids prior to the onset of the regulations that effectively prohibited resale and comparing them to bids after the regulations took effect.

support t_i for all i, then there exists an inverse bid function, $\phi_i(\cdot)$, that is also strictly increasing and differentiable on the support of bids. Denote the set of strategies followed by other firms as B_{-i}, then the probability that firm i wins the auction is

$$Q_i(b_i) \equiv \Pr\left[t_j < \phi_j(b_i) \forall j \neq i\right] = \prod_{j \neq i} G_j\left[\phi_j(b_i)\right] \tag{1}$$

Without resale, firm i's expected profits from participating in the auction are given by

$$E\left[\pi_i(b_i, B_{-i})\right] = (t_i - b_i) Q_i(b_i). \tag{2}$$

In equilibrium, this imposes a structure on the relationship between a given firm's bid and the probability of that bid winning the auction.

However, when bidders have resale opportunities, equation (2) must be adjusted to reflect expected profits from resale trade. Haile (2003) highlights two opposing effects of resale on bidder valuations and hence strategies – the *resale seller effect* (the option value of selling in the resale market) and *the resale buyer effect* (the option value of buying in the resale market). Intuitively, whenever the resale seller effect dominates, the expected value of winning any auction is a combination of an agent's private use value and the expected profits of selling the commodity on the secondary market. Provided that the expected profit of resale is non-zero, this option value leads to higher overall bids.

In the interior of British Columbia, Category 2 bidders trade logs harvested from SBFEP auctions at bilaterally negotiated prices. Since Category 2 mills specialize in processing certain types of timber and end products, exchange occurs along specialization lines for species that comprise a minority percentage of the total harvested timber on any plot. Further, whenever secondary trade occurs, both the buyer and seller have better information about market prices for processed timber than at the time of bidding in the primary auction - trade in timber occurs at the time it is processed whereas bidding in auctions occurs months before final production.

As such, we hypothesize that the resale seller effect dominates the resale buyer effect for Category 2 bidders. In such an environment, firm *i*'s expected profits can be rewritten as

$$E[\pi_i(b_i, B_{-i})] = (t_i^{'} - b_i)Q_i(b_i), \tag{2'}$$

where $t_i^{'} \geq t_i$ is a combination of an agent's private use value (t_i) and the expected profits of selling the commodity on the secondary market and $Q_i(b_i)$ is again the probability that bidder *i* wins the auction. The resulting optimal bid strategies differ from those of an equivalent first-price auction without resale. Across all signal (valuation) ranges, bids in the former environment weakly dominate those in the latter.

To identify this comparative static, we employ a general approach that is in the spirit of, for example, Porter and Zona (1993, 1999). We employ reduced-form methods to infer the nature of resale effects by differences in bidding patterns across subsets of firms facing different outside options.[57] Our identification strategy approximates equilibrium bidding behavior as a linear function of both observed and unobserved auction-specific and firm effects assumed to affect firm *i*'s valuation and/or probability of winning a given auction.

[57] An alternate approach to identifying resale effects would be to employ structural methods similar to those developed in Laffont, Ossard, and Vuong (1995) or Guerre, Perrigne, and Vuong (2000). However, to use these methods, we would have to specify the structure of the resale market which is based upon bilateral exchange between mills that is unobserved in our data.

As the true equilibrium bidding function is unknown in practice, we consider two different specifications to approximate observed bidding behavior given by:

$$P_{ij} = X_{ij}\beta + \tau D_i + \varepsilon_{ij} \tag{3}$$

$$P_{ij} = D_i\left[X_{ij}\beta_1\right] + \left(1 - D_i\right)\left[X_{ij}\beta_0\right] + \varepsilon_{ij} \tag{3'}$$

where P_{ij} is the ith bidder's total bid in auction j. X_{ij} is a set of regressors underlying the i^{th} firm's valuation for tract j, D_i is a dummy variable that equals one for any bid placed by a mill in a Category 2 auction; $\varepsilon_{ij} = \alpha_i + u_{ij}$; $E[\alpha_i] = 0$, $E[\alpha_i^2] = \sigma_\alpha^2$, $E[\alpha_i\alpha_k] = 0$ for $i \neq k$; α_i and u_{ij} are orthogonal for all i and j. α_i is a random effect assumed to capture heterogeneity that would be left uncontrolled in a standard cross-sectional model and u_{ij} represents private information such as idiosyncratic shocks to the expected valuation for firm i in auction j.

Specification (3) indicates that the effect of resale acts only as an intercept shift for mills in Category 2 auction, while (3') permits the resale effect to enter both the slope and intercept terms. Theoretically, if the resale seller effect dominates for Category 2 bids, then we would expect that $\hat{\tau} > 0$ and $\hat{\beta}_1 \neq \hat{\beta}_0$. In particular, we should observe that the estimated comparative static effect of competition (and any other covariate that is positively related with t_i') on observed bids is smaller for interior Category 1 auctions than it is for otherwise equivalent Category 2 auctions generating predicted bids that are greater for the latter subset of auctions.

<u>The SBFEP Auction Data - Empirical Results</u>

We observe 2,671 SBFEP sealed-bid tender first-price auctions conducted in the interior of British Columbia for the period January 1996 through December 2002. These auctions provide more than 10,000 individual bids, from which we eliminate any bids submitted in auctions with only a single bidder, any bids submitted in an auction with an estimated net cruise volume of less than 1,000 m^3, any bids submitted in an auction employing a format other than a first-price sealed tender, and any bids submitted by a bidder that participates in both Category 1 and Category 2 auctions. This results in a sample of nearly 1,250 firms that submit nearly 5,500 bids.

To generate the data for the empirical model, we combine information from a number of sources. First, a list of all bidders currently registered to participate in SBFEP timber auctions was provided by the Ministry of Forests (MOF) in BC. This listing was used to generate unique identification codes for each bidder in the data set. Second, the MOF provided bid sheets for each of the 2,671 auctions. The bid sheets provide information on (i) the regional office holding and date of the auction, (ii) the estimated net cruise volume of timber on the plot, (iii) the announced upset rate for the auction, and (iv) the identity and bonus bid per m^3 for each participant in the auction. Finally, the MOF provided a database that contains detailed information on the characteristics of each plot and the required deadline to complete the harvest of the specified timber.

We were careful to follow the Canadian hedonic specification when specifying our reduced form approximation of the equilibrium bidding function. Auction covariates included in the vector of regressors include:

- UPSET RATE: announced reservation price per m^3
- NCV: estimated net cruise volume (divided by 1000)
- VPH: estimated volume of trees per hectare (divided by 1000)
- LNVPT: log of estimated volume per tree

- LSPI: the average selling price index for timber harvested
- DC: deflated development costs (divided by NCV)
- SLOPE: weighted average slope
- BWDN: estimated percent of volume blown down
- BURN: estimated percent of volume burned
- CY: estimated percent of volume to be extracted via cable
- HP: estimated percent of volume to be extracted via helicopter
- HORSE: estimated percent of volume to be extracted via horse
- UTIL: estimated capacity utilization for firm *i* – ratio of current backlog of timber contracts in m^3 to maximum backlog of timber contracts in m_3
- CYCLE: estimated cycle time for harvested timber
- LNB: natural log of the number of bidders.

Table 1 provides parameter estimates for equations (3II) and (3III) estimated for different subsets of the 5,524 observations.[58]

Empirical results presented in Table 1 suggest an important difference in the behavior of mills versus loggers that are consistent with resale possibilities for the former set of bidders. First, the indicator variable for Category 2 bidders (mills) in Column A suggests that such agents submit bids that are *ceteris paribus* \$1.68 greater than those submitted by a registered logger in a Category 1 auction with this difference statistically significant at the $p < 0.05$ level. Second, measured at the sample means using parameter

[58] The number of bidders in SBFEP auctions is likely an endogenous measure. Taking the number of bidders as an exogenous measure is thus somewhat problematic. However, in the current context, solving for endogenous entry using an instrumental variables approach is equally problematic since n affects bids directly through equilibrium bidding strategies, a relation with resale prospects, and the correlation of entry with unobserved characteristics.

estimates from Columns 3 and 4, the estimated marginal effect of adding an additional bidder in a Category 2 auction is \$2.11 (4.4% increase in the predicted bid) as opposed to \$0.91 (2.01% increase in the predicted bid) for a Category 1 auction. Equilibrium bidding strategies in auctions with resale are conditioned upon information related to other bidders in the market that is absent in the strategy of a firm bidding in a market without resale. Hence, we would expect greater competition in the former case when the secondary market institution is an OA continuation game of complete information (or a similar analog), as we assume for the Category 2 mills. This finding is consonant with the predictions and analysis employed by Haile (2001) to identify resale effects for U.S. timber auctions. Combined with other parameter estimates in Table 1 (e.g., the estimated increase in predicted bids for Category 2 auctions), we take the empirical results to suggest that resale opportunities influence bidding in the direction that theory would predict.

III. Risk, Resale, and Bidder Behavior – First-Price Auctions

Although we are able to identify empirical differences across Category 1 and Category 2 bidders consistent with comparative static effects of resale opportunities on bids, the field data on SBFEP auctions do not allow us to control properly for many underlying determinants like the (expected) value from using the good, the division of surplus on the resale market, or the risk-posture of the firms. [59] The effects of resale are inferred from differences in reduced-form parameters across the subsets of bidders.

[59] For example, there is a possibility that loggers are able to mitigate risk in the field by entering into contractual relationships with processors and/or logging companies that Category 2 bidders are unable to mitigate. If so, then one could argue that loggers are less risk averse than are mills. Hence, it is possible that estimated differences in behavior across these two groups are generated by differences in unobserved risk posture rather than differences in *ex post* resale opportunities.

Our analysis is sensitive to issues of model specification and the interpretation of estimated parameters. In practice, both the "true" underlying model specification and its associated interpretation are often unknown and/or unobserved in naturally occurring data. Furthermore, while there is a recent literature (see e.g., Campo et al. 2002 or Perrigne 2003) that enables structural estimation of first-price auction models allowing for risk aversion, such methods rely upon strong restrictions to identify risk preference.

To make more powerful inference of whether predicted comparative statics are generated by the existence and nature of secondary markets, one can examine behavior in a controlled environment. We follow this line of reasoning and complement the field results with lab experiments. An advantage of our laboratory experiments is that we are able to directly control for the secondary market structure, individual valuations, and the risk preference of subjects when evaluating the impact of changes in the existence and nature of secondary market exchange. This provides a validity check on model applied to the timber auction data as it enables us to examine whether misspecification of individual risk preference affects the coherence of the relevant hypotheses tests. To derive testable predictions, we first develop a model that allows risk aversion.[60]

Consider a first-price auction with resale opportunities with *n* symmetric, risk-averse players. We assume that players are risk averse with constant absolute risk

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[60] As noted by Haile (2003), auction markets with resale have components of both common and private value auctions. It is well documented in the experimental literature that in a private value setting risk-averse agents submit bids that first-order stochastically dominate those of risk-neutral agents. In common value settings, however, this tendency is reversed. Risk-averse agents in a common value auction submit bids that are stochastically dominated by risk-neutral counterparts. Given the persistence of risk-averse behavior on the part of student subjects in the lab, and lacking an *a priori* theoretical prediction/conjecture about the effects of risk aversion on behavior in our setting, it is important to develop such theory to enable us to filter out the effects of risk aversion from those of resale opportunity. Without such theory, empirical tests are potentially confounded and do not permit a direct test of our desired treatment effect.

aversion (CARA=σ). That is, the Bernoulli utility function is given by $\rho(z) = -\exp(-\sigma z)/\sigma$. Prior to bidding, each player *i* receives a signal X_i on her use value U_i. The signals $X_i \in [x_l, x_u]$ are independently and identically distributed according to a differentiable and strictly increasing distribution $F(\cdot)$. Use values $U_i \in [u_l, u_u]$ are assumed to follow the conditional distribution $G(\cdot \mid X_i)$, which is differentiable with $G_u > 0$ on the support $[u_{\min}(X_i), u_{\max}(X_i)]$. Furthermore, we assume that $G(u \mid x)$ is continuous and decreasing in x, i.e., $G(u \mid x)$ stochastically dominates $G(u \mid y)$ if $x > y$.[61] This implies that both $u_{\min}(x), u_{\max}(x)$ are increasing in x.

We make the following assumption on the probability distributions:

Assumption (A1): $\frac{d}{du} \log G(u \mid x)$ is increasing in *x*.

Note that Assumption (A1) is satisfied in particular for all uniform distributions: $G(u \mid x) = \frac{u - u_{\min}(x)}{u_{\max}(x) - u_{\min}(x)}$. Here, $\frac{d}{du} \log G(u \mid x) = \frac{1}{u - u_{\min}(x)}$, which increases in *x* as $u_{\min}(x)$ is increasing in *x*.

We consider three different first-prize auctions which differ with respect to the resale opportunity: (i) the reference case (N) in which there is no secondary resale market. (ii) a case where resale is possible and the seller extracts all the surplus in the resale market, i.e. the resale market is structured as an optimal auction continuation game (R=OA), and (iii) the case in which there is an English auction in the resale market which provides the buyer in the resale market with the maximal surplus (R=EA). Under resale, the value the bidder places on the commodity in the primary auction market depends on

[61] $G(u \mid x)$ is assumed strictly decreasing on the interior of the support.

the price at which resale can take place. As discussed in Haile (2003), such prices are dependent upon the informational structure and trading institution assumed on the secondary market. By concentrating on the two cases described above, we capture the two extreme distributions of surplus between buyer and seller in the secondary market, i.e. extreme assumptions on the expected value from winning (and losing) the good.

Bidder Behavior in Markets without Resale

Without resale opportunity, the distribution of the use value is given by $G(\cdot | X_i)$. A player with signal x who wins the auction with a bid of b has expected utility given by

$$\begin{aligned}\int_{u_l}^{u_u} \rho(u-b)dG(u|x) &= \frac{\rho(0)}{\rho(b)}\int_{u_l}^{u_u} \rho(u)dG(u|x) \\ &= \frac{\rho(0)}{\rho(b)}K_N(x)\end{aligned} \tag{1}$$

where $K_N(x)$ refers to the expected utility from consuming the good given a signal x.

Using standard derivation techniques, we obtain the following result:

Proposition 1 [corresponds to Theorem 14 of Milgrom and Weber (1982)]**:** *If no resale is possible, the unique differentiable symmetric separating equilibrium bid function $b_N(x)$ is implicitly defined by*

$$\frac{1}{\rho(b_N(x))} = -\sigma \exp(\sigma b_N(x)) = \frac{1}{F(x)^{n-1}}\int_{x_l}^{x} \frac{1}{K_N(z)} dF(z)^{n-1} \tag{2}$$

Proof: (see Appendix A)

To later discuss the effect of risk-aversion, consider the extreme cases of risk-neutrality and infinite risk-aversion which can be derived using l'Hospital's rule (see Appendix A). Under risk neutrality, an optimal bidding function is given by

$$b_N(x) = \frac{1}{F(x)^{n-1}} \int_{x_l}^{x} \int_{u_l}^{u_u} u dG(u \mid z) dF(z)^{n-1} \text{ for } \sigma = 0 . \tag{3}$$

For infinitely risk-averse agents, however, bids converge towards the minimal possible use value given a signal *x*, i.e., $b_N(x) \to u_{\min}(x)$ for $\sigma \to \infty$.

Bidder Behavior in Auctions with Resale

We first study the case of complete information on a resale market characterized by an OA continuation game. That is, we assume that *ex post* use values are common knowledge among players and that the seller extracts the entire surplus by selling to the opponent with the highest use value on the resale market whenever such trade is profitable. The value of the good to the winner is therefore not given by his own use value but by the maximal use value of all players.

To derive the relevant probability distributions, let us denote the distribution of the use value of a single player given that her signal is less than or equal to *y* as

$$M(u \mid y) = \frac{\int_{x_l}^{y} G(u \mid z) dF(z)}{F(y)} \tag{4}$$

The distribution of highest use value of an opponent of a player given that *y* is the maximal signal to an opponent is then given by

$$\overline{G}_1(u \mid y) = G(u \mid y) M(u \mid y)^{n-2} \tag{5}$$

Finally, the distribution of highest use value of all players given signal *x* to one player and *y* being the maximal signal to an opponent is given by

$$G_1(u \mid x, y) = G(u \mid x) \overline{G}_1(u \mid y) \tag{6}$$

The expected utility of a player with signal x – facing opponents with maximal signal y – who wins an auction with a bid of b is given by

$$\begin{aligned}\int_{u_l}^{u_u} \rho(u-b)dG_1(u \mid x,y) &= \frac{\rho(0)}{\rho(b)}\int_{u_l}^{u_u} \rho(u)dG_1(u \mid x,y) \\ &= \frac{\rho(0)}{\rho(b)} K_{OA}(x,y)\end{aligned}, \tag{7}$$

where $K_{OA}(x,y)$ refers to the expected utility from consuming the good given a signal x, where y is again the maximal signal of all opponents. Not winning the auction yields a payoff of zero.

If an English auction is carried out on the resale market, the second highest use value is decisive. Similarly to the OA case, the expected utility from obtaining the good given a signal x and y being the maximal signal of all opponents can be written as

$$K_{EA}(x,y) = \int_{u_l}^{u_u} \rho(u)dG_2(u \mid x,y), \tag{8}$$

where $G_2(u \mid x,y)$ is the probability that neither the use value of a player with signal x nor the second highest use value of opponents whose highest signal is y exceeds u. This probability distribution can be written as

$$G_2(u \mid x,y) = G(u \mid x)\bar{G}_2(u \mid y) \tag{9}$$

where $\bar{G}_2(u \mid y)$ denotes as the distribution of second highest use value of an opponent of a player given that y is the maximal signal to an opponent and is given by

$$\bar{G}_2(u \mid y) = M(u \mid y)^{n-2} + (n-2)G(u \mid y)M(u \mid y)^{n-3}\left[1-M(u \mid y)\right] \tag{10}$$

If a player does not win the auction, she can acquire the good on the resale market if she has the highest use value. For the English auction, the expected value from losing the auction is therefore given by

$$\int_{u_l}^{u_u}\left[\int_{u_l}^{u}\rho(u-z)d\overline{G}_1(z\mid y)+\int_{u}^{u_u}\rho(0)d\overline{G}_1(z\mid y)\right]dG(u\mid x) \\ = L_{EA}(x,y)\rho(0) \tag{11}$$

Using the definitions of $K_R(x,y)$ and $L_R(x,y)$ with $R\in\{OA,EA\}$ and $L_{OA}(x,y)=1$, we obtain the optimal bids for both types of resale markets.

Proposition 2: [corresponds to Theorem 2 of Haile (2003)] *If the resale market is organized via an optimal or English auction, the unique differentiable symmetric separating equilibrium bid function* $b_R(x)$ ($R\in\{OA,EA\}$)is implicitly defined by

$$\frac{1}{\rho(b_R(x))}=\left[\int_{x_l}^{x}\frac{\exp\left(\int_{x}^{z}K_R(y,y)/\int_{x_l}^{y}K_R(y,w)dF(w)^{n-1}dF(y)^{n-1}\right)}{\int_{x_l}^{z}K_R(z,y)dF(y)^{n-1}}L_R(z,z)dF(z)^{n-1}\right]. \tag{12}$$

Proof: See Appendix A

The effect of risk-aversion can again be identified by looking at the extreme cases of risk-neutrality and infinitely risk-averse agents. For risk-neutral agents, l'Hospital's rule for $\sigma\to 0$ yields:

$$b_{OA}(x)=\frac{1}{F(x)^{n-1}}\int_{x_l}^{x}\int_{u_l}^{u_u}udG_1(u\mid z,z)dF(z)^{n-1}\ \text{for}\ \sigma=0, \tag{13}$$

which mimics the optimal bid function derived by Haile (2003) for a first-price auction followed by an OA continuation game with complete information (see Appendix A). For the English auction continuation we obtain similarly

$$b_{EA}(x)=\frac{1}{F(x)^{n-1}}\int_{x_l}^{x}\left[\int_{u_l}^{u_u}udG_2(u\mid z,z)-\int_{u_l}^{u_u}\int_{u_l}^{u}(u-w)d\bar{G}_1(w\mid z)dG(u\mid z)\right]dF(z)^{n-1}\quad\text{for } \sigma=0, \quad (14)$$

which is in line with Haile's result.

Furthermore, for agents with preferences characterized by infinitely high risk aversion, bids converge towards the minimal possible use value given a signal *x* for both continuation games, i.e., $b_R(x)\to u_{\min}(x)$ for $\sigma\to\infty$ ($R\in\{OA,EA\}$) (see Appendix A).

Implications for Optimal Bidding Strategies: Resale vs. No Resale

For infinitely high risk-aversion and independently of the underlying resale market, bids converge towards the minimal possible use value given a signal *x*, i.e.,

$$b_N(x)=b_R(x)=u_{\min}(x) \text{ for } \sigma=\infty.$$

Therefore, resale has no effect on bidding strategies if players are infinitely risk averse. Hence, since a large majority of agents in the population are risk averse, the differences due to the possibility of resale are generally overstated if only risk neutrality is considered.[62]

Further, with a perfectly informative signal $u_{\min}(x)=\int_{u_l}^{u_u}udG(u\mid x)=u_{\max}(x)$ for all x, resale also has no effect on optimal bids, independent of the level of risk aversion. To see this, note that in such a case $K_R(x,y)=\rho(x)$ for $x>y$ and $L_{EA}(x,y)=1$. Hence, the bidding function (11) reduces to

[62] There are a number of studies that suggest risk aversion on the part of agents in varying contexts. Bingswanger (1980) finds levels of relative risk aversion above 0.32 for farmers in rural India. Estimates of relative risk aversion for private value auctions in the lab range from 0.52 to 0.67 (Cox and Oaxaca, 1996; Goeree et al., 1999). Campo et al. (2000) estimate relative risk aversion of 0.56 from field data on timber auctions.

$$\frac{1}{\rho(b_R(x))} = \frac{1}{F(x)^{n-1}} \int_{x_l}^{x} \frac{1}{\rho(z)} dF(z)^{n-1},$$

which coincides with the bid function for markets without resale given by (2) with $K_N(x) = \rho(x)$. The intuition behind this result is that with resale opportunities an agent wins the auction only if she receives the highest signal, i.e., she has the largest use value. The resale value, therefore, coincides with the use value without resale opportunity. With an imperfectly informative signal, however, resale generally increases bids since the expected resale value is not smaller than the expected use value of an agent.

Implications for Optimal Bidding Strategies: The Effect of Risk Aversion

The qualitative effects of risk aversion depend on whether the risk-neutral bids for a signal *x* exceed or equal the minimal use value given by $u_{\min}(x)$, which depends on the specific distributions of use values and signals. However, using the limiting case of infinite risk aversion, the following cases might occur for a treatment $t \in \{N, OA, EA\}$:

1. Under risk neutrality, $b_t(x) > u_{\min}(x)$: Risk aversion decreases bids for high degrees of risk aversion.
2. Under risk neutrality, $b_t(x) < u_{\min}(x)$: Risk aversion eventually increases bids.

In our experimental markets described below, case 1 is expected to hold for the lower range of the signal space whereas case 2 applies to the higher range of the signal space. The effects of risk aversion therefore are predicted to qualitatively change over the range of signals. In our experimental markets, we would thus predict a crossing of optimal bid functions for both the resale and no-resale treatments for agents that demonstrate high levels of risk aversion.

Implications for Optimal Bidding Strategies: Minimal Observable Bids

The minimal bids in both resale and no-resale cases are given by the lowest signal type. Note that, using l'Hospital's rule again, the equilibrium bid functions lead to

$$\frac{1}{\rho(b_N(x_l))} = \frac{1}{K_N(x_l)}$$

and

$$\frac{1}{\rho(b_R(x_l))} = \frac{L_R(x_l, x_l)}{K_R(x_l, x_l)}.$$

Unless the signal is perfectly informative at $X = x_l$, we have that $K_R(x_l, x_l) > K_N(x_l)$, and further that $L_R(x_l, x_l) \leq 1$. Therefore, the smallest observable bid should be higher if resale is possible. In our experimental market, a bidder who receives a signal of $X = x_l$ knows with certainty that his use value will be \$10. Thus bids in all treatments should coincide at this lowest signal level.

IV. Experimental Design and Results

Experimental Design

A total of 90 subjects participated in our laboratory experiment, which was conducted during the Fall 2003 and Spring 2004 semesters at the University of Maryland. Each session consisted of two experimental parts: a first-price auction market with or without resale opportunity and the Holt and Laury (2002) experimental procedure to elicit risk preference. Each part of the laboratory experiment is described below.

Part I: The Auction Market

Each subject's experience typically followed four steps: (1) consideration of an invitation to participate in an experiment, (2) learning the auction rules, (3) actual market participation, and (4) conclusion of the experiment and completion of the Holt and Laury (2002) risk-aversion experiment. In Step 1, undergraduate students from the University of Maryland were recruited using e-mail solicitations and flyers hung in academic buildings across the campus. Once the prerequisite number of subjects had responded, a second e-mail was sent to each participant inviting them to participate in an experimental session to be held at a given date/time. After subjects were seated in a room, in Step 2 a monitor thoroughly explained the experimental instructions and auction rules (included in Appendices B and C).

Before proceeding, a few key aspects of the experimental design should be highlighted. First, all bidders were informed that earnings from the auction experiment would be added to earnings from a second, unrelated experiment to determine total earnings for the session. Second, individuals were informed that they would be bidders in the experiment. In each of the 12 rounds (2 practice and 10 that count towards earnings), they would be given a bidder's card that contained a number, known only to that bidder, representing a signal of the value of one unit of the fictitious commodity. Importantly, all agents were informed that this information was strictly private and that both signals and use values would change each round. They were also informed about the number of other bidders in the market (4), that they would bid against the same four bidders for all ten rounds, and that agents may have different signals (use values).

Third, the monitor explained how signals were determined in each market period and how these signals related to the agent's final reservation (use) value. Subjects were informed that in each period, they would receive a signal from the interval [$0, $50]. These signals were determined by adding a random integer generated from a uniform distribution on the interval [-$10, $10] to the agent's final use value which was itself an integer value randomly drawn on the uniform interval [$10, $40]. Several examples illustrated the relationship between a given use value and the range of signals that the bidder could receive in the first stage, and vice versa.

Fourth, the monitor explained how earnings were determined. In the baseline, no resale treatment, the highest bidder earns the difference between their end use value and their bid. All other bidders earn zero. In the resale treatment with OA continuation game, the bidder who submits the highest bid earns the difference between the highest use value of all bidders and the winning bid. All other bidders earn zero. In the resale treatment with EA continuation game, the bidder who submits the highest bid receives the maximum of her use value and the second highest use value of all participants minus her winning bid. The bidder who does not submit the high bid but has the highest use value receives the difference between this value and the second highest use value of all other participants. All other bidders earn zero for the round. Total earnings for each treatment are computed by summing the earnings across the 10 periods.

In the resale treatment with OA continuation game, it was publicly announced that following the completion of each round, ownership of the good would be sold to the agent with the highest use value in the group at a price equal to her value. In the resale treatment with EA continuation game, it was publicly announced that following the completion of each round, ownership of the good would be sold to the agent with the highest use value in the group at a price equal to the second highest use value of all agents in the group.[63] In the baseline no-resale treatment, several examples were provided that illustrated the irrationality of bidding more than $10 above a received signal. In the resale treatments several examples were provided that illustrated the workings of the resale market and how prices for resale exchange and earnings for each bidder would be determined.[64] Fifth, individuals participated in 2 practice rounds of bidding to gain experience with the auction market and rules.

In Step 3, subjects participated in the market. Each market consisted of 10 rounds of bidding that lasted about 3 minutes each. After each 3-minute period, a monitor privately gathered each subject's bidder card and gave the bidder a second card containing the subject's final use value that was within [-$10, $10] of the original signal. Once all bidder cards were collected, a monitor publicly announced all bids and awarded the good to the highest bidder. Final use values were publicly announced and, in the resale treatment, ownership of the commodity transferred to the agent with highest use value.

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[63] Two important features of our experimental design that we should highlight include: i) our choice to limit participation on the secondary market to bidders from the primary auction market and ii) our decision to execute trades on the secondary market at the theoretical benchmarks for both the OA and EA game of complete information. We elected to limit participation on the secondary market to maintain consistency with theory and our naturally occurring data—the interior secondary market for timber in BC is comprised of bidders registered to participate in the primary auctions. We elected to execute trade on the secondary market at the theoretical benchmarks to maintain consistency with our conceptual model. The focus of this analysis is on first-stage bidding strategies rather than secondary market exchange. Allowing the endogenous determination of prices on the secondary market would surely have an influence on bidding strategies, as it is likely that rents would not be divided on the secondary market as predicated by theory. Anticipating this, bidders would adjust first-stage bidding strategies. We hope that future work analyzes behavior in markets where prices are endogenously determined on the secondary market and new participants are allowed to enter the second-stage continuation game.

[64] An important consideration in designing our auction markets was the issue of bankruptcy and bidder behavior. Theoretically, bankruptcy was not an issue if subjects played the risk-neutral Nash equilibrium. However, equilibrium payouts in a number of the periods were low enough to raise concern if subjects determined bids with a degree of error. For reasons outlined in Hansen and Lott (1991), we decided to employ an unlimited liability rule and allow subjects to have negative earnings for Part I of the experiment.

It should be noted that throughout each session careful attention was given to prohibit communications between bidders that could induce collusive outcomes. Step 4 concluded the experiment – after subjects completed the Holt and Laury (2002) experiment (described in Part II of this section), they were paid their earnings in private.

This simple procedure was followed in each of three treatments, which are summarized in Table 2. Table 2 can be read as follows: row 1, column 2 of Table 2 contains treatment NR, denoting a no-resale auction market with 5 bidders, who each have unit demand for the good. Table 3 presents buyer induced values and signals for each market period. All signals were drawn and assigned using the following procedure. We first drew 50 integer numbers on the uniform distribution between [$10, $40] using Excel's random number generator. We added an integer drawn on the uniform distribution [-$10, $10] to this number to obtain signal values. These values were then assigned so that unbeknownst to bidders, in each session (i) every bidder received the highest signal twice, (ii) each bidder received the highest use value but a lower ordered signal, and (iii) resale trade was potentially profitable in half of the periods.

Part II: The Holt-Laury Risk Experiment

Upon completion of Part 1 of the session, instructions and a decision sheet were handed out for the second part of the experiment. This second part was designed to elicit subjects' risk preferences. In this part of the session, the low-payoff treatment of Holt and Laury (2002) was used (see Appendix C for instructions).[65] The treatment is based

[65] We elected to use the low-payoff treatment of the Holt and Laury (2002) experiment to measure risk preference since the domain of earnings because this treatment [$0.10 to $3.85] approximates the equilibrium domain of per period earnings for our auction markets. We also collected data for a higher-payoff treatment of the Holt and Laury (2002) experiment, where the domain of earnings [$0.40 to $15.40] approximates the equilibrium domain of earnings at the session level in our auction markets. In what

on ten choices between paired lotteries. The paired choices are included in Appendix C. The payoff possibilities for Option A, $2.00 or $1.60, are much less variable than those for Option B, $3.85 or $0.10, which was considered the risky option. The odds of winning the higher payoff for each of the options increased with each decision, and the paired choices are designed to determine degrees of risk aversion. Holt and Laury (p. 1649) provide a table that will be used to categorize subjects' CARA risk preference levels based on their ten decision choices.

After the instructions were read and questions were answered, the subjects were asked to complete their decision sheets by choosing either A or B for each of the ten decisions. The subjects were instructed that one of the decisions would be randomly selected *ex post* and used to determine their payoffs. Part of a deck of cards was used to determine payoffs, cards 2-10 and the Ace to represent "1". After each subject completed his or her decision sheet, a monitor would approach the desk and randomly draw a card twice, once to select which of the ten decisions to use, and a second time to determine what the payoff was for the option chosen, A or B, for the particular decision selected. After the first card was selected, it was placed back in the pile, the deck was reshuffled, and the second card was drawn. For example, if the first draw was an Ace, then the first decision choice would be used. Suppose the subject selected A in the first row. The second draw would then be made. If the Ace was drawn, the subject would win $2.00. If a card numbered 2-10 was drawn, the subject would win $1.60. The subjects were aware that each decision had an equal chance of being selected.

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follows, we report only the empirical results for risk preference based upon individual response to the low-payoff Holt and Laury (2002) design. However, all tests and results are robust to the use of response to the higher-payoff experiment.

After all the subjects' payoffs were determined, they combined their payoff from Part 1 with that of Part 2 to compute their final earnings. The final payoffs were then verified against records maintained by a monitor, and subjects were paid privately in cash for their earnings. Each of the sessions lasted approximately 75 minutes and average earnings were roughly $13.

Theoretical predictions for the laboratory auction markets

Figure 1 provides theoretical predictions for risk-neutral bidders in our experimental markets conditioned upon the signal. Across all but the lowest level of the signal space ($0.00), bidders in markets with resale opportunities represented by an OA continuation game are predicted to submit bids that are on average higher than those submitted by an equivalent bidder in a market without resale options. These differences range from mere pennies for signals less than $5.00 to a maximum of about $3.40 for bids submitted in the signal range around $23.[66] For signal ranges above $40.00 or below $20, the predicted differences in bids between the no-resale and resale treatments are less than $2.75. In resale treatments represented by an EA continuation game, risk-neutral bidders are predicted to submit bids that are on average higher than those submitted by an equivalent bidder in a market without resale at ranges of our signal space of less than $27.00. For signals larger than $27.00, risk-neutral bidders in our resale treatment with EA continuation game are predicted to submit bids that are on average less than those submitted by an equivalent bidder in the no-resale treatment. These differences range

[66] The optimal bid functions were derived numerically. Using the theory developed in Section III, we first calculated $K_R(x, y)$ and $L_R(x, y)$ on a grid with 0.1 increments. Using interpolating functions, we then solved the respective differential equations.

from a maximum of $0.70 at a signal of approximately $20.00 to a minimum of $-0.56 at a signal of approximately $46.00.

<u>Experimental Results</u>

Table 4 provides summary statistics for the experimental data. Entries in Table 4 are at the period level and include average bid level and its standard deviation, the average winning bid and its standard deviation, average resale price, and average earnings for the auction winner and resale buyer. Table 4 can be read as follows: on average, in period 1 of the No Resale treatment, subjects submit a bid of $20.84 (standard deviation = 9.32) and the average winning bid is $30.77 (standard deviation = 3.17). Perusal of the data summary in Table 4 leads to our first two results:

> *Result 1: Bids in a first-price auction followed by resale exchange in an OA continuation game are greater than those submitted in equivalent markets without resale.*
>
> *Result 2: Bids in a first-price auction followed by resale exchange in an OA continuation game are greater than those submitted in equivalent markets with an EA continuation game.*

These results can be seen most directly by examining both per period average and winning bids across our three laboratory treatments. Across all ten market periods, both average and winning bids in the resale treatment with OA continuation game are greater than bids in both the baseline, no-resale treatment and the resale treatment with EA continuation game.

Figure 2 provides a comparison of bids submitted in our baseline no-resale treatment and our resale treatment with OA continuation game. The figure illustrates the first part of result 1: bids in the resale treatment with OA continuation game are greater than those in the baseline no-resale treatment. Interestingly, these differences are greatest at lower and intermediate ranges of the signal domain. For signals above \$32-35, there is no discernable difference in bids across the two treatments.

Our last piece of evidence to support Results 1 and 2 comes from a random effects bid equation:

$$B_{\mathrm{it}} = \mathrm{v}(Z_{\mathrm{it}}) + \varepsilon_{\mathrm{it}}, \tag{15}$$

where B_{it} is the bid of the *i*th buyer in period *t*. The vector Z_{it} includes treatment dummy variables, the induced signal received by the agent, the square of this signal, and session fixed effects; $\varepsilon_{it} = \alpha_i + u_{it}$; $E[\alpha_i] = 0$, $E[\alpha_i^2] = \sigma_\alpha^2$, $E[\alpha_i\alpha_j] = 0$ for $i \neq j$; α_i and u_{it} are orthogonal for all i and t. The random effects α_i capture important heterogeneity across agents that would be left uncontrolled in a standard cross-sectional model.

Columns A-D in Table 5 present regression results which provide support for Results 1 and 2. For example, parameter estimates in columns A-D suggest that bids in the OA treatment are either \$3.91 (\$5.85) higher than bids in the baseline treatment (the omitted categorical variable) depending on whether or not we explicitly control for session fixed effects. As indicated in the table, both of these differences are statistically significant at the $p < .05$ level. Furthermore, using a Chow test of coefficient equality, we find that OA bids are larger than EA bids at the $p < .05$ level. As columns A-D show, these differences are robust across several different empirical specifications.

Empirical results in Table 5 suggest that baseline bids and EA treatment bids are isomorphic. Yet, when bids are analyzed over ranges of signals less than (greater than) \$26, where our theory predicts that bids from the EA treatment are predicted to be greater than (less than) those submitted in the baseline treatment, we find evidence consonant with the theory. Over lower ranges of the signal space, bids from the EA treatment are, on average, \$2.73 larger than those from the baseline treatment, a statistically significant difference at the $p < 0.05$ level.[67] Over higher ranges of the signal space, bids from the EA treatment average \$2.07 less than those from the baseline treatment, with these differences statistically significant at the $p < 0.05$ level. These data patterns lead to the next result:

> *Result 3: Over lower (higher) signal ranges, bidders in a first-price auction followed by an EA continuation game submit bids that are higher (lower) than those submitted by agents in an equivalent baseline market without resale opportunity.*

Figure 3 provides a comparison of bids submitted in the baseline no-resale treatment and bids in the resale treatment with EA continuation game which highlights this result. Over the entire signal domain, there is little discernable difference between bids, as suggested by the empirical estimates of the pooled data. At lower ranges of the signal space, however, the highest bids from the EA treatment are greater than the highest bids from our baseline auction markets. And, at a higher range of the signal space, the lowest bids from the EA treatment are less than the lowest bids from our baseline market.

Combined, these first three results lead to our fourth result:

> *Result 4: Differences in bidder behavior across auction markets without resale and equivalent auction markets with both an OA and EA continuation game of complete information are consistent with the comparative static predictions of Haile (2003).*

Risk Aversion and Bidder Behavior

Having found general support for the comparative static predictions of Haile (2003), we now examine more closely the predictions of the theory by exploring the data conditioned upon underlying risk preference. Figure 4 provides an illustration of bids in

[67] Statistical significance is evaluated using the Mann-Whitney test of significance evaluated at the session level. Test statistics are thus based upon a comparison of average bid levels for signals less than (greater than) \$26 in each of our six EA sessions versus comparable averages from our six baseline sessions.

our baseline no-resale market relative to the theoretical predictions for risk-neutral equilibrium bids. As can be seen from the figure, risk-neutral point predictions do not fit the data well. Over lower ranges of the signal space, realized bids are *less* than the risk-neutral theoretical predictions while over higher ranges, realized bids are *greater* than the risk-neutral theoretical benchmarks. This pattern of behavior is consistent with our theoretical model for risk-averse bidders. Similar patterns, albeit less pronounced, emerge for both the OA and EA treatments.

This raises two important questions for evaluating bidder behavior in auction markets. First, to what extent can such behavior be explained by the underlying risk preference of bidders? There has been considerable debate over the ability of traditional theory to account for observed patterns of bidding in experimental first-price, private value auctions.[68] A number of studies have attempted to recover and infer the level of risk-aversion that rationalizes the observed bids of subjects in controlled laboratory environments (i.e., Cox and Oaxaca, 1996; Cox, Smith, and Walker, 1985; Goeree et al., 1999; Harrison, 1990). In the current study, we directly elicit individual measures of risk posture and use this information as a control in estimating observed bidding behavior. As such, we are able to correlate differences in observed bids with variations in risk preference across agents.[69]

Second, to what extent is the coherence of hypothesis tests affected by the specification of factors such as the underlying risk preference of bidders typically

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[68] See Kagel (1995) or Harrison (1989) for an overview of this debate.

[69] Of course, unlike our experimental treatments risk posture should not be regarded as something we can exogenously impose on subjects. Thus, we exercise caution when interpreting the data in that risk posture could be systematically related to individual-specific unobservables that cause the data patterns discussed below.

unobserved by the econometrician? The theoretical model outlined in Section III indicates that the effect of resale on bidding strategies is attenuated by risk aversion. The difference between the bids of a risk averse agent in both the OA and EA treatments and the same agent in treatment NR is a decreasing function of any signal greater than $20. Incorrectly assuming the risk-preference of agents can thus impact the magnitude of estimated resale effects. For example, models that specify agents as risk neutral will tend to underestimate the effect of resale on bids whenever agents are in fact risk averse.

To evaluate the impact of risk preference on observed bids, we augment equation (15) by including an interaction of an individual's risk preference with his/her signal, the signal squared, and the treatment dummies.[70] Empirical estimates from this model are contained in column E in Table 5. We obtain the following insight from these results:

> *Result 5: Over all but the lowest range of signals, risk averse agents submits bids that greater than a risk-neutral counterpart with this difference increasing in the level of individual risk-aversion.*

Support for Result 5 can be garnered by examining the marginal effect of our risk proxy on realized bids. For example, in Column E of Table 5 the estimated marginal effect of risk preference on realized bids in the baseline treatment is given by [$-2.94 + 0.20$*Signal $- 0.002$*Signal^2]. This marginal effect is strictly positive for any signal greater than $15. For any signal greater than $15 in the baseline treatment, the bid of a risk averse agent should exceed that of a risk neutral counterpart receiving the same induced signal. Similar patterns arise for both the EA and OA treatments. In the EA (OA) treatment, risk

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[70] To proxy each agent's risk posture, we use the number of safe choices "Lottery A" selected by the agent in the Holt-Laury experimental design. An increase in the number of safe choices represents an increase in the individual's implied risk posture. For example, an agent who selects option A for the first four choice occasions is considered risk neutral whereas an agent who selects option A for the first six choice options has CARA risk preference of approximately 0.5. Figure 5 provides the frequency distribution of the implied CARA preference for the 90 subjects in our experiment auctions.

averse agents submit bids that are greater than a risk-neutral counterpart for any signal above \$19 (\$21). Our data thus suggest a direct correlation between observed over-bidding and individual risk preference.

Having found evidence that risk-aversion is correlated with observed patterns of bidding behavior in our experimental auctions, we now turn to examining the influence of individual risk preference on our estimated treatment effects. Our theoretical model suggests that increased risk aversion serves to attenuate the effect of resale on observed bids. Perusal of the estimates presented in Table 5 generates a sixth result:

> *Result 6: Risk averse agents in both the EA and OA treatments submit bids that are conditionally less responsive to induced signals than do risk-averse agents in the baseline no-resale treatment. Models that fail to account for the risk preference of agents thus underestimate the effect of resale on bidder behavior.*

The first part of result 6 follows from estimated differences on the coefficients for the risk_signal variable and the associated interaction of this variable with the treatment dummies (OA_risk_signal and EA_risk_signal). For example, in Column E of Table 5 the estimated marginal effect of the signal on realized bids in the baseline treatment is given by [risk_preference*(0.20 – 0.002*signal)]. In contrast, the estimated marginal effect of the signal on realized bids in our OA treatment is given by [risk_preference*(0.14 – 0.002*signal)]. This difference is statistically significant at the $p < 0.05$ level using an F-test.

The second part of result 6 follows from a comparison of the estimates on the treatment dummies in model specifications that control for individual risk preference (Columns E and F) with those that do not condition behavior on risk preference (Columns A – D). For example, the estimated treatment effect on the OA dummy variable is at least \$7.21 greater in models that explicitly control for individual risk preference with this difference statistically significant at the $p < 0.05$ level. Hence assuming that agents are risk-neutral as in Columns A – D will tend to underestimate the impact of resale opportunities on observed bids.

Our theoretical model is based upon the assumption of homogenous risk preference across agents. However, subjects in our experiment and in many other settings demonstrate heterogeneities in risk posture. An important issue is thus evaluating how such variations affect the observed behavior of agents. To address this issue, we calculate the average risk preference for subjects in each session and augment equation (15) by including an interaction of the average risk preference with induced signal and an interaction of these values with a measure of individual risk preference. Empirical estimates from this model are contained in column F in Table 5. We obtain the following insight from these results:

> *Result 7: There is an interaction between individual and average risk preference on observed bids. For agents with individual CARA risk posture less than one, bids are decreasing (increasing) in the average risk posture of competitors over lower (higher) ranges of signals.*

Support for result 7 is garnered by examining the marginal effect of average risk preference at the session level (Mean Risk) on realized bids. For example, in column F of Table 5, the estimated marginal effect of mean risk on realized bids is given by the following:

$$\frac{\partial bid_{NR}}{\partial Mean\_Risk} = -1.61 + 0.12 \cdot Signal - 0.01 \cdot Ind\_Risk \cdot Signal$$

which is positive for a risk-neutral agent – i.e., an agent who selects Option A in the Holt-Laury experiment for the first four choice occasions – for any signal greater than twenty four. In fact, for any agent that selects Option A fewer than nine times, this expression is positive over some range of the induced signal space.

Although our theory does not consider asymmetric agents, the observed patterns of behavior in our experimental markets are consistent with the expected impact of risk-aversion. Intuitively, a change in risk-posture of any opponent impacts an agent mainly if the two receive similar signals. Over lower ranges of the signal space, an opponent who is risk-averse will submit bids that are less than a risk-neutral counterpart. In reaction, an agent can decrease bids over lower signal ranges while keeping constant the chances of winning the auction. Over higher ranges of the signal space, the opposite result holds – risk averse agents submit bids that are greater than a risk-neutral counterpart. In reaction, an agent competing with risk-averse counterparts must increase bids when receiving higher signals to hold constant the chances of winning the auction.

V. Conclusions

Auctions are ubiquitous. Yet whether and to what extent the introduction of secondary resale markets influences bidding behavior when private values are uncertain remains largely unknown. We begin by exploring a novel data set that provides insights into the importance of the resale effect. Reduced-form empirical estimates suggest that bidding patterns are consistent with theoretical predictions. Yet, akin to many empirical exercises, the strength of inference is attenuated when one considers the set of maintained assumptions needed to generate confident conclusions from these field data.

Our approach to this problem is to make use of a laboratory experiment. Such an effort gives up much of the realism associated with field data, but it permits us to investigate whether the resale market by itself can lead to such predicted consequences.

We find that extant theory has considerable predictive power, but the accuracy of the theory is enhanced if we control for individual risk preferences. Besides their obvious importance normatively, these results have practical policy significance as well. For example, a necessary condition to lift the countervailing duty and anti-dumping ruling against Canadian softwood lumber exporters (who export to the U.S.) is that their auction markets be robust and not influenced unduly by collusion. Without a proper understanding of the resale opportunities of the various bidders, the modeler may very well earmark bidding disparities among certain bidder types as evidence of collusion when it is in fact due merely to secondary market considerations.

Table 1: Random Effects Regression Estimates: Interior SBFEP Auction Data

	Model (3II) Only Bid in Cat 1 or 2	Model (3III) Pooled Data	Model (3III) Loggers Only	Model (3III) Mills Only
Constant	4.40** (1.15)	5.65** (1.17)	5.90** (1.19)	1.55 (5.31)
Upset	0.87** (0.01)	0.82** (0.01)	0.82** (0.14)	0.74** (0.05)
LSPI	0.04** (0.01)	0.06** (0.01)	0.06** (0.01)	0.11** (0.03)
DC	-0.13** (0.05)	-0.18** (0.05)	-0.26** (0.05)	0.97** (0.20)
VPH_1000	4.07** (1.07)	5.20** (1.07)	5.02** (1.08)	8.13 (5.32)
NCV_1000	-0.02 (0.02)	-0.01 (0.02)	-0.01 (0.02)	-0.06 (0.06)
Slope	0.06** (0.02)	0.03** (0.015)	0.04** (0.02)	-0.06 (0.06)
LNVPT	1.52** (0.30)	1.85** (0.30)	1.86** (0.31)	0.22 (1.19)
Bwdn	-0.79 (1.16)	-1.93* (1.16)	-1.49 (1.17)	-10.91 (6.93)
Burn	-3.22* (1.36)	-3.86** (1.33)	-3.19** (1.40)	-11.50** (4.23)
Cy	-2.88** (0.74)	-3.20** (0.73)	-3.22** (0.78)	-1.03 (2.37)
Horse	-2.49** (0.66)	-3.93** (0.65)	-3.67** (0.66)	-10.81 (7.15)
Cycle	-0.41** (0.08)	-0.53** (0.08)	-0.53** (0.08)	-0.33 (0.43)
LNB	5.63** (0.24)	5.22** (0.24)	5.18** (0.25)	7.81** (1.29)
Util2	-2.70** (0.36)	-2.69** (0.36)	-2.62** (0.36)	-2.86** (1.49)
Category 2	1.68** (0.68)			
Buyer Random Effects	Yes	Yes	Yes	Yes
# of Firms	1245	1245	1105	140
# of Obs	5524	5524	5148	376
Log Likelihood	-20537.2	-19632.73	-18225.7	-1371.3
Predicted Bid		45.69 (16.02)	45.51 (16.19)	48.25 (13.22)

Note: Cell entries indicate marginal effect of model covariates (see text for description of covariates) on total bid level.

Table 2: Experimental Design – Laboratory Markets

Resale Structure	*Market Summary*
No Resale	*NR* 5 bidders N=30
Resale – OA Continuation: Resale price = High use value	*ROA* 5 bidders N=30
Resale – EA Continuation: Resale price = Second highest use value	*REA* 5 bidders N=30

Notes: Each cell represents one unique treatment in which we gathered data in different sessions. For example, "*NR*" in row 1, column 2, denotes that the no-resale treatment had 30 subjects in groups of 5 competing in auction markets where *ex post* resale of the commodity was prohibited. No subject participated in more than one treatment.

Table 3: Bidder Signals and Use Values (in dollars)

	Pd. 1	Pd. 2	Pd. 3	Pd. 4	Pd. 5	Pd. 6	Pd. 7	Pd. 8	Pd. 9	Pd. 10
Buyer 1	36 (29)	9 (19)	25 (26)	36 (37)	14 (13)	14 (21)	44 (39)	32 (24)	40 (33)	23 (17)
Buyer 2	17 (27)	4 (12)	14 (19)	42 (32)	36 (38)	10 (14)	25 (34)	29 (22)	32 (24)	44 (40)
Buyer 3	19 (18)	41 (36)	20 (12)	32 (29)	39 (31)	32 (39)	22 (23)	25 (17)	22 (22)	26 (34)
Buyer 4	12 (10)	34 (26)	38 (37)	26 (20)	29 (28)	34 (34)	18 (22)	29 (39)	21 (17)	23 (25)
Buyer 5	37 (36)	25 (33)	33 (34)	6 (13)	25 (21)	24 (28)	23 (14)	38 (33)	35 (40)	28 (23)

Notes: Each cell entry represents the signal received by the bidder in a given period and her induced use value (in parentheses). For example, buyer #1 received a signal of \$36.00 and an induced use value of \$29.00 in market period 1 (column 2, row 2). Each buyer received the high signal in 2 of the market periods and the high use value in 2 of the market periods. In five of the market periods (4, 5, 6, 8, and 9) we would *ex ante* predict resale exchange, as the agent who received the high signal did not receive the high induced use value.

Table 4: Mean Performance Measures—Lab Markets

	Pd 1	Pd 2	Pd 3	Pd 4	Pd 5	Pd 6	Pd 7	Pd 8	Pd 9	Pd 10
No Resale										
Avg. Bid	$20.84	$20.07	$24.31	$26.42	$27.68	$22.35	$24.80	$29.51	$28.03	$28.31
	(9.32)	(12.74)	(7.13)	(10.02)	(8.12)	(8.89)	(9.56)	(3.40)	(6.17)	(7.36)
Win Bid	$30.77	$36.63	$33.00	$35.55	$35.47	$32.23	$37.77	$34.10	$34.15	$37.25
	(3.17)	(3.07)	(3.87)	(1.58)	(0.82)	(2.64)	(1.52)	(2.14)	(1.21)	(2.27)
Resale Price	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
OA										
Avg. Bid	$26.97	$25.86	$30.24	$29.10	$31.80	$30.64	$32.47	$35.57	$33.75	$34.39
	(7.49)	(9.38)	(5.46)	(9.97)	(6.98)	(7.25)	(5.38)	(2.86)	(7.58)	(5.41)
Win Bid	$35.68	$37.18	$36.92	$36.73	$38.68	$36.55	$38.02	$37.87	$38.15	$39.30
	(4.94)	(2.55)	(3.94)	(2.47)	(1.22)	(2.06)	(1.91)	(1.52)	(1.66)	(1.08)
Resale Price	$36	$36	$37	$37	$38	$39	$39	$39	$40	$40
EA										
Avg. Bid	$24.06	$21.55	$24.11	$26.26	$26.69	$22.11	$26.51	$29.63	$26.77	$26.96
	(9.72)	(12.47)	(10.48)	(9.31)	(7.78)	(10.99)	(7.86)	(6.53)	(7.63)	(9.27)
Win Bid	$35.48	$36.25	$35.25	$36.27	$33.52	$32.00	$34.68	$36.35	$33.17	$34.67
	(2.94)	(3.97)	(4.37)	(3.34)	(1.54)	(3.74)	(3.25)	(3.48)	(1.75)	(3.22)
Resale Price	$29	$33	$34	$32	$32	$34	$34	$33	$33	$34

Note: Entries in the table provide mean performance measures across our three experimental treatments. The data are summarized by period and can be read as follows: in period 1 of the No Resale treatment the average bid was $20.84 with a standard deviation of $9.32. The average winning bid for the round was $30.77 with a standard deviation of $3.17.

Table 5: Random Effects Regression – Lab Bid Levels

	Model A	Model B	Model C	Model D	Model E	Model F
Constant	21.93** (1.02)	21.75** (1.66)	3.86** (1.42)	3.66** (1.88)	16.44** (1.68)	8.72* (4.98)
OA Treatment	5.85** (0.92)	3.91* (2.06)	5.85** (0.88)	3.91** (1.97)	13.54** (1.44)	13.06** (1.41)
EA Treatment	0.23 (0.92)	-2.42 (2.06	0.23 (0.88)	-2.42 (1.97)	6.28** (1.44)	5.48** (1.48)
Signal			0.98** (0.10)	0.98** (0.10)		
Signal2			-0.008** (0.001)	-0.008** (0.002)		
Risk_Signal					0.20** (0.02)	0.14** (0.03)
Risk_Signal2					-0.001 (0.0003)	-0.001** (0.0003)
OA_Risk_Signal					-0.06** (0.008)	-0.05** (0.008)
EA_Risk_Signal					-0.04** (0.008)	-0.04** (0.008)
Signal_MeanRisk						0.12** (0.01)
Risk_Signal_ MeanRisk						-0.01** (0.005)
Individual Risk					-2.94** (0.33)	
MeanRisk						-1.61* (0.96)
Period Effects	Yes	Yes	Yes	Yes	Yes	Yes
Session Effects	No	Yes	No	Yes	No	No
Sigma_U	2.51	2.07	2.94	2.58	4.49	3.24
Sigma_E	7.93	7.93	5.52	5.52	5.65	5.32
Log Likelihood	-3171.53	-3163.68	-2874.89	-2866.39	-2924.98	-2851.86

Note: Cell entries indicate the marginal effect of model covariates (see text for description of covariates) on recorded bid level. For example, in row 2 of Column A the estimated marginal effect of being in the OA treatment is an increase of $5.85 on bids, ceteris paribus.

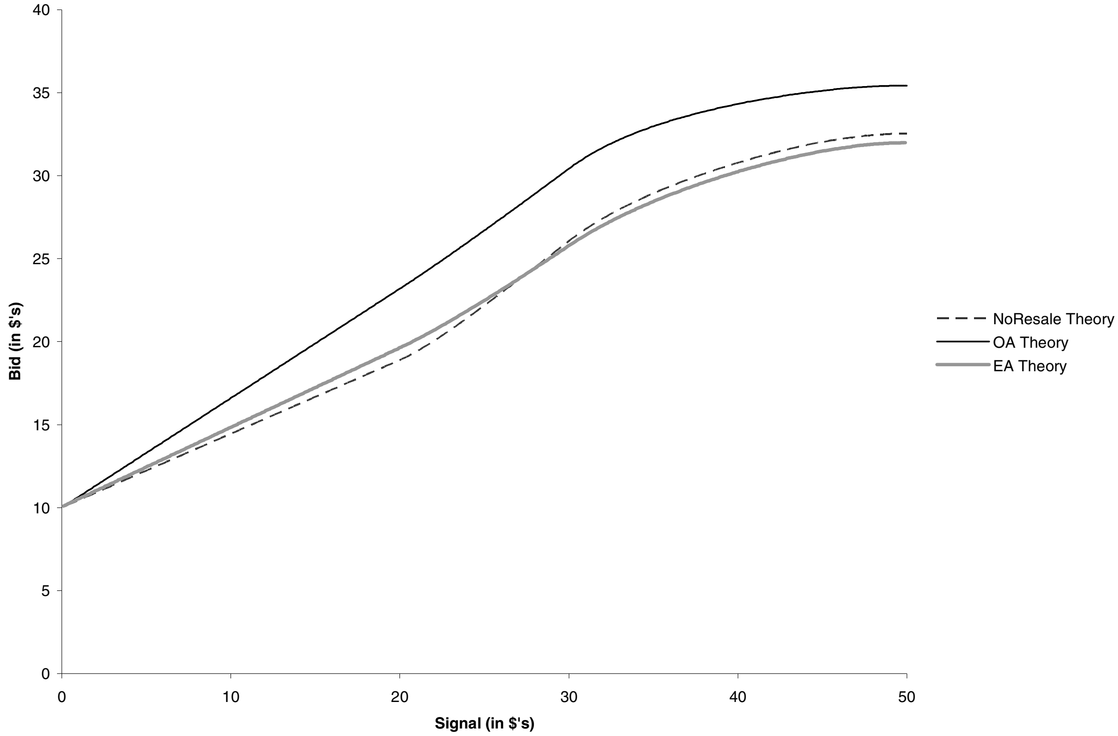

Figure 1: Risk-Neutral Predictions for Bids

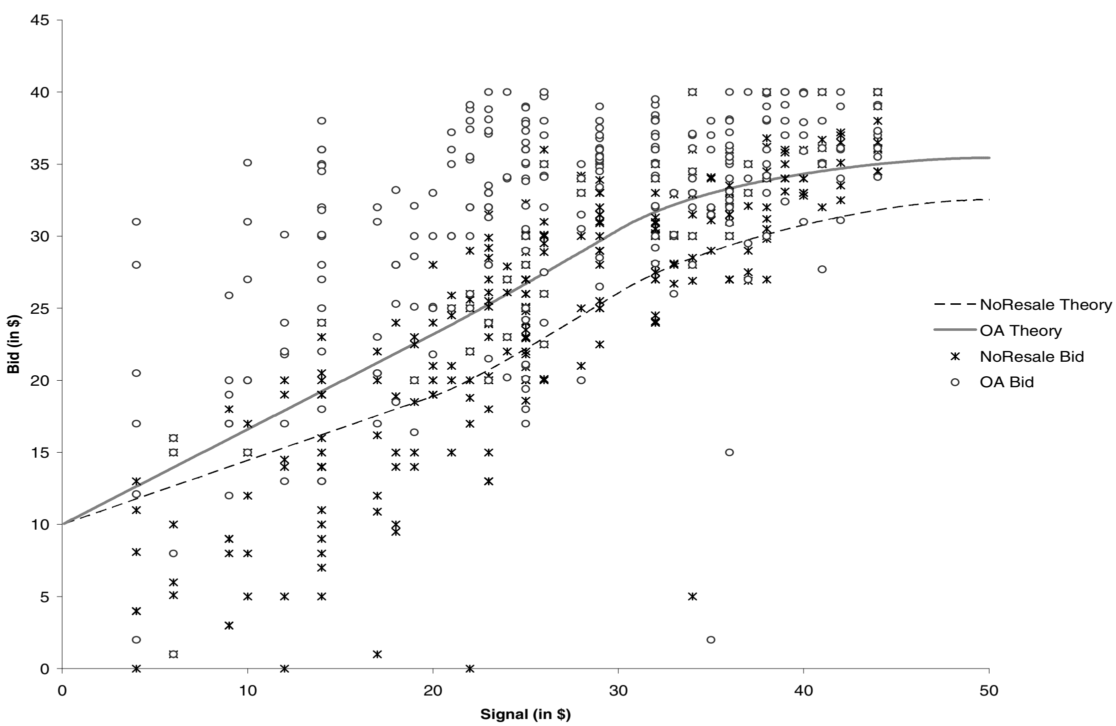

Figure 2: No Resale vs. OA Bids

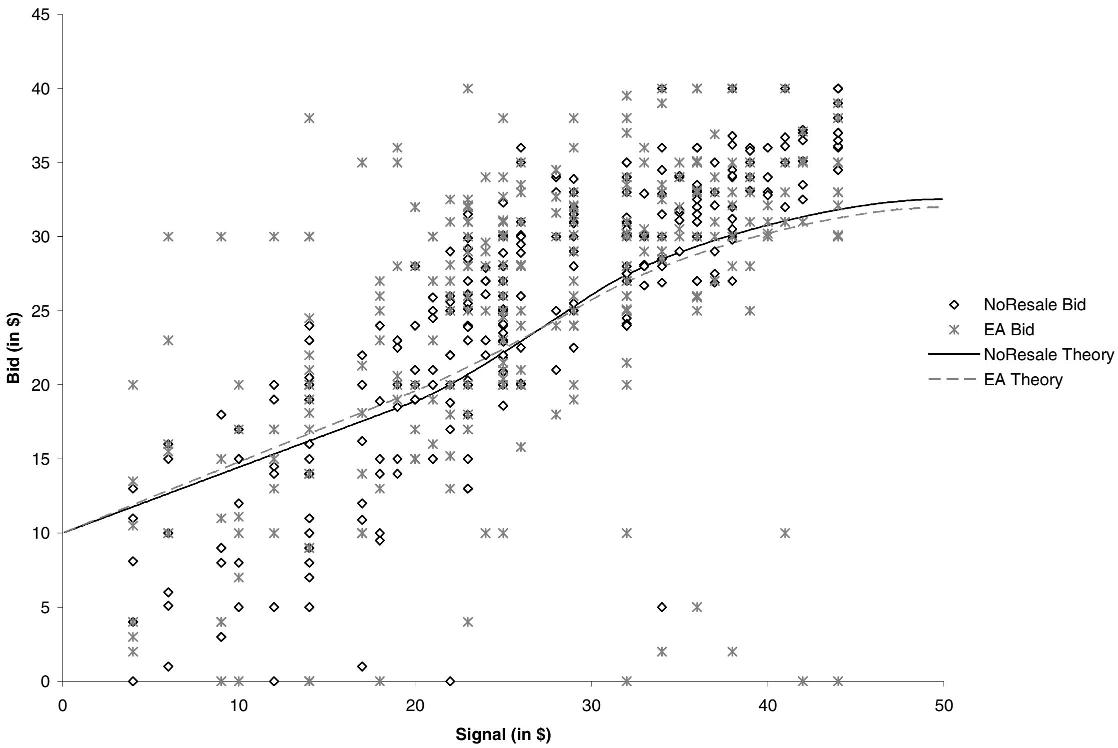

Figure 3: Bids No-Resale vs. EA Treatment

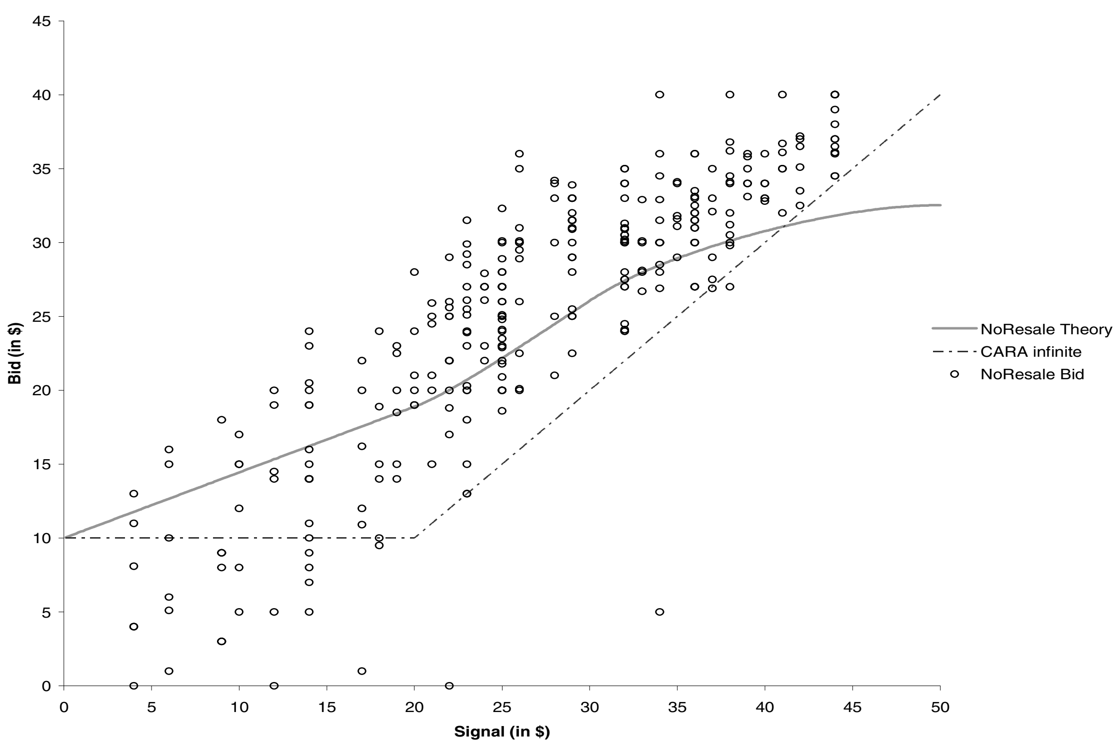

Figure 4: All Bids No-Resale Treatment

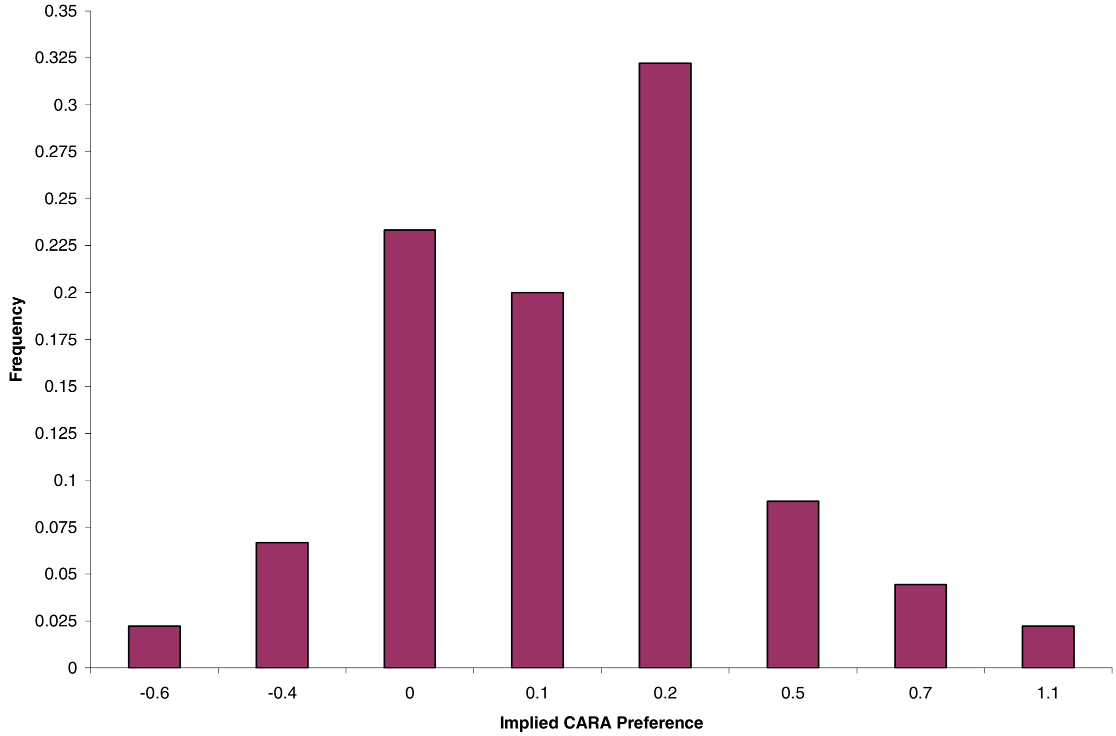

Figure 5: Frequency Distribution of CARA Preferences of Experimental Subjects

APPENDIX A. Theoretical Derivations

In the proofs, we use the following characteristic of stochastic dominance:

Lemma 1:

For any given strictly increasing function ϕ *and distributions* $H_1(u)$ *and* $H_2(u)$ *on* $u \in [u_{\min}, u_{\max}]$, where $H_1(u)$, *which stochastically dominates* $H_2(u)$, $\int_{u_{\min}}^{u_{\max}} \phi(u) dH_1(u) > \int_{u_{\min}}^{u_{\max}} \phi(u) dH_2(u)$ *whenever the expectation exists.*

Proof of Proposition 1 (Optimal bid without resale):

Given an increasing equilibrium bid function $b_N(\cdot)$, the expected utility of a player with signal x who bids $b_N(\tilde{x})$ is given by

$$\begin{aligned} &\left[\frac{1}{\rho(b_N(\tilde{x}))} \int_{x_l}^{\tilde{x}} K_N(x) dF(z)^{n-1} + (1 - F(\tilde{x})^{n-1}) \right] \rho(0) \\ &= \left[\frac{1}{\rho(b_N(\tilde{x}))} K_N(x) F(\tilde{x})^{n-1} + (1 - F(\tilde{x})^{n-1}) \right] \rho(0) \end{aligned} \quad . \tag{A1}$$

Shading by a bidder of type x leads to a tradeoff between earnings and the probability of winning the auction. Maximizing the expected utility with respect to $\tilde{x}$ and setting $\tilde{x} = x$ yields the following differential equation defining an optimal bidding function $b_N(x)$:

$$\left(\frac{1}{\rho(b_N(x))} F(x)^{n-1} \right)' = \frac{\left(F(x)^{n-1} \right)'}{K_N(x)} . \tag{A2}$$

From this expression, we obtain the following implicit definition of the optimal bidding function without resale opportunity:

$$\frac{1}{\rho(b_N(x))} = -\sigma \exp(\sigma b_N(x)) = \frac{1}{F(x)^{n-1}} \int_{x_l}^{x} \frac{1}{K_N(z)} dF(z)^{n-1} . \tag{2}$$

We must ensure that $b_N(x)$ is increasing in x and that the first-order condition describes an optimum. For the latter (with standard arguments in Haile 2003 and Milgrom and Weber 1982), it is sufficient to show that $\frac{\partial^2 EU}{\partial x \partial \tilde{x}}(x=\tilde{x})>0$ where, EU refers to the expected utility given signal x and claiming signal $\tilde{x}$.

$$
\begin{aligned}
& b_N\,'(x)>0 \\
\Leftrightarrow\; & \frac{d}{dx}\frac{1}{F(x)^{n-1}}\int_{x_l}^{x}\frac{1}{K_N(z)}dF(z)^{n-1}<0 \\
\Leftrightarrow\; & \frac{1}{K_N(x)}F(x)^{n-1}-\int_{x_l}^{x}\frac{1}{K_N(z)}dF(z)^{n-1}<0
\end{aligned}
$$

which follows since $K_N(x)$ is increasing in x (from Lemma 1).

$$
\begin{aligned}
& \frac{\partial^2 EU}{\partial x \partial \tilde{x}}(x=\tilde{x}) \\
& \qquad =\left[(\frac{F(x)^{n-1}}{\rho(b_N(x))})'\,K_N\,'(x)\right]\rho(0) \\
& \qquad =(F(x)^{n-1})'/\underbrace{K_N(x)}_{<0}\underbrace{K_N\,'(x)}_{\geq 0}\underbrace{\rho(0)}_{<0} \\
& \qquad \geq 0
\end{aligned}
$$

Optimal bid for risk-neutral players without resale - Condition (3)

$$
\lim_{\sigma\to 0} b_N(x)=\lim_{\sigma\to 0}\frac{1}{\sigma}\log\left[\int_{x_l}^{x}\frac{1}{\int\exp(-\sigma u)dG(u\mid z)}dF(z)^{n-1}\right]
$$

$$
=\lim_{\sigma\to 0}\frac{\int_{x_l}^{x}\frac{\int\exp(-\sigma u)udG(u\mid z)}{\left(\int\exp(-\sigma u)dG(u\mid z)\right)^2}dF(z)^{n-1}}{\int_{x_l}^{x}\frac{1}{\int\exp(-\sigma u)dG(u\mid z)}dF(z)^{n-1}}=\frac{1}{F(x)^{n-1}}\int_{x_l}^{x}\int udG(u\mid z)dF(z)^{n-1}
$$

Optimal bid for infinitely risk-averse players without resale:

Analogously to the limit $\sigma\to 0$,

$$
\lim_{\sigma\to\infty} b_N(x)=\lim_{\sigma\to\infty}\frac{\int_{x_l}^{x}\frac{\int\exp(-\sigma(u-u_{\min}(x)))udG(u\mid z)}{\left(\int\exp(-\sigma(u-u_{\min}(x)))dG(u\mid z)\right)^2}dF(z)^{n-1}}{\int_{x_l}^{x}\frac{1}{\int\exp(-\sigma(u-u_{\min}(x)))dG(u\mid z)}dF(z)^{n-1}}=u_{\min}(x)
$$

Lemma 2: $G_1(u \mid x, y)$ *and* $G_2(u \mid x, y)$ *are decreasing in* x *and* y.

Proof of Lemma 2:
Since $G(u \mid x)$ is decreasing in *x* by assumption, the same property follows immediately for $G_i(u \mid x, y)$, *i*=1,2. To see that $G_i(u \mid x, x)$ as defined in (6) and (9) are also decreasing in *x*,

$$\frac{\partial}{\partial y}\bar{G}_1(u \mid y)$$
$$= \frac{\partial}{\partial y}G(u \mid y)M(u \mid y)^{n-2} + G(u \mid y)(n-2)M(u \mid y)^{n-3}\frac{\partial M(u \mid y)}{\partial y}$$

where both summands are negative since $G(u \mid y)$ and $M(u \mid y)$ are decreasing in *y*.
Similarly, using $G(u \mid y) \le M(u \mid y)$:

$$\frac{\partial}{\partial y}\bar{G}_2(u \mid y)$$
$$= \frac{\partial}{\partial y}G(u \mid y)(n-2)M(u \mid y)^{n-3}(1-M(u \mid y))$$
$$+G(u \mid y)(n-2)M(u \mid y)^{n-4}\left[M(u \mid y) + G(u \mid y)(n-3) - G(u \mid y)(n-2)M(u \mid y)\right]\frac{\partial M(u \mid y)}{\partial y}$$
$$\le \frac{\partial}{\partial y}G(u \mid y)(n-2)M(u \mid y)^{n-3}(1-M(u \mid y))$$
$$+G(u \mid y)(n-2)^2 M(u \mid y)^{n-4}G(u \mid y)\left[1-M(u \mid y)\right]\frac{\partial M(u \mid y)}{\partial y}$$
$$\le 0$$

Lemma 3: $\dfrac{\bar{G}_2(u \mid x)}{\bar{G}_1(u \mid x)}$ is decreasing in u.

Proof: Using the definitions in (5) and (10), we obtain

$$\frac{\bar{G}_2(u \mid x)}{\bar{G}_1(u \mid x)} = \frac{M(u \mid x)^{n-2} + (n-2)G(u \mid x)M(u \mid x)^{n-3}\left[1-M(u \mid x)\right]}{G(u \mid x)M(u \mid x)^{n-2}}$$
$$= \frac{1}{G(u \mid x)} + \frac{(n-2)}{M(u \mid x)} - (n-2)$$

where the first two terms are decreasing in u.

Lemma 4: $\dfrac{\bar{G}_1(u \mid x)}{\int_{u_l}^{u} \rho'(-z)\,d\bar{G}_1(z \mid x)}$ is decreasing in u.

Proof:

$$\frac{d}{du}\frac{\bar{G}_1(u \mid x)}{\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z \mid x)} < 0$$

$$\Leftrightarrow \frac{d}{du}\bar{G}_1(u \mid x)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z \mid x) - \bar{G}_1(u \mid x)\rho'(-u)\frac{d}{du}\bar{G}_1(u \mid x)$$

$$= \frac{d}{du}\bar{G}_1(u \mid x)\left[\int_{u_l}^{u}\underbrace{\rho'(-z)}_{<\rho'(-u)}d\bar{G}_1(z \mid x) - \bar{G}_1(u \mid x)\rho'(-u)\right] < 0$$

Lemma 5: Let $\phi_1(u)$ and $\phi_2(u)$ be positive and decreasing functions and $\mu(u)$ a positive function on $u \in [u_{\min}, u_{\max}]$. Then $\int_{u_{\min}}^{u_{\max}}\phi_1(u)\phi_2(u)\mu(u)du\int_{u_{\min}}^{u_{\max}}\mu(u)du > \int_{u_{\min}}^{u_{\max}}\phi_1(u)\mu(u)du\int_{u_{\min}}^{u_{\max}}\phi_2(u)\mu(u)du$ *whenever the expectations exist.*

Proof: Define $\bar{\mu}(u) = \mu(u)/\int_{u_{\min}}^{u_{\max}}\mu(z)dz$ and let $\tilde{u}$ solve $\phi_2(\tilde{u}) = \int_{u_{\min}}^{u_{\max}}\phi_2(z)\bar{\mu}(z)dz$**.** Then we have

$$\int_{u_{\min}}^{u_{\max}}\phi_1(u)\phi_2(u)\bar{\mu}(u)du - \int_{u_{\min}}^{u_{\max}}\phi_1(u)\bar{\mu}(u)du\int_{u_{\min}}^{u_{\max}}\phi_2(u)\bar{\mu}(u)du$$

$$= \int_{u_{\min}}^{u_{\max}}\phi_1(u)\bar{\mu}(u)\underbrace{\left[\phi_2(u) - \int_{u_{\min}}^{u_{\max}}\phi_2(z)\bar{\mu}(z)dz\right]}_{>0\Leftrightarrow u<\tilde{u}}du$$

$$\geq \int_{u_{\min}}^{u_{\max}}\phi_1(\tilde{u})\bar{\mu}(u)\underbrace{\left[\phi_2(u) - \int_{u_{\min}}^{u_{\max}}\phi_2(z)\bar{\mu}(z)dz\right]}_{>0\Leftrightarrow u<\tilde{u}}du = 0$$

Lemma 6: $\frac{K_{OA}(x,x)}{L_{OA}(x,x)}$ *is increasing in x. Given (A1),* $\frac{K_{EA}(x,x)}{L_{EA}(x,x)}$ *is also increasing in x.*

Proof: From Lemma 1 and 2, we know that $K_R(x,y)$ is increasing in both arguments, whereas $L_R(x,y)$ is constant for *R=OA* and decreases in *x* but increases in *y* for *R=EA*. It remains to show that $\frac{\partial}{\partial x}\frac{K_{EA}(x,y)}{L_{EA}(x,y)}\Big|_{y=x} \geq 0$.

For this, we consider the derivative with respect to x and show that $\frac{\partial}{\partial x}K_{EA}(x,x)L_{EA}(x,x)-K_{EA}(x,x)\frac{\partial}{\partial x}L_{EA}(x,x)\geq 0$. Using the definitions of K_{EA} and L_{EA} in (7), (8), and (11), we have:

$$K_{EA}(x,y)=\int_{u_l}^{u_u}\rho(u)dG_2(u\mid x,y)=\rho(u_{\max}(x))-\int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\mid x)\bar{G}_2(u\mid y)du$$

$$\frac{\partial}{\partial x}K_{EA}(x,x)=-\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\mid x)\bar{G}_2(u\mid x)du$$

$$L_{EA}(x,y)=\int_{u_l}^{u_u}\left[\rho'(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid y)+(1-\bar{G}_1(u\mid y))\right]dG(u\mid x)$$

$$=\rho'(u_{\max}(x))\int_{u_l}^{u_u}\rho'(-z)d\bar{G}_1(z\mid y)-\int_{u_l}^{u_{\max}(x)}\left[\rho''(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid y)\right]G(u\mid x)du$$

$$\frac{\partial}{\partial x}L_{EA}(x,x)=-\int_{u_l}^{u_{\max}(x)}\left[\rho''(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid x)\right]\frac{\partial}{\partial x}G(u\mid x)du$$

(A3)

and, therefore,

$$\frac{\partial}{\partial x}K_{EA}(x,x)L_{EA}(x,x)-K_{EA}(x,x)\frac{\partial}{\partial x}L_{EA}(x,x)$$
$$=-\rho'(u_{\max}(x))\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\mid x)\left[\bar{G}_2(u\mid x)\int_{u_l}^{u_u}\rho'(-z)d\bar{G}_1(z\mid x)-\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid x)\right]du$$
$$-\sigma\left[\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\mid x)\bar{G}_2(u\mid x)du\right]\left[\int_{u_l}^{u_{\max}(x)}\left[\rho'(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid x)\right]G(u\mid x)du\right]$$
$$+\sigma\left[\int_{u_l}^{u_{\max}(x)}\rho'(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid x)\frac{\partial}{\partial x}G(u\mid x)du\right]\left[\int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\mid x)\bar{G}_2(u\mid x)du\right]$$

(A4)

We can apply Lemma 5 to the last two summands with $\mu(u)=\rho'(u)\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid y)G(u\mid x)$, $\phi_1(u)=\frac{\bar{G}_2(u\mid x)}{\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\mid x)}$ and

$\phi_2(u)=-\frac{\frac{\partial}{\partial x}G(u\mid x)}{G(u\mid x)}$, where we use (A1), Lemma 3, and Lemma 4 to guarantee the required properties Lemma 5. We then obtain:

$$\frac{\partial}{\partial x}K_{EA}(x,x)L_{EA}(x,x)-K_{EA}(x,x)\frac{\partial}{\partial x}L_{EA}(x,x)$$

$$\geq -\rho'(u_{\max}(x))\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\underbrace{\left[\bar{G}_2(u\,|\,x)\int_{u_l}^{u_u}\rho'(-z)d\bar{G}_1(z\,|\,x)-\int_{u_l}^{u}\rho'(-z)d\bar{G}_1(z\,|\,x)\right]}_{>0\text{ (Lemma 3,4)}}du$$

$$\geq 0$$

Lemma 7: $\dfrac{\int_{x_l}^{x}\frac{\partial K_{EA}}{\partial x}(x,z)dF(z)^{n-1}}{\int_{x_l}^{x}K_{EA}(x,z)dF(z)^{n-1}}\leq\dfrac{\frac{\partial K_{EA}}{\partial x}(x,x)}{K_{EA}(x,x)}<0$

Proof: In the proof of Lemma 6, equation (A3), we have shown that

$$K_{EA}(x,z)=\rho(u_{\max}(x))-\int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\bar{G}_2(u\,|\,z)du$$

$$\frac{\partial}{\partial x}K_{EA}(x,z)=-\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\bar{G}_2(u\,|\,z)du$$

which implies

$$(1/F(x)^{n-1})\int_{x_l}^{x}\frac{\partial K_{EA}}{\partial x}(x,z)dF(z)^{n-1}=-\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\hat{G}_2(u\,|\,x)du$$

$$(1/F(x)^{n-1})\int_{x_l}^{x}K_{EA}(x,z)dF(z)^{n-1}=\rho(u_{\max}(x))-\int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\hat{G}_2(u\,|\,x)du$$

where $\hat{G}_2(u\,|\,x)=(1//F(x)^{n-1})\int_{x_l}^{x}\bar{G}_2(u\,|\,z)dF(z)^{n-1}$ denotes the distribution of the second highest use value of an opponent given that the maximal signal of an opponent does not exceed x. This distribution is given by

$$\hat{G}_2(u\,|\,x)=(n-1)M(u\,|\,x)^{n-2}-(n-2)M(u\,|\,x)^{n-1}$$

To show the claim, note first that $\hat{G}_2(u\,|\,x)\geq\bar{G}_2(u\,|\,x)$ and, in addition

$$\frac{\partial}{\partial u}\frac{\bar{G}_2(u\,|\,x)}{\hat{G}_2(u\,|\,x)} = \frac{\partial}{\partial u}\frac{1+(n-2)\dfrac{G}{M}(1-M)}{n-1-(n-2)M}$$

$$= \frac{(n-2)(1-M)}{n-1-(n-2)M}\underbrace{\frac{\partial(G/M)}{\partial u}}_{\geq 0} - \underbrace{\frac{(n-2)(G/M-1)}{[(n-1)-(n-2)M(u\,|\,x)]^2}}_{\leq 0}\frac{\partial M}{\partial u}$$

$$\geq 0$$

We therefore obtain:

$$-\rho(u_{\max}(x))\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\hat{G}_2(u\,|\,x)du \leq -\rho(u_{\max}(x))\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\bar{G}_2(u\,|\,x)du$$

Further, we can apply Lemma 5 with $\mu(u) = \rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\bar{G}_2(u\,|\,x)$, $\phi_1(u) = -\frac{\partial}{\partial x}G(u\,|\,x)/G(u\,|\,x)$ (decreasing by assumption (A1)), and $\phi_2(u) = \hat{G}_2(u\,|\,x)/\bar{G}_2(u\,|\,x)$ (decreasing as shown above)to show that

$$\int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\bar{G}_2(u\,|\,x)du\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\hat{G}_2(u\,|\,x)du$$

$$\leq \int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\hat{G}_2(u\,|\,x)du\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\bar{G}_2(u\,|\,x)du$$

We therefore obtain:

$$K_{EA}(x,x)(1/F(x)^{n-1})\int_{x_l}^{x}\frac{\partial K_{EA}}{\partial x}(x,z)dF(z)^{n-1}$$

$$= -\left[\rho(u_{\max}(x)) - \int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\bar{G}_2(u\,|\,x)du\right]\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\hat{G}_2(u\,|\,x)du$$

$$\leq -\left[\rho(u_{\max}(x)) - \int_{u_l}^{u_{\max}(x)}\rho'(u)G(u\,|\,x)\hat{G}_2(u\,|\,x)du\right]\int_{u_l}^{u_{\max}(x)}\rho'(u)\frac{\partial}{\partial x}G(u\,|\,x)\bar{G}_2(u\,|\,x)du$$

$$= (1/F(x)^{n-1})\int_{x_l}^{x}K_{EA}(x,z)dF(z)^{n-1}\frac{\partial K_{EA}}{\partial x}(x,x)$$

which immediately implies the claimed relationship.

Proof of Proposition 2 (Optimal bid function with resale)

Assuming an increasing bid function $b_R(\cdot)$, the expected utility of a player with signal x who bids $b_R(\tilde{x})$ can be written as

$$\left[\frac{1}{\rho(b_R(\tilde{x}))}\int_{x_l}^{\tilde{x}} K_R(x,z)dF(z)^{n-1} + \int_{\tilde{x}}^{x_u} L_R(x,z)dF(z)^{n-1}\right]\rho(0)\,. \tag{A5}$$

Differentiating with respect to $\tilde{x}$ and setting $\tilde{x}=x$ leads to a differential equation for the optimal bidding function $b_R(x)$:

$$\left(\frac{1}{\rho(b_R(x))}\right)'\int_{x_l}^{x} K_R(x,z)dF(z)^{n-1} + \frac{1}{\rho(b_R(x))}K_R(x,x)(F(x)^{n-1})' = L_R(x,x)(F(x)^{n-1})', \tag{A6}$$

which reduces to the following linear equation:

$$\left(\frac{1}{\rho(b_R(x))}\right)' + \frac{1}{\rho(b_R(x))}H_1(x) = H_2(x), \tag{A7}$$

where $H_1(x)$ is given by

$$H_1(x) = \frac{K_R(x,x)(F(x)^{n-1})'}{\int_{x_l}^{x} K_R(x,z)dF(z)^{n-1}} \tag{A8}$$

and $H_2(x)$ is given by

$$H_2(x) = \frac{(F(x)^{n-1})' L_R(x,x)}{\int_{x_l}^{x} K_R(x,z)dF(z)^{n-1}}\,. \tag{A9}$$

By a standard solution we thus obtain that an optimal bidding function $b_R(x)$ is given by

$$\frac{1}{\rho(b_R(x))} = \left[\int_{x_l}^{x} H_2(z)\exp\left(\int_{x}^{z} H_1(y)dy\right)dz\right] + c_1\exp\left(-\int_{x_l}^{x} H_1(z)dz\right) \tag{A10}$$

for some constant c_1. Noting that $\rho(u_l) \le K_R(x,y) \le \rho(u_u)$, there exists a constant c_2 such that $\int_{x_l}^{x} H_1(z)dz \le c_2 \int_{x_l}^{x} F(x)'/F(x)dz = c_2[\ln(F(x) - \ln(F(x_l)] = -\infty$. The second summand in (A10) therefore vanishes and we arrive at the implicit definition for $b_R(x)$:

$$\frac{1}{\rho(b_R(x))} = \left[\int_{x_l}^{x} \frac{\exp\left(\int_{x}^{z} K_R(y,y) / \int_{x_l}^{y} K_R(y,w) dF(w)^{n-1} dF(y)^{n-1} \right)}{\int_{x_l}^{z} K_R(z,y) dF(y)^{n-1}} L_R(z,z) dF(z)^{n-1} \right]. \tag{12}$$

To show that $b_R(x)$ is the unique differentiable symmetric separating equilibrium, we again have to show that

$b_R\,'(x) > 0$ and $\frac{\partial^2 EU}{\partial x \partial \tilde{x}}(x = \tilde{x}) \ge 0$. We have:

$$b_R\,'(x) > 0 \Leftrightarrow \left(\frac{1}{\rho(b_R(x))}\right)' < 0 \Leftrightarrow L_R(x,x) - \frac{K_R(x,x)}{\rho(b_R(x))} > 0$$

From Lemma 6 we know that $\frac{K_R(x,x)}{L_R(x,x)}$ is increasing in *x*. We therefore obtain:

$$\begin{aligned}
&\frac{K_R(x,x)}{\rho(b_R(x))} \\
&\le \int_{x_l}^{x} \exp\left(\int_{x}^{z} K_R(y,y) / \int_{x_l}^{y} K_R(y,w) dF(w)^{n-1} dF(y)^{n-1} \right) \frac{K_R(z,z)(F(z)^{n-1})'}{\int_{x_l}^{z} K_R(z,y) dF(y)^{n-1}} L_R(x,x) dz \\
&= L_R(x,x) \left[1 - \exp\left(\int_{x}^{x_l} K_R(y,y) / \int_{x_l}^{y} K_R(y,w) dF(w)^{n-1} dF(y)^{n-1} \right) \right] \\
&< L_R(x,x)
\end{aligned}$$

Further, we have to show:

$$\frac{\partial^2 EU}{\partial x \partial \tilde{x}}(x=\tilde{x})$$

$$= \left[\left(\frac{1}{\rho(b_R(x))}\right)' \underbrace{\int_{x_l}^{x} \frac{\partial K_R}{\partial x}(x,z) dF(z)^{n-1}}_{>0} + \left[\frac{1}{\rho(b_R(x))} \frac{\partial K_R}{\partial x}(x,x) - \frac{\partial L_R}{\partial x}(x,x) \right] (F(x)^{n-1})' \right] \underbrace{\rho(0)}_{<0}$$

$$> 0$$

***Optimal auction* (R=OA)**

The claimed relationship follows immediately as we have already shown that $\left(\frac{1}{\rho(b_R(x))}\right)' < 0$ and further that $\frac{1}{\rho(b_R(x))} \frac{\partial K_{OA}}{\partial x}(x,x) \leq 0$ and $\frac{\partial L_{OA}}{\partial x}(x,x) = 0$.

***English auction* (R=EA)**

Using (A6), we have to show that

$$\frac{\int_{x_l}^{x} \frac{\partial K_{EA}}{\partial x}(x,z) dF(z)^{n-1}}{\int_{x_l}^{x} K_{EA}(x,z) dF(z)^{n-1}} \underbrace{\left[\frac{K_{EA}(x,x)}{\rho(b_{EA}(x))} - L_{EA}(x,x) \right]}_{<0} > \frac{1}{\rho(b_R(x))} \frac{\partial K_{EA}}{\partial x}(x,x) - \frac{\partial L_{EA}}{\partial x}(x,x).$$

Using Lemma 7 we obtain:

$$\frac{\int_{x_l}^{x} \frac{\partial K_{EA}}{\partial x}(x,z) dF(z)^{n-1}}{\int_{x_l}^{x} K_{EA}(x,z) dF(z)^{n-1}} \underbrace{\left[\frac{K_{EA}(x,x)}{\rho(b_{EA}(x))} - L_{EA}(x,x) \right]}_{<0} - \left[\frac{1}{\rho(b_{EA}(x))} \frac{\partial K_{EA}}{\partial x}(x,x) - \frac{\partial L_{EA}}{\partial x}(x,x) \right]$$

$$\geq -L_{EA}(x,x) \frac{\frac{\partial K_{EA}}{\partial x}(x,x)}{K_{EA}(x,x)} + \frac{\partial L_{EA}}{\partial x}(x,x)$$

$$\geq 0$$

where the last inequality was proven in Lemma 6.

Optimal bids with resale for extreme CARA-levels : $\lim_{\sigma \to 0} b_R(x)$ and $\lim_{\sigma \to \infty} b_R(x)$:

Note that $\exp\left(\int_{x}^{z} K_R(y,y) / \int_{x_l}^{y} K_R(y,w) dF(w)^{n-1} dF(y)^{n-1} \right) \to \frac{F(z)^{n-1}}{F(x)^{n-1}}$ for $\sigma \to 0$. The proof is therefore analogous to that of the no-resale case (see above).

$$\lim_{\sigma\to 0} b_R(x)$$

$$= \lim_{\sigma\to 0} \frac{\int\limits_{x_l}^{x} \frac{F(z)^{n-1}}{F(x)^{n-1}} \frac{\frac{\partial}{\partial\sigma} L_R(z,z) \int\limits_{u_l}^{z} (-\sigma) K_R(z,y) dF(y)^{n-1} - L_R(z,z) \int\limits_{u_l}^{z} \frac{\partial}{\partial\sigma} ((-\sigma) K_R(z,y)) dF(y)^{n-1}}{\left((-\sigma) \int\limits_{u_l}^{z} K_R(z,y) dF(y)^{n-1} \right)^2} dF(z)^{n-1}}{\int\limits_{x_l}^{x} \frac{F(z)^{n-1}}{F(x)^{n-1}} \frac{L_R(z,z)}{(-\sigma) \int\limits_{u_l}^{z} K_R(z,y) dF(y)^{n-1}} dF(z)^{n-1}}$$

$$= \int\limits_{x_l}^{x} \frac{1}{F(x)^{n-1}} \frac{\left[\lim_{\sigma\to 0} \frac{\partial}{\partial\sigma} L_R(z,z) \right] F(z)^{n-1} - \int\limits_{u_l}^{z} \left[\lim_{\sigma\to 0} \frac{\partial}{\partial\sigma} (-\sigma) K_R(z,y) \right] dF(y)^{n-1}}{F(z)^{n-1}} dF(z)^{n-1}$$

from which one obtains the claimed relationships for R=*OA* and R=*EA*. The proof for $\sigma \to \infty$ is follows a similar line.

Appendix B: Experimental Instructions – OA Resale Treatment

Welcome to Lister's Auctions! You have the opportunity to bid in a series of experimental auctions today and you can earn cash by participating.

Auction Rules:
In this auction you will bid against four (4) other people and the person with the highest bid is the winner, and pays the amount of their bid for the "fictitious" commodity. The auction is a sealed bid auction so you don't know the bids of the other participants. We will repeat the auction for 10 rounds. At the end of the session, your earnings from this experiment and another unrelated experiment will be summed and paid to you in cash.

There are six steps in the auction process, each of which is explained in detail below. The six steps include: (i) determining your signal of the value of the fictitious commodity, (ii) determining your bid, (iii) determining your use value for the fictitious good, (iv) determining the winner, (v) the resale market, and (vi) determining your payouts for the round.

1. **Determining your signal of the good's value:** At the beginning of each period, a monitor will hand you a card numbered from zero dollars ($0) to fifty dollars ($50) in one dollar ($1) increments. The value on the card handed to you will be a signal of your use value for the fictitious good. The other bidders in your auction will have their signals determined in exactly the same way. Signals are private and independent across buyers, and your signal will change across rounds.

    Signals and Use Values

    Use values, V, in each round are drawn from a uniform distribution on the interval [10, 40]. That is, every dollar value between 10 and 40 is equally likely to be drawn as your use value. These values are independently drawn for each subject and will differ across periods.

    The signal you will receive is determined by adding a random number drawn on the interval [-10, 10] to your use value. Again, each dollar value between -10 and 10 is equally likely to be drawn and added to your use value. Your first-stage signal, S, is hence given by:

$$S = V + random\ number$$

    Your signal, S, is thus distributed on the interval [$0, $50].

    Given your signal, you can compute the expected use value. For example, if you were to receive a signal of $30 in the first stage, you know that your final use value must lie somewhere in the interval [$20, $40]. Since each of these values is equally likely to have been selected as your use value, on average your use value is $30. However, any value in this range could have been assigned as your use value.

2. Determining your bid value: After receiving your signal, you will choose your bid value for the fictitious good. In order to choose your bid, consider how your earnings for each period are calculated. If you are the person with the highest bid you are the winner of the auction. Your earnings are equal to your use value minus your bid amount if you have the highest end use value:

Earnings = your good's use value (V) – your bid

If you are the person with the highest bid but do not have the highest use value, your earnings are equal to the highest use value of all participants minus your bid amount:

Earnings = highest use value – your bid

If you are not the high bidder in a round, your earnings for the period are zero. If there is a tie, the winner will be determined by the flip of a coin (if more than two people tie we will draw a card to determine the winner). Your bid can be any amount in the range from zero ($0) to forty dollars ($40) in ten cent ($0.10) increments.

3. Determining your use value: Once all bids have been received, a monitor will hand you a second slip of paper numbered from ten dollars ($10) to forty dollars ($40) that gives your final use value, V. Your use value will lie within $\pm$$10 of your signal, S.

4. Determining the auction winner: All bids will be publicly announced and recorded by a monitor on the blackboard. Your bid will be compared with those of the four other participants in the auction. The person with the highest bid amount is the winner.

5. The resale market: In the resale market, each participant can see the use values for all other participants. The highest bidder in the auction market will sell the "fictitious" commodity to the individual with the highest use value. In this experiment this happens automatically. The payoff for the winner is the highest use value of all participants minus his/her bid amount. If you did not win the auction, your payout for the period will be zero. The payout for the auction winner can be positive even if your bid was greater than your use value.

6. Determining your payouts: If you are the auction winner, you will receive the difference between the highest use value and your bid. If you did not win the auction, you receive zero for that period. Your total earnings for this experiment are the sum of your earnings for each of the 10 periods.

Do you have any questions about the auction process?

Appendix C: Experimental Instructions for Risk Aversion Experiment

Record your subject number from the previous part on your decision sheet. Your decision sheet shows ten decisions listed on the left. Each decision is a paired choice between OPTION A and OPTION B. You will make ten choices and record these in the final column, but only one of them will be used in the end to determine your earnings. Before you start making your ten choices, please let me explain how these choices will affect your earnings for this part of the experiment.

We will use part of a deck of cards to determine payoffs; cards 2-10 and the Ace will represent "1". After you have made all of your choices, we will randomly select a card twice, once to select one of the ten decisions to be used, and a second time to determine what your payoff is for the option you chose, A or B, for the particular decision selected. (After the first card is selected, it will be put back in the pile, the deck will be reshuffled, and the second card will be drawn.) Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used. Obviously, each decision has an equal chance of being used in the end.

Now, please look at Decision 1 at the top. OPTION A pays \$2.00 if the Ace is selected, and it pays \$1.60 if the card selected is 2-10. OPTION B yields \$3.85 if the Ace is selected, and it pays \$0.10 if the card selected is 2-10. The other decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, the cards will not be needed since each option pays the highest payoff for sure, so your choice here is between \$2.00 or \$3.85.

To summarize, you will make ten choices: for each decision row you will have to choose between OPTION A and OPTION B. You may choose A for some decision rows and B for other rows, and you may change your decisions and make them in any order. When you are finished, we will come to your desk and pick a card to determine which of the ten decisions will be used. Then we will put the card back in the deck, shuffle, and select a card again to determine your money earnings for the OPTION you chose for that decision. Earnings for this choice will be added to your previous earnings, and you will be paid all earnings in cash when we finish.

So now please look at the empty boxes on the right side of the record sheet. You will have to write a decision, A or B in each of these boxes, and then the card selection will determine which one is going to count. We will look at the decision that you made for the choice that counts, and circle it, before selecting a card again to determine your earnings for this part. Then you will write your earnings in the blank at the bottom of the page.

Are there any questions? Now you may begin making your choices. Please do not talk with anyone else while we are doing this; raise your hand if you have a question.

Chapter 4:

Using Tontines to Finance Public Goods: Back to the Future?[71]

> ***tontine:*** *An annuity scheme wherein participants share certain benefits and on the death of any participant his benefits are redistributed among the remaining participants; can run for a fixed period of time or until the death of all but one participant.* Webster's Online Dictionary

I. Introduction

The oldest standing bridge in London (Richmond Bridge), numerous public buildings and other municipality projects throughout the U.S., Britain, the Netherlands, Ireland, and France, and several wars, including the Nine Years' War, all share a common thread: they were wholly, or partially, funded by tontines. The idea of the tontine is believed to have originated in 1652, when an expatriate banker, Lorenzo Tonti, proposed a new mechanism for raising public funds to Cardinal Mazarin of France.[72] Tonti advertised his idea as "A gold mine for the king….a treasure hidden away from the realm." The salesmanship of Tonti coupled with the difficulties associated with raising taxes in seventeenth century France led to an enthusiastic endorsement from King Louis XIV. While the idea, and many affiliated derivatives, prospered as major tools for financing public goods for several decades, tontines have since been banned in Britain

[71] This essay was written with Andreas Lange and John List.

[72] Similar mechanisms are believed to have been employed in the Roman Empire several centuries earlier. Tonti's mechanism should not be confused with the *tontines* in Western Africa, which are small, informal savings and loan associations similar to ROSCAs (Rotating Savings and Credit Associations).

and the United States due to the potential incentive for investors to kill one another in order to increase their shares.[73]

In essence, a tontine is a mixture of group annuity, group life insurance, and lottery. While the use and economic operation of each of these components is understood as a vehicle for individual investment/leisure, as a means to fund public goods, the tontine itself has largely been ignored. It is well established that relying upon voluntary contributions for the provision of public goods generally results in the under provision of such goods relative to first-best levels. Numerous mechanisms have been proposed to alleviate the tendency of agents to free-ride (see e.g., Groves and Ledyard 1977; Walker 1981; Bagnoli and McKee 1991; Varian 1994; Falkinger 1996).

This study adds to the literature on voluntary provision of public goods by formally investigating the performance and optimal design of the tontine as a fundraising mechanism for private charities. It is not our purpose to provide a theoretical model of the tontine as a mechanism to finance government debt or to provide a lifetime annuity for subscribers. Rather, our purpose is to provide a model of a variant of the historical tontine that can be used by private charities to finance the provision of public goods. In this spirit, we provide information about the history and modeling results of tontines in order to encourage usage of the best characteristics of the institution in the future. We begin by outlining the conditions that define an optimal tontine—one that maximizes total group contribution levels—when symmetric risk-neutral agents have quasi-linear preferences. Properties of tontines are also explored upon relaxation of symmetry and

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[73] As an aside, this allure of the tontine has led to a fantastic plot device for detective story writers (the interested reader should see, e.g., *The Wrong Box* by Robert Louis Stevenson, which was made into a film in 1966 starring Peter Cook, Dudley Moore, Ralph Richardson, Michael Caine, and Tony Hancock).

risk neutrality. We then compare the performance of the tontine to a popular fundraising scheme used today: lotteries (see, e.g., Morgan 2000 and Lange et al. 2004).[74]

Our main findings are as follows: (i) the optimal for tontine for agents with identical valuations of the public good consists of all agents receiving a fixed "prize" amount in the first period equal to a percentage of their total contribution, (ii) contribution levels in this optimal tontine are identical to those of risk-neutral agents in an equivalently valued single prize lottery, (iii) contribution levels for the optimal tontine are independent of risk-aversion, and (iv) with sufficient, and plausible, risk-aversion or asymmetry in individual valuations of the public good, tontines yield higher contributions than the optimal lottery. Further, one can obtain full participation in the tontine mechanism compared to partial participation in the lottery mechanism. These results have clear implications for empiricists and practitioners in the design of private fundraising campaigns. Further, they provide useful avenues for future theoretical work on voluntary provisioning of public goods.

We test our theoretical conjectures in a controlled laboratory experiment. The laboratory experiment consists of two parts. The first part of the experiment employs a series of experimental treatments to examine the contribution decisions of agents across a number of settings. These treatments compare the outcomes of the voluntary contribution mechanism (VCM), the single fixed-prize lottery (SPL) and the optimal tontine for agents who have symmetric valuations for the public good, but who may differ in revealed risk preference. The second part of the experiment was designed to

[74] Relatedly, Engers and McManus (2002) and Goeree et al. (2004) explore the use of auctions to raise money to finance public goods, and Andreoni (1998) and List and Lucking-Reiley (2002) explore the voluntary contributions mechanism with and without announcements of "seed" money.

lend insights into subjects' risk postures using the Holt and Laury (2002) paired lottery choice design and link those preferences to behavior in the public goods game.

The experimental results provide mixed support for our theoretical model. First, in contrast with our theoretical predictions, we find that the single prize lottery is a superior fundraising mechanism to the optimal tontine. Contributions in the lottery treatment are approximately 42% greater than those recorded in the tontine treatment. However, as predicted by theory, contributions in the optimal tontine are independent of individual risk-aversion whereas contributions in the lottery treatment are strictly decreasing in the level of individual risk. Furthermore, contributions in the optimal tontine exceed those of an equally valued VCM. Finally, we find that free-riding is more likely in the lottery treatment than in the optimal tontine.

The remainder of our study is crafted as follows. Section II provides a brief historical overview of tontines. Section III describes a theoretical model of the tontine and compares the performance of an optimal tontine with that of lotteries. Section IV summarizes the experimental design and results. Section IV concludes.

II. Tontines throughout History

Lorenzo Tonti was a Neapolitan of little distinction until his sponsor, Cardinal Mazarin of France, who was responsible for the financial health of France, supported his position in the court of the French King in the 1650s. In this position, Tonti proposed a form of a life contingent annuity with survivorship benefits, whereby subscribers, who were grouped into different age classes, would make a one-time payment of 300 livres to the government. Each year, the government would make a payment to each group equaling five percent of the total capital contributed by that group. These payments would be distributed among the surviving group members based upon each agent's share of total group contributions. The government's debt obligation would cease with the death of the last member of each group. Although the plan was supported enthusiastically by Louis XIV, Tonti's plan was rejected by the French Parliament for two reasons: (i) the uncertain nature of total government debt obligations and (ii) the proposed rate of return was low in comparison with rates on life annuities (Weir, 1989). While the Netherlands started a successful tontine in 1670, it was not until 1689, when France was engaged in the Nine Years' War, that France offered its first national tontine. The design was quite similar to that originally proposed by Tonti. Later offerings in France coincided with peaks in national capital demand during periods of war and were generally successful in raising the sought-after capital. During France's four major wars of this period, national tontine offerings raised approximately 110 million livres from around 110,000 individuals.

Contrary to the relative success enjoyed by France, tontine offerings in England often failed to raise the desired capital. England provided its first national tontine in 1693; this initial tontine generated but a tenth of the one million pounds set as its goal. Yet England did successfully use the tontine to fund many public projects, including construction of the Richmond Bridge, claimed to be the oldest standing "London" Bridge. Unlike many of the early French tontines, English tontines frequently allowed agents to purchase numerous shares.

While the use of tontines to finance government projects was predominately a European endeavor, the notion that tontines could be used as a means to finance national debt has a historical basis in the U.S as well. Faced with growing principal liability on national debt, Alexander Hamilton proposed a national tontine in the U.S. in his 1790 Report Relative to a Provision for the Support of Public Credit (Jennings et al., 1988). Hamilton's proposal was to reduce principal repayments on national debt by converting old debt with principal that was repayable at the discretion of the government into debt demanding no return on principal.

The structure of the tontine that Hamilton proposed was inspired by a tontine originally proposed by William Pitt in 1789. The proposed tontine included six age classes, and shares in the tontine would be sold for $200 with no limit on the number of shares that any agent could purchase. Individuals could subscribe on their own lives or on the lives of others nominated by them. However, Hamilton proposed a freeze component on debt repayment: the annuities of subscribers who passed away would be divided among living subscribers until only twenty percent of the original subscribers remained. Once this threshold was reached, the payments to remaining survivors would be frozen for the duration of their lives (Dunbar, 1888).

Tontine Insurance in the United States

While tontines proper were not used after the eighteenth century, an adaptation of the tontine was implemented in the U.S. life insurance market in 1868. Tontine insurance was introduced in 1868 by the Equitable Life Assurance Society of the U.S. Under tontine insurance, premiums served two distinct purposes: (i) provision of standard life insurance benefits and (ii) creation of an individual investment fund. Under tontine insurance, policyholders deferred receipt of the dividend payments of standard premium insurance policies. The deferred dividends were pooled and invested by the insurance company on behalf of the policyholders for a specified time period. At the end of this period, the fund plus the investment earnings were divided proportionately among the entire active, surviving policyholders. Investment earnings could be received as either cash or as a fully paid life annuity. Beneficiaries of policyholders that passed away before the end of the tontine period received the specified death benefits, but had no claim on the tontine fund money (Ransom and Sutch, 1987).

Conceptually, tontine insurance had several advantages relative to a standard life insurance policy. Policyholders were able to secure life insurance plus create a retirement fund. Survivors could receive a generous rate of return on these investments if a large proportion of other group members were to pass away or allow their policy to lapse. Tontine insurance provided an opportunity for young individuals to save for retirement by providing a low-risk, high-yield investment fund available on an installment plan. Unfortunately, corruption by the insurance companies led to the prohibition of tontine insurance sales by 1906 (Ransom and Sutch, 1987).

III. Tontine Theory

To model a tontine as an instrument to fund public goods, we must define the utility structure of agents and their probability of survival in a particular period. For the former, we consider n agents $i = 1,...,n$ whose utility is assumed additively separable in monetary wealth and the benefits from the public good:

$$u_i = y_i + h_i(G),$$

where y_i is a numeraire and G the provision level of the public good. We assume $h_i(G)$ to be increasing and concave ($h_i'(\,) > 0$, $h_i''(\,) \leq 0$).[75] We make the standard public good

[75] For studies that relax the assumption of utility being dependent upon only the level of the public good see Sugden (1982; 1984) and Andreoni (1990); these theories suggest that if one were to rewrite utility such

assumption—that it is socially desirable to provide a positive amount of the public good, i.e., $\sum_i h_i'(0) > 1$

Given an initial endowment w of wealth (income), the choice facing the agent is to determine the amount b_i of wealth to invest in the tontine. Investment b_i in the tontine provides the agent with an uncertain monetary return x_i that is dependent upon her own contributions and those of all other members of a group:

$$u_i = w + x_i - b_i + h_i(G).$$

We assume that the tontine pays $P_t \geq 0$ in period *t* with a total of $\sum_{t=0}^{T-1} P_t = P$. Payments are covered by the players' contributions, i.e., the level of public good provision equals the total contribution minus the aggregate prize level:

$$G = B - P = \sum_{i=1}^{n} b_i - \sum_{t=0}^{n-1} P_t$$

In each period *t*, some individuals might die (exit the game). All survivors receive a payment that is determined by their relative contribution level. That is, for a total tontine payment P_t in period *t*, a surviving player *i* receives a payment $\frac{b_i}{B_t} P_t$ where B_t is the sum of the contributions made by the remaining players in period *t*.

We assume that each agent has a perish probability in period *t* given by μ_t where $\sum_{t=1}^{T} \mu_t = 1$. The probability that an agent will die no later than period *t* is denoted by M_t where $M_t = \sum_{s=1}^{t} \mu_t$. The probability of agents' deaths is i.i.d. Finally we assume

that it is a function of both the level of the funds raised and own individual contributions, then the standard result of free-riding behavior can be reversed.

for simplicity that agents are risk-neutral and payments are perfectly substitutable across periods. Denoting the set of $k \le n$ participating agents (with positive contributions) by S_0 ($k = \# S_0$),[76] the *ex ante* expected utility of a player *i* is given by

$$EU_i = w - b_i + h_i(B-P) + \sum_{t=0}^{T-1} P_t \sum_{l=0}^{k-1} M_t^{\,l} (1-M_t)^{k-l} \left[\sum_{S \subseteq S_0 \setminus i, \# S = l} \frac{b_i}{B - B(S)} \right].$$

We immediately obtain the following equilibrium conditions:

$$1 - h_i'(B-P) = \sum_{t=0}^{T-1} P_t \sum_{l=0}^{k-1} M_t^{\,l} (1-M_t)^{k-l} \left[\sum_{S \subseteq S_0 \setminus i, \# S = l} \frac{B - B(S) - b_i}{(B - B(S))^2} \right] \quad \text{for} \quad i \in S_0$$

$$1 - h_i'(B-P) \ge \sum_{t=0}^{T-1} P_t \sum_{l=0}^{k-1} M_t^{\,l} (1-M_t)^{k-l} \left[\sum_{S \subseteq S_0 \setminus i, \# S = l} \frac{1}{B - B(S)} \right] \quad \text{for} \quad i \notin S_0$$

$$k - \sum_{i \in S_0} h_i'(B-P) = \sum_{t=0}^{T-1} P_t \sum_{l=0}^{k-1} M_t^{\,l} (1-M_t)^{k-l} \left[\sum_{S \subseteq S_0, \# S = l} \frac{k-l-1}{B - B(S)} \right] \tag{1}$$

In the following we will first consider the case of symmetric risk-neutral agents. Both assumptions are relaxed in later sections.

III.1 Tontines for symmetric risk-neutral agents

If all agents value the public good identically ($h_i(G) = h(G)$), we can concentrate on symmetric equilibria. Here, all *n* agents contribute at a level *b* such that total contributions *B=nb* is given by the symmetric version of first-order condition (1):

$$B(1 - h'(B-P)) = \sum_{t=0}^{T-1} P_t \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-l-1}{n-l} \right]. \tag{2}$$

We now consider the optimal design of a tontine. In particular, we address the question of how an organization—government or private charitable fundraiser—with a

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[76] We will later show that all agents participate: $k = n$ if there is (at least) one *t* for which $P_t > 0$ and $0 < M_t < 1$.

fixed prize budget, $P=\sum_{t=0}^{T-1} P_t$, should allocate this prize money across $t \geq 0$ distinct time periods so as to maximize total contributions. We obtain the following result:

Proposition 1 (Optimal tontine—Symmetric risk-neutral agents)

> *If agents are symmetric and risk-neutral, contributions to the public good using a tontine are maximal if all the payments are made in the first period, i.e., before anybody has passed away.*

Proof of Proposition 1:

Contributions to the public good are clearly increasing in the right-hand side of the equilibrium condition (2). Thus, we obtain:

$$
\begin{aligned}
&\sum_{t=0}^{T-1} P_t \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-l-1}{n-l} \right] \\
&= \sum_{t=0}^{T-1} P_t \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-l-1}{n-l} \right] \\
&\leq \sum_{t=0}^{T-1} P_t \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-1}{n} \right] \\
&\leq \frac{n-1}{n} P
\end{aligned}
$$

which coincides with the right-hand side if all payments are made before any agent has perished, i.e., $P = P_0$.

The optimal tontine for symmetric agents, therefore, has a simple structure: All agents receive a rebate proportional to their contributions relative to those of the total group. This optimal structure implies that agents are not subject to any risk – all subjects receive their payment with certainty. Given the contribution of all other agents, the payoffs for an agent i under the tontine are given by $\frac{b_i}{B} P$, where P denotes the prize level.

The certain payoff is therefore given by $w - b_i + h(B - P) + \frac{b_i}{B}P$ which can also be interpreted as the expected payoff in Morgan's (2000) risk-neutral one-prize lottery. All of his results therefore apply. In particular, using his δ-financing rule, the tontine will always be carried out and the contributions will increase in the prize level, P, (see Morgan 2000, lemma 5).[77]

Reconsidering the first-order condition for a symmetric equilibrium (2), the individual ($\bar{b}$) and the total ($\bar{B}$) contribution levels for the optimal tontine are given by

$$n\bar{b} = \bar{B} \qquad \bar{B}(1 - h'(\bar{B} - P)) = \frac{n-1}{n}P. \tag{3}$$

Note that the tontine raises a positive amount of money for the public good net of prize payments, as

$$P(1 - h'(0)) > \frac{n-1}{n}P \quad \Leftrightarrow \quad nh'(0) > 1,$$

which coincides with the condition for a public good.

We summarize these results as follows:

Proposition 2 (Contribution levels for optimal tontines—Symmetric players)

For symmetric players, the optimal tontine will always be carried out and raises contributions in excess of the prize-level P. The provision level of the public good is increasing in P.

Historically, tontines clearly have not reflected the *optimal* features derived in Proposition 1. In the seventeenth and eighteenth centuries, tontine "prize" payments

[77] The optimal tontine that we study in this paper provides a rebate (subsidy) on individual contributions to the public good. This feature resembles the study relating government subsidies and contributions to a public good by Andreoni and Bergstrom (1996). In their case, however, subsidies are financed by taxes, whereas in our model the rebates are taken out of the contribution to the public good. The provision of the public good therefore does not depend on the possibility of enforcing tax payments. To balance the budget, subsidy rates in our model are not exogenously fixed but endogenously given by the individual relative to total contributions.

were made over a long time span. That is, the tontines differed significantly from the optimal tontine in that repayments were made annually to the surviving subscribers instead of making all repayments before anybody died. In the oft-used tontine repayment system, however, subscribers could die in any period s (even before any payment was received) and thus would forego payments in all periods $t > s$ with positive probability. To model this aspect of the mechanism, let us assume that the aggregate prize amount P is spread evenly across $\tilde{T} \leq T-1$ periods. In other words, $P_t = P/\tilde{T}$ for $1 \leq t \leq \tilde{T}$.

Then, the contributions in equilibrium are given by the first-order condition:

$$B(1-h'(B-P)) = \frac{P}{\tilde{T}} \sum_{t=1}^{\tilde{T}} \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-l-1}{n-l} \right] , \tag{4}$$

for which we obtain the following result:

Proposition 3 (Suboptimal tontines – Effect of $\tilde{T}$ and n)

Contributions to the public good using a tontine that pays a fixed prize-level in $\tilde{T} \leq T-1$ periods are decreasing in $\tilde{T}$. For any given $\tilde{T}$, they converge towards the contributions to an optimal tontine (or lottery) if the number of (potential) participants, n, increases.

Proof of Proposition 3:

In order to show that contributions decrease in $\tilde{T}$, it is sufficient to show that the right-hand sides of (4), $\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \frac{n-l-1}{n-l}$, are decreasing in *t.* As we know that M_t increases in *t*, we must demonstrate that:

$$\frac{\partial}{\partial M} \sum_{l=0}^{n-1} \frac{n!}{l!(n-l)!} M^{l} (1-M)^{n-l} \frac{n-l-1}{n-l}$$
$$= \sum_{l=0}^{n-1} \frac{n!}{l!(n-l)!} M^{l-1} (1-M)^{n-l-1} (l-nM) \frac{n-l-1}{n-l} < 0.$$

It is clear that for $(n-1)/n \le M$ all the summands are negative. For $(n-1)/n > M$, however, we obtain:

$$
\begin{aligned}
&\sum_{l=0}^{n-1} \frac{n!}{l!\left(n-l\right)!} M^{l-1}(1-M)^{n-l-1}(l-nM)\frac{n-l-1}{n-l} \\
&\le \frac{n-nM-1}{n-nM} \sum_{l=0}^{n-1} \frac{n!}{l!\left(n-l\right)!} M^{l-1}(1-M)^{n-l-1}(l-nM) \\
&= \frac{n(1-M)-1}{(1-M)^2}\left[\sum_{l=0}^{n-2} \frac{(n-1)!}{l!\left(n-l-1\right)!} M^{l}(1-M)^{n-l-1} - \sum_{l=0}^{n-1} \frac{n!}{l!\left(n-l\right)!} M^{l}(1-M)^{n-l}\right] \\
&= -\frac{n-1-nM}{1-M} M^{n-1} < 0.
\end{aligned}
$$

To prove the convergence result, we compare the right-hand side of the optimal tontine with the one that pays in all periods $1 \le t \le \tilde{T}$:

$$
\begin{aligned}
&\frac{\dfrac{P}{\tilde{T}} \sum_{t=1}^{\tilde{T}} \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \, \dfrac{n-l-1}{n-l} \right]}{P\dfrac{n-1}{n}} \\
&= \frac{n}{n-1}\frac{1}{\tilde{T}} \sum_{t=1}^{\tilde{T}} \left[\sum_{l=0}^{n-1} \binom{n}{l} M_t^{\,l} (1-M_t)^{n-l} \, \frac{n-l-1}{n-l} \right].
\end{aligned}
$$

It is therefore sufficient to show that

$$
\sum_{l=0}^{n-1} \binom{n}{l} M^{l} (1-M)^{n-l} \, \frac{1}{n-l}
$$

converges to zero for all $0 \le M < 1$ when n goes to infinity. This is easily demonstrated numerically.

Proposition 3 highlights that the inefficiency of tontines that pay in later periods is less severe when many participants are expected to participate. As a further feature of such tontines, the expected payments in period t, conditional on agent survival, are clearly small in the beginning (as the likelihood of others' survival is high) but increase rapidly toward the terminal period. As an investment instrument for retirement funds, the tontine therefore provides advantages compared to other instruments. In particular, if one relaxes the assumption of risk-neutrality and perfect substitutability across periods, the tontine is quite practical economically if agents have decreasing external income (salary, pension) and can use the tontine to flatten their temporal payoff streams.

Example 1

We consider contributions to a linear public good when the probability of dying is uniformly distributed: $\mu_t = \dfrac{1}{T}$ for all $1 \le t \le T$. Assume that there are $n = 50$ symmetric agents and $T = 50$ periods. Figure 1 shows the contribution level to the $\tilde{T}$-tontine relative to the contribution level to the optimal tontine. For the $\tilde{T} = 50$-tontine, Figure 2 illustrates the expected payments in period *t* given survival (payments relative to payment in period 1).

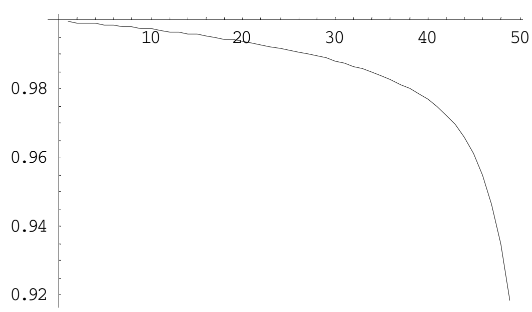


Figure 1:

Total contributions as a function of $\tilde{T}$ (normalized)

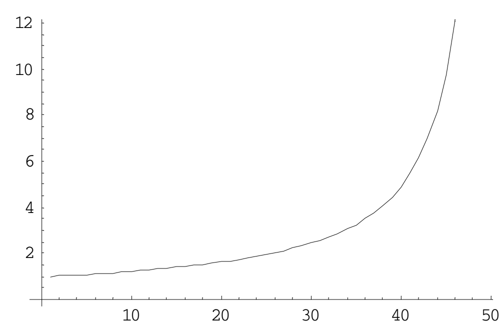


Figure 2:

Expected payments in period *t* given survival (normalized)

Figure 1 reveals that contributions remain above 90 percent of the optimal levels even if one spreads the tontine payment over the whole potential lifespan of agents. Figure 2 shows that expected payments in period t, given that the agent survives until then, increase rapidly toward the end of an agent's lifespan.

III.2 Tontines and risk-aversion

Lange et al. (2004) have shown that contributions to lotteries are decreasing in the level of risk-aversion. For the optimal tontine, however, players are not exposed to any risk. The optimal tontine is therefore a more efficient instrument for fundraising than any lottery.

Proposition 4 (Tontines for risk-averse players)

> *Individual contributions under the tontine that pays only before any agent has died are independent of the risk posture of agents. If agents are risk-averse but symmetric with respect to their valuation of the public good, it dominates any lottery as a fundraising instrument.*

Besides this superiority of tontines for risk-averse agents, a fundraiser does not need any prior beliefs over the risk preference of a potential donor pool when designing the fundraising instrument.

III.3 Tontines with heterogeneous agents

We have seen in the previous section that the optimal tontine for symmetric risk-neutral players coincides with a single-prize lottery or—equivalently—a rebate scheme. In this section, we consider the performance of tontines for agents with heterogeneous valuation of the public good. Conditions are derived under which the rebate scheme, i.e., the degenerate tontine, is optimal.

Reconsidering the individual first-order conditions (1), first observe that if there is (at least) one t for which $P_t > 0$ and $0 < M_t < 1$, all players will contribute. The intuition is that there is a chance that in period t only one agent will survive. An agent can secure himself this prize $P_t > 0$ by contributing. More formally, looking at the first-order condition for $i \notin S_0$, the right-hand side is clearly infinite (consider $S = S_0 \setminus i$).

Proposition 5 (Participation in tontines):

If there is period t for which $P_t > 0$ and $0 < M_t < 1$, then all players contribute to the tontine.

Even a slight deviation from the degenerate tontine ($P_0 = P$) (alias the rebate scheme) towards $P_t > 0, P_0 < P$ can therefore lead to a discontinuous change in participation and therefore contribution levels. In general, we obtain the following result when a tontine should pay out part of the prizes in later periods:

Proposition 6 (Tontines—Heterogeneous agents):

If agents are sufficiently heterogeneous with respect to their valuation of the public good, the optimal tontine pays $P_t > 0$ for some $t > 0$ with $0 < M_t < 1$. In particular, if a set S_0 of players participates for $P_0 = P$, then contributions can be increased by changing to $P_t > 0$ ($P_t + P_0 = P$)

(i) *if $k < n$, i.e., there is (at least) one agent $i \notin S_0$ who does not contribute if $P_0 = P$:*

$$h_i'(B-P) \le \frac{1}{k-1}\sum_{j \in S_0} h_j'(B-P) = \frac{H-1}{k-1}$$

(ii) *if $k = n$ for $P_0 = P$ and*

$$\sum_{0 \le l \le n-1} M_t^{\,l}(1-M_t)^{n-l}\,\frac{n-l-1}{n-l}\sum_{S \subseteq S_0, \#S = n-l}\frac{1-H/n}{1-H+(n-1)H(S)/(n-l)} > \frac{n-1}{n}$$

$$where\ \ H(S)=\sum\nolimits_{i\in S} h_i\,'(\ )\ \ and\ \ H=\sum\nolimits_{i\in S_0} h_i\,'(\ )$$

Proof:

We analyze the tontine that pays $P_t=\varepsilon$ and $P_0=P-\varepsilon$. Here, the first-order conditions (1) are given by:

$$\begin{aligned}1-h_i\,'(B-P)&=(P_0-\varepsilon)\frac{B-b_i}{B^2}+\varepsilon\sum\nolimits_{l=0}^{n-1} M_t{}^l(1-M_t)^{n-l}\left[\sum\nolimits_{S:i\notin S,\#S=l}\frac{B-B(S)-b_i}{(B-B(S))^2}\right]\\ n-\sum_i h_i\,'(B-P)&=(P_0-\varepsilon)\frac{n-1}{B}+\varepsilon\sum\nolimits_{l=0}^{n-1} M_t{}^l(1-M_t)^{n-l}\left[\sum\nolimits_{S:\#S=l}\frac{n-l-1}{B-B(S)}\right]\end{aligned}\tag{5}$$

<u>Case 1</u>

Consider first the case in which there is $i\notin S_0$ with $h_i\,'(B-P)<\dfrac{H-1}{k-1}$. Then there is a discontinuity in participation and contribution at $\varepsilon=0$ when $P_t=\varepsilon$ and $P_0=P-\varepsilon$. We therefore study the limit of the first-order conditions (5) from above ($\varepsilon\quad 0$) and get $k(\vec{S}_0)>k(S_0)$, where $\vec{S}_0$ is the set for which $\vec{b}_i=\lim_{\varepsilon\quad 0} b_i(\varepsilon)>0$. Now we have

$$\begin{aligned}1-h_i\,'(\vec{B}-P)&=P\frac{\vec{B}-\vec{b}_i}{\vec{B}^2}\quad \text{if}\quad \vec{b}_i>0\\ k(\vec{S}_0)-\sum\nolimits_{i\in\vec{S}_0} h_i\,'(\vec{B}-P)&=P\frac{k(\vec{S}_0)-1}{\vec{B}},\end{aligned}$$

from which the claim follows immediately.

<u>Case 2</u>

Consider now the case in which there is $h_i\,'(B-P)\geq\dfrac{H-1}{k-1}$ for all *i* at $P_0=P$. Then, the first-order conditions (5) also hold for $P_0=P$ (as all individual first-order conditions (1) hold with equality). For $P_t = \varepsilon$ and $P_0 = P - \varepsilon$, we study the derivative of B with respect to ε at $\varepsilon = 0$:

$$\frac{\partial B}{\partial \varepsilon}(\varepsilon = 0) = \frac{1}{\frac{n-1}{B^2} P - \sum_i h_i''(\ )}\left[-\frac{n-1}{B} + \sum_{l=0}^{n-1} M_t^{\ l}(1-M_t)^{n-l}\left[\sum_{S:\#S=l} \frac{n-l-1}{B-B(S)}\right]\right]$$

Therefore, $\frac{\partial B}{\partial \varepsilon}(\varepsilon =$ $>$ i f

$$\sum_{l=0}^{n-1} M_t^{\ l}(- \quad - \left[\sum \quad = \frac{n- \ -1}{B- \ (} \ t\right] \overset{n}{=} \sum_{= S:}^{-} 1 \quad M \quad - \left[\sum^{n\ 1}_{= \#S} \frac{l - \ -}{B \quad)}\right] > \frac{-}{} \qquad t\ l$$

Using the equilibrium conditions

$$1 - h_i \quad - \quad = \frac{B - \quad i}{B^2} \qquad - \quad = \frac{n-1}{B} \quad , \qquad b \qquad ($$

we obtain the claimed relationship,

$$\sum_{0 \le l \le \ -} M_t^{\ l}(- \quad - \frac{n- \ -1}{n- \quad n\ 1} \sum_{\subseteq \ 0 \quad = \ -} \frac{n \quad - \quad 1 \ M \quad l}{1^t - \ + \ - \quad - \ l} > \frac{-}{} \quad S \ , \qquad ($$

and completes the proof.

We have demonstrated above that the tontine with $P_0 =$ - c

p r i

u n -p r d r y

A s

p r o

$$(k - \sum_{i \in \ 0} \frac{b_i}{B - \quad i} > \quad \Leftrightarrow \quad \underset{S}{-} \quad \sum_{\in} \frac{1}{1 - \ 2} > \frac{k - \quad ^2}{b \quad 0} k \qquad i \qquad)\ i$$

On the one hand, we immediately see that one can design a tontine that outperforms any lottery if $k<n$ and $(1-H/k)\sum_{i\in S_0}\frac{1}{1-h_i}<\frac{(k-1)^2}{k-2}$. Alternatively, if $k=n$ and $(1-H/n)\sum_{i}\frac{1}{1-h_i}>\frac{(n-1)^2}{n-2}$, one can increase the contributions to the lottery by offering a second prize, but cannot improve upon the degenerate tontine if

$$\sum_{0\le l\le n-1} M_t^{\,l}(1-M_t)^{n-l}\,\frac{n-l-1}{n-l}\sum_{S\subseteq S_0,\#S=n-l}\frac{1-H/n}{1-H+(n-1)H(S)/(n-l)}<\frac{n-1}{n}$$

for all t. This, for example, would be the case if M_t is close to one for all t. In such cases, the right-hand side of the inequality would be close to zero.

We therefore can summarize our findings in the following Proposition:

Proposition 7 (Tontines vs. lotteries—Heterogeneous agents):

If agents are risk-neutral and heterogeneous with respect to their valuation of the public good, then there exist situations in which appropriately designed tontines outperform lotteries and vice versa.

Note that in real-world applications there will always be agents who have no valuation, or only a below average valuation, for specific public goods. In such cases, one can always improve upon a single prize lottery by using a tontine with $P_t>0$ *and* $0<M_t<1$. In this case all agents will contribute under the tontine.

III.4 *Tontine as a fundraising instrument*

A charity that seeks to fundraise using a literal version of the historical tontine to replace lotteries might find the simulation of the "probabilities to die" problematic since in each round one must have a random draw for all survivors. The structure of the tontine can be used, however, to design a fundraising instrument (which we also call tontine) whose implementation is quite simple.

For this, we abstract from the independent and identical probabilities of dying considered in the previous section. Instead, sequentially draw one of the *k* participating persons which must leave the game. That is, in period *t* the number of players is *k-t*. For the payments, the sequence of "dying" is decisive.[78] Each sequence has the same probability given by $1/k!$ if *k* players contribute. As in the previous section, a certain amount of money is distributed among the remaining players according to their share in each period (i.e. before the next person leaves).

Compared to the preceding analysis, we only have to change the probability of a certain set *S* of players having passed away until period *t* from $M_t^{\#S}(1-M_t)^{n-\#S}$ to $1/\binom{k}{\#S}$. The first-order conditions (1) therefore convert to

$$1-h_i'(B-P)=\sum_{t=0}^{T-1}P_t\sum_{l=0}^{k-1}\left[\sum_{S\subseteq S_0\setminus i,\#S=l}\frac{B-B(S)-b_i}{(B-B(S))^2}\right]/\binom{k}{l}\quad\text{for}\quad i\in S_0$$

$$1-h_i'(B-P)\geq\sum_{t=0}^{T-1}P_t\sum_{l=0}^{k-1}\left[\sum_{S\subseteq S_0\setminus i,\#S=l}\frac{1}{B-B(S)}\right]/\binom{k}{l}\quad\text{for}\quad i\notin S_0$$

$$k-\sum_{i\in S_0}h_i'(B-P)=\sum_{t=0}^{T-1}P_t\sum_{l=0}^{k-1}\left[\sum_{S\subseteq S_0,\#S=l}\frac{k-l-1}{B-B(S)}\right]/\binom{k}{l}$$

while all the qualitative results remain valid. In particular, payments should be made before anybody leaves the game ($P_0=P$) if agents have similar valuation of the public good. If agents are sufficiently heterogeneous, one can improve upon this degenerate tontine—and possibly upon any lottery—by choosing $P_0<P$.

[78] For example, given identical contributions, a person who leaves last gets the highest payment, the person who leaves first receives the lowest payment.

To summarize, our theoretical model provides a number of testable hypotheses regarding the performance of the tontine as a fundraising mechanism relative to a VCM and a single prize lottery. First, contributions in the optimal tontine (single-prize lottery) are greater than those for an equivalent valued VCM. Second, contributions in the tontine are independent of individual risk-preference since an agent receives a private return on any contribution to the public good with certainty. Finally, the optimal tontine dominates the single prize lottery as a fundraising mechanism when agents are risk-averse and have symmetric valuations for the public good.

IV. Experimental Design and Results

We design an experiment that is closely linked with our theoretical model to examine these three conjectures – Table 1 provides a design summary. We begin with the traditional control treatment that induces symmetric MPCR's across agents in a voluntary contribution mechanism (denoted VCM). We cross this treatment with comparable single-prize (denoted SPL) and optimal tontine (denoted OT) treatments, leading to a total of 3 treatments.[79]

All treatments were conducted at the University of Maryland—College Park. The experiment consisted of multiple sessions held on separate days with different subjects. Each session consisted of two parts, the first to gather information on individual contribution decisions across the various treatments. The second part was included to gather information on individual risk postures. We describe, in turn, each part of the session.

[79] The data for the FPL and VCM treatments come from the symmetric single prize lottery and symmetric VCM in Lange et. al (2005). The data for the OT treatment were collected for the current study.

Part 1:

The first part of the experiment was designed to compare contribution levels across the single fixed-prize lottery, the optimal tontine, and the voluntary contribution mechanism. The voluntary contribution mechanism treatment and the single fixed-prize lottery treatment followed the instructions from Morgan and Sefton (2000) to enable direct comparison. Table 1 summarizes the key features of our experimental design and the number of participants in each treatment. Subjects were recruited on campus using posters and emails that advertised subjects could "earn extra cash by participating in an experiment in economic decision-making." The message stated that students would be paid in cash at the end of the session and that sessions generally take less than an hour and a half. The same protocol was used to ensure that each session was run identically.

Each subject was seated at linked computer terminals that were used to transmit all decision and payoff information. All sessions were programmed using the software toolkit *z-Tree* developed by Fischbacher (1999). The sessions each consisted of 12 rounds, the first two being practice. The subjects were instructed that the practice rounds would not affect earnings. Once the individuals were seated and logged into the terminals, a set of instructions and a record sheet were handed out. The subjects were asked to follow along as the instructions (included in Appendix A) were read aloud. After the instructions were read and the subjects' questions were answered the first practice round began.

At the beginning of each round subjects were randomly assigned to groups of four. The subjects were not aware of whom they were grouped with, but they did know that the groups changed every round. Each round the subjects were endowed with 100 tokens. Their task was simple: decide how many tokens to place in the group account and how many to keep in their private account. The decision was entered in the computer and also recorded on the record sheet. When all subjects had made their choice, the computer would inform them of the total number of tokens placed in their group account, the number of points from the group account and the private account, as well as any bonus points that were earned. The payoff for the round was determined by summing the points from the group account, points from the private account, and any bonus points received. Once each of the subjects had recorded all of this information on their record sheets, the next round would begin.

The points for each round were determined as follows. For all sessions, subjects received 1 point for each token placed in their private account. They were awarded 0.3 points for each token placed in the group account by themselves and the other members of their group. Additionally, each session had a different method for earning bonus points.

We follow Morgan and Sefton (2000) by adding the value of the prize (80 tokens) to the group account in the VCM, which makes the VCM treatment comparable to the SPL and OT treatments. Therefore, in the voluntary contribution mechanism session, all subjects, regardless of their contributions to the group account, earned 24 bonus points, which represent the 80 tokens placed in the group accounts. In the single fixed-prize lottery sessions, group members competed for a lottery prize of 80 points. Each subject's chance of winning the prize was based on his or her contribution to the group account compared to the aggregate number of tokens placed in the group account by all group members. For the optimal tontine session, group members competed for an endogenously determined share of the 80 bonus points. As in the single fixed-prize lottery sessions, subjects' share of the bonus was equal to his or her contribution to the group account compared to the aggregate number of token placed in the group account by all group members. Hence, any individual that contributed to the group account was guaranteed to receive some bonus points.

At the end of the last round, one of the non-practice rounds was chosen at random as the one that would determine earnings. Subjects were paid $1.00s for every 15 points earned. They recorded their earnings for *Part 1* of the session and prepared for *Part 2*.

Part 2

The second part of the experiment was designed to lend insights into subjects' risk postures and link those preferences to behavior in the public goods game described above. Attempting to measure risk postures in one game and applying them to more closely explore behavior in another is not novel to this study. Previous authors have attempted this approach with varied success. There are issues with such an approach even if "successful," including whether risk preferences are stable across games, over time, etc. Yet, because risk posture is not exogenously imposed on players (such as MPCR's are induced in the public goods game) an important caveat must be placed on the results from such an exercise.

In this part of the session, the low-payoff treatment of Holt and Laury (2002) was replicated with all values multiplied by a factor of four (see Appendix B for instructions).[80] In each of the eleven sessions this part was conducted in an identical

[80] The payoffs for the Holt and Laury experiment were multiplied by a factor of four so that the domain of earnings from this experiment ($0.40, $15.40) would correspond with the domain of potential earnings from the public goods game ($1.20, $29.33).

manner. The treatment is based on ten choices between paired lotteries. The paired choices are included in the appendix. The payoff possibilities for Option A, \$8.00 or \$6.40, are much less variable than those for Option B, \$15.40 or \$0.40, which was considered the risky option. The odds of winning the higher payoff for each of the options increase with each decision. In the first decision, there is only a 1/10 chance of winning the higher payoff, so only the most risk-loving individuals should choose Option B. The expected payoff difference for choosing Option A is \$4.66. As the probabilities of winning the higher payoff increase, individuals should cross over to Option B. The paired choices are designed such that a risk-neutral individual should choose Option A for the first four decisions and then switch to Option B for the remaining six decisions. The paired choices are also designed to determine degrees of risk aversion.

Upon completion of Part 1 of the session, instructions and a decision sheet were handed out. After the directions were read and questions were answered, the subjects were asked to complete their decision sheets by choosing either A or B for each of the ten decisions. The subjects were instructed that one of the decisions would be randomly selected *ex post* and used to determine their payoffs. Part of a deck of cards was used to determine payoffs, cards 2-10 and the Ace to represent "1". After each subject completed his or her decision sheet, a monitor would approach the desk and randomly draw a card twice, once to select which of the ten decisions to use, and a second time to determine what the payoff was for the option chosen, A or B, for the particular decision selected. After the first card was selected, it was placed back in the pile, the deck was reshuffled, and the second card was drawn. For example, if the first draw was an Ace, then the first decision choice would be used, and the subject's decision, A or B would be circled. Suppose the subject selected A for the first decision. The second draw would then be made to determine the subject's payment. If the Ace were drawn, the subject would earn $8.00. If cards 2-10 were drawn the subject would earn $6.40. The subjects were aware that each decision had an equal chance of being selected.

After all the subjects' payoffs were determined, they combined their payoff from Part 1 with that of Part 2 to compute their final earnings. The final payoffs were then verified against the computer records, and subjects were paid privately in cash for their earnings. Each of the sessions took approximately 75 minutes.

Experimental Results

Our experimental design enables us to test a number of theoretical predictions regarding contribution levels across our various treatments. We craft the results summary by first pooling the data across subjects of all risk postures, but later explore the effects of risk preference on contribution schedules. This approach permits a comparison of our results with the voluminous public goods literature, which implicitly assumes risk neutrality, and therefore represents joint hypothesis testing in some cases.

Our first hypothesis is that the lottery and tontine treatments introduce a compensating externality that serves to attenuate the tendency to "free-ride". This hypothesis is directly testable using our experimental data and implies that:

H1a: Mean contributions in the FPL sessions are greater than mean contributions in the VCM sessions.

H1b: Mean contributions in the OT session are greater than mean contributions in the VCM sessions.

Table 2 provides mean contribution levels for our experimental data and Figures 3 and 4 provide a graphical depiction of the data. As can be seen in the table and the figures, contribution levels in the lottery and tontine treatment are greater than those in the VCM treatment. Mean contribution levels in the FPL (OT) treatment were 42.18 tokens (29.63 tokens) respectively. Mean contribution levels in the VCM treatment were 22.85 tokens. The difference in mean contributions for the FPL and VCM treatment is statistically significant at the $p < 0.01$ level using a Mann-Whitney test of significance. The difference in mean contributions between the OT and VCM treatments is statistically significant at the $p < 0.10$ level using.[81] These data generate our first result:

> *Result 1: Both the single-prize lottery and optimal tontine are superior fundraising mechanisms than the voluntary contribution mechanism.*

The first part of result 1 replicates previous findings in the experimental literature about the dominance of the charitable lottery as a fundraising mechanism (e.g., Morgan and Sefton, 2000; Dale, 2004). The second part of result 1 is novel to the literature.

A second testable implication of hypotheses 1a and 1b is that agents are less-likely to "free-ride" and contribute nothing to the group account in the lottery treatment and the optimal tontine than in the VCM treatment. Further, since the tontine is predicted to induce full participation whereas the lottery does not ensure participation by all agents, we would expect a lower tendency for agents to "free-ride" in the OT treatment than the SPL treatment.

[81] The unit of observation for the Mann-Whitney test is the average contribution level for each agent. Since groups are randomly rematched in each period, the average contribution level for agents within a treatment should be independent observations.

To examine whether the tendency to strongly free-ride is attenuated by the lottery incentives, we estimate a random effects probit model. In estimating the model, we make use of Butler and Moffit's (1982) random effects probit specification:

$$T_{it} = \beta' X_{it} + e_{it} \qquad e_{it} \sim N[0,1]$$

where T_{it} equals unity if agent i donated zero in period t, and equals zero otherwise, and X_{it} are model covariates. We specify $e_{it} = u_{is} + \alpha_i$, where the two components are independently and normally distributed with mean zero. It follows that the variance of the disturbance term e_{it} is $\mathrm{Var}(e_{it}) = \sigma_u^2 + \sigma_\alpha^2$. By construction, the individual random effects α_i will capture important heterogeneity across subjects that would be left uncontrolled in a standard cross-sectional model. The vector X_{it} includes treatment dummies, a one-period lagged value of the donations for agent *i*, and controls for period effects.

Table 3 provides empirical results for our estimated random effects probit model. Testing the hypothesis that individuals are less likely to strongly free-ride in our lottery and optimal tontine treatments is equivalent to testing the following set of hypotheses:

H2: $\hat{\beta}_{OT} < \hat{\beta}_{SPL} < 0$

Results from our model support this hypothesis. As indicated in columns 2 and 3 of Table 3, the estimated coefficients on the lottery and optimal tontine treatment indicators are each negative and statistically significant at the p < 0.05 level. These negative coefficient estimates suggest that conditioned on underlying model covariates, individuals in the lottery (tontine) treatment are less likely to contribute zero than agents in an equivalent VCM. These findings generate our next result:

Result 2: The introduction of a charitable lottery or tontine attenuates the tendency of agents to "strongly free-ride", i.e. increases the number of contributing agents, with the optimal tontine inducing greater participation than the single-prize lottery.

Further support for *Result 2* is provided by t-tests comparing the estimated parameter values for the lottery (tontine) treatment with the parameters for the equivalent VCM treatment. The estimated coefficients for both the single prize lottery and optimal tontine are smaller than the associated parameter for the VCM (the constant term in the regression) at the $p < 0.05$ level of significance. Furthermore, parameter estimates are consistent with our prediction that strong free-riding incentives are lower in the optimal tontine treatment compared to the single-prize lottery treatment. Agents in the optimal tontine are approximately 3.45 percent less likely to free-ride than are agents in the single-prize lottery with this difference statistically significant at the $p < 0.05$ level.[82]

Tontines, Lotteries, and Risk Aversion

The above analysis follows the spirit of the literature in that all agents are pooled and a series of hypotheses are tested jointly—risk assumptions and direction of treatment effects. We can examine our data at a level deeper based on our theoretical predictions and subjects' revealed risk preference in Part II of our experiment. Risk preferences, summarized in Figure 5, are based on the number of safe choices "Option A" selected by the agent in the Holt/Laury experimental design. Under the basic Holt/Laury design, an increase in the number of safe alternatives selected by an agent implies an increase in his implied risk preference.

[82] Estimated probabilities are evaluated at the mean value for one-period lagged donations in each of the treatments respectively. These mean values are 42.18 tokens in the single-prize lottery and 29.63 tokens in the optimal tontine.

Our theory provides two testable implications of risk aversion on contributions in our experimental treatments; (i) contributions in the optimal tontine are independent of individual risk preference, and (ii) contributions in the single prize lottery are strictly decreasing in the level of risk aversion. To evaluate these hypotheses, we estimate a linear random effects regression model of individual contribution levels:

$$C_{\mathrm{it}} = \mathrm{v}(Z_{\mathrm{it}}) + \varepsilon_{\mathrm{it}},$$

where C_{it} is the contribution level of the ith agent in period t. Z_{it} includes treatment dummy variables, the interaction of the treatment indicators with the number of safe alternatives agent i selected in the Holt-Laury experiment (a proxy for individual risk-preference), one-period lagged values for individual contribution levels, and controls for the experimental round; $\varepsilon_{it} = \alpha_i + u_{it}$; $E[\alpha_i] = 0$, $E[\alpha_i^2] = \sigma_\alpha^2$, $E[\alpha_i\alpha_j] = 0$ for $i \neq j$; α_i and u_{it} are orthogonal for all i and t. The random effects α_i capture important heterogeneity across agents that would be left uncontrolled in a standard cross-sectional model.

Table 4 provides results for this model across two different specifications. Testing the theoretical conjectures that contributions in the optimal tontine are independent of risk-preference and contributions in the single-prize lottery are strictly decreasing in individual risk-preference is equivalent to testing the following hypothesis:

H3: $\hat{\beta}_{Risk\_Tontine} = 0, \hat{\beta}_{Risk\_Lottery} < 0$

Results from this model provide support for this hypothesis. Across both model specifications (Columns 1 and 2), the estimated coefficient on the interaction of risk preference and the tontine indicator is statistically insignificant. However, the estimated coefficient on the interaction of risk preference and the indicator for the single-prize lottery is negative and significant at the $p < 0.05$ level in both specifications suggesting that lottery contributions are decreasing in the level of individual risk aversion. Combined, this leads to our third result:

> *Result 3: Contributions in the optimal tontine (single-prize lottery) are independent of (strictly decreasing in) individual risk preference.*

One additional result presented in Tables 2 and 4 not predicted by our theory is the superiority of the single prize lottery as a fundraising mechanism even when agents are risk-averse. The theoretical model presented in Section III suggests that risk-averse agents should strictly prefer the optimal tontine to the single-prize lottery. However, observed contribution levels in our single-prize lottery treatment strictly dominate those observed in treatment OT despite the fact that a non-trivial percentage of the agents in each treatment are characterized as risk-averse using the Holt-Laury experimental procedure.

IV. Concluding Remarks

This article provides a theoretical exploration of tontines, a popular method of financing public goods that was introduced more than three centuries ago. Even though tontines were once quite popular—the name "tontine" remains prominently displayed on several publicly funded projects around the world—little is known about their formal structure and whether it would be apropos to reintroduce tontines today.

In this study, we highlight the best characteristics of the tontine that might be utilized in future fundraising drives by deriving the optimal tontine and formally linking the tontine to a popular modern fundraising scheme used by both government and charitable fundraisers: lotteries. We show that the optimal tontine generates contributions that are equivalent to those under a single prize lottery when agents are symmetric and risk neutral. For symmetric risk-averse agents, contributions under the optimal tontine strictly dominate contributions raised under any lottery type. Further, the design of an optimal tontine is independent of underlying risk posture and generates contributions that weakly dominate those of *any* lottery. If agents are sufficiently asymmetric, tontines yield higher contribution levels than the optimal lottery—having a chance of being the only survivor in a period with positive payment provides incentives for *all* players to contribute. If a fundraiser also seeks a high participation rate in order to collect the names of potential contributors for future fundraising drives, then the tontine has an additional "hidden" advantage in that it maximizes participation rates.

We test our theory using a series of laboratory treatments and find evidence in favor of many of our theoretical predictions. Perhaps most importantly, contribution levels under both the single-prize lottery dominate and tontine dominate those of the VCM. Moreover, we find that risk posture is a critical component determining the performance of the single prize lottery but has no influence on contribution levels in the optimal tontine. One finding inconsistent with our theoretical predictions is the dominance of the single-prize lottery as a fundraising mechanism even when some subset of agents are risk-averse. This finding warrants additional research and may suggest a behavioral aspect of the charitable lottery not considered in extent theory.

While this article has addressed the performance of tontines as a fundraising mechanism, there are a number of outstanding issues. For example, under the optimal tontine each agent receives a positive monetary payment with certainty. The *ex post* allocation of wealth is thus more equitable than that which results from any *k*-prize lottery. Given that inequality-averse preferences have been found to be prevalent among agents in laboratory experiments (see, e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), there are reasons to suspect that contribution levels under a tontine would exceed even those predicted by our model. We hope that future work examines this issue in greater detail and evaluates the performance of tontines in the laboratory and in the field.

Table 1: Experimental Design

	Session 1	**Session 2**
VCM *MPCR = 0.30* *Endowment = 100*	*N = 20 Subjects* *10 Rounds* *200 Observations*	
SPL *MPCR = 0.30* *Endowment = 100* *Prize = 80*	*N = 20 Subjects* *10 Rounds* *200 Observations*	*N = 16 Subjects* *10 Rounds* *160 Observations*
OT *MPCR = 0.30* *Endowment = 100* *Prize = 80*	*N = 16 Subjects* *10 Rounds* *160 Observations*	

Note: Cell entries provide the experimental design and parameters for each treatment. For example, in the VCM treatment the MPCR = 0.30 and the subjects were endowed with 100 tokens. In this treatment there was one session of 20 subjects that lasted for 10 rounds.

Table 2: Experimental Results - Mean Contribution Levels by Treatment

	All Periods Pooled	***First Five Periods***	***Last Five Periods***
VCM	22.85 Tokens (31.11)	32.35 Tokens (34.29)	13.34 Tokens (24.24)
SPL	42.18 Tokens (20.30)	45 Tokens (33.40)	40.29 Tokens (32.16)
OT	29.63 Tokens (32.82)	30.76 Tokens (21.06)	28.5 Tokens (19.99)

Note: Cell entries provide the mean and standard deviation for each treatment. For example, in the VCM-treatment the average token contribution was 22.85 with a standard deviation of 31.11 tokens.

Table 3: Random Effects Probit of Free-Riding Behavior

	$T_{it} = 1$ if agent *i* free-rides in period *t*	$T_{it} = 1$ if agent *i* free-rides in period *t*
Constant	-0.24 (0.25)	-0.79** (0.34)
SPL Treatment	-1.11** (0.31)	-1.16** (0.32)
Optimal Tontine Treatment	-1.71** (0.41)	-1.76** (0.43)
1-Period Lagged Donation	-0.007** (0.003)	-0.005* (0.003)
Round		0.08** (0.03)
Log Likelihood	-221.82	-225.96
# of Observations	639	639
# of Agents	71	71

** Denotes statistically significant at the $p < 0.05$ level.
* Denotes statistically significant at the $p < 0.10$ level.

Note: Cell entries provide parameter estimates from a random effects probit model where $T_{it} = 1$ if agent I contributed zero to the public good in period t. For example, the negative and significant coefficient on the symmetric single prize lottery treatment dummy variable suggests that, relative to the VCM, agents in this treatment are less likely to free-ride in any given period.

Table 4: Random Effects Regression Model – Individual Contribution Levels

	Specification A	Specification B
Constant	16.53** (7.93)	25.15** (8.33)
Optimal Tontine Treatment	-2.81 (12.18)	-3.02 (12.09)
SPL Treatment	20.75** (10.07)	21.25** (10.01)
Risk_OT	0.46 (1.34)	0.46 (1.34)
Risk_SPL	-2.16* (1.13)	-2.25** (1.12)
Risk_VCM	-0.81 (1.37)	-0.86 (1.37)
1-Period Lagged Donations	0.38** (0.04)	0.37** (0.04)
Round		-1.32** (0.42)
R-squared	0.24	0.25
# of Observations	639	639
# of Groups	71	71

** Denotes statistical significance at the $p < 0.05$ level
* Denotes statistical significance at the $p < 0.10$ level

Note: Cell entries provide marginal effects from a linear random effects regression on individual contribution levels.

Figure 3: Average Contribution Levels by Period

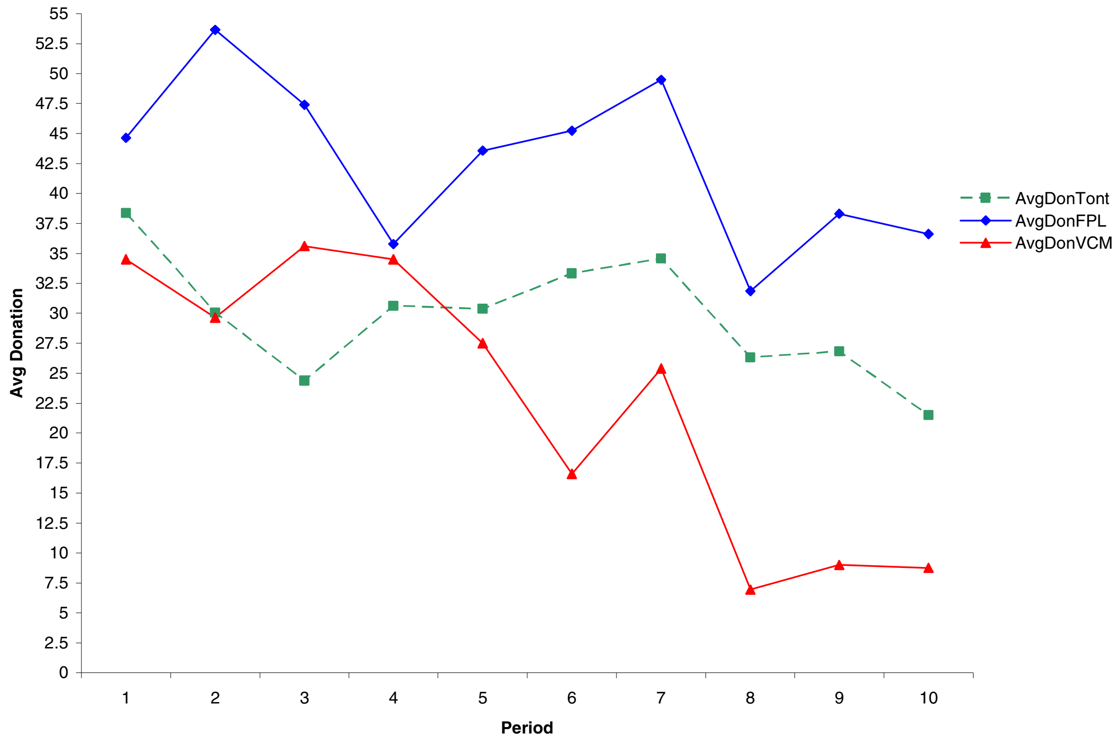
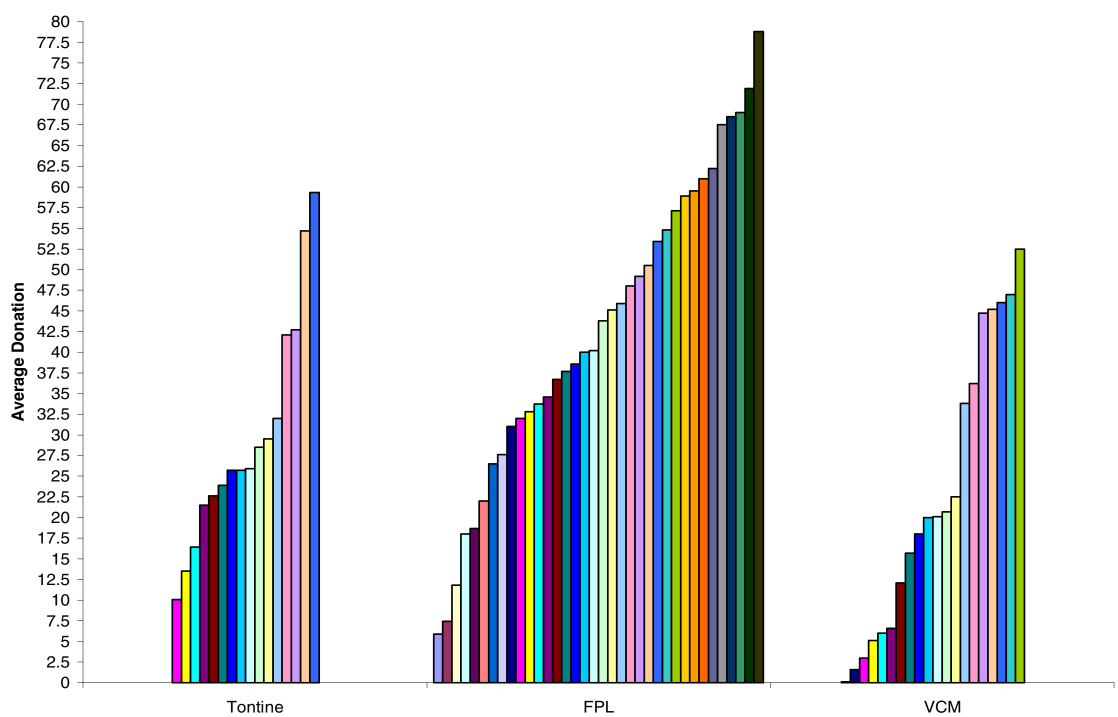
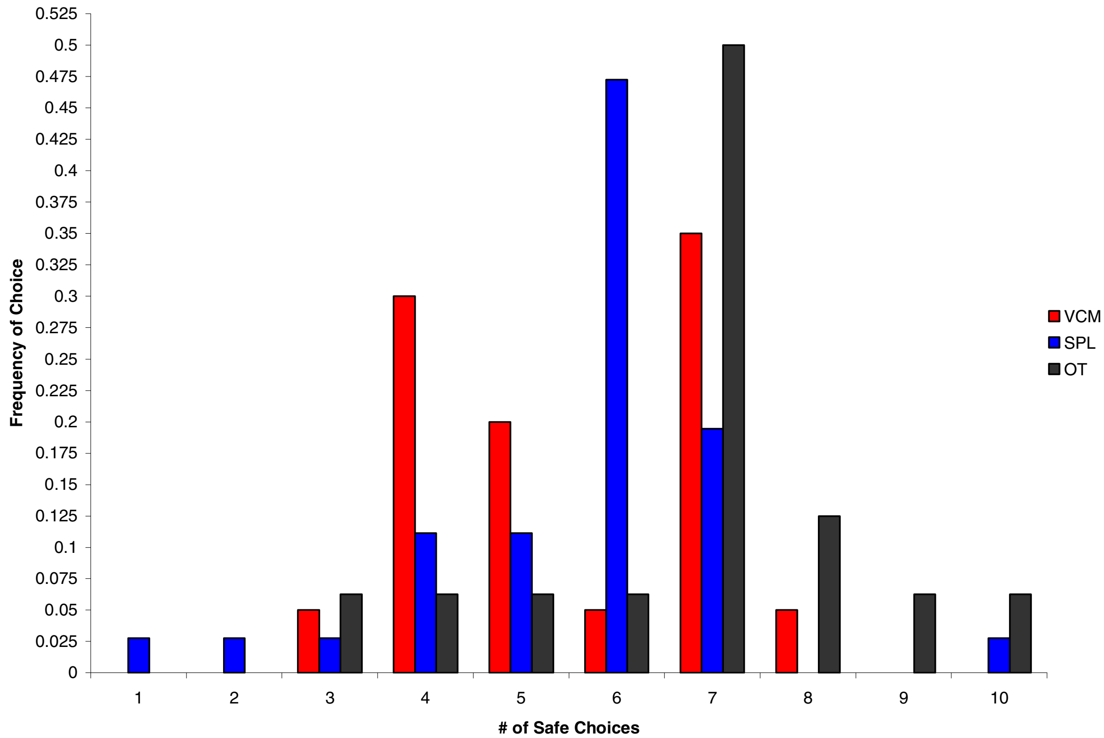


Figure 4: Average Contribution Levels by Individual



Note: The figures summarize average contribution levels by period and across individual for each of the experimental treatments.

Figure 5: Frequency Distribution of Risk Preference – by Treatment



Note: The figure provides the frequency of the number of safe choices (Option A) in the Holt/Laury experiment for agents in each of our three experimental treatments. An increase in the number of safe choices is associated with an increase in the implied level of risk preference.

Appendix A. Instructions—Part 1

General Rules

This is an experiment in economic decision making. If you follow the instructions carefully and make good decisions you can earn a considerable amount of money. You will be paid in private and in cash at the end of the session.

It is important that you do not talk, or in any way try to communicate, with other people during the session. If you have a question, raise your hand and a monitor will come over to where you are sitting and answer your question in private.

The experiment will consist of 12 rounds. The first 2 rounds will be practice. In each round, you will be randomly assigned to a group of 4 people. These groups will change each round. You will not know which of the other people in the room are in your group and the other people in the session will not know with whom they are grouped, in any round.

In each round, you will have the opportunity to earn points. At the end of the session, one of the non-practice rounds will be randomly selected and you will be paid in cash an amount that will be determined by the number of points you earn during the randomly selected round.

Description of each round

At the beginning of the first trial a subject number will be given on your terminal. Record that number on your record sheet. Each round you will be given an endowment of 100 tokens. At the beginning of each round, the computer will prompt you to enter the number of tokens you want to contribute to the group account. Enter a whole number between 0-100, record the number in column (b) on your record sheet, and click continue. Any tokens you do not place in your group account are placed in your private account. Once your decision is recorded, it cannot be changed. After everyone in your group has recorded their decisions, a screen will appear informing you of the number of tokens contributed to the group account by all group members, whether any bonus points have been earned, and your profit for the round. Record the information from that screen onto your record sheet as follows:

Tokens in Private Account:	Column A
Your Contribution to Group Account:	Column B
Total Tokens in Group Account:	Column C
Private Account Points:	Column D
Group Account Points:	Column E
Bonus Points:	Column F
Profit for Round:	Column G

Once everyone has recorded his or her information, the next round will begin.

How earnings are determined

VCM:

The number of points you earn in the round will be determined as follows. For each token placed in your private account you will earn 1 point. This amount is recorded in column (d) on your record sheet. You will receive 0.3 points for each token placed in your group account by you and the other people in your group. The group account points are recorded in column (e) on your record sheet. In addition, in each round you will also receive 24 bonus points regardless of how you and the other people in your group place your tokens. This amount is recorded in column (f). Your profit for the round is computed by summing the private account points, the group account points and the bonus points. This total is recorded in column (g) on the record sheet.

SPL:
The number of points you earn in the round will be determined as follows. For each token placed in your private account you will earn 1 point. This amount is recorded in column (d) on your record sheet. You will receive .3 points for each token placed in your group account by you and the other people in your group. The group account points are recorded in column (e) of the record sheet.

In addition, in each round you have the chance to win 80 bonus points. At the end of each round a lottery will be drawn. Your odds of winning the lottery are determined by how much you contributed to the group account in that round. Specifically, your chances of winning the bonus points will be equal to the number of tokens you place in the group account, divided by the total number of tokens placed in the group account by you and the other people in your group. For example, if you place 30 percent of the tokens into the group account, you will have a 30 percent chance of winning the bonus. If no tokens are placed in the group account, each member of the group will have an equal chance of winning the bonus. Record any bonus points earned in column (f) on your record sheet. Your profit for the round is computed by summing the private account points, the group account points and the bonus points. This total is recorded in column (g) on the record sheet.

OT:
The number of points you earn in the round will be determined as follows. For each token placed in your private account you will earn 1 point. This amount is recorded in column (d) on your record sheet. You will receive 0.3 points for each token placed in your group account by you and the other people in your group. The group account points are recorded in column (c) of the record sheet.

In addition, at the end of each round you will receive a share of 80 bonus points. Your share of the bonus points is determined by how much you contributed to the group account in that round. Specifically, your share of the bonus points will be equal to the number of tokens you place in the group account, divided by the total number of tokens placed in the group account by you and the other people in your group. For example, if your contribution is 50 percent of the total tokens placed in the group account, you will receive 50 percent of the bonus (40 points). If no tokens are placed in the group account, each member of the group will receive an equal share of the bonus. Record any bonus points earned in column (f) on your record sheet. Your profit for the round is computed by summing the private account points, the group account points, and the bonus points. This total is recorded in column (g) on the record sheet.

At the end of the session we will draw a ticket from the box. In the box there is a numbered ticket for each round played (1-10). The number on the ticket that is drawn will determine the round for which you will be paid. Record the selected round and then your profit for that round in the space provided at the bottom of the record sheet. You will receive $1.00 in cash at the end of the session for every 15 points you earn in that round. This amount is recorded in the space titled earnings.

Appendix B. Instructions—Part II (Risk Aversion Measures)

Record your subject number from the previous part on your decision sheet. Your decision sheet shows ten decisions listed on the left. Each decision is a paired choice between OPTION A and OPTION B. You will make ten choices and record these in the final column, but only one of them will be used in the end to determine your earnings. Before you start making your ten choices, please let me explain how these choices will affect your earnings for this part of the experiment.

We will use part of a deck of cards to determine payoffs; cards 2-10 and the Ace will represent "1". After you have made all of your choices, we will randomly select a card twice, once to select one of the ten decisions to be used, and a second time to determine what your payoff is for the option you chose, A or B, for the particular decision selected. (After the first card is selected, it will be put back in the pile, the deck will be reshuffled, and the second card will be drawn). Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used. Obviously, each decision has an equal chance of being used in the end.

Now, please look at Decision 1 at the top. OPTION A pays $8.00 if the Ace is selected, and it pays $6.40 if the card selected is 2-10. OPTION B yields $15.40 if the Ace is selected, and it pays $0.40 if the card selected is 2-10. The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, the cards will not be needed since each option pays the highest payoff for sure, so your choice here is between $8.00 or $15.40.

To summarize, you will make ten choices: for each decision row you will have to choose between OPTION A and OPTION B. You may choose A for some decision rows and B for other rows, and you may change your decisions and make them in any order. When you are finished, we will come to your desk and pick a card to determine which of the ten Decisions will be used. Then we will put the card back in the deck, shuffle, and select a card again to determine your money earnings for the OPTION you chose for that Decision. Earnings for this choice will be added to your previous earnings, and you will be paid all earnings in cash when we finish.

So now please look at the empty boxes on the right side of the record sheet. You will have to write a decision, A or B in each of these boxes, and then the card selection will determine which one is going to count. We will look at the decision that you made for the choice that counts, and circle it, before selecting a card again to determine your earnings for this part. Then you will write your earnings in the blank at the bottom of the page.

Are there any questions? Now you may begin making your choices. Please do not talk with anyone else while we are doing this; raise your hand if you have a question.

Decision Sheet

OPTION A	OPTION B	DECISION
1/10 of $8.00, 9/10 of $6.40	1/10 of $15.40, 9/10 of $0.40	
2/10 of $8.00, 8/10 of $6.40	2/10 of $15.40, 8/10 of $0.40	
3/10 of $8.00, 7/10 of $6.40	3/10 of $15.40, 7/10 of $0.40	
4/10 of $8.00, 6/10 of $6.40	4/10 of $15.40, 6/10 of $0.40	
5/10 of $8.00, 5/10 of $6.40	5/10 of $15.40, 5/10 of $0.40	
6/10 of $8.00, 4/10 of $6.40	6/10 of $15.40, 4/10 of $0.40	
7/10 of $8.00, 3/10 of $6.40	7/10 of $15.40, 3/10 of $0.40	
8/10 of $8.00, 2/10 of $6.40	8/10 of $15.40, 2/10 of $0.40	
9/10 of $8.00, 1/10 of $6.40	9/10 of $15.40, 1/10 of $0.40	
10/10 of $8.00, 0/10 of $6.40	10/10 of $15.40, 0/10 of $0.40	

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